43. P. Heelan, "Quantum and Classical Logic: Their Respective Roles," *Synthèse*, 21 (1970), pp. 2–33.
44. We are here, in effect, combining a possible world machinery with supervaluations; this was first done, to handle similar problems in tense logic, by R. H. Thomason, "Indeterminist Time and Truth-Value Gaps," presented at the APA (Western Division) annual meeting, May 1970.
45. Most of the relevant mathematics can be found in two places: Jauch, *Foundations of Quantum Mechanics*, ch. 11, and H. Everett III, "'Relative State' Formulation of Quantum Mechanics," *Reviews of Modern Physics*, 29 (1957), pp. 454–62. I am largely indebted to these two authors for the formal part of my approach; my interpretation seems to agree largely with Jauch, but to be in almost direct opposition to Everett's; see especially the latter's "'Relative State' Formulation," pp. 459–60, note added in proof.
46. Early in 1968, Professor R. H. Thomason suggested to me that the quantum-mechanical attributions of state ought perhaps to be explicated as modal statements, and early in 1969 Professor N. Grossman (University of Illinois at Chicago Circle, then my student at Indiana University) suggested the same idea independently. In both cases the suggestion concerned the relation between pure states and measurement outcomes as well as the relation between mixtures and pure states, and the objections I had concerned the former. I am much indebted to conversations with Professors Grossman and Thomason and with Nancy Delaney Cartwright and Professor C. Hooker, University of Western Ontario.

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On the Conceptual Structure of Quantum Mechanics

Thoughts without content are empty, intuitions without concepts are blind.

—Kant
Critique of Pure Reason

Of course, whatever we can do in the way of abstraction is for some purposes useful—provided that we know what we are about.

—Whitehead
Adventures of Ideas

Nature loves to hide.
—Heraclitus

I. Quantum mechanics commands philosophic interest for several reasons, among which probably the best known—although not necessarily therefore the best reason—is the persistence, after more than forty years, of serious unresolved questions of theoretical interpretation. That serious problems remain is not, however, a universally acknowledged proposition. Part of my intention here is to support it, and to suggest reasons for believing that the questions still unresolved are (in the long run) important *for physics*. (In my childhood the legend was current that only twelve men in the world understood Einstein's theory. Nowadays, relativity is quite tame; but I shall argue

I am deeply indebted to my friend Abner Shimony, under whose tutelage I learned quantum mechanics, and with whom, over a period of several years, I have discussed the philosophic issues of that theory with profit and pleasure.
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presently that *nobody* yet understands the quantum theory.) This is certainly not the view of the majority of physicists, who consider the critical issues of interpretation to have been settled in the late twenties and early thirties, chiefly through the analyses of Niels Bohr. At any rate, a weaker statement may pass unchallenged: namely, that as in the days when Newton's theory of gravity was regarded unfavorably by Huygens and Leibniz, controversy that deserves to be considered serious (even if possibly misguided) does continue.

As I have suggested, however, its (arguably) problematic character is not the only philosophic motive for attention to quantum mechanics. In its attempt to understand science as a human intellectual enterprise, philosophy has the obligation to consider science as it actually is: the essential data for the philosophy of science can only come from the history of science; including, of course, its current history, which it could not be the part of wisdom to ignore. But the point in the present case is really much stronger. Quantum mechanics represents an altogether extraordinary occurrence in intellectual history: not only because it has entailed a very deep-going change in the fundamental conceptual scheme of physics—a fact that is widely known; but also (as should be more widely appreciated than I think it is) because with this change it has brought the realization, to a very remarkable degree, of what has been the dream of the science of physics from the days of Newton.

Hence a third reason, and perhaps the best of all, for attributing philosophical importance to quantum mechanics: in realizing that ancient dream, and realizing it through a profound conceptual revolution, quantum mechanics has revealed deep and surprising things about the world of which we are part. Concern for such things belongs traditionally to philosophy; and in my view of philosophy, this concern is a proper and a central one. It is, of course, shared by scientists; but this ought not to be taken as objectionable: that scientists are not properly philosophers is a modern, and I think an unfortunate, doctrine. On the other hand, for philosophy, whose name does still evoke the names of Plato and Aristotle, of Descartes, Spinoza, Leibniz, and Kant—for philosophy itself to be construed as a professional specialty, with its own technical concerns (logical or methodological or linguistic), but divorced from serious interest in the veritable structure of the universe: this would, in my opinion at least, be not only unfortunate but perverse.

If my insistence upon the profundity and the richness of achievement of quantum mechanics seems to conflict with the claim that the theory lacks a satisfactory interpretation, the appearance rests upon a misunderstanding; and an important part of my purpose is to combat this misunderstanding. It seems to be widely believed among philosophers that there is in the conceptual structure of quantum mechanics something loose or muddled. Accounts of the theory that stress the "wave-particle duality"—or, more generally, Bohr's notion of "complementarity" (between waves and particles, between position and momentum, between "causal description" and "space-time description")—appear to have created an impression of quantum mechanics as shuffling incompatible systems of concepts, as it passes from one problem to the next, and escaping inconsistency only by some mysterious art of judicious selection. On the contrary, the *internal* coherence of the conceptual structure of quantum mechanics is truly marvelous: looseness occurs only in antiquated or semi-popular expositions; and muddle (I think) in *discussion* of the conceptual difficulties—the difficulties themselves, as I see them, have nothing to do with muddle.

The following discussion, then, has three broad aims: to exhibit with as much clarity as possible the conceptual structure of the theory; to indicate the character of the epoch-making achievement I have referred to; and to state, again as clearly as possible, the nature of the conceptual difficulties that remain and the case for regarding them as real and unsolved difficulties. In particular, I shall develop more fully the distinction I have made elsewhere[1] between the "interpretation of quantum mechanics" in an epistemological sense—which, I have argued, presents no difficulties not encountered throughout physics—and its interpretation in a full physical (or "metaphysical") sense, which does present quite peculiar difficulties. These three aims will not be reflected in a corresponding tripartite structure of the paper: by far the largest space will be devoted to the first purpose. It is obvious that a clear appreciation of conceptual difficulties should follow upon a clear grasp of conceptual structure; and we shall find that the achievement of quantum mechanics is reflected back into its own conceptual analysis in a most remarkable manner.

II. In my view, conceptual clarity is served by far the best if one gives, for the *internal* structure of a theory, as exact as possible a mathematical characterization: in problems of interpretation, it is a great help at least to have a sharp statement of what is to be interpreted. But long and completely abstract mathematical expositions are hard to follow and hard to hold in mind. There is a well-known remedy: to sketch, as one goes, a preliminary rough "intuitive" interpretation of the abstract structural notions—as in geometry, for instance, one attaches pictorial spatial images to the geometrical terms. We shall do something of that sort here. But this brings with it a danger, when interpretation is to become a serious issue: the danger of *assuming* later that features introduced in the preliminary sketch are well understood and soundly based. The matter is a little delicate. In the mathematical exposition, such interpretations as are offered cannot be subjected to rigorous criticism; one must suspend disbelief at this stage and embrace the interpretations sympathetically as mnemonic and heuristic aids. But one must also withhold belief and must prepare to subject the preliminary interpretations to ruthless examination when an adequate basis shall be available: an examination that has to consider whether those interpretations can in fact be sustained by the fully developed structure.

III. The mathematical structure of "classical" quantum mechanics—which is all that we shall be concerned with here—was essentially established by von Neumann in 1927. In expounding the structure, I shall follow (with minor variations) the account given by George Mackey in his published Harvard lecture notes.[2] In this order of exposition, one starts from a very general notion of *statistical theory of a physical system.* Informally, we are to think of "a system" as something that has a definitely specified physical constitution—definite composition and definite laws of evolution: for instance, a freely moving electron; a hydrogen atom (or, more properly, an electron and a proton, with the laws of their interaction fully specified, but their condition in other respects—for example, their total energy—not necessarily given); or—towards an opposite extreme—the solar system. (It may be objected that if, in the last case, we mean the actual solar system in its full concreteness, we cannot specify its composition and laws of evolution—for this would be to have solved, in principle, all the problems of geology and many of the problems of astrophysics. Nevertheless, precisely because the system is an actual

one, reference to "the constitution of the solar system" is to be regarded as *eo ipso* "specific": in other words, we presuppose that the problems of geophysics and astrophysics are, in principle, solvable. To this extent at least, quantum mechanics—contrary to a widely diffused impression—joins classical mechanics in assuming an objectively given physical reality, with objectively definite character independent of our state of knowledge.)

The constitution of a system must be supposed to entail for it a definite complement of *testable attributes*—each accessible to empirical determination, by the help (in general) of suitable physical apparatus, which we bring to interact with the system in order to evoke the manifestation of one or more of these attributes. A test of a single attribute is in effect a yes-or-no question put to the system; the answer will ordinarily be influenced, of course, not only by the constitution of the system, but by its more special actual conditions or "state." *We shall say that we have a determinate "statistical state" of the system, if we possess information that determines a probability for each possible attribute under all possible empirical tests.*

We do not assume prima facie, however, that to affirm or deny a given attribute of a given system at an arbitrary time is meaningful: it is only "under suitable conditions" that the attributes manifest themselves empirically, and it is only under such conditions that we postulate the meaningfulness of affirming or denying them; whether they can be extended in a natural way to all possible circumstances is a question we do not prejudge. Their status is in one way like that of "dispositional predicates": corresponding to each of them (we may suppose) there is a set of Carnapian "bilateral reduction sentences." But the application of such predicates, in a natural sense, to cases where the test conditions do not hold, depends upon empirical laws—and therefore upon propositions established by inductive reasoning: it makes sense to regard "being magnetic" as an honest property because (to oversimplify in the customary way) a thing that attracts iron once or a few times will always, at least for some time, do so. It is easy to think of situations or notions for which the analogous inductive inference is unavailable ("being lucky," for instance); and it proves to be unavailable in general for our quantum-mechanical attributes. It would therefore be misleading to think of the latter as "properties" of a system, in the ordinary logical sense of "property": on the domain of all possible conditions of the system, they are best conceived of as "partial," or "conditional," properties—functions into the set of truth values, defined on (possibly proper) subsets of that domain.[3]

But this account is already oversimplified and misleading. I have described the notion of "attribute"—and this (or a corresponding treatment of the notion of "measurable quantity") is quite customary—as meaning, essentially, "conditional, or partially defined, empirical predicate." But the latter conception, although of undoubted methodological value, is *extremely* difficult to employ in contexts demanding a high degree of logical sophistication and precision. This fact should be clear, from the extensive body of discussion of the language of theoretical science; however, since I am not sure that the seriousness of the difficulty is sufficiently appreciated, a brief digression to labor the point seems worthwhile. The chief problems are these: (1) that the condition laid down for a test is supposed to constitute an *observable sufficient condition* for the occurrence of an *observable criterion* of affirmation or denial; (2) that a (finite) disjunction of such conditional criteria is supposed to *exhaust the meaning* of the predicate so characterized (or its "present meaning," since the specification may be regarded as subject to later supplementation). These two suppositions are the two great stumbling blocks of operationalism: the first because it is truly a daunting task to write out, explicitly and precisely, a set of conditions sufficient to define an absolute experimental test for a property, free of all loopholes—even in the rather simple case of "magnetic," so often used as a prototype in these discussions, the formulations given in the philosophical literature are enormously far from meeting the requirement; and the second *a fortiori*, because if it is practically impossible to specify a single sufficient test, to describe *all* eligible tests (leaving no loopholes in any) must be practically impossible to at least the second power. Where high precision is not at issue, these problems of sufficiency and exhaustiveness need not be of much concern; but in theoretical physics, the introduction of "conditional empirical predicates" by reduction sentences is really out of the question—except perhaps as a Platonic myth: that is, an unattainable pattern, which can still do useful service in the analysis of concepts, but which may lead to obfuscation if that unattainability is not clearly recognized.

Of the two stumbling blocks, the second is critical here. In a preliminary view, the Platonic myth of attributes as testable under some well-defined experimental conditions is rather helpful; but it is fundamental to the structure of quantum mechanics that each of the attributes to which probabilities are assigned by statistical states is conceived as testable by *infinitely many essentially different kinds of experiment.* I regard it as a defect of the standard expositions that they do not make this explicit.

To signalize the peculiarities of what I have so far called "attributes," I want to introduce somewhat unconventional terminology. In probability theory, the subjects to which probabilities are ascribed are ordinarily referred to as "events." To suggest the "potential" character of our attributes—which are phantoms in limbo until they are called forth by suitable test procedures—I shall prefer to use the word "eventuality"; and shall say that *a statistical state assigns a probability to each eventuality of the system.* I shall speak of an experiment as "realizing" an eventuality if the experiment can be regarded as a test for that eventuality: thus the realization can prove affirmative or negative; and the probability is to be construed as *the conditional probability of affirmative realization, on the hypothesis that an experiment realizing the eventuality in question is performed.*

It is crucial in quantum mechanics that certain collections of eventualities are capable of being realized together in a single experiment, whereas other collections are not. Let us call two eventualities "corealizable" if a simultaneous test is possible. In quantum mechanics, corealizability of pairs of eventualities is not an equivalence relation: it is (obviously) reflexive and symmetric, but is not transitive. *This circumstance already shows that a single eventuality cannot be associated with a single type of experimental test.* For suppose that eventualities e_1 and e_2 are corealizable, and likewise e_2 and e_3, but that e_1 and e_3 are not so. In the terminology of Bohr, the last clause means that tests for e_1 and e_3 require "mutually exclusive experimental arrangements." If, then, A is an "experimental arrangement" realizing e_1 and e_2 together, and A' is such an arrangement realizing e_2 and e_3 together, it is clear that A and A' must differ in essential respects; and yet each counts as a realization of e_2.

On the other hand, quantum mechanics does suppose that corealizability of sets of eventualities is determined by corealizability of pairs: that is, that a set of eventualities is jointly realizable if and only if each pair included in it is so. In the light of what we have just considered, this postulate cannot lay claim to very great intrinsic plausibility; for if, in the example we have discussed, e_1 and e_3 were also corealizable, say through the experimental arrangement

A'', we could perfectly well imagine the experimental arrangements A, A', A'', to be mutually exclusive in pairs; and we could imagine the same to hold of any other experimental arrangements realizing pairs of our three eventualities: in other words, there is no difficulty in conceiving three eventualities to be corealizable in pairs, but not corealizable all together. The supposition here made by quantum mechanics is recommended, not by its qualitative plausibility, but by the fact that it follows from a (very much stronger) postulate about the structure of the set of eventualities—a postulate which is itself absolutely central to the theory and all its successes, and recommended just by *that* fact.

With all this in the background, let us proceed to the mathematical theory—bearing in mind not only that, as I have already warned, we must not take it for granted that the theory will bear the interpretation thus informally sketched for it in advance; but also that the validity of the theory does not necessarily depend upon this informal interpretation: in other words, that we have to be prepared to refine or revise the interpretation in the light of our discussion of the theory.

IV. First, then, we suppose given a set $\mathcal{E}$ whose elements we call "eventualities." In formulating postulates for the structure of this set, it will be convenient to assume as a basic or primitive notion a relation, to be called "disjointness," between pairs of eventualities. In terms of our preliminary interpretation, that two eventualities are disjoint is to be thought of as meaning that they are corealizable, but can never both be affirmed. Our first structural postulate is:

1) *Disjointness is a symmetric relation.*

We shall call an eventuality "impossible," or "absurd," if it is disjoint from every eventuality. (Note that we do not merely say "disjoint from those eventualities with which it is corealizable": an absurd eventuality is required to be corealizable with all others. Quantum mechanics in fact identifies the "impossible" or "contradictory" outcomes of all possible experiments.) We call a collection of eventualities "exhaustive" if every eventuality disjoint from all the members of the collection is absurd. We call a collection "disjoint" if all pairs of distinct members are disjoint. We can now formulate our second postulate:

2) *For every countable* (i.e., finite or countably infinite) *disjoint set of eventualities, S, there is a unique eventuality $n(S)$* ("the conjoint negation of S"), *which is disjoint from each member of S, and such that the set formed by adjoining the element $n(S)$ to the set S is exhaustive.*

Informally, we think of the eventualities in such a set S as jointly realizable in one experiment; and we think of $n(S)$ as corresponding to the outcome: "all members of S are negated." But although a joint realization of the members of S is thus of itself a realization of $n(S)$, the converse is by no means presumed to be the case: the possibility is admitted, in principle, of realizations of $n(S)$ which do not realize *any* of the members of S. (This is another instance, and a striking one, of the fundamentally non-"operational" character of quantum-mechanical concepts: the theoretical identification, as "realizations of the same eventuality," of outcomes of altogether different experiments.)

For any single eventuality e, the "conjoint negation" of the set whose only member is e is called simply "the negation of e" and is denoted by "e'." It is clear that in an experiment jointly realizing the members of S, and thus realizing $n(S)$, the eventuality $(n(S))'$ is affirmed if and only if one of the members of S is affirmed. This informal reflection leads us to define $d(S)$, "the disjunction of the countable disjoint set S," as the negation of $n(S)$: $d(S) = (n(S))'$. (The definition does not, of course, depend upon the informal considerations leading to it. That the definition makes sense is guaranteed by postulate (2), and by the proposition—an immediate consequence of our definition of "disjointness"—that a set containing just one eventuality is always disjoint.) Since the empty set is clearly disjoint, we may form its disjunction, which we call **0**, as well as its conjoint negation, which we call **1**. It is easy to see that **0** is the unique absurd eventuality, and **1** the unique eventuality whose unit set is exhaustive (that is, whose negation is absurd).

Our previous remark about realizations of $n(S)$ can obviously be put in terms of $d(S)$, since every realization of either of these eventualities is a realization of the other: our informal interpretation admits in principle that an experiment might permit us to affirm the disjunction of a collection of eventualities and yet not permit us to affirm any member of the set. Now, in quantum mechanics, as it turns out, this (so far merely abstract) possibility is in fact exemplified, and very strongly. First of all, for quantum mechanics, if S is any countable disjoint set of eventualities containing more than one nonabsurd member, there exists an eventuality e that is corealizable with $d(S)$ (in the *formal* sense of "corealizability" to be explained below), but not with any of the members of S different from 0. In our interpretation, this should mean that there is an experimental arrangement which, realizing e and $d(S)$, tests the disjunction of the members of S, although it not only fails to test any of those members individually, but is actually incompatible with any experimental arrangement that could test those members. Second, there are particular cases of this sort in quantum mechanics for which *actual* experimental tests of the type described are *feasible in practice*, and in which the asserted incompatibility is crucial to the theory.

Our third structural postulate, which completes what is needed for the general part of the theory (the part that is common to classical and quantum mechanics), restricts to some extent this possibility that eventualities are corealizable with a disjunction but not with its terms. The postulate imposes a stronger logical bond than we have had so far between eventualities and their disjunction; one of its consequences is that, although an eventuality may be corealizable with a disjunction and yet fail to be corealizable with its terms, no eventuality can be *disjoint* from a disjunction without being corealizable with its terms. The full postulate is this:

3) *For any eventuality, e, and any countable disjoint set of eventualities, S: e is disjoint from d(S) if and only if it is disjoint from each member of S.*

The structure defined by these postulates[4] is often referred to in the literature as the "logic" of the system to which the eventualities pertain. This use of the word "logic" seems quite natural, in view of the formal analogy of the structure to that of the algebra of classical logic, and the analogy of our informal interpretative notions of "affirmation" and "negation" of eventualities to the corresponding classical logical notions. But one must beware of reading too much into the word: we shall see that quantum-mechanical *discourse* is in fact carried on in a *classical* logical framework; and although there are difficulties of interpretation, it has certainly not been shown that these might be avoided by the adoption of a nonclassical framework for such discourse. In fact, it has not been shown how one could employ such a framework as a logic of discourse at all. The first step

would be the extension of the structure beyond mere propositional logic to a suitable analogue of quantification theory; and I am not aware that any attempt has been made to do this—much less to incorporate mathematics proper (for example, in the form of a suitable version of set theory). One argument that has been made for attempting such a program is that the structure of & corresponds more faithfully to the exigencies of empirical observation, and therefore gives promise of providing a more adequate framework for empirical discourse, than two-valued Boolean logic.[5] This argument would seem to lose much of its force in the light of remarks we have already made. There is no doubt that the structure of & does have a significant correspondence to the relations among possible experiments; and this is of the highest importance for quantum theory, because that theory tells us some critical, and novel, things about the relations in question. To say this is hardly to say more than that the structure & is part of the conceptual theoretical apparatus of quantum mechanics. But when an issue is made of *empirical* logic, the implied criterion is adequacy to the standards of empirical method. If, with this criterion in mind, we contemplate the gulf between the concept of "disjunction" we have just been examining, on the one hand, and, on the other hand, the honest empiricist maxim of the mathematical constructivists—that to affirm a disjunction is to claim that one is in a position to affirm one of the disjuncts—then, I think, "quantum logic" will appear to be in flagrant delict.

The formal analogy to classical logic merits some explication. A Boolean algebra is called "σ-complete" if countable disjunctions are defined—that is, if every countable set of elements has a least upper bound in the Boolean order structure. Now, every σ-complete Boolean algebra satisfies our three postulates, provided one defines "a is disjoint from b" to mean that the intersection of a and b is the zero element. Such a logic has the following special property: for every finite subset S of &, there is a unique finite disjoint $S_1 \subset$ &, such that (1) each element of S is the disjunction of some subset of S_1, and (2) if T is a subset of & satisfying the analogue of (1) (with T substituted for S_1), then each element of S_1 is the disjunction of a subset of T. Conversely, any logic—that is, any structure satisfying our three postulates—for which, in addition, this special property holds, is a σ-complete Boolean algebra. (The Boolean structure is defined from our logical structure in an obvious way: the Boolean disjunction of the arbitrary finite set S above is the disjunction in our sense of the associated disjoint set S_1; the treatment of countably infinite disjunctions is a little trickier, but not difficult.) Turning to the general case, we may exploit these relationships as the basis for a formal definition of "corealizability": we say that a set of eventualities, S, is *corealizable*, if there exists a set T, containing **0** and **1** as elements and including S as a subset, such that, with the structure induced from $\mathcal{E}$ (i.e., with the same relation of disjointness), T is a σ-complete Boolean algebra. (These technical details will not be needed for our subsequent general discussion; but one point is worth noting strongly: *the logic of corealizable sets of eventualities is Boolean logic.*)

V. We come next to the notion of a statistical state. Each such state is to assign probabilities to eventualities; we have therefore to specify, in formal terms, what such a probability assignment shall mean. Our heuristic informal notion is that the number assigned to each eventuality as its probability is the *conditional* probability that that eventuality will be affirmed *if it is in any way realized.* Now, in any realization—still pursuing the informal train of considerations—we conceive some set of corealizable eventualities to be in fact realized. These have among themselves logical relationships—and, as we have seen (or rather, in our formal treatment, just stipulated), Boolean logical relationships. The probabilities assigned to the eventualities of such a set ought therefore to satisfy, with respect to the logic of the set, the conditions that are imposed by the classical theory of probability; for the reasons that justify the imposition of those conditions in the classical context apply fully here. If we then treat arbitrary sets of "corealizable" eventualities in the sense of our formal definition as—or "as if they were"—*in fact* corealizable, we are led to the following definition:

A PROBABILITY MEASURE *on the set* $\mathcal{E}$ *is a function from that set to the real unit interval which, on every subset of* $\mathcal{E}$ *having the structure of a* σ*-complete Boolean algebra, satisfies the conditions of the classical theory of probability.*

It is very easy to see that this definition is equivalent to the following:

A probability measure on $\mathcal{E}$ *is a function* p *from* $\mathcal{E}$ *to the real unit interval such that* $p(\mathbf{1}) = 1$ *and such that, for any countable disjoint subset* S *of* $\mathcal{E}$, $p(d(S)) = \sum_{e \in S} p(e)$.

Adopting this definition, we shall, in accordance with previous remarks, henceforth use the expressions "statistical state of the system" and "probability measure on the set of eventualities" as synonymous.

VI. One more notion of quite fundamental importance finds its natural place in this general context: the notion of a (presumably) measurable quantity, or dynamical variable, or—a most unfortunate and misleading term, which I shall nevertheless use in deference to the prevailing custom—an *observable.*

The motivating idea for the definition of observables is this: to measure a measurable quantity is to obtain the answers to an array of yes-or-no questions—namely, questions about the value of that quantity. As prototype of such questions we may take: "Does the value belong to a certain (specified) interval of real numbers?" Within the general conceptual scheme so far developed, this means that we have to suppose our quantity q to associate with each (bounded or unbounded) real-number interval I an eventuality $q(I)$. For measurement to be possible, all the eventualities assumed as values by this function q must of course be corealizable; Boolean logical operations are therefore performable upon them, and we must obviously require compatibility between the logical relationships of the $q(I)$'s and the analogous relationships of the intervals they come from. This leads to the stipulation that *q assign an eventuality to each member of the Boolean algebra of sets generated by the intervals*; that *the eventualities assigned in this way themselves constitute a Boolean-algebraic substructure of the logical structure of* $\mathcal{E}$; and that *the mapping q be a homomorphism of the one Boolean algebra into the other.* But further reflection shows that something more is necessary. The requirement that q be a Boolean homomorphism guarantees that the image of the empty set is **0**, the absurd eventuality—that is, that the measured value cannot be a member of the empty set!—and that $q(\mathbf{R})$, the image of the whole real line, is **1** (the "tautologous" or "necessary" eventuality, the negation of **0**); and this is as it should be. But if we partition the real numbers into, say, the intervals of unit length terminated by integers, then the conditions we have postulated thus far do not (for instance) rule out the possibility that q assigns the eventuality **0** to each of them. This anomaly can occur, despite the fact that $q(\mathbf{R}) = \mathbf{1}$, because the crucial "logical" relation of **R** to the set of intervals in question—namely, that **R** is their union—is not a finite Boolean relation. We therefore need a stronger postulate that will take account of some "infinite" operations. The following stipulation clearly suffices to take care of the case we have just considered, and is also clearly "justified," from the point of view of our informal interpretation, in its general application: *If* $S_1, S_2, \ldots$, *is an "increasing" sequence of sets* (i.e., with each included in the following one), *all of them belonging to our Boolean algebra, and if the "limit," S, of this sequence* (i.e., the union of all the sets of the sequence), *belongs to it as well, then any eventuality disjoint from each* $q(S_n)$ *is disjoint from* $q(S)$.

Now suppose that we have a function q satisfying the conditions laid down by the italicized passages of the preceding paragraph. The eventualities taken as values by q are (formally) corealizable. If we assume (unrealistically) that this means that an experiment is practically feasible in which they are all realized, then it is clear that any such experiment can be regarded as an *infinitely precise measurement* of a *physical quantity*: for as a result of the experiment we shall have, for each open, closed, or half-closed interval of real numbers, a yes or a no to the question "Is the value in that interval?"—and all these answers will be logically compatible with one another and with the hypothesis that there is a unique value.[6] Approaching a little closer to what might be regarded as reasonable, we may take the formal corealizability to mean that an actual experimental realization is ("in principle") possible of any subset of the range of q that corresponds to a partition of the real line into intervals of nonzero length. This would amount to the assertion of the possibility—in principle—of an *approximate* measurement, to any desired degree of approximation (short of absolute precision); and this possibility would be quite enough to justify us in taking q to define a quantity subject to measurement, hence an "observable."

For the sake of the mathematical theory, it proves desirable to strengthen our conditions still further, by requiring the collection of subsets of the real line for which "questions" are defined—a collection that includes, by stipulation, all the intervals—to be closed not only under the finite Boolean operations, but also under countable

unions (hence under the operation referred to in our last italicized stipulation—which, however, did not require this operation to be performable within our collection, but only stated a requirement conditional upon that performability). When this new requirement is imposed, the sets for which q is assumed to be defined are what are called the "Borel sets": the sets belonging to the smallest σ-complete Boolean algebra containing the intervals. Now, Borel sets can interlace very intricately with their complements, and to assume that it is "empirically meaningful" to ask whether the value of a quantity lies in an arbitrary Borel set (example: Is the value a rational number?) is really quite far-fetched. But the new requirement can be justified, for the special cases of classical and quantum mechanics, by a theorem: namely, that in these two cases—that is, for the particular structures of & that these theories posit—any mapping q with the properties we had previously demanded has a unique extension satisfying the new requirement as well. Thus our new condition neither rules out anything previously allowed nor introduces distinctions where previously there were none: it can be legitimately regarded, for classical and quantum mechanics, as purely conventional, introduced for technical convenience only.[7] I do not know whether an analogous proposition holds for the general case (the proofs for classical and quantum mechanics are very different: each exploits the special structure involved, in a way that does not admit of obvious generalization). If, however, waiving scruples, we adopt the new condition as a requirement for observables in our general theory, it becomes possible to give the following simpler definition:

An OBSERVABLE *is a function, q, from the collection of all Borel sets of real numbers to the set* &, *such that:*

1) *if* B_1 *and* B_2 *are disjoint sets, then* $q(B_1)$ *and* $q(B_2)$ *are disjoint eventualities* ("q preserves disjointness");
2) *if B is the union of a countable disjoint collection of Borel sets, and if C is the image of that collection under the mapping q, then* $q(B) = d(C)$ ("q transforms countable disjoint union to countable disjoint disjunction");
3) $q(\mathbf{R}) = \mathbf{1}$ ("q is normalized").

The analogy to the ordinary notion of a measure is obvious; and indeed any function with properties (1) and (2) is called an "&-valued measure" on the real line. We shall call a function satisfying all three conditions of this definition—whether or not the set & in question is the "set of eventualities" of a physical system (but always assuming $\mathcal{E}$ to have the formal structure we have defined)—a *normalized $\mathcal{E}$-valued measure* on the real line.

VII. The structure so far described—the set of eventualities, $\mathcal{E}$, with the basic relation of disjointness, satisfying our three postulates; and the associated notions of statistical state as probability measure on $\mathcal{E}$ and observable as normalized $\mathcal{E}$-valued measure on the real line—is common to classical and quantum mechanics. Our conditions on the structure of $\mathcal{E}$ leave open a great multitude of more specific possibilities; and it is in this further specification of $\mathcal{E}$ that the characteristic difference of the two theories appears.

Classical mechanics assumes as given, for a system of given constitution, the notion of a precise state of that system: namely, the notion of an instantaneous configuration and state of motion, or what has come to be called a "phase"; and the set of all such states, the "phase space," carries a rather elaborate intrinsic structure (essentially the structure of the cotangent bundle of a differentiable manifold). Questions about the system are, then, presumed to be questions about the *state* of the system, and to be translatable into the form: "Is the system in a phase belonging to a specified subset of the phase space?" Eventualities, therefore, simply correspond to subsets of the phase space; and it is clear that disjointness of eventualities must correspond to disjointness of the respective sets. It is not perfectly evident which subsets should be taken to represent eventualities. Our postulates for $\mathcal{E}$ will be satisfied if we choose for it the set of *all* subsets of the phase space; but this would not fit well with our requirement that statistical states assign probabilities to every eventuality—it is well known that most "natural" measures cannot be extended to all the subsets of a manifold. In a fashion analogous to that exemplified in our discussion of the notion of observable, a combination of considerations based upon the "empirical interpretation" of the theory, and requirements suggested by the mathematical theory of probability, leads to the Borel subsets of the phase space—the sets belonging to the smallest σ-complete Boolean algebra containing all open sets—as our best choice for the eventualities of the system. This identification of eventualities with the Borel subsets of the phase space (and of disjointness with disjointness of sets) has the following fundamental consequences:[8]

1. The "logic" of the classical eventualities is Boolean propositional logic; that is, the classical $\mathcal{E}$ "is," in the sense defined in section IV above, a σ-complete Boolean algebra.
2. The notion of a statistical state reduces to the ordinary classical notion of a probability measure on the phase space—that is, a probability distribution over the possible "exact states" of the system.
3. The observables can be regarded as just *functions on the phase space* (more precisely, real-valued Borel functions): that is, an observable can be regarded simply as a quantity—subject to certain technical constraints loosely related to the qualification "measurable"—that has a definite value in every "exact state."

In quantum mechanics, none of these three fundamental principles is true. The "logic" of eventualities is *radically different* from the structure of a Boolean algebra; the statistical states *cannot* be regarded as probability measures on a state space; and the observables *cannot* be construed as functions on such a space. This is all pretty well known; but one circumstance has tended—despite the absolute clarity of the theoretical situation—to induce a certain confusion in its public discussion: for notwithstanding the facts just recited, there does occur in quantum mechanics a notion of state and state space, different from the general notion of statistical state that we have so far considered, which plays a role in the theory analogous in important ways to the role of the classical phase space. How (from the point of view we have adopted) this notion arises—and in what ways it is, in what ways it is not, like the classical phase space—we shall consider presently.

VIII. In order to characterize the structure that quantum mechanics postulates for $\mathcal{E}$, we require the mathematical concept of what (following von Neumann) is called "Hilbert space."[9] The concept itself is quite elementary (although the theory it leads to goes rather deep); a review of the definition is perhaps in order: (1) *A Hilbert space is* (in the first place) *a vector space*—that is, its elements ("vectors") can be added and subtracted and can be multiplied by the numbers of a certain number field ("the field of scalars"), the result of the operation being in each case a vector; under addition (and subtraction) the vectors form a commutative group; multiplication of scalars and multiplication of vector by scalar are (together) associative; multiplication of vector by scalar is distributive both over addition of vectors and over addition of scalars; multiplication by the unit of the field is the identity operation. Further, the field of scalars is not arbitrary; and while usage varies on this point, for our purposes it is as well to make the specific stipulation that *the field of scalars for a Hilbert space is the field of the complex numbers.* (2) *A Hilbert space is endowed with a positive definite Hermitian inner product*—that is, a function assigning to each pair of vectors, **x** and **y**, a scalar, which we designate simply by (**x**,**y**), in such a way that: (i) the function is *linear* in its first argument—i.e., $(a\mathbf{x} + b\mathbf{y}, \mathbf{z}) = a(\mathbf{x},\mathbf{z}) + b(\mathbf{y},\mathbf{z})$ (where a and b are scalars; **x**, **y**, and **z**, vectors); (ii) the function is *Hermitian-symmetric*—i.e., (**x**,**y**) and (**y**,**x**) are complex conjugates (their sum is real, their difference imaginary); (iii) the function is *positive definite*—i.e., for every nonzero vector **x**, its "inner square" (**x**,**x**) is a strictly positive real number. On the basis of this much structure, one can introduce the basic notions of *orthogonality* and of the *length* of a vector: $\mathbf{x} \perp \mathbf{y}$ means that $(\mathbf{x},\mathbf{y}) = 0$; the square of the length of **x** is (**x**,**x**). The postulated algebraic properties ensure that these two notions are related by the analogue of the *Pythagorean theorem*: the square of the length of the sum of mutually orthogonal vectors is the sum of the squares of their lengths. Further, with the notion of length we have that of "distance": the distance between two vectors is just the length of their difference; and the properties postulated ensure that this notion satisfies the general requirements upon a distance-function (principally the so-called "triangle inequality"), so that our vector space is in a natural way a *metric space.* This prepares the way for our last requirement: (3) *A Hilbert space, in its natural metric, is a complete metric space*—every sequence of vectors that is eventually contained in an arbitrarily small sphere (or: such that, given any positive radius, there is a sphere of that radius containing all but a finite number of its terms) converges to a limit vector.

The technical notions, connected with the structure of a Hilbert space, that we shall have to refer to, are the following:

A. The notion of a *closed linear subspace*: A closed linear subspace is any nonempty subset of the whole space closed under (1) the linear operations—addition, and multiplication by scalars; and (2) the operation of passing to the limit (in the sense of the metric) of a convergent sequence of vectors. (The closed linear subspaces are thus the subsets that are themselves Hilbert spaces under the operations "induced" from the main space.) Special case: for any nonzero vector **x**, the set of all scalar multiples of **x** is a closed linear subspace: the one-dimensional subspace, or "ray," generated (or "spanned") by **x**.

B. The notion of *dimension*: The dimension of a Hilbert space is the cardinal number of a maximal set of mutually orthogonal nonzero vectors. *Any such set "spans" the space*: that is, the whole space is the smallest closed linear subspace containing all the vectors of the set. It is easy to show that the dimension by itself constitutes a full set of invariants for Hilbert spaces: for any cardinal number, there is a Hilbert space having that cardinal number as its dimension, and—up to isomorphism—there is only one (any two Hilbert spaces of the same dimension "look the same"). *The Hilbert space that quantum mechanics is concerned with is of countably infinite dimension* (so-called "*separable* infinite-dimensional Hilbert space"—still more explicitly, "separable infinite-dimensional complex Hilbert space"); it is thus a precisely defined, fully specific structure.[10]

C. *The relation of orthogonality between closed linear subspaces*: Two closed linear subspaces are said to be orthogonal if every vector in the one subspace is orthogonal to every vector in the other.

D. The notion of a *linear operator* on a Hilbert space: A linear operator is a function, L, whose arguments are vectors and whose values are vectors, such that $L(a\mathbf{x} + b\mathbf{y}) = aL(\mathbf{x}) + bL(\mathbf{y})$ (where a and b are scalars, **x** and **y** vectors). It is not, however, implied that the domain of definition of the mapping L is necessarily the entire space. A full explication of the considerations that are important here would take us too far into the substantive mathematical theory of Hilbert space; the following elaboration of special cases will suffice for our later purposes. (i) A *unitary operator* is a linear operator which constitutes an "automorphism" of the structure of the space: an operator defined on the whole space, having the whole space as image, and taking each vector to a vector of equal length (from which it follows that all inner products are preserved—that is, that $(L(\mathbf{x}),L(\mathbf{y})) = (\mathbf{x},\mathbf{y})$). (ii) A *self-adjoint operator* is a linear operator, whose domain is not necessarily the whole space, which satisfies the following condition: for any given vectors **y** and **z**, we have that $(L(\mathbf{x}),\mathbf{y}) = (\mathbf{x},\mathbf{z})$ for every **x** in the domain of L if and only if **y** is in the domain of L and $\mathbf{z} = L(\mathbf{y})$. Unitary operators are obviously continuous (in the natural metric of the Hilbert space). Self-adjoint operators need not be continuous; and one can show that a self-adjoint operator is in fact continuous if, and only if, it is everywhere defined. Discontinuous linear operators are also (and more usually) called "unbounded" (and continuous ones "bounded"—the connotation is that, for such an operator, the lengths of the images of vectors of unit length are bounded). The domain of definition of an unbounded self-adjoint operator is never a closed subspace: it is a linear subspace *topologically dense* in the whole space (i.e., although—as already remarked—not every vector belongs to the domain in question, every vector can be approximated as closely as one likes by a vector in that domain). One special class of (bounded) self-adjoint operators is of quite particular importance for us: (ii*a*) If M is any closed linear subspace, each vector **x** of the Hilbert space can be represented in a unique way as the sum, $\mathbf{x}_1 + \mathbf{x}_2$, of a vector $\mathbf{x}_1$ in the subspace M and a vector $\mathbf{x}_2$ orthogonal to that subspace (i.e., to every vector in it). The mapping $E_{\mathbf{M}}$ that takes **x** to $\mathbf{x}_1$—the *orthogonal projection on* M—is linear, continuous, and self-adjoint; and furthermore it is *idempotent*—that is, $E_{\mathbf{M}}E_{\mathbf{M}} = E_{\mathbf{M}}$ (where the "multiplication," as always henceforth for operators, is to be understood as the composition of mappings). Conversely: if E is any idempotent self-adjoint linear operator, and if $\mathbf{M}_E$ is the image of the entire Hilbert space under the mapping E, then $\mathbf{M}_E$ is a closed linear subspace, and E is the orthogonal projection on $\mathbf{M}_E$ (in particular, any such E is necessarily bounded). It follows that we have a one-to-one correspondence between the set of all closed linear subspaces and the set of all idempotent self-adjoint linear operators—or *projection operators* (or, shorter still, *projections*), as we shall call them from now on. It is obvious that the zero operator, **0**, which maps every vector to the zero vector, corresponds to the zero-dimensional subspace (which consists of the zero vector alone); and that the identity operator, **1**, which maps every vector to itself, corresponds to the whole space. We call two projections (mutually) *orthogonal* if the subspaces they correspond to are orthogonal. In terms of the projections themselves, this reduces to the very simple algebraic condition: *E_1 and E_2 are orthogonal if and only if $E_1E_2 = \mathbf{0}$* (it is the case—despite the noncommutativity of operator multiplication in general—that, for projections, $E_1E_2 = \mathbf{0}$ if and only if $E_2E_1 = \mathbf{0}$).

IX. It is an elementary exercise in the theory of Hilbert space to show that the set of all closed linear subspaces (or, equivalently, the set of all projection operators), with the relation of orthogonality, satisfies the three postulates we have assumed for the set of eventualities of a physical system, with the relation of disjointness. It is a fundamental assumption of quantum mechanics—made explicit by von Neumann, in his path-breaking and profound study of the mathematical structure of the theory[11]—that *the "logic of eventualities" of a physical system is isomorphic to the system of all the closed linear subspaces of a separable infinite-dimensional Hilbert space, with the relation of disjointness corresponding to the relation of orthogonality.*

On this assumption, we must ask, what follows about the statistical states and the observables? These questions lead to rather deep—eventually, to very deep—inquiries into the structure of Hilbert space. To take the observables first: our definition of this notion says—in the new context—that *an observable is a normalized projection-valued measure on the real line.* But there is a most fundamental theorem, called the "spectral theorem for self-adjoint operators," which associates such projection-valued measures one-to-one with the self-adjoint linear operators on the Hilbert space.[12] It follows that *the observables stand, in a natural way, in one-to-one correspondence with the self-adjoint operators.* It further turns out that this correspondence preserves the algebraic (and analytic) relationships among observables: (1) Observables q_1 and q_2 are *compatible* (i.e., all questions about q_1 and all questions about q_2 form, together, a corealizable set of eventualities) if and only if the associated operators Q_1 and Q_2 *commute*—that is, if and only if $Q_1Q_2 = Q_2Q_1$;[13] and arbitrary sets of observables—finite or infinite—are compatible if and only if they are compatible in pairs (hence if and only if they commute in pairs). (2) For compatible observables, it clearly makes sense—in terms, that is, of our (tentative) informal interpretation—to speak of sums and products; and if we start from the "logically natural" definition of sums and products (applicable in our *general* logical framework), it can be proved in the special quantum-mechanical case that the sum of two observables is represented by the sum of their corresponding operators, the product of two observables by the product of their operators.[14] (3) It is similarly possible, in a natural way, to discuss "limiting operations" on systems of compatible observables—for example, the summing of infinite series; and these, too, have natural analogues in the calculus of operators; so that a very wide class of functions—including, for instance, square roots, logarithms, exponentials, trigonometric functions, etc.—applicable to generate quantities from other quantities, apply in the very same way to generate the corresponding operators from the corresponding other operators.

Turning now to the statistical states, what we require is some information about the possible probability measures on the set of closed subspaces, or of projections, of Hilbert space. In his analysis of the foundations of quantum mechanics, von Neumann, in 1927, defined a class of such probability measures.[15] These are easy to describe: If **x** is any (fixed) vector of unit length in Hilbert space, then—this is an almost immediate consequence of the Pythagorean theorem—the assignment to each projection, E, of the square of the length of the projected vector, $E\mathbf{x}$, is a probability measure. We have thus a mapping that associates with each unit vector a statistical state; let us (provisionally) refer to these as "vector states." Now, it is easy to see that if one takes an arbitrary countable collection of probability measures, and if, assigning to each of them a nonnegative "weight" in such a way that the sum of the weights is unity, one forms their "weighted sum" (or "weighted average"; but the best generic term for such an operation is "countable convex combination"), then the result of this process will be again a probability measure. The statistical states considered by von Neumann were just the vector states and their countable convex combinations; and von Neumann—and, independently, Weyl[16]—remarked that, within this class of states, the vector states are distinguished by the fact that they cannot be obtained as convex combinations of states distinct from themselves. Following Weyl, it has become customary to call the vector states "pure states," or "pure cases," and the others "mixed states," or "mixtures."

One ought to ask whether the vector to which a given pure state is associated is itself unique—that is, whether the mapping of vectors to states is one-to-one; and if not, to just what extent it is ambiguous. These questions are very easily resolved: the association is not unique, for it is obvious that two different unit vectors that lie in the same one-dimensional subspace (ray)—each being a scalar multiple of the other, with the factor a complex number of absolute value one—define the same probability measure; and on the other hand, this is the precise extent of the ambiguity: two unit vectors in different rays obviously do not define the same probability measure. (Indeed: on

the first point, if $\mathbf{y} = a\mathbf{x}$, where a has absolute value one, then $E\mathbf{y}$—namely, $aE\mathbf{x}$—has the same length as $E\mathbf{x}$; and on the second, if **x** and **y** lie in different rays, and if E is the projection on the ray in which **x** lies, then the square of the length of $E\mathbf{x}$ is one, and the square of the length of $E\mathbf{y}$ is smaller than one.) *The pure states are, therefore, in natural one-to-one correspondence, not with the vectors of our Hilbert space, but with its rays.*

The next question one ought to ask is whether the states we have so far defined are the only ones there are. Now, von Neumann—still in 1927—did give an argument to prove that the pure cases and mixtures are the only possible statistical states for quantum mechanics; and in his book of 1932, this argument underlies his celebrated "proof" of the impossibility of reducing quantum mechanics to a nonstatistical theory.[17] But von Neumann's argument is unquestionably invalid; that is to say, the mathematical argument he gives, although itself correct, establishes another theorem than the one required:[18] that argument neither shows nor attempts to show that the probability measures formed by countable convex combination of pure states are the only ones that exist. Nevertheless, this last proposition is true: the question was raised explicitly in 1957 by Mackey, and answered in the same year by A. M. Gleason, who proved—by a most intricate argument, the heart of which is a *tour de force* of elementary geometry of the sphere in three-dimensional Euclidean space—that *for a finite- or countably-infinite-dimensional Hilbert space of at least three dimensions, the probability measures of von Neumann are the only ones there are.*[19]

To summarize, then, the results of the present section: we see that von Neumann's postulate—the identification of the logic of eventualities with the structure of the projections of a Hilbert space—leads, through a quite deep mathematical analysis, to rather sharp results about both the observables and the statistical states: identifying the former (even in their algebraic and more general functional relationships) with the self-adjoint operators, and characterizing the latter as the pure states—which correspond one-to-one with the rays of the Hilbert space—and the mixtures, obtainable as countable convex combinations of pure states.

X. Nothing has been said so far about the evolution of a physical system with time—that is, about *dynamics*; this omission must now be made good. The basic postulate of quantum dynamics is the postulate of statistical determinism: that *for every statistical state, S, and every time interval, t, there is a well-determined S_t, the state to which S evolves in time t.* In the spirit of our tentative interpretation, a few comments can be made about the informal content and plausibility of this assumption (and of its further specifications, as they are added below). To "have" a characterization of a system as in a definite statistical state is, we conceive, to "possess information" adequate to determine an assignment of probabilities to all the eventualities of the system. Suppose that we have such a characterization—the state S—for our system at a certain time, and that we ask about the same system after a lapse of time t (t being a positive interval). Let e be any eventuality that we might choose to realize for the system at this later time. Then the following description can be given of an experiment upon the system *at the earlier time*—that is, in the state S: "Wait for the time interval t; then perform the procedures of an experimental realization of the eventuality e." It is clear that this describes a realization of e at the later time; but if we recognize that every experiment takes time, and if we impose no a priori restrictions upon the length of time allowable, then it also describes what may quite reasonably be considered as the realization of a certain eventuality e' "at the earlier time."[20] In this case, the postulate of statistical determinism can be justified by pointing out that the probability of e being affirmed after the time-lapse t, supposing e then realized, is nothing but the probability assigned by the state S to the realization of e', supposing e' realized. This would show not only that S_t is indeed determined, given S and t, but further—and it is a most important point for the theory—that to know *how* S_t is determined (to know, that is, the *dynamical law*: the function assigning to S and t the value S_t) is the same as to know the law associating e' to e and t. As instructive as these considerations may be, however, there is an immediate serious rub. For quantum dynamics (as it exists in fact) requires us to make the determinate assignment of S_t (to S and t) *also for the case where the time-interval t is negative*; and one does not see—I, at least, do not see—any possibility of an analogous "justification" for this. (It should be noted that this assumption of "statistical determinism of the past"—which Mackey not quite appropriately calls "reversibility"[21]—can be expressed equivalently as the requirement that the *forward* mapping, that of S to S_t for any *positive* interval t, is always one-to-one, and onto the set of all the states.)

Taking the postulate in this strong sense, then, it is certainly clear from the meaning we attach to the notion of dynamical evolution that, for fixed S, the assignment of S_t to t must have the character of a *group action*: i.e., that if t is the zero interval then $S_t = S$, and that (for all t and t') $S_{t+t'} = (S_t)_{t'}$. A further supplementary requirement, although falling short, perhaps, of the same degree of inevitability, is also very reasonable: the requirement that dynamical evolution be *continuous in time*, in the sense that, for any fixed initial state S and eventuality e, the probability assigned to e by S_t is a continuous function of t.

We need one more supplementary stipulation, which can be most clearly motivated by assuming the standpoint of the "statistical frequency" interpretation of probability. Suppose we have a state S that is a convex combination of states S_n, each with its corresponding weight w_n (all nonnegative, and summing to unity). Then, from the "frequency" point of view, the state S can be (approximately) "prepared" as follows: Take an enormous collection of systems in each of the states S_n. Then extract from each collection a certain number of specimens, taking care to make the proportions of these numbers among themselves (approximately) the same as the proportions of the corresponding weights. (This of course cannot, in general, be done exactly.) Now define a new collection to consist of just those systems one has so chosen. From the point of view of the frequency theory of probability, this new collection (or "collective") *constitutes* (approximately) the "statistical state S." But even from a more critical—or at least more cautious—point of view, it can be said that *from the sole information, about a given physical system, that it belongs to the "new collection" just described, one would be justified in ascribing to the eventualities of the system those probabilities that constitute the statistical state S.* Now, if we think of the new collection—that is, all of its members—as evolving in time in accordance with the dynamical law of the system, it is clear that after time t we shall have a collection of systems, a fraction w_n of which "are in" the statistical state $(S_n)_t$; hence, a collection which, in the same sense as above, "constitutes" or represents the statistical state formed by convex combination of the states $(S_n)_t$ with weights w_n. The postulate these considerations suggest, then, is the following—which we formally adopt: *If the state S is formed by convex combination of states S_n with respective weights w_n, then S_t is the state formed by convex combination of the states $(S_n)_t$ with the same respective weights.*

These dynamical postulates fit the context of our previous *general* discussion: they are independent of the postulate of von Neumann, which characterizes the quantum-mechanical logic of eventualities; and they hold in fact for classical mechanics (as they had better, if our "plausibility" arguments are to carry any sort of conviction!). But when the dynamical assumptions are combined with von Neumann's postulate, new consequences of fundamental importance result:

1) *Dynamical evolution always takes pure states to pure states; and the evolution of a mixture is determined by the evolution of the pure states that compose it.*

This gives the sense in which the set of pure states plays a really basic role in the physical theory: every state can be expressed in terms of pure states, and when the dynamical law of the pure states is known, that for all other states is thereby determined; moreover, the dynamical law of the pure states does not require any reference to states other than pure ones. It is, then, *as underlying the dynamics* that *the space of pure states* (which, it should be remembered, is the space of rays—the "projective space"—associated with a Hilbert space) *is a close quantum-mechanical analogue of the phase space of classical mechanics.* In its relation to observable properties, measurable quantities, and statistical states in general, the quantum-mechanical space of pure states stands *altogether differently* from the classical phase space.[22]

2) *The dynamical evolution of the pure states of a system can be reduced to the action of a one-parameter group of unitary operators, acting upon the Hilbert space of the system.*

This means the following: With each time-interval t there is associated a unitary operator, U_t, on the Hilbert space. The association satisfies the condition: $U_{t+t'} = U_t U_{t'}$ (it is this condition—which, it should be noted, implies that the zero interval goes to the identity operator—that makes of the association what is called a "one-parameter group"). The dynamical evolution—pure state S to pure state S_t in time t—is derived from this one-parameter group acting upon the *vectors* of the Hilbert space in a pretty obvious way: if $\mathbf{x}$ is any vector in the ray corresponding to the pure state S, then S_t is the pure state corresponding to the ray that contains the vector $U_t\mathbf{x}$. This proposition, with its (otherwise by no means obvious) corollary that dynamical action—which by its definition affects the *rays* of our Hilbert space—can be represented in a systematic and simple way by an action upon the *vectors*,[23] explains (partly) why the vector space, that is, the Hilbert space itself, rather than its ray space, comes to occupy the center of the stage for the physicists in the technical elaboration of the quantum theory. And this proposition leads, in its turn, through a fundamental theorem due to M. H. Stone (1930), to the following:

3) *The dynamics of a quantum system is fully determined by one particular self-adjoint operator on the Hilbert space of the system, the "infinitesimal generator" of the dynamical group.*

But according to an earlier conclusion, a self-adjoint operator means an observable. The operator in question here is called the "Hamiltonian operator" of the system; the associated observable is called "energy"; and the relation expressing how the Hamiltonian operator determines the evolution of states is known as the (time-dependent) Schrödinger equation.[24]

XI. We have now reached the point at which the serious philosophical questions about the interpretation of quantum mechanics may be said to *begin.* The immediate most urgent question is how, on the basis thus far constructed,—how on earth, one might even say—we are to obtain a grip on any possible interpretation at all: in other words, how our "eventualities" and our "observables," hitherto inhabiting the pure ethereal realm reserved for the hypothetical entities of mathematics, are to be coaxed into the grosser empirical air and endowed with "empirical content."[25]

It is in respect of this general problem that the deepest mathematical considerations enter the theory. These are the considerations of symmetry, introduced into quantum mechanics by Eugene Wigner and Hermann Weyl.[26] It was Weyl who conceived the program of *characterizing* the fundamental dynamical variables of quantum mechanics with the aid of symmetry principles.[27] In its subsequent development, the program has deviated in significant respects from the path indicated by Weyl: the essential mathematical theory, that of infinite-dimensional group representations, did not yet exist in the 1920s, and the results obtained by that theory have placed some of the relationships in a new light. The results have also substantiated, not only the fundamental soundness of Weyl's program, but the altogether remarkable power and fruitfulness of the principles he introduced.[28]

What one does, in the order of development we are now considering, is to specify defining conditions for a system of a certain type, to be called a "system of n interacting particles": one postulates the existence, for such a system, *first* of a collection of eventualities corresponding to all the possible "questions about the position" of each particle, and *second* of a representation of the group of symmetries of Euclidean three-dimensional space—or of Galilean space-time—acting upon the states and having the appropriate effects upon the positions. The eventualities of position are assumed to form a corealizable set, and to correspond, one-to-one, to the n-tuples of Borel subsets of Euclidean three-dimensional space (where each component of an n-tuple is associated with one particle, and represents the question: "Is the particle located in this Borel subset of physical space?"). A symmetry of Euclidean space can be thought of as a possible way of displacing and tilting the camera used to photograph the system; the action of such a symmetry, g, upon a state, S, is supposed to produce a state gS which, if photographed by the displaced and tilted camera, will "look the same" as S photographed by the original camera. The assumption that this action has "the appropriate effects" upon the position eventualities means that, for any Borel subset B of physical space, and any state S, and any Euclidean symmetry g, the probability assigned by the transformed state gS to the eventuality associated with the transformed set gB—applied, say, to the kth particle of the system—is the same as that assigned by S to the eventuality correspondingly associated with the set B. This requirement of Euclidean symmetry is then to be strengthened by allowing—to put it roughly (but not incorrectly)—the several particles to be photographed each by a different camera, and allowing these cameras to be displaced and tilted independently. In proceeding to the Galilean group—which contains the Euclidean group as a subgroup—we introduce, in the first place, transformations of the velocity; this amounts to assuming that our camera (now taken as single) can, as it were, be put on roller skates. As to the "translations in time," which (added to the preceding operations) complete the Galilean group, we identify these, in their action upon the states, with the transformations of the dynamical group. It is not difficult

to see that requiring this action to be compatible with the structure of the Galilean group as a whole is just the assumption that *the laws of dynamics are invariant under Euclidean symmetries and under transformations of velocity* (the "principle of Galilean relativity").

Two things should be especially noted about these assumptions (or this *definition*). First, from the formal point of view, it is indeed just a definition that we have to deal with; and therefore we have not left the realm of the abstractly mathematical. (On the other hand, of course, the definition is motivated by reflections about very basic properties of space and time; and we shall see that the abstract systems to which we are eventually led in this way have properties resembling in such important respects the properties of physical systems, that this resemblance itself is enough to define their empirical interpretation.) Second, although we have borrowed from classical mechanics the notion that particles have "position questions," as well as the various assumptions of symmetry or relativity (assumptions that involve the ideas of displacement and velocity *of the whole system*), we have *not* borrowed the notion that a particle "has a position" at each (or at any) time. We have assumed nothing about the states of the system, in their relation to the position eventualities, except what our general theory requires: that each state assigns to each eventuality a probability. We cannot, therefore, for example, use the assumptions we have made to introduce, in something like the classical way, such elementary and classically indispensable concepts as the velocity or momentum of a particle. Although we have "position questions," and (in this sense) "possible positions," we have nothing at all like "possible trajectories" (we have—at least prima facie—no "questions about trajectories"); indeed, there is not so far in our repertoire of theoretical themes and devices any obvious way of expressing the temporal course of our system in terms of physical space (or of configuration space—which is just the set of possible arrangements of n points in physical space).

The derivation of the mathematical consequences of our symmetry assumptions is an exceedingly intricate affair. Some general points about it are simple enough, and important enough, to be worth making here. (1) When we postulate that a group of symmetries "acts upon the states," the object upon which this action has primarily to be conceived as taking place is not the Hilbert space of our system, but the associated projective space or "ray space"—since this is the "space of pure states." In the technical language of the subject, we are concerned primarily with what are called "projective representations." This introduces, not only appreciable technical complications, but quite essential technical possibilities: some, but *not* all, projective representations can be reduced to ordinary representations on the underlying vector space; and some of those that cannot be so reduced play a crucial role in the theory. (2) The action of our group operators upon the ray space has to preserve the relation of orthogonality between rays.[29] Now, every *one-parameter* group of such transformations on the space of rays (i.e., every representation, by such transformations, of the additive group of the real numbers) *can* be reduced to a representation on the Hilbert space—and, what is more, to a representation by unitary operators. The groups we are concerned with—belonging as they do to the class called "Lie groups" (of dimension greater than zero)—are, in an important sense, "full of one-parameter subgroups"; and these can therefore be considered as acting on the Hilbert space, even when the parent group cannot. But a one-parameter group of unitary transformations on a Hilbert space always has, by Stone's theorem, an infinitesimal generator: just as the dynamical action led us to a special observable, the energy, so *each one-parameter subgroup of the postulated group of symmetries leads to an associated observable*—the observable whose corresponding self-adjoint operator is the infinitesimal generator of the one-parameter group action concerned.[30] (3) Adding the assumption that a particular one-parameter group of symmetries is invariant under the dynamics turns out to imply a very special behavior of the associated observable in the course of time: namely, that for any eventuality e belonging to that observable, and for any state S, the probability assigned to e by the state S_t is independent of the time t—a situation that one describes by saying that the observable in question is a *conserved quantity*.[31]

Let us now pass in review the principal conclusions that result from the intricate analysis I have mentioned:

A. For the special case of a one-particle system, the postulate of Euclidean symmetry leads to the determination of three observables for each direction, ξ, in physical space (it being understood that a point has been fixed, once for all, as "spatial origin"). The first of these observables is the *position coordinate in the direction* ξ; this—with its corresponding self-adjoint operator Q_ξ—is determined (in an obvious way) by the postulated eventualities of position. The other two are the observables associated with the one-parameter groups of *translations in the direction* ξ and *rotations about the* ξ*-axis through the origin*; the corresponding self-adjoint operators, the infinitesimal generators of these respective groups, are denoted by P_ξ and J_ξ; the observables themselves are called (anticipating later results) the ξ-components of *linear momentum* and *angular momentum* respectively.[32] The position coordinates and linear momentum components satisfy a system of relationships of fundamental importance, known as the *Heisenberg commutation relations*:[33]

1) For every pair of directions in space, ξ and η, $Q_\xi Q_\eta - Q_\eta Q_\xi = 0$ and $P_\xi P_\eta - P_\eta P_\xi = 0$.
2) For every pair of mutually perpendicular directions in space, ξ and η, $P_\xi Q_\eta - Q_\eta P_\xi = 0$.
3) For every direction in space, ξ, $i(P_\xi Q_\xi - Q_\xi P_\xi) = 1$ (where i is the imaginary unit).

From these relations, it follows that the spectra—roughly speaking, the systems of possible values—of the positions and linear momenta all consist of the entire real line. The angular momenta, on the other hand, have *purely discrete* spectra, consisting exclusively of whole-number multiples of 1/2 (*absolute quantization of angular momentum*).

B. Under the same assumptions, the Hilbert space of the system has a certain natural (and instructive) representation;[34] namely, a representation as what is called the "tensor product" of two Hilbert spaces, $\mathcal{H}_1$ and $\mathcal{H}_2$, each having a special relation to the observables we have been discussing. The vectors of the space $\mathcal{H}_1$ are represented by *complex-valued functions on physical space, whose absolute squares are integrable over all of space*; the vector-space operations in $\mathcal{H}_1$ are represented by the corresponding standard operations (of addition, and multiplication by a scalar) on these functions; and the square of the length of a vector is given by the integral of the absolute square of the corresponding function. In the simplest case the space $\mathcal{H}_2$ is one-dimensional, and the tensor product therefore reduces to $\mathcal{H}_1$, whose vectors—or, rather, representative functions[35]—are called "wave functions." Let the direction ξ be that of the x-axis; then the operators Q_ξ, P_ξ, J_ξ, respectively take the wave function ψ to $x\psi$, $-i\frac{\partial \psi}{\partial x}$, $i\left(z\frac{\partial \psi}{\partial y} - y\frac{\partial \psi}{\partial z}\right)$.[36] In the general case, $\mathcal{H}_2$ supports a *projective representation of the group of rotations of physical space about the origin*; and this leads, for each direction ξ in space, to a self-adjoint operator S_ξ on $\mathcal{H}_2$. The position and linear momentum operators remain exactly as before: they "act only upon the first factor" of the tensor product, and by the same multiplication and differentiation operations specified above. But the angular momentum now presents two parts: the differential operator already given, which acts upon $\mathcal{H}_1$, and which in the more general context is denoted by L_ξ and called the ξ-component of *orbital angular momentum*; and the operator S_ξ, acting upon $\mathcal{H}_2$, whose corresponding observable is called the "intrinsic angular momentum" or *spin* of the particle. The angular momentum operator J_ξ is then the sum of the operators L_ξ and S_ξ. The spectrum of each orbital angular momentum component consists of all the integers (positive, negative, and zero). As to the spin, nothing more can be inferred from our general assumptions (except, of course, that spin has the same general characteristics as angular momentum; and, in particular, that the spectra of all the spin components are the same[37]). But the case of greatest interest, both because of its role in the general theory and because it is the only case that is encountered in reality for one-particle systems, is that in which the representation of the rotation group on the rays of $\mathcal{H}_2$ is "irreducible." *We shall now adopt the auxiliary hypothesis that every particle has "irreducible spin."* It follows that the space $\mathcal{H}_2$ has finite dimension n ($\geqslant 1$), and the spectrum of the spin components consists of all numbers from $-(n-1)/2$ through $(n-1)/2$, in steps of $+1$ (so that the numbers in the spectrum are either all integers or all halves of odd integers).[38] The number $(n-1)/2$—the maximum possible value of a spin component—is referred to as "the spin of the particle." Our first case, then (namely, the case in which $\mathcal{H}_2$ is one-dimensional), is that of a spin-zero particle; these, we have seen, are completely described by wave functions. In the general case, by choosing a set of base vectors in the finite-dimensional "spin space" $\mathcal{H}_2$, we can represent the vectors of our Hilbert space as n-tuples of vectors of $\mathcal{H}_1$; so a particle of spin s is described by a "$(2s+1)$-component wave function"—and we have a very complete and explicit account of all the position, momentum, and orbital and intrinsic angular momentum operators for such a particle.

C. For an n-particle system, our assumption of "independent Euclidean symmetry" leads to the representation of the Hilbert space of the system as a tensor product of n Hilbert spaces, associated one-to-one with the particles (i.e., with the independent component representations of the Euclidean group); and each of these n spaces has the structure we have just discussed for the space of a single particle. We have thus, for each direction in physical space, n corresponding position coordinates, n components of linear momentum, n components of orbital angular momentum, and n components of spin (namely, one of each of these for each of the n particles). The position and linear momentum operators again satisfy the Heisenberg commutation relations—with the supplementary conditions saying that any two operators corresponding to quantities for two different particles commute with one another. When we combine all the Hilbert spaces $\mathcal{H}_1$ of all our particles on the one hand and all the spaces $\mathcal{H}_2$ on the other hand, the latter tensor product is still a finite-dimensional Hilbert space (the "total spin space" of the system), and the former tensor product is naturally represented as the space of square-integrable functions on a Euclidean space of $3n$ dimensions—in point of fact, as the space of *wave functions on the ("classical") configuration space*. Position, linear momentum, and orbital angular momentum operators on this space are entirely analogous to those in the case of a single particle. Finally, when we consider the action of the Euclidean group on the system as a whole (instead of independently on the several particles), we derive, for each direction in space, the component of *total linear momentum*, the component of *total orbital angular momentum*, and the component of *total spin*, in that direction. Each of these is the sum of the corresponding components for all of the n particles; and the sum of the component of total orbital angular momentum and that of total spin in a given direction is the component, in that direction, of *total angular momentum*.

D. Passing now from the consideration—which has so far exclusively occupied us—of the relationships of states and observables *at a single instant*, to the consideration of *changes with time*, we first examine the consequences of assuming that the action of the Euclidean group (on the system as a whole—*not* independently on the particles) is invariant under the dynamics of the system. What follows is simply the conservation of each of the quantities associated with a one-parameter subgroup of this group action: i.e., we have that *each component of total linear momentum and each component of total angular momentum is a conserved quantity.* (It should be noted that the energy of the system, as the quantity associated with the dynamical group itself, is ipso facto conserved.)

E. Far more follows from the full postulate of Galilean invariance (which includes in itself all that has gone before). For a one-particle system, the form of the Hamiltonian operator is completely determined by this postulate: in the representation of the Hilbert space that we have already discussed, that operator acts only upon the factor $\mathcal{H}_1$, and (up to the inherently arbitrary additive constant[39]) it is given by $(P_\xi^2 + P_\eta^2 + P_\zeta^2)/k$, where ξ, η, ζ, are any three mutually perpendicular directions in space, and k is a parameter associated with the particular projective representation of the Galilean group that occurs (the parameter k can have any nonzero real value—although further considerations lead to the restriction, or rather to the possibility of the convention, that k is to be assumed to be positive). Calculating, from this dynamical operator, the rates of change with time of the probabilities assigned by any state to the eventualities of position (as that state evolves in time), one finds that they are just what they ought to be if the observables whose operators are $2P_\xi/k$, $2P_\eta/k$, $2P_\zeta/k$, are the respective components of the *velocity* of the particle. We are therefore led to make this identification, and accordingly also to identify the parameter $m = k/2$ as the *mass of the particle*. The operator given above as the Hamiltonian of our one-particle system we shall call the *kinetic energy operator*.

F. The n-particle case is considerably more complicated. It is necessary, for strong conclusions, to introduce auxiliary assumptions concerning the relation of the velocity components to other observables and to the action of the Galilean group. It will suffice for us to confine our attention to the simplest case, in which all the particles have spin zero and in which there are no "velocity-dependent forces." This case is obtained by postulating—besides spin zero—that the operators corresponding to the velocity components of each particle are just the ones already described for a one-particle system. The Hamiltonian operator of such a system then turns out to be the sum of two terms: the first of these is completely determined, and is just the sum of the kinetic energy operators of all the particles (*total kinetic energy of the system*); the second term is a function of the position observables of the particles, restricted only by the condition of invariance under the action of the Euclidean group—that is, it is a function of the "internal configuration" of the system (*potential energy of the system*). The properties of a system of this type are therefore completely determined when one knows the mass of each particle and the potential-energy function of the system. In the more general case—allowing nonzero spins, and velocity-dependent forces—the possibilities open for the Hamiltonian are wider: roughly speaking, the spins introduce a matrix of functions, in place of a single function, for the potential energy; and the velocity dependence complicates the connection of velocity with momentum, introducing a new system of functions (or, in the nonzero-spin case, system of matrices of functions) into that relationship. This occasions a change in the kinetic energy operator, which does continue to be half the sum of the products of the masses into the squares of the velocities, but does not continue to be half the sum of the quotients of the squares of the momenta by the masses. With these modifications, the Hamiltonian is still the sum of the kinetic energy and potential energy operators.

XII. The results reviewed in the preceding lengthy section are highly technical, and the arguments needed to establish them constitute a rather formidable piece of mathematics. The results are of great technical importance. It should be emphasized that these results do *not* require that formidable mathematical derivation for their own subsistence: they were attained by other heuristic routes—perhaps conceptually less pure, but of easier access (at least to the well-trained physicist)—long before the mathematical theory we have reviewed was created; and once attained, their justification as principles of an empirical science depends far more upon their consequences than it depends upon their antecedents. Moreover, once they are established, their further consequences—in particular, such conclusions as we may succeed in drawing from them about the interpretation of quantum mechanics—are of course also established (no matter again what the prior heuristic considerations may have been). If, therefore, the present sketch of an epistemological analysis of the theory (to be completed—as a sketch—in sections XIV–XVIII below) is correct, it would be a misinterpretation and a mistake to conclude that symmetry principles provide *the* basis for, or "the key to," satisfactory understanding of quantum mechanics. The legitimate conclusion would rather be that those principles provide, on one line of approach to the theory, *a clue* to its satisfactory interpretation. This more modest claim seems to me sufficient, in view of the remarkable character of the mathematical connections themselves, to justify bringing these technicalities to the attention of a philosophical audience.

At the beginning of this paper, however, I have ascribed to quantum mechanics three sorts of philosophic interest, and the preceding remark concerns only the first of these. But the reasoning we have considered is, in my view, of more especial import for the third (or "metaphysical") interest—and, as a consequence, also to a certain extent for the second (that is, for what may be called general epistemology or methodology).

I have said that the results of that reasoning are of great technical importance. From the point of view of the analogy between quantum mechanics and classical mechanics, which dominated the heuristics of the actual development of the theory, some of the technical features (the concept of spin; the absolute quantization of angular momentum; the mere possibility of a discontinuous energy spectrum) are flatly irreconcilable with classical mechanics; and the empirical evidence bearing upon these features constituted, in the 1910s and 1920s, that "anomalous" or "critical" situation which signalized the need for a radical change in the theory. On the other hand, some of the features correspond to long-established characteristics of classical mechanics—but characteristics which, although fundamental to the latter theory, appear rather arbitrary there; more precisely, appear there as so many independent postulates. For instance, classical mechanics offers no clue to why the differential equations of motion should be of the second order. Galilean invariance indeed implies that these equations must contain no first order terms (and so must be of at least the second order); but that they are precisely of the second order—i.e., that acceleration is the dynamical effect *par excellence*—has to be posited expressly. The discovery of this postulate, or its disengagement from the special results of his predecessors, was one of Newton's great achievements, and in the form of the "second law of motion" it stands at the head of Newtonian mechanics. But given this principle, with Galilean symmetry, one cannot deduce the conservation of linear and angular momentum (roughly, the "third law of motion"); and given these in addition, one cannot deduce the conservation of energy. In short, that the equations of motion are of the type that can be expressed in the form of the "canonical equations" of Hamilton is a proposition involving a quite material set of further restrictions, within the framework of Newtonian mechanics.

In quantum mechanics, the analysis we have reviewed in the pre-

ceding section derives all these features, with the help of the symmetry principles, from just one postulate of a comparably "arbitrary" sort: namely, von Neumann's postulate that the structure of eventualities is the same as the structure of the closed linear subspaces of a Hilbert space. So, from the point of view here taken, this postulate—specifying, according to our tentative interpretation, the logical structure of the collection of all possible experimental tests on a physical system—deserves to be regarded as *the fundamental discovery* of the quantum theory: almost everything of a general character in the theory follows from this postulate, with the help of auxiliary assumptions of a *qualitative* kind. Moreover, the quantum-mechanical relationships can plausibly claim to "explain" the occurrence of those otherwise arbitrary features of classical mechanics. Indeed, only such classical systems as have these features can be obtained as "limiting cases," in an appropriate sense, of quantum systems; and it is only as (approximately) such a limiting case that a classical system, according to quantum mechanics, can exist at all.[40]

XIII. I characterize the point of view of the foregoing remarks—a point of view that contemplates the analysis of physical principles, and their representation as derivable from those among them that can be considered most fundamental—as "meta-physical" (with or without a hyphen). Analysis of that sort seems to me a very important philosophic function. It also seems to me that such analysis must always be taken as speculative—not only in the etymological and traditional sense, "concerned with (intellectual) *seeing* or θεωρία," but also in the modern colloquial sense: tentative and even risky. In the present case, we know that it is not really Galilean invariance but Lorentz invariance—whose implications involve in part still unsurmounted mathematical difficulties—that ought to be taken as characterizing nature. And what is worse, we know that if general relativity is correct, Lorentz symmetry can only be regarded as holding of the *infinitesimal* structure of the world; but how to employ this idea in the framework of the quantum theory remains altogether dark. (It is in reflection upon the occurrence of problems of this order; upon the methodological character of "explanations" like the one just sketched; and upon the import of distinctions like that of "arbitrary vs. plausible or qualitative" assumptions, or "more vs.

less fundamental" principles; that I see a lesson—mainly a cautionary one—for general epistemology or methodology, in the developments we have been discussing.)

But I want to turn, now (still deferring direct consideration of the theory's interpretation), to the consequences I had in mind in my opening statement about the achievement of the theory—that it has brought the realization, to a very remarkable degree, of what has been the dream of physics since the days of Newton. The further assumptions required to obtain these consequences are strikingly few. First it is necessary to make a *fundamental correction* of the results I have already reported: those results do stand; but they apply only to systems of particles *of different kinds*—and the restriction is in effect quite drastic, since the total number of "kinds" of particles, in the sense here required, is not large. When some of the particles in a system are of like kind, the previous result has to be modified by restricting the functions constituting the Hilbert space of the system either to such as are symmetric, or to such as are antisymmetric, in the position coordinates of the like particles (separately for each "kind" of particle in the system). Passing over technical discussion of this point, let me just make three remarks about it: (1) The requirement is closely related to the Leibnizian "principle of the identity of indiscernibles." (2) The choice between symmetry and antisymmetry —which makes a big difference—is *empirically* found to be determined by the character of the spin of the particle concerned: when the spin is an integer, the proper requirement is symmetry; when the spin is half an odd integer, the proper requirement is antisymmetry. (This empirical connection in its turn receives a "fundamental" derivation, or "explanation," at the hands of the "relativistic" theory—i.e., when the Galilean group is replaced by the inhomogeneous Lorentz group.) (3) In the special—and, for the theory of atomic structure, crucial—case of the electron, the correct requirement is antisymmetry; this leads to the famous "Pauli exclusion principle," which was first formulated within the framework of the older quantum theory, and which underlies the theoretical derivation of the chemical periodic table of Mendeleev.

With this correction made, what remains, in order to gain the promised treasure, is a suitable specification of the undetermined functions entering into the Hamiltonian operator. A small matter indeed, one may think: for this corresponds precisely, if transposed into the framework of classical physics, to the whole program of Newton: *to discover the laws governing all natural forces, and to deduce the laws of all natural phenomena from these laws of force and from an analysis of the phenomena as manifestations of the motions of particles*. This program, stated with perfect clarity and explicitness by Newton,[41] dominated the main tradition of classical physics. But the fundamental interparticle forces actually discovered by classical physics were in point of fact inadequate—it is surprising how little this is known—to account for *any example whatever* of a stable structure of more than one particle.[42] The quite fantastic advance made here by quantum mechanics lies in the fact that, within this new framework, *a Hamiltonian operator that expresses nothing but the quantum-mechanical analogue of the Coulomb force of electrostatics is already sufficient to account* (not only for the occurrence of some stable structures, but) *for the actual structures of atoms and molecules, for the principal qualitative facts of chemistry, and for the existence of ordinary solid bodies.*[43] Thus the new conceptual scheme, to which physics was driven (in a way that I have made no attempt to review here) by a series of experimental results whose implications in the classical framework seemed a tangle of contradictions, has not only resolved those rather esoteric technical puzzles, but has led us to a solution of the *first*—and most elusive—problem of classical physics: the problem of accounting for the very existence of matter as we know it, and for its qualitative and quantitative characteristics.[44]

XIV. There is clearly a conceptual gap in what I have said so far: I have still not spoken of the interpretation of that elaborate mathematical edifice whose architecture we have surveyed—except in the sense of the rough tentative indications given along the way; and yet I have just referred to the solution of real, objective problems by this theory. In a coherent account, must not physical interpretation of a theory precede its physical application?

Now, the description I have given of the mathematical structure of quantum mechanics is very close to a complete one. I have of course glossed over many details; but these details are themselves standard mathematics: for a mathematician well versed in the requisite mathematical disciplines, but entirely ignorant of quantum mechanics (indeed, of physics), the preceding account, supplemented by very few additional words, would suffice to put him essentially in command of the entire quantum-mechanical "formalism." If I should then be required to explain to this mathematically sapient physical ignoramus how the formalism is used, the following odd situation would confront me: if I try to say what all, or even some, of the fundamental concepts of the formalism mean—to state "rules" for the empirical application of such notions as those of "position observables," "momentum observables," "energy," and so on—the task is stupendously difficult (*at least* as difficult as the writing of a volume of the *Handbook of Experimental Physics*); if, however, I try to explain *directly* in what sense quantum mechanics has solved such problems as that of the existence of solid bodies, I find that quite easy to do. Indeed, for an *n*-particle system there are, besides the observables introduced as associated with particles, other observables—obtained as functions of the former ones—that we should naturally regard as attached to the system as a whole (e.g., "position of the center of gravity"—*defined* as the average of the positions of the particles, weighted by their masses; total momentum of the system; and so on) or to its more or less large-scale parts (e.g., position of the center of gravity, or total momentum or angular momentum components, of some subset of the total collection of particles). If, then, one specifies appropriately the constitution of the system (the masses and spins of the particles, their respective numbers, and the Hamiltonian operator), and *if one calculates the "behavior" of such derived observables belonging to large-scale parts, one finds a strong formal analogy with the "behavior" of the correspondingly-named properties of macroscopic bodies.* As to the nature of this formal analogy, it too is easy to characterize: When we "know" an ordinary macroscopic state of a body (or, more generally, a system), we have values for macroscopic observables, fixed with high probability as lying within more or less narrow limits. We must then consider quantum-mechanical states whose probability-assignments to the eventualities of these observables have that same character (i.e., probability near zero outside a small interval). The quantum-mechanical formalism allows us to calculate the temporal evolution of any such state, and hence of the probability distributions. I say that there is "formal analogy" with macroscopic behavior if this formal evolution is in close agreement, under the already indicated correspondence of observables and of states, with the empirical behavior of the ordinary bodies in question.

XV. An objection is to be anticipated, especially from the physicist: that the application of the theory is not, in fact, such a cut-and-dried formal affair; that we do not and cannot really perform such calculations from basic principles as the preceding discussion imagines, and that in actual practice the application of the quantum-mechanical formalism to an experimental situation may call upon the highest skills of the theoretical physicist.

The remark is correct; the issue is whether it ought to be regarded as standing against the view I have presented. That view incorporates a Platonic myth: the myth of the "sapient mathematician," who can calculate or derive—as real mathematicians cannot—all the consequences of the Schrödinger equation in all possible cases, and even the general consequences that characterize all given classes of cases (a kind of semi-Laplacian demon: not required to have the factual information, but having all the logical-mathematical prowess of the being Laplace imagined). If this mythical presupposition is granted, the rest of the story does follow. Of course, the myth being unrealized, we do not really *know* that the asserted formal analogy holds: the theory of solids, for instance, is based in part upon special assumptions;[45] but (1) it is certain that the basic principles of the theory do in fact determine, one way or the other, the tenability of such auxiliary assumptions as are made—so that in making these assumptions we make, in effect, a *mathematical conjecture*, and in basing empirical expectations upon them we wager upon the truth of that conjecture; and (2) the wager is not a blind one: we have "good heuristic reasons" for the auxiliary assumptions (coming partly from comparison with cases where rigorous deductions from the theory can be made, partly from those nonrigorous approximation procedures at which physicists have developed great skill). Our story, regarded in this light, not only stands uncontradicted by the fact that the application of quantum mechanics can demand the highly developed art of the physicist, but even "explains" that fact. Yet further: even if our "Platonic" mathematician did exist, cultivated physical taste would still be needed to find and to recognize the right representation, in the formalism, of some actual physical phenomenon: the mathematician is able, we have supposed, to derive all the consequences of the formalism for any given formally described system; to find the appropriate formal description to begin with is quite another matter. (Of course, our mathematician might, by studying the consequences of many kinds of formal starting-points, and by comparing these with ordinary reality, eventually learn how to account for real phenomena; but this would precisely be to acquire—in a rather unusual way—the skill of the physicist.)

I attach considerable importance to the situation we have been discussing, because it seems to me characteristic not only of the relation of formal theory to physical application in quantum mechanics, but of that relation in general: that is, I believe that we are dealing here with a matter of general methodology—and one that has been often misunderstood, to the detriment of our philosophical understanding in such fundamental special subjects as the geometry of space and of space-time, the foundations of mechanics, and the interpretation of the concept of probability; and also in the consideration of fundamental general questions about the relationships of theories ("reduction," comparability versus incommensurability of content, etc.). The point concerns the relation of any "formalism" —that is, any *mathematically formulated* theory—to the reality of ordinary experience; and it is connected with the remarks already made (in section III) about operationalism. In the earlier context, the requirement of "exhaustiveness" was the critical stumbling block to the operationalist account of theoretical terms. In the present one, it is the requirement of "sufficiency" that is critical; and here the point affects not only the operationalist doctrine (which has, I think, been largely abandoned by philosophers of science), but all views that presuppose the existence of clearly defined deductive relationships between statements in the "theoretical language" ("formalism") and statements in the "observation language" (or the language of everyday life).[46] My reference, in the preceding section, to the magnitude of the difficulty of writing a volume of the *Handbook of Experimental Physics*, is quite serious in intention. Its point is not merely that sufficient conditions for inference—in either direction—between theoretical and observational statements are hard to formulate, and require much space; its point is, beyond this, that such conditions (in the significant cases) are, in practice, *impossible* to formulate: for the *Handbook of Experimental Physics* itself does not contain them. On the other hand, extremely precise formulations of physical theories can be given very compendiously; and so can indications of the conditions of what I have called "formal analogy" with phenomena, quite sufficient to function as guides to the cogent application of the theories in actual cases. In short, the Platonic myth involved in this

type of account seems to me more fruitful—more clarifying of the relationships really encountered—than the standard myth of deductive "analytic" relationships among theoretical and observational statements. (The whole analysis of quantum mechanics sketched in the present paper stands as one test of this claim.)

I have elsewhere[47] pointed to the question how "correspondence rules" for a theory are found as one that merits more attention than it has received from philosophers—the usual assumption having been that such rules, providing the theory with "empirical content" or "meaning," are part of the conceptual foundation upon which the logical structure of the theory is built (a shadow of the idea of the "Aufbau"). My suggestion is, rather, that physical theories typically contain a set of principles *generative of* correspondence rules—so that, as a theory unfolds deductively and as experimental results yield more information about regularities of phenomena, *new "interpretative" rules of application of theory to phenomena* (supplementary paragraphs to our *Handbook* volume) *are obtained in a systematic way* (and no one regards the theory itself as having been essentially modified thereby). This point of view conforms with the clear historical fact that experimental technique (and perhaps above all, *fundamental technique of measurement*) develops under the principal auspices of physical theory. It is, I think, in the sense of this suggestion that one can understand such a statement as the following (by Bohr's collaborator Leon Rosenfeld): "[I]solated from their physical context, the mathematical equations are meaningless: but if the theory is any good, the physical meaning which can be attached to them is *unique*."[48] But the suggestion remains imprecise; and the detailed logic of these relationships (or of the notion of "formal analogy") seems to me to deserve investigation.

XVI. For quantum mechanics, the situation (viewed in the way we have been discussing) is especially remarkable: in so far as quantum mechanics provides a fundamental account of the existence and properties of *ordinary bodies*, it must follow that the theory contains, in principle, an account of every *experiment*; and the problem of interpretation would seem therefore—"in principle"—to be solved, *intrinsically and definitively*. Setting aside the fact that the non-relativistic theory cannot be regarded as really correct (whereas the relativistic theory remains a torso), this conclusion, in my opinion, is right; it underlies the statement I have made elsewhere[49] that "quantum mechanics poses no special problem of an epistemological kind."

In pursuing this line of analysis of the theory, we shall of course want to apply it to the conceptions with which we began, in order to see whether our preliminary interpretative notions can be maintained. Among these, the crucial one is the notion of *realization of eventualities*; and an explication of this lies immediately at hand. Consider any set S of disjoint eventualities of a given system (the "object"). By a "formal realization" of S let us mean an *interaction* of the object with a second system (the "apparatus"), in a suitable initial state ("well-primed apparatus"), given together with a one-to-one mapping of S onto a set S' of disjoint eventualities of the apparatus (taking e in S to e' in S'), satisfying the following conditions:

1) The interaction occupies a finite interval of time—from t_0 (which we regard as the "time of realization") to t_1 (the "time of registration"); before t_0, and after t_1, the object and the apparatus evolve independently.
2) Whenever the object, at the time of realization, is in a state assigning probability p to an eventuality e in S (and the apparatus is well primed at that time), the state of the joint system at the time of registration assigns the same probability, p, to the corresponding eventuality, e', in S'.

A formal realization is, thus, an interaction so constituted that the eventualities of interest in the object system are *mirrored* by suitable eventualities of the measuring apparatus. If, for instance, we are to study the state of the object that results from specified procedures of "preparation," then to obtain inductive evidence for the probabilities thereby associated with the eventualities in the set S we may carry out a statistical investigation of the eventualities in S', over a population of apparatuses used in formal realizations of S (each with its own object, correctly prepared at the time of realization; with the eventualities in S' studied at the time of registration). Condition (2) assures that the probabilities inductively derived for the members of S' can be transferred to the corresponding members of S. That condition may, in fact, be replaced by the following one (which was formally weaker):

2′) Whenever the object, at the time of realization, is in a state assigning probability *one* to an eventuality e in S (and the apparatus is well primed at that time), the state of the joint system at the time of registration assigns probability one to the corresponding eventuality, e', in S'.

For the fact that quantum dynamics is *linear* on the Hilbert space ("principle of superposition"), and also preserves countable convex combinations of statistical states, easily yields the conclusion that (2′) entails (2). The "mirroring" may therefore be expressed by the statement that *whenever e is certain initially, e' is certain upon registration.* Generalizing this connection, we stipulate that the question of *affirmation or denial* of e (as of the time of realization) is reduced to the corresponding question for e' (as of the time of registration).

The explicative notions so far introduced belong altogether to the formalism; but they can be given empirical content through the interpretation sketched in section XIV. The latter is characterized by the identification of certain eventualities of certain types of systems—at least approximately—with "ordinary" properties of ordinary bodies. Let us call such eventualities "decidable." Then we may define: *a realization is a formal realization by a set of decidable eventualities*; and affirmation or denial of decidable eventualities means affirmation or denial, in the ordinary sense, of the corresponding ordinary properties.

XVII. Unfortunately, we are dealing here with yet another Platonic myth; for although I do not know of any rigorous impossibility theorem bearing upon the question, there are reasons of principle for doubting that *any* interactions having the character of realizations in the above sense can *ever* exist.[50]

The trouble comes from the fact that, whereas our concept of realization imposes a limitation upon the initial state of the apparatus (or rather, a special initial state of the apparatus forms part of that concept), no such limitation is involved for the initial state of the object: that is supposed to be absolutely arbitrary. But "arbitrary state" means *total lack of information*—the object might be on Mars! How, then, can one expect with assurance that there will be any interaction of object with apparatus at all? And if there is none, how can an eventuality of the apparatus after the experiment mirror an eventuality of the object before?

A possible solution to this problem may seem to be this: that, although the objection is valid against the realizability of *all* eventualities, for some—and a sufficiently comprehensive class—realization is possible: namely for eventualities corresponding to questions of the form, "Is the object either such-and-such, or else not there at all?" Then "object not there" will be an appropriate and acceptable message from the apparatus.

Unfortunately, this suggestion fails. It is first of all plain that it will not work when the "such-and-such" in the above formula itself corresponds to an eventuality that is not corealizable with eventualities of position. But this excludes an enormous class: for instance, *no* nontrivial linear momentum or energy eventuality of a particle is corealizable with *any* position eventuality that involves limitation of all three spatial coordinates. (I call "trivial" the eventualities corresponding to the operators **0** and **1**; these, indeed, can—trivially!—be realized.) And next, even for position itself the suggestion appears, at least in present practice, to break down. I do not know any example of a procedure for determining something about a particle's position that does not make some assumption about its state of motion: for example, that it is moving towards, not away from, a screen with an aperture, from the side opposite that on which a detector is placed; or that it has great enough kinetic energy to cause ionizations in a cloud or bubble chamber or a counter. And such combinations of assumptions and tests, formally combined as a single "question," encounter just the same obstacle as above: the components of the question correspond to non-corealizable eventualities.

The general situation, then, for the fundamental eventualities of microscopic systems, appears to be that *every experimental procedure having the character of a measurement or a test is such only on the stipulation of some prior restrictions upon the state of the "object."* But the same is even more clearly true for the tests of "decidable" eventualities of macroscopic systems: I can determine the approximate position of the center of gravity of a snowflake by looking; but there is *no* question about a corresponding system of fundamental particles (identified, e.g., as "those particles that constituted the snowflake on my windowpane yesterday"—here waiving difficulties about interchangeability), in an antecedently arbitrary state, that can be answered by that procedure.

This situation seems to me in no way damaging to the basic position that I have been concerned to expound; on the contrary, it seems to me a new indication of the soundness of the track indicated in sections XIV et seq. For this point of view, the existence of empirically feasible procedures having the character of a "realization of quantum-mechanical eventualities" is not crucial to the successful interpretation of the theory. (I have therefore said, in the article several times cited,[51] that in my view the epistemological importance of the technical problems of the quantum theory of measurement has been overestimated.) Nevertheless, even if it is not crucial, the issue of affirmations and denials of eventualities cannot be merely dismissed; for in practice we do execute what we call measurements of position or momentum or spin of microparticles—and the interpretation of the theory through "formal analogy," as described in section XIV, identifying only those eventualities that correspond to ordinary observable properties, does not of itself provide an explication of this usage. That task of explication does remain, therefore; and in relation to it, the "Platonic myth" of realization in the sense of the preceding section seems to me of use: for it is as "approximations" to realization in that sense that the interactions we regard as measurements deserve that title. The task of explication is, then, partly that of analyzing the actual cases to find and exhibit their structure, and partly that of formulating in general what ought to be meant by "approximation" in the foregoing statement.

XVIII. In the literature on the quantum theory of measurement, a good deal of attention has been given to what is often called the "projection postulate" of von Neumann. In our terms, this is a further restriction upon the notion of a formal realization—that is, an additional clause in the *definition* of realization. There is, of course, room enough for all sorts of definitions (postulates are, in this respect, quite different!); and although controversies do arise over which words go with which definitions, my own conviction is that such controversy is all too often obscurantist: that the serious issue—or, at least, the *prior* serious issue—is to understand the relationships that hold among the several concepts involved, letting the words fall as they may. What, then, is the von Neumann condition?

There are in fact two versions, a weaker and a stronger. (The stronger is the one that occurs in von Neumann's discussion of measurement, and is also the one naturally described as a "projection" condition.) Each can best be attached to clause (2′) in our definition of a formal realization (section XVI), as a further requirement whenever the object at the time of realization is in a state assigning probability 1 to eventuality e in the set S being realized. (It should be noted that, since S is a disjoint set, every eventuality distinct from e in S then necessarily receives probability 0.) The weakened condition of von Neumann requires the state of the joint system at the time of registration, in this case, to assign probability 1 not only to e' but also (again) to e: thus probabilities assigned in S initially must not only be mirrored in S', but also preserved (or, rather, restored) in S. The strong condition requires that the state at the time of registration (still under the same assumption: "all eventualities in S initially definite, with one of them 'true'") assign to *every* eventuality of the object—not just the ones in S—the same probability that was assigned to it by the initial state: in other words, that the interaction leave the object in a definite statistical state, identical with its initial state.

Now, it is obvious that interactions of the (weaker or stronger) von Neumann type would be of particular interest; they allow direct and simple inferences bearing upon the subsequent behavior of the object system: for both types, we should know the values, after measurement, of any measured quantities—and know that they are just the values we read off in the measurement; for the stronger type, we should also know that any other quantities compatible with the ones measured, and definite before the interaction (e.g., as the result of a previous measurement of the von Neumann type), have those same definite values when the interaction is over.[52] On the other hand, we now possess a group of theorems ruling out the possibility of strong von Neumann realizations for a very large class of eventualities, and either ruling out or restricting the possibility of weak von Neumann realizations as well (namely, restricting their possibility for the same class of eventualities, and excluding them for a significant subclass).[53] In the light of the considerations of the preceding section, these negative results seem of no great *philosophical* importance; for even without the formal impossibility theorems, the informal arguments against the existence of realizations (in the sense of section XVI) apply *a fortiori* to these more special realizations—whereas in point of "approximate" feasibility the von Neumann types appear to stand on the same footing as the general notion.

The issue associated with the "projection postulate" that does have fundamental philosophical interest is just the issue of prediction: what is important for us to consider is *how, on the interpretation we are examining, predictions about the future course of a physical system can be based upon information gathered from experiments on the system.* To simplify the discussion of this point, I shall make certain unrealistic idealizing assumptions, among which one will be the "Platonic myth" that realizations are possible. The whole aim of such a procedure, it must be understood, is to adumbrate the principles that can serve—although in a more intricate way—to gain the same point in real cases. It will be seen that the "projection postulate" is quite unnecessary to this end.

Suppose, then, that the object system of interest interacts with some apparatus in a way that constitutes a realization of the set S of eventualities by the set of decidable eventualities S'. Our question is: what does the result of the experiment tell us about the subsequent state of the object system? The result, we of course assume, will have been the affirmation of a member e' of the set S' (and therefore, by our definition, the affirmation *as of the time of realization* of the corresponding member of S). Now, to ask about the subsequent state of the object is to ask for an assignment of probabilities to its eventualities at a later time; and this—at least "Platonically"—is to ask for statistical predictions governing the outcomes of future experimental realizations on the object. But the *combination* of our realization of S by S' with a later experiment on the object system can itself be regarded as one big experiment—whose outcome can be described by the "readings" taken from both pieces of apparatus involved. If we assume that we possess information about the initial state of the object (and, of course, of both pieces of apparatus), and that the dynamics of the interactions are known, then the theory allows us to determine a probability distribution over all possible outcomes of the big experiment. (Such information about the initial state may, for instance, be based upon the procedures of preparation used in setting up the experiment, by way of the kind of evidence briefly indicated in section XVI.) From a probability assignment to the outcomes of the big experiment, however, we may pass—given the outcome of its earlier part—to a probability distribution over the outcomes of the later part, by a straightforward application of the rules of conditional probability. In favorable circumstances, we can even infer in this way, from the result of the first experiment, a *unique pure state* for the object system. This state need not, and typically will not, be the same as the state of the object before the first experiment (as the "projection postulate" would require); nor will it (in general) be the state to which the object would have evolved if the first experiment had not been performed; it will be the result, rather, of the interaction with the first measuring apparatus—an interaction that definitely perturbs the system. The possibility of prediction as a result of measurement depends, therefore, (not upon the restoration of the state of the object by the measurement process, but) just upon conceptual control of the dynamics of measurement. To say that such control is indeed possible is only to say that the quantum theory does, in practice, work; and this even the strongest (competent) philosophical opponents of the theory concede.[54]

XIX. This completes what I have to say, on the positive side, concerning the epistemological interpretation of quantum mechanics; and (therewith) my defense of the internal coherence and the empirical intelligibility of that theory. It remains for me to explain, briefly, the sense in which I nevertheless believe that the theory is still not "understood."

In one way, the focal point of what is not understood is the problem generally known in the literature by the name of "reduction of the wave packet." I do not wish, here, to discuss this famous problem at length, or to set forth in detail my dissatisfaction with all the solutions that have been offered; I reserve this for another occasion. It will suffice for the present to show, in the context we now have available, what the problem is—and how it is related to the whole conceptual framework we have been using. My principal aim is, in fact, to present what I conceive to be the unsolved issues of "interpretation" as problems *about that framework*.

Consider again the situation of measurement-cum-prediction, as described in the preceding section. We have a coupled system, object plus apparatus; and at time t_0, we have a characterization of the statistical state of this joint system—a characterization, moreover, that has the form of an ascription of a definite (we may even suppose *pure*) state to the object, and of one to the apparatus. We have an eventuality e of the object, to which the initial state assigns probability p; and this is realized by a ("decidable") eventuality e' of the apparatus: that is, the dynamics of the joint system transforms the initial state to a state, at the time t_1, assigning the same probability, p, to e' (and after t_1, the systems evolve independently). If many replicas of this experiment are performed, we may, for each of them (at the time corresponding to t_1), "look to see" whether the eventuality e' of the apparatus is affirmed or denied; finding it affirmed in

approximately $100p$ percent of the cases, we conclude that the quantum-theoretic account of this interaction has been confirmed—or, if we did not previously know that probability, we conclude that the state resulting from our procedures in "preparing" the object is one that does assign probability approximately p to e (a result that can be used in subsequent experiments of an analogous kind).

All this fits perfectly our general conceptual scheme, and even conforms to our preliminary interpretation of the notions of "eventuality" and "state." But when we return to the single experiment and regard it in its *predictive* aspect (i.e., as the first part of a larger experiment), the matter is rather different. We have indeed seen how this process can be analyzed within our conceptual framework, and with our interpretation of that framework. But according to this analysis, our prediction of the subsequent course of the object from the result of the first (partial) experiment upon it crucially involves the derivation of new probabilities from old ones by the rules of conditional probability. The "old probabilities" here in question come from *a particular quantum-mechanical state*: that to which the initial state of our joint system is transformed, by its dynamics, in the time-interval from t_0 to t_1. The "new probabilities," in turn, themselves *constitute* a quantum-mechanical state—not of the joint system, but of the object: the state that governs our predictions from the result of the measurement. It is this transition from one quantum-mechanical state to another, *at the same time* (namely, t_1), by conditionalization of probabilities on the basis of the contents of an observation, that is called "reduction of the wave packet"; and the problem it raises is the problem of the *physical* nature of this transition.

The view that I believe is most widely—although not universally—held by physicists is that the reduction of the wave packet does not correspond to any physical event at all. According to this view, the *meaning* of the state of the joint system at time t_1 obtained from quantum dynamics is exhausted by its assignment of probabilities to the possible observations (or "decidable eventualities") of the apparatus—and, through these, to the subsequent possible statistical predictions ("pure states" in our idealized example) for the object. What happens, then, when "the wave packet is reduced," is simply that *a less specific characterization of the system is replaced by a more specific one, on the basis of new information* (obtained "by looking").

Now, according to the general conceptual scheme of quantum mechanics, a state is not in fact characterized fully by the data which, on the view just explained, "exhaust its meaning." These data—the probabilities assigned to "decidable" eventualities—form only a part of that meaning; the latter is "exhausted" only by the probabilities assigned to *all* eventualities. The foregoing view of the reduction therefore implies a serious departure from the general conceptual scheme we have employed so far. In particular, on this view, *certain distinctions between states in our scheme are regarded as devoid of physical meaning*; and in such a way—this is essential to the account of reduction—that *some pure states of the formalism are regarded as identical in their physical meaning with "mixtures."* It would follow that the very character of the pure states as the statistical states of maximal specificity, the "extreme points" in the convex set of all statistical states of a system, had (in general) gone by the board: more precisely, that *some*, but *not all*, pure states have that character. That this is indeed a "serious departure" from our general conceptual scheme will be plain when one reflects upon the profound consequences we have traced from von Neumann's postulate and Gleason's theorem.

These comments do not imply the invalidity, or even the implausibility, of the view that reduction of the wave packet is (as it were) an epistemological, rather than a physical, phenomenon. What they do imply is that if this is so, then the physical theory faces an important question, to answer which would entail a deep-going revision of the theory. The question is to identify, on the basis of some clear principle, which among the "states" admitted by our theory are physically meaningful, and which distinctions among such states are physically meaningful distinctions. It is plain from what has already been said that such a principle would involve a deep-going revision of the theory; I shall make a few further comments on this point in the next (and concluding) section.

But if, on the contrary, reduction is a physical phenomenon—if, that is, the "pure state" that results by quantum dynamics for our joint system, object plus apparatus, at time t_1, is physically different from the "mixed state" in which the several possible results of observation are weighted with their respective probabilities—then it is no less plain that the theory faces a penetrating question and a deep revision. For the pure state that quantum mechanics predicts is not what in fact we observe; the view that there is a real physical difference therefore implies that quantum dynamics itself is not strictly correct. And the true dynamics, if it is to account for reduction, must differ *radically*—must differ in its fundamental conceptual structure—from quantum dynamics: for to account for reduction, it would have to be nonlinear (in the physicists' terminology, it could not satisfy the principle of superposition).

In one point, what I have just said is not entirely correct. In the schematic experiment we have considered, it is not right to compare "the state of the joint system that quantum mechanics predicts" with "what we in fact observe"; for by the act of observation, we are (however slightly) coupled to the system—and quantum mechanics does not then predict a pure state for "object plus apparatus," but only for "object plus apparatus plus us." For this reason, those who regard reduction as a real physical process are not constrained *by any presently known facts* to admit a departure from quantum dynamics, except when a sentient observer is involved in the interaction (a solipsist, indeed, need not admit reductions except when he himself is involved!). This is the origin of the view, often encountered in the literature, that reduction of the wave packet is precipitated by the registration of a content in the consciousness of the observer, and is hence essentially a mind-body interaction. I hope that my presentation of the theory and of the problem has made it apparent upon how slight a foundation of evidence this contention rests.[55] On the other hand, in a problem where next to nothing is solidly known, I think we ought to allow any cogent speculation—provided it is recognized as speculative: neither "fact" nor well-supported "theory"—the privilege of respectable consideration; there tends in such matters, just where clear lines of evidence fail, to occur a sort of ideological vehemence, against which the recollection of the unfortunate polemics of Ostwald and Mach should serve as a cautionary example. In any case, the point I wish to make about this speculation is that it falls under the general head of the preceding paragraph, and in respect of the conclusion of that paragraph makes the conjecture that the departure from quantum dynamics will occur only in psychophysics.

The alternatives we have reviewed are not exhaustive. There remains at least one more: namely, that the quantum-mechanical formalism is fully correct, and that (consequently) reductions of the wave packet never occur at all. Although at first glance this position seems to be in flat contradiction to our experience in general and our applications of quantum mechanics in particular, it has been seriously put forward and defended.[56] Explanation of this view is foreign to my present purpose and would take us too far afield. It should suffice here to remark, with reference to the account I have given, that whether reductions occur or not, our *use* of quantum mechanics essentially involves the *appearance* of reduction (yes-or-no observation, and conditionalization of probability); therefore a theory that denies its occurrence must nonetheless account for its appearance—and this seems to raise the previous questions over again.

XX. I have described this *crux interpretum* of reduction of the wave packet in terms of the quantum-mechanical states. It can of course be described as well in terms of the eventualities: Gleason's theorem determines the states from von Neumann's postulate, which specifies the structure of the eventualities. Our preliminary interpretation—which followed the standard explication of the theory in this respect—took the eventualities to stand for "possible experimental tests" on the system. It is rather natural to think—I myself once thought, and even said in an earlier draft of the present paper—that the question which eventualities have "real physical meaning" reduces to the question (itself dependent upon the forces of nature—i.e., upon identification of the Hamiltonians that govern actual processes): "Which eventualities can be realized experimentally?" The discussion in section XVII should suffice to show the inadequacy of this view of the matter. I should like to conclude this paper with a series of reflections—although not, unfortunately, very conclusive ones—on the nature and role of the eventualities.

We have seen, even in our preliminary discussion, that the simple "operational" version of the nature of the eventualities does not fit the structure of the theory. We have seen later that "experimental realization" of eventualities, in the strict sense that the preliminary discussion seemed to presuppose, is practically impossible. To complete the demythologizing of this matter, it should be remarked that there exist quite straightforward "experimental questions" that can be "put to" a system, but that do not correspond to any eventuality of the system at all.[57] For instance (waiving the difficulties of section XVII—i.e., speaking "approximately," in the unexplicated sense of "approximation"), placing a screen with an aperture in the way of

a moving particle is ordinarily regarded as a "position measurement" on the particle in the plane of the screen: the *experimental* question is, "Did the particle get past the screen?"—or, "Did a flash occur on the detector?"; but the experiment *realizes* (roughly) a position eventuality of the particle at a time *before* it reaches the screen. However, a perfectly analogous experiment that uses a pair of screens, one behind the other, cannot be regarded as a "measurement" or realization on the particle (in its initial condition) at all. One has an equally definite question: "Did it get past both screens?" (or, again, "Did a flash occur?"); and this question does correspond to an eventuality—a "decidable" one—of the entire system, apparatus included (so that the formalism with our general interpretative principles is perfectly adequate to discuss the whole process—we do not have a fundamental unsolved epistemological problem of interpretation!); but it does not correspond to an eventuality *of the particle*.[58] Why does it not? The answer is that, according to the formalism, for each eventuality there must be a state assigning to that eventuality probability 1; but according to quantum mechanics, no conceivable state of the particle before it reaches the first screen can ensure that it will pass both screens. And the lesson would appear to be that, although eventualities do unquestionably have something to do with experimental tests, their "role" in quantum mechanics—their "import," to borrow a convenient current cliché—is essentially bound up with their *theoretical* connections.

Next, quite apart from the problems of section XVII (which I have often "waived," or hand-waved aside, by speaking of Platonic myths or approximations), it is important to note that there are eventualities of undoubted physical significance that do not come within the scope of even approximate direct *presently known* experimental realization. For example, a spin component of a particle can be approximately measured. But for a pair of spin-½ particles, there is an eventuality whose affirmation is tantamount to the statement that the total spin component of the two particles together vanishes for every direction. I do not know of any experimental procedure capable of testing this eventuality. Yet it plays a very essential role in the theory of particle interactions; and the corresponding *states*—that is, states ascribing probability 1 to this eventuality—are very well known to occur (for instance, the two electrons in a helium atom in its ground state are related in this way, and that fact is crucial to the properties of helium according to quantum mechanics).

And yet, eventualities unquestionably have something to do with experimental tests. The standard discussion of the uncertainty relations—which I have not here reviewed—shows this; and the coherence of the whole "preliminary" account I have given in the first part of this paper, with its structural analysis of the "logic" of (classes of) experimental tests, shows it too. How to put these things together is what seems to me a genuine "philosophical" mystery about quantum mechanics in its present state (and in our present state of analysis of it).

Coming back for a moment to physics: One is inclined to believe that "decidable" eventualities must be "definite" always—that is, whether one "looks" or not (contra Berkeley). This is also logically comforting: decidable eventualities are all corealizable, hence have entirely Boolean logical relationships. There is a problem, as we have seen in the preceding section (for it is the counterpart of the problem of saying which states are really distinct), about defining precisely what, in the formalism, the decidable eventualities are (the strategy of section XIV leads only to what we have in practice: piecemeal ad hoc identification of decidable eventualities). But there is also a deeper problem. For quantum mechanics accounts for ordinary observable *things*, in all their thingishness and solidity, with their "decidable" properties, in terms of their constitution out of micro-components whose fundamental nature is radically different: the position of the center of gravity is built up out of position eventualities of particles, and we *know* that these are not "always definite." What is extremely hard to see is how there can be a radical change in this respect at some point in the process of building up. And this is a philosophical mystery that is *also* a *physical* mystery.

It is possible to adopt a rather indifferent attitude towards such problems. We have a physical theory that works very well over a vast domain. Where known classes of phenomena resist its application, physicists are exerting intense efforts to resolve the problems. The issues we have been discussing are of a different character; for they do not attach themselves to clear experimental situations that pose a difficulty, but to ideally conceived—quite far-fetched—experiments that no one knows how to perform, but that would make difficulties if they could be performed ("What if we could realize an eventuality not corealizable with a definite position of the center of gravity of my desk?"). In other words, the situation—as it seems to me—is the following: Because we do have a working interpretation of quantum

mechanics, it is possible simply to disregard conceptual questions that fail to connect with classes of definite known phenomena. Yet, within the framework of the theory itself (and of the best interpretation we possess), conceptual questions of a quite fundamental sort can be posed, which we just do not know how to answer. The state of affairs can be compared somewhat instructively with the problem, within classical electromagnetic theory, of the constitution of the ether. "Maxwell's theory," as we all know, "is just the system of Maxwell's equations." This doctrine, when set forth by Hertz, was in fact illuminating.[59] But puzzling over the questions about the ether that the fundamental notions of Maxwell's theory seemed to raise did not prove to be a useless exercise for physicists (even though much theorizing on the matter followed paths that led nowhere); for, eventually, puzzles about the ether's state of motion as a whole, and about the relative motions of its parts, came to be seen in such a light—and in such relation to a new accumulation of phenomena—that answers could be found. And those answers revolutionized the theory and deepened our understanding of nature very considerably.

One cannot be sure of such an event. There is no logical impossibility in the assumption that what I have displayed as a present impasse in the physical interpretation of quantum mechanics will remain a permanent impasse—indifference to which would then be the most reasonable posture. But the processes of scientific understanding are not advanced by such an assumption, and our historical experience does not recommend it. For (Heraclitus was right) nature does love to hide; and as to our success in ferreting her out—it is (unfortunately) hard to think of another serious human concern in which history so strongly suggests optimism as the expectation most likely to be fulfilled.

NOTES

1. "Is There a Problem of Interpreting Quantum Mechanics?" *Noûs*, 4 (1970), p. 93.
2. George W. Mackey, *Mathematical Foundations of Quantum Mechanics* (New York: W. A. Benjamin, 1963); see esp. secs. 2-2 and 2-3, pp. 61–85.
3. It is, of course, always possible to *extend* a "partial function" or "partial property," so that it becomes defined everywhere. Thus, we may agree to call a man "lucky" only while he is enjoying specific good luck and "unlucky" at all other times (or, alternatively, if we are pessimists, "lucky"

whenever he is not suffering specific misfortune and "unlucky" only when he is); and we may agree to call "not odd," or "even," all numbers that are not odd rational integers—or (in accordance with Frege's demand that every function be defined on the domain of all objects) everything in the world that is not an odd rational integer. But the conceptual construction of quantum mechanics on the lines we are following has no need for any extension of the domains. What is more, two propositions that emerge from the mathematical analysis of the quantum-mechanical framework yield rather deep objections to the point of view that insists upon extension to the domain of all possible conditions of the system as a philosophical requirement (or—less dogmatically—aims at such extension as a desideratum; for an example of the latter sort see Joseph D. Sneed, "Quantum Mechanics and Classical Probability Theory," *Synthese*, 21 [1970], esp. pp. 39–44). The first objection is that any such extension must be *arbitrary*, in a very strong sense. The examples of extension given above are arbitrary in that one sees no reason to restrict the application of "lucky" or "odd" and to extend that of "unlucky" or "even"; but the difference between the two members of each of these pairs of contraries is a conceptual difference, and the extensions are therefore conceptually defined. The "arbitrariness" in the quantum-mechanical context, however, consists in the fact that there *conceptually defined extension for all pairs of contraries is demonstrably impossible*; one can even name a *single* pair of contraries for which no such extension can be conceptually defined. The second objection is that there can be no extension at all—arbitrary or not—satisfying certain conditions without which most of the philosophical motives for making the extension would be frustrated. This is the content of the negative results concerning the possibility of "hidden variables" in quantum mechanics. I hope to discuss these matters on another occasion.

4. This structure is equivalent to that of an *orthocomplemented partially ordered set*, as defined by Mackey, *Mathematical Foundations*, p. 67: if we define $e_1 \leqslant e_2$ to mean that e_1 is disjoint from e_2' (i.e., informally, that affirmation of e_1 entails affirmation of e_2), then our postulates are equivalent to the statement that the relation $\leqslant$ is a partial ordering and negation an orthocomplementation in Mackey's sense. Conversely, given any orthocomplemented partially ordered set, if we define "a is disjoint from b" to mean that $a \leqslant b'$, then our three postulates are satisfied—negation coinciding with complementation; and the ordering defined from disjointness by the foregoing prescription coincides with the given ordering.

   It is worth remarking that, in an order structure of this type, least upper and greatest lower bounds of pairs of elements *may or may not* universally be present: in other words, an orthocomplemented partially ordered set *may, but need not, be a lattice*. Now, both in classical mechanics and in quantum mechanics the "logic" of eventualities does in fact have the structure of a lattice; and in the literature on the logic of quantum mechanics, this property has tended to be emphasized. In my opinion, that emphasis is seriously misleading. The "logical operations" that can make some claim to having a clear meaning within the theory are just the operations that can be explicitly defined from negation and from disjunction of disjoint eventualities—this is the consideration that has motivated the particular form of axiomatization I have given for the structure. The lattice operations cannot, in general, be so defined. In the classical case, to be sure, the least upper bound and greatest lower bound of a pair are, respectively, its disjunction and its conjunction. It is precisely this that leads to what I consider a serious confusion: for quantum mechanics, least upper and greatest lower bounds are present; but there is no justification (in the interpretation of the theory) for identifying these with logical disjunctions and conjunctions—except (see the end of this section) in the case of *corealizable* eventualities; yet these identifications are regularly made, and play a strong part in the proofs of theorems, in the lattice-theoretic accounts of quantum logic (notably, for instance, in connection with the problem of hidden variables).

5. Cf. also n. 22, paragraph (4).
6. Whether this means that such an "infinitely precise measurement" would yield such a unique value is, however, a somewhat ticklish question. If one could survey the *uncountably* infinite set of "answers" relating to, say, open intervals, then indeed one would find a single real number r such that an open interval receives the answer "yes" if and only if r belongs to it; clearly, then, r is "the value of the quantity q" in this measurement. But—unfortunately—it is entirely possible that the same measurement gives the answer "no" to the question: "Is the value precisely r?"—i.e., "Does the value lie in the closed interval bounded both above and below by r?" This possibility (of what might be called "ω_1-inconsistency") comes from the fact that we have not required logical compatibility under *arbitrary* infinite operations, but only under *countable* operations. The anomaly cannot be removed by strengthening our stipulations—not, at least, if we are to preserve the existing structure of quantum mechanics. Indeed, in quantum mechanics the possibility in question becomes, in typical cases, a *necessity*: any quantity that has what is called a "purely continuous spectrum"—and among such are, for instance, the coordinates of position and the components of linear momentum—assigns to each single real number (i.e., each definite value) the *absurd* eventuality; hence each definite value is negated in all possible cases.

   This situation, which undoubtedly looks very queer, is essentially related to the well-known paradox of the theory of probability: that in a continuous probability-distribution, each punctiform event has probability zero—despite the fact that it is certain that some such event must occur. For the statistical theories we are discussing, "absurdity" really means, not "logical impossibility," but "having probability zero in all possible states." The paradox is mitigated, on the empirical side, by the remark that for a continuously distributed quantity any *real* experiment will produce *at best* a localization of the value to within some finite interval—never a point value, nor even a nonzero probability for any point value. (Cf. also the following note.)
7. One of my themes has been that the claims of exemplary "empiricist" or "operational" character for quantum mechanics are specious. The point can be made here once again. If one defines the notion of "observable" without laboring too hard the issue of closure under countable operations, the latter easily passes as a harmless technicality; and, in point of fact, it appears to *be* harmless—given the other things we have assumed—in the light of the remark just made (in the main text). But, one may ask, why the restriction to countable operations? Intrinsically, it would seem natural to extend our logical operations to arbitrary disjoint sets (and for quantum mechanics—but *not* for classical mechanics!—this would be all right, since in the logic of quantum mechanics every disjoint set of eventualities is ipso facto countable), and then to require that the mapping q respect the structure of the set of all subsets of $\mathbf{R}$ as a *complete* Boolean algebra: why admit all Borel

sets and stick at admitting all sets? The answer is that neither "intrinsic naturalness" nor "operational meaning" really determines our choices here; they are determined by a whole network of systematic interconnections (within which, to be sure, considerations of "naturalness" or "simplicity" and of empirical significance do at certain junctures play a strategic part). We do not require compatibility with the complete Boolean structure, but only with the (conceptually less "natural") countable structure, because the former requirement would lead to contradiction (cf. n. 6).

From the point of view of an honest concern with empirical meaningfulness, the conceptual difficulties in this construction really seem to enter at a more primitive stage than that of the "infinite" operations. Some attention to the latter appears inevitable; and for both classical and quantum mechanics, this leads us straight to σ-completeness. But it was with the introduction of the *finite* Boolean operations that the trouble really began. A case can be made out, on empiricist grounds, for starting with questions about *open intervals only*. Finite disjunctive questions—finite unions of open intervals—are surely a harmless extension; and finite conjunctions then lead to nothing new. But *negation* changes open sets to closed sets; and negation together with disjunction leads very quickly from open intervals to individual points. If one wishes to develop a "logic" for physics that is truly adapted to the characteristics of empirical measurement, this suggests a program far more radical than that of building upon the logic of eventualities of quantum mechanics in its current state: it suggests the attempt to develop and apply a logical structure in which "exact negation" does not occur. Any such attempt will obviously have to give up the notion of definite questions associated even with open intervals. In principle, one can question whether an experiment ever fixes the value of a quantity as lying within an interval. In any case, it is clear that one cannot (for a continuously distributed quantity) do the following: a finite open interval being specified, perform an experiment that will determine whether the value of the quantity falls inside the interval. However wide the interval, the value of the quantity may lie too close to its boundary for the experiment actually performed to succeed in the required discrimination.

8. Of these propositions, the first and second are immediate consequences of the definitions of the notions involved in them; the third is not immediately obvious but can be proved as follows: Let q be an observable, x a phase of the system; and let us consider which real numbers r could possibly be "the value of the quantity q in the state x." (We shall find that there is exactly one such; and this will show the existence of the function associated with q.) For any real number r, if $\{r\}$ is the set whose only member is r, $q(\{r\})$ will be the subset of the phase space comprising (presumably) just those states in which q has the value r; thus, to find the possible values of q in the state x, we have to find all the numbers r for which $x \epsilon\, q(\{r\})$. I claim that there is *at most one* such r. Indeed, if r_1 and r_2 are distinct numbers, $\{r_1\}$ and $\{r_2\}$ are disjoint sets; therefore (by our definition of observables) $q(\{r_1\})$ and $q(\{r_2\})$ are disjoint, and x cannot belong to both of them. Next, I claim that there is *at least one* such r. For $q(\mathrm{R})$ is the whole phase space; therefore $x \epsilon q(\mathrm{R})$; but R is the countable disjoint union of the intervals $[n,n+1)$ (where n ranges over the signed integers); therefore $q(\mathrm{R})$ is the union of the $q([n,n+1))$, and x must belong to one of these. By successive bisection, choosing each time a subinterval to the q-image of which x belongs, we obtain a sequence of nested intervals, of length converging to zero—having, therefore, at most one number in their intersection I; but since x belongs to each of their q-images, and they are countable in number, we must have $x \epsilon q(I)$ (this is an easy consequence of our requirements on an observable); therefore, I cannot be empty, so it has the form $\{r\}$, and we have $x \epsilon q(\{r\})$.

9. This section reviews rather technical material, as background for what follows. A reader unfamiliar with the technical matters surveyed here will probably do better to go on to section IX, referring back to the present section only when a briefing on the technical context may seem desirable. (To facilitate such reference, the principal terms and propositions of the section have been set in italics.)
10. This, in fact, is the structure to which von Neumann gave the name "Hilbert space"; and it is also the structure Hilbert studied—not, however, from the point of view of its general or "abstract" definition, which von Neumann was the first to give, but in two special embodiments: the "Lebesgue space" L^2 of (classes of) square-integrable functions on the unit interval or the real line, with

$$(f, g) = \int f(x)\overline{g(x)}dx;$$

and "Hilbert's space of sequences" $x = (x_1, x_2, \ldots)$ of complex numbers the sum of whose absolute squares converges, with

$$(x,y) = \sum x_n \overline{y}_n.$$

It was the fact that these two spaces are (in a natural sense) isomorphic—a fact itself long established (theorem of Riesz and Fischer)—and the significance of this fact for quantum mechanics (in particular, its connection with the equivalence of the formally very different theories of Heisenberg-Born-Jordan and of Schrödinger) that led von Neumann to introduce the abstract notion of Hilbert space. (See J. von Neumann, "Mathematische Begründung der Quantenmechanik," *Nachrichten der Gesellschaft der Wissenschaften zu Göttingen, Mathematisch-physikalische Klasse*, Session of May 20, 1927, pp. 1–57; also reprinted in von Neumann, *Collected Works*, 1 [New York: The Macmillan Company, 1961], pp. 151–207; on the point in question, see esp., in the latter edition, pp. 154–64. Cf. also von Neumann, *Mathematical Foundations of Quantum Mechanics*, trans. Robert T. Beyer [from the German edition, 1932; Princeton: Princeton University Press, 1955], pp. 28–33.)
11. "Mathematische Begründung der Quantenmechanik," pp. 154–64, 192–95 (where, however, the account is in terms of observables and states, and eventualities do not appear); *Mathematical Foundations of Quantum Mechanics*, pp. 28–33, 196–206 (roughly corresponding to the foregoing)—and, for the explicit introduction of eventualities ("propositions"), pp. 247–54. Cf. also Garrett Birkhoff and John von Neumann, "The Logic of Quantum Mechanics," *Annals of Mathematics*, 37 (1936), pp. 823–43; reprinted in von Neumann, *Collected Works*, 4 (1962), pp. 105–25.
12. This theorem is already of some depth. It was first proved by Hilbert, in 1906, for *bounded* self-adjoint operators (whose associated projection-valued measures assign the operator 1 to some finite interval, and therefore assign 0 to every set disjoint from that interval). For the unbounded case, it was necessary to find the "correct" *definition* of self-adjointness; this was done by Erhard Schmidt, and the theorem was proved by von Neumann:

"Allgemeine Eigenwerttheorie Hermitescher Funktionaloperatoren," *Mathematische Annalen*, 102 (1929), pp. 49–131; *Collected Works*, 2 (1961), pp. 3–85 (the spectral theorem is Satz 36, p. 46 of the latter edition).

13. When the operators are unbounded, there are technical complications which, here and later, we shall ignore.

14. Sums and products of *incompatible* observables do *not* make obvious sense. But sums and products nevertheless do exist for noncommuting bounded operators, and sometimes, or in some sense, for unbounded ones too; moreover, addition of operators is commutative (multiplication, of course, is not), and the sum (but not the product) of bounded self-adjoint operators is always self-adjoint. In quantum mechanics we therefore have a kind of adventitious possibility of defining addition for a wide class of pairs of incompatible observables; and it turns out, despite the obstacles noted, that such a possibility exists for multiplication too—although for a smaller class and with more complications. This has considerable importance for the technical development of the theory: for instance, the Hamiltonian operator, which corresponds to the energy and which governs the dynamics of a quantum system (see sec. X), is the sum of two parts—the kinetic energy operator and the potential energy operator—which (except in physically trivial cases) do not commute. For the conceptual foundations of the theory, it is important to understand that algebraic combination of incompatible observables has no prima facie meaning. Conceptually indefensible presuppositions about such combinations have more than once impaired the cogency of analyses of the foundations of quantum mechanics. (The most notable case is that of the "no hidden variables" theorem of von Neumann—see below; cf. also n. 4.) Not least among the merits of the group-theoretic account of quantum-mechanical concepts—for a review of which see sec. XI—is that it can derive such facts as the additive decomposition of the energy operator into noncommuting components *without* presupposing that addition of observables has in general a physical meaning.

15. "Wahrscheinlichkeitstheoretischer Aufbau der Quantenmechanik," *Nachrichten der Gesellschaft der Wissenschaften zu Göttingen, Mathematisch-physikalische Klasse*, Session of November 11, 1927, pp. 245–72; *Collected Works*, 1, pp. 208–35 (in particular, in the latter edition, pp. 215–21). *Mathematical Foundations of Quantum Mechanics*, pp. 295–323.

16. H. Weyl, "Quantenmechanik und Gruppentheorie," *Zeitschrift für Physik*, 46 (1927), pp. 1–46 (introductory remarks and Part I).

17. *Mathematical Foundations of Quantum Mechanics*, pp. 323–25 (based upon the analysis cited in n. 15 above—i.e., upon pp. 295–323).

18. Cf. n. 14. The critical assumption, in von Neumann's argument cited in the preceding note, is postulate E. on p. 309. A very clear and decisive critique of von Neumann's argument is given both by John S. Bell, "On the Problem of Hidden Variables in Quantum Mechanics," *Reviews of Modern Physics*, 38 (1966), secs. II and III (pp. 447–49), and by Simon Kochen and E. P. Specker, "The Problem of Hidden Variables in Quantum Mechanics," *Journal of Mathematics and Mechanics*, 17 (1968), sec. 6 (pp. 75–82).

19. A. M. Gleason, "Measures on the Closed Subspaces of a Hilbert Space," *Journal of Mathematics and Mechanics*, 6 (1957), pp. 885–93. (The progress of Gleason's investigation can be traced through Mackey's article, "Quantum Mechanics and Hilbert Space," *American Mathematical Monthly*, 64 [supplement: October 1957], pp. 45–57; see footnotes on pp. 51 and 57.)

20. It should be emphasized that these remarks fall under the general caveat issued at the beginning about the status of our "informal interpretation"; we shall presently see that reflections of this sort have to be taken with a grain of salt. Nevertheless, taken with such a prophylactic granule, this way of viewing the situation seems to me to have much in its favor. It will be recognized by those familiar with discussions of the philosophical foundations of quantum mechanics that this point of view is incompatible with insistence upon the so-called "projection postulate" for quantum-mechanical measurement. The issue will be discussed more fully below.

21. *Mathematical Foundations*, pp. 1, 81; "not quite appropriately" because the postulate demands merely that inferences can be made to the past as well as to the future—not that, for each possible history of a system, there is another that follows (in a suitable sense) the reverse course.

22. These differences, which seem to me to constitute the main pedagogical barrier to the understanding of quantum mechanics, deserve more complete discussion:

1. In classical mechanics, observable properties and measurable quantities are functions on the phase space—the latter real-valued, the former "truth-value"-valued (or $\{0,1\}$-valued). Now, it is easy to see that *if* all measurable quantities are representable, in their functional relationships, as real-valued functions on some set, then all observable properties must be representable, with their logical relationships, as $\{0,1\}$-valued functions on that set—or, what comes to the same thing, as *subsets* of that set; and the "logic" of these properties must be Boolean, in the sense that *the structure of $\mathcal{E}$ can be embedded in the structure of a complete Boolean algebra.* It is, however, an immediate consequence of Gleason's theorem about the probability measures on the closed subspaces of a Hilbert space, that *the von Neumann logic of quantum mechanics cannot be embedded in a Boolean algebra.* And from this it clearly follows that the measurable quantities and observable properties of quantum mechanics are not functions on the space of pure states; indeed, that *those quantities and properties cannot be represented, in their functional relationships, as functions on any set at all.*

2. If one considers a quantum-mechanical "mixed state" S, composed of pure states S_n with weights w_n (nonnegative and summing to 1), it is possible to think of a system in the state S as being "with probability w_n in the state S_n"; and this (with the classical case as analogue) makes it tempting to suppose that a mixed state is essentially a probability distribution over pure states. But such a supposition would be quite incorrect. Mixed states, like pure states, are characterized fundamentally by the probabilities they assign to eventualities: states S and S' are identical, for quantum mechanics, if they assign the same probability to each eventuality. In this sense of identity, the same mixed state can always be represented in infinitely many different ways as a convex combination of pure states: the representation is unique only when the state is pure (and the weighted sum reduces to a single term). It follows that if we were to interpret the weights as specifying a probability distribution we should be led to contradictions: one representation of the state S (for instance) would "tell us" that a system in that state has probability 1 of "being in one of the states $S_1, S_2, \ldots$," whereas another representation of the same S would give the same event probability 0.

3. In quantum mechanics, according to Gleason's theorem, every statistical state is a countable convex combination of pure states. In classical statistical mechanics, on the other hand, states obtained by countable convex combination of pure—i.e., "punctiform"—states are exceedingly special; indeed, from the statistical point of view, such states are degenerate, and they are ordinarily not considered at all—the probability measures that one does

consider assign probability 0 to all countable sets of points of the phase space: we may say that the classical point states are not "genuinely statistical" states at all. In the class of genuinely statistical states, then, there are no extreme points. The quantum-mechanical pure states, on the contrary, are "genuinely statistical"; and the quantum-mechanical class of genuinely statistical states, in its convex structure, not only contains extreme points, but (with countable combination) is generated by them.

4. It is remarkable that the pure states of quantum mechanics, since they correspond to rays—i.e., one-dimensional linear subspaces—of the Hilbert space, stand in natural one-to-one correspondence with a certain class of eventualities (for each ray also represents an eventuality). From this association arises the following way of speaking, which is very widespread in the literature: if the pure state S corresponds to the eventuality e, an experimental realization of e is said to test *whether the system is in the state* S. For classical mechanics, the analogous formulation is entirely appropriate: the eventuality associated with a point state is represented by the subset of the phase space containing just that point (and is hence disjoint from the eventualities associated with other point states). But in quantum mechanics this way of speaking leads to trouble, for a reason much like that encountered in paragraph (2) above. If we know that our system is in the pure state S, whose associated eventuality is e, then, to be sure, we shall predict with probability 1 that e, if realized, will be affirmed. But if S_1 is a pure state different from S, and e_1 the corresponding eventuality, we shall generally have from S a nonzero probability—and we may have a probability close to 1—that e_1 will be affirmed if it is realized. Can we then say that our system, known to be in the state S, may be (or probably is) in the state S_1? We cannot, without contradiction. Suppose, for instance, that S assigns to e_1 probability ½. There will be some pure state S_2, with associated eventuality e_2, such that S_1 assigns e_2 probability ½, and such that the eventualities e and e_2 are *disjoint*: so that S assigns to e_2 probability 0. To say that our system is in the state S, then, implies that we can predict with probability 1 that e_2, if realized, will be denied—and accordingly implies that in a population of like systems, e_2, if realized for each of them, will with practical certainty never be affirmed. To say that our system is in the state S_1 implies that, in a moderate-sized class of like systems, e_2, if realized for each of them, will with very high probability be affirmed about half the time. And to say that our system has probability ½ of being in the state S_1 therefore implies that in any such collection of experiments it can be confidently expected that the fraction of affirmative realizations of e_2 will lie somewhere between not much below ¼ and not much above ¾. Clearly, then, to regard realization of e_1 as testing for S_1 leads us into contradiction. It is precisely this situation that has led many writers to suggest the use of a nonstandard logic *of discourse* as the way out (cf. sec. IV). As I have already said, I do not think this proposal has been satisfactorily developed; and I do not believe it necessary to introduce a novel logic of discourse in order to avoid inconsistency here. The following distinctions and usages seem to me appropriate and adequate to that end:

A) Neither in classical nor in quantum mechanics ought one to speak of *testing whether* a system "is in" a given (genuinely statistical) state. A *statistical test*, carried out upon a population, can be regarded as answering a yes-or-no question about that population ("Is a certain property distributed with a certain frequency?"); but such a question does not, of itself, concern a "genuinely statistical state" *of the population*. The question may, to be sure, in some cases (namely when it is sufficiently detailed), be regarded as (approximately) a yes-or-no question about the statistical state of the *members* of the population: i.e., an affirmative answer may lead to comprehensive probabilistic predictions about those members. This, however, does not amount to a single test, performed upon a single object, to determine *its own* statistical state.

B) Both in classical and in quantum mechanics, *inference to* "the statistical state of a system"—i.e., to probabilistic predictions about the system—is quite possible. Such inference will be based upon specific information about the system and upon general laws about such systems. (We shall consider in secs. XVI, XVIII, and XIX how experimental results on quantum-mechanical systems can lead to such a determination of a statistical state.)

C) Both in classical and in quantum mechanics, there is a distinguished class of states—represented by points in the phase space, rays in the Hilbert space—that (i) play a fundamental role in the dynamics, (ii) are states of maximal specificity, and (iii) have—unlike any state not in the distinguished class—a special association with eventualities of the system (so that a state of the distinguished class is fully determined by reference to the associated eventuality, and conversely). *But:*

D) In classical mechanics, the distinguished states—the phases—are not "genuinely statistical": for *it does make sense to speak of a test to determine whether or not a system is in a given phase* (or even to determine *which phase it is in*); and testing of the associated eventuality has just the character required for this (former) purpose. Accordingly, *in classical mechanics no "genuinely statistical" state is associated with an eventuality.*

E) In quantum mechanics, on the other hand, *all the states there are are genuinely statistical*. Therefore *there are no states that can properly be regarded as subject to yes-or-no test.* But—and this situation is unprecedented—there are states which, although genuinely statistical, nevertheless stand in natural one-to-one association with "questions" of maximal specificity, each such state predicting with probability 1 an affirmative response to a test of the corresponding question. Like the classical phases, then, the quantum-mechanical pure states are—in our still tentative, informal, and idealized version—associated with definite tests of definite eventualities; but the associated eventuality can in the classical case, and in the quantum-mechanical case cannot, be regarded as the eventuality "that the system is in the corresponding state."

23. One should ask whether the association to a given dynamical law—taking (t,S) to S_t—of a suitable unitary action—taking t to U_t—is unique; and if not, how wide is the latitude? The answer is that the association is not unique, but the ambiguity is very manageable: if r is an arbitrary real number, the mapping that takes t to $e^{irt}U_t$ defines a one-parameter group of unitary transformations having the same effect upon the rays as the group that takes t to U_t; and these are the only ones with the same effect.

24. It has been mentioned above (sec. IX) that a wide class of functions, including the exponential function in particular, have analogues in the calcu-

lus of operators. The relation of the self-adjoint operator H to the one-parameter unitary group U of which it is the infinitesimal generator can be expressed, in terms of this functional calculus, as follows: $U_t = e^{-itH}$ (the negative sign is chosen merely by convention). The ambiguity in U—see n. 23 above—leads to an ambiguity in H: if we replace U_t, by $U_t^* = e^{irt}\, U_t$, we shall have $U_t^* = e^{irt}e^{-itH} = e^{-it(H-r1)}$; hence instead of H we shall have $H^* = H - r1$ as infinitesimal generator (1 being the identity operator): the multiplicative ambiguity in U gives rise to an additive ambiguity in H, by a real additive constant. For the observable associated with H—the energy—this means that the definition of a zero point for energy is (so far as these considerations are concerned) arbitrary.

As to the "time-dependent Schrödinger equation," what the physicists call by this name is obtained as follows: for a given "state vector" $\mathbf{x}$, we define: $\mathbf{x}_t = U_t\mathbf{x} = e^{-itH}\mathbf{x}$. Differentiation of this relationship (which can be justified) gives: $\frac{d}{dt}(\mathbf{x}_t) = -iH\mathbf{x}_t$. This differential equation is the time-dependent Schrödinger equation; in conjunction with the "initial condition" $\mathbf{x}_0 = \mathbf{x}$, it is *equivalent* to the integrated relation: $\mathbf{x}_t = e^{-itH}\mathbf{x}$. (In the standard "Schrödinger representation," the Hilbert space of the system is thought of as the space of classes of square-integrable functions, ψ, on the "classical configuration space." The time dependence—of ψ [replacing $\mathbf{x}$] on t—introduces a new "independent variable" t, and the equation is written as a partial differential equation: $\frac{\partial\psi}{\partial t} = -iH\psi$.)

25. For the analogous concepts of Newtonian mechanics, we do not ordinarily see a corresponding problem—unless we are more or less extreme positivists. For the eventualities and the dynamical variables of Newtonian mechanics are defined theoretically in terms of the phase space—i.e., in terms of the positions and motions of the particles; and we are persuaded that we have a clear understanding of what these notions mean. The problem of specifying empirical procedures for testing or measuring properties or quantities then just looks like a technical problem for the experimentalist. Of course, the positivist example—notably the polemic against the meaningfulness of the kinetic-molecular theory of matter—shows that the problem of "clothing naked abstractions with empirical significance" *can* be said to exist *on the same philosophical footing* for classical as for quantum mechanics; but the substantive failure of that polemic has tended to discredit its principles, and thus to discredit the view that that problem *should* be said so to exist.

In my view, there is an irony in this situation. The antiatomist polemic was surely wrong in substance; and as surely *one-sided* in its epistemological and methodological principles. It does not follow that nothing in those principles was sound. The overwhelming triumph of the atomic theory has shown conclusively that (e.g.) *Mach's dogmatic rejection of atoms was thoroughly misguided.* But in the very midst of the triumph of atomic physics, Newtonian mechanics itself failed; and this failure—the specific failure that led to, and has been set right by, the quantum theory—is traced by the quantum-mechanical analysis precisely to a breakdown in "physical meaning" of those fundamental concepts of position and motion which are supposed to give the classical theory its clear intelligibility. Does this not show Mach's *skepticism* (in contrast to rejection) of the ultimate "metaphysical" meaningfulness and reliability of the classical concepts to have been thoroughly justified?

26. E. Wigner, "Einige Folgerungen aus der Schrödingersche Theorie für die Termstrukturen," *Zeitschrift für Physik*, 43 (1927), pp. 624–52; H. Weyl, "Quantenmechanik und Gruppentheorie."

27. Weyl, "Quantenmechanik und Gruppentheorie," introductory remarks and Part II; note the following (here translated from pp. 1–2): "In quantum mechanics one can clearly distinguish two questions from one another: 1. How do I arrive at the matrix, the Hermitian form [i.e., self-adjoint operator], that represents a given quantity in a physical system of known constitution? 2. When I have the Hermitian form, what is its physical meaning; what sort of physical information can I get from it? . . . The second Part treats of the deeper question 1. It is most intimately connected with the question of the essence and the right definition of the *canonical variables* [i.e., positions and momenta]. . . . Here I believe I have reached, with the help of the *theory of groups*, a deeper insight into the true state of affairs. [*Note:*] This connection with the theory of groups lies in an entirely different direction from the investigations of Mr. *Wigner*, which show that the structure of spectra is qualitatively determined by the subsisting group of symmetries."

See also Hermann Weyl, *The Theory of Groups and Quantum Mechanics*, trans. H. P. Robertson (New York: Dover Publications), pp. 272–80.

28. See George W. Mackey, "Infinite-Dimensional Group Representations," *Bulletin of the American Mathematical Society*, 69 (1963), pp. 628–86, esp. pp. 664–70; "Group Representations and Non-Commutative Harmonic Analysis" (Mimeographed lecture notes, Department of Mathematics, University of California at Berkeley, August 1965), ch. 14 (pp. 157–99); *Induced Representations of Groups and Quantum Mechanics* (New York: W. A. Benjamin, Inc., 1968).

29. This is not obvious from our formulation of the definition of an n-particle system; but it can be seen as follows. Our assumption of an action upon the states has to be understood to mean all states, mixtures as well as pure cases, and to require that the convex structure of the class of states be preserved—for otherwise we should have a statistical method of distinguishing absolute position or absolute orientation in space or absolute velocity. Now, the relation of orthogonality between pure states is definable in terms of the convex structure of the class of all states: pure states S_1 and S_2 are orthogonal if and only if for every pair of pure states, S_1' and S_2', and every pair of real numbers between 0 and 1, r and r', if the states $rS_1 + (1-r)S_2$ and $r'S_1' + (1-r')S_2'$ coincide then the "weights" r' and $1-r'$ fall between the weights r and $1-r$. It follows that our group operators preserve the relation of orthogonality of rays.

30. As in the case of energy (cf. n. 24), these considerations determine the associated observable up to the arbitrary choice of a zero point. (When the action of a larger group is considered, a reduction of this arbitrariness may occur; e.g., for the projective representations of the Euclidean group there is no ambiguity in the observables associated with one-parameter subgroups.) It is worth noting that, once units of distance and time have been chosen (as is of course necessary in order to relate the structure of the Galilean group in a numerically unique way to the group of real numbers), no further specification of units of measurement is required. Thus energy, and the remaining observables and parameters that we obtain in this way, are numerically

determined once units of length and time are given: there is no need, as in classical physics, to choose a conventional unit of mass. In the relativistic theory, just *one* choice is required; for distance and time are there numerically related in a natural way. Just as the effect of this "natural" relation in special relativity is to eliminate the constant c, the velocity of light, from the laws of nature, so the effect of the use of "natural" units in the present context is the elimination of Planck's constant h, the elementary quantum of action.

31. The technical situation here is more complicated than most accounts show. Invariance under the dynamics means that the one-parameter group in question and the dynamical group commute *in their action on the rays*; and therefore generate a projective representation of the additive group of a two-dimensional real vector space. If one knew that this could be reduced to an ordinary Hilbert-space representation, the conclusion would follow very simply by the argument usually given. But what actually follows from just the premises we have is that *there is a real number λ, the same for all states* (and constant in time), *such that in any time-interval t the observable in question changes by λt.* Upon what basis do we conclude that λ must be zero? The answer is the following:

    1) Consideration (not just of single one-parameter groups, but) of the entire Euclidean group shows that for the dynamics to leave the action of the Euclidean group invariant, each one-parameter subgroup of the latter must in fact generate, with the dynamical group, a projective representation that does reduce to an ordinary representation; hence each observable that comes from the Euclidean group has $\lambda = 0$—i.e., is a conserved quantity.
    2) The result in (1) suffices as a basis for the further analysis that obtains results about the possible forms of the Hamiltonian operator. This operator, one finds, can never have a purely continuous multiplicity-free spectrum extending over the whole real line. But it is very easy to show that if any observable increases by λt in each time t—with the same λ for all states—the Hamiltonian must have precisely that forbidden spectrum, *unless* $\lambda = 0$. Hence the conclusion that $\lambda = 0$ holds for any quantity generating a group that is preserved by the dynamics (not only for those coming from the Euclidean group).

    On the result stated in (2), cf. also Wolfgang Pauli, "Die allgemeinen Prinzipien der Wellenmechanik," *Handbuch der Physik*, ed. H. Geiger and K. Scheel, vol. 24, part I; reprinted in Pauli, *Collected Scientific Papers*, ed. R. Kronig and V. F. Weisskopf (New York: Interscience, 1964), vol. 1 (see, in the latter edition, p. 830, n.): *there is no dynamical variable "canonically conjugate" to the energy*; or: *time cannot be regarded as a quantum-mechanical observable*. (This is not, although it may at first seem, a surprising fact. It says that no observable of any system increases uniformly with time *in all states*, and at a rate independent of the state. We ordinarily measure time, of course, by using an observable—typically a position—that changes uniformly with time, and at a known rate, *when the system starts from a suitable state*: "clock correctly adjusted and wound.")

32. Choice of a spatial origin is necessary in order (i) to fix the zero point of each position coordinate and (ii) to define the action of the subgroup of "rotations about the origin." After this choice (and that of a unit of length), all else is determined; thus the "natural" quantum-mechanical units of linear

and angular momentum do not depend upon the unit of time. The angular momentum operators, indeed, are independent of the choice of a unit of length as well (although they do depend—not merely for zero point, but for their essential definition—upon the choice of an origin). The linear momentum components, on the other hand, depend upon the unit of length (but not upon the choice of a spatial origin).

    In the presentation of the theory sketched here, linear momentum appears to have a "natural" zero point. This has to be regarded as an accident—and an unfortunate one. It is a consequence of our starting from the naïve concept of "physical space," and thus from an assumed absolute distinction between rest and motion. In this order of proceeding, justice is done to the relativity of motion only by the subsequent postulate of Galilean symmetry. It would be rather more satisfying—at least in point of elegance and clarity—to carry out the construction on a basis that is invariant under velocity transformations from the beginning. What the proper mathematical framework is for such an enterprise is not obvious, and seems to deserve consideration. (The first notion to suggest itself—that one associate observables with subsets of space-time, rather than space—has to be rejected: as we have seen in n. 31, there is no observable corresponding to time.)

33. For the history of the Heisenberg relations—in whose discovery Born and Jordan on the one hand, Dirac on the other, played a role comparable with that of Heisenberg (and in whose most general formulation Pauli and Weyl also had a part)—see the illuminating account in B. L. van der Waerden, *Sources of Quantum Mechanics* (Amsterdam: North-Holland Publishing Co., 1967; New York: Dover Publications, 1968), pp. 36–42, 52–54, 58–59.
34. All separable infinite-dimensional Hilbert spaces are isomorphic; what is special in the association of this representation of the Hilbert space with a system of this type consists, not in the form taken by the *vectors* of the space, but in the form taken by the *operators that correspond to physically important dynamical variables*. (The same point holds for later cases.) Cf. Weyl's formulation of his "question 1," in the passage quoted in n. 27.
35. The wave functions cannot, strictly speaking, be identified with the vectors of $\mathcal{H}_1$, because the correspondence of functions to vectors is not one-to-one: the vectors of $\mathcal{H}_1$ are associated with equivalence classes of wave functions, two functions being equivalent if the absolute square of their difference has integral 0 (i.e., if the values of the functions are equal "almost everywhere").
36. A little more precisely: the images, by these operators, of the function whose value at the point (a,b,c) is $\psi(a,b,c)$, are the functions whose values at that point are $a\psi(a,b,c)$, $-i\frac{\partial\psi}{\partial x}(a,b,c)$, and $i(c\frac{\partial\psi}{\partial y}(a,b,c) - b\frac{\partial\psi}{\partial z}(a,b,c))$, respectively. The operators in question are all unbounded, and therefore not everywhere defined. The first, for instance, has for its domain of definition the set of square-integrable functions for which $x\psi$ is also square-integrable. (Full specifications for the other two require a more elaborate discussion, because of the occurrence of differential operators.)
37. This follows from the isotropy of space, hence from our assumption of Euclidean symmetry.
38. The possibility of half-integral spins turns upon the existence of projective representations of the rotation group that cannot be reduced to ordinary representations.
39. See n. 24.

40. It is sometimes denied—from the point of view of Bohr and the "Copenhagen school"—that classical mechanics can be regarded as an approximation to quantum mechanics. But that denial consists in the contention that in any realization of an eventuality, the experimental apparatus—not being a part of the quantum system under study—has to be treated by classical rather than quantum mechanics. Now, I do not think that this latter contention is sound (on this point, see the—rather terse—discussion on p. 99 of the paper cited in n. 1); but for our present purposes, this issue is irrelevant. For, granting the correctness of the Copenhagen position here, it would remain the case that any physical system could, in principle, be accorded quantum-mechanical treatment—say, the system A, whose behavior we know to be accounted for well by classical mechanics. To perform quantum-mechanical observations upon A, we should have to use another system, B, as experimental apparatus; and according to the Copenhagen view we should have to use classical physics to discuss the behavior of B; but this latter circumstance does not prevent us from giving a quantum-mechanical account of the behavior of A—and showing that this account does justice to the circumstance that classical physics works for A (indeed, if we could not, in principle, do this, then our theory would certainly be inconsistent). It is sometimes argued that Bohr's point of view—or the correct point of view—would forbid the use of quantum-mechanical notions for any "macroscopic" system. This, in my opinion, is a gross misunderstanding of Bohr, and an obscurantist attitude to the substantive question. For we have no clear line between "microscopic" and "macroscopic" systems; and, what is more important, the program that physics has successfully pursued is that of dealing with the properties of macroscopic systems in terms of their constitution out of microconstituents.

41. It is stated in the Preface to the *Principia*, and it dominates the more speculative parts of the *Opticks*, one of whose major concerns is the analysis of the structure of bodies in terms of particles and the forces between them. Indeed, it seems surprisingly little known—or little recognized—that *the study of the interactions of light and matter to derive information about both the fundamental particles and the forces of nature* was a central systematic aim of the investigations reported by Newton in the *Opticks*. (Cf., on this point, the brief discussion in my paper, "On the Notion of Field in Newton, Maxwell, and Beyond," in *Historical and Philosophical Perspectives of Science*, ed. Roger H. Stuewer [Minneapolis: University of Minnesota Press, 1970], vol. 5 of *Minnesota Studies in the Philosophy of Science*: see pp. 269–72.)

42. The celebrated classical problem of the stability of the solar system—still unsolved—concerns a quite different concept of "stability" from the one meant here. In that problem, one wants to know (roughly) whether the structure of the system is such that (a) its dimensions—the average distances among its constituent bodies over suitable intervals of time—will remain approximately the same for all time, provided no externally occasioned disturbances occur, and (b) relatively small external disturbances will cause relatively small permanent changes in those dimensions. In this sense, for example, a system of two bodies under gravitational attraction is stable. But the "stability" that is required to account for the properties of matter is, rather, that of a configuration of a system which, if perturbed, will tend to *return* to that configuration. (I here use the term "configuration" in a broader sense than that of its technical usage in mechanics, so that "dynamic," as well as "static," stability is included under the concept.) With this meaning, there would be no question at all about the planetary system's stability: that system certainly possesses *no* tendency towards restoration of its former state after a disturbance.

43. To make the account quantitatively correct, and to include electromagnetism and light in the scheme, the relativistic theory is required; and this turns out to involve very serious difficulties indeed, which are far from being resolved.

It should be added that there is a mathematical gap in the theory so far as its ability to account for solid bodies is concerned. In the case of atoms, one has rigorous results for the simplest cases, and these well justify the devices employed in the more complicated cases to obtain approximate numerical information; and for molecules, a similar statement can be made. But the properties of solids depend essentially upon the structure of crystals; these—as everyone knows—are structures of a periodic kind; and in the present state of the theory, the existence of periodic solutions of the quantum-mechanical conditions for a stable state (the "time-independent Schrödinger equation")—and of periodic solutions, moreover, corresponding to a minimum for the energy of the system—has to be just postulated, in order to gain any results. Such postulation, of course, has to be regarded as provisional, for the mathematics of the theory itself determines whether the postulate is true or false; and the fact that one gets *correct* results, as attested by experiment, leads to the fairly confident expectation that the mathematics will confirm the guess. (Cf. sec. XV.)

I am indebted to my colleague Professor Paul Kantor for pointing out to me the hypothetical character of the quantum theory of solids as it stands at present.

44. Although description of this problem as "the first" lays no claim to profundity, it does have somewhat more than rhetorical justification. Conceptually, classical physics is about bodies; and the *only* bodies entering into evidence in the empirical base of classical physics are "ordinary" ones. Historically, it is worth remembering the place this problem occupied—before Newton—in the "Monde" and in the *Principia Philosophiae* of Descartes; and the fact that it forms the subject of Day I of Galileo's *Two New Sciences*.

45. Cf. n. 43.

46. The difficulties involved in all such views have been recently discussed by C. G. Hempel in his Carus Lectures (delivered to the annual meeting of the Western Division of the American Philosophical Association, in St. Louis, May 1970).

47. "Is There a Problem . . . ?" (cf. n. 1 above), p. 98.

48. Leon Rosenfeld, "Misunderstandings About the Foundations of Quantum Theory," in *Observation and Interpretation in the Philosophy of Physics*, ed. S. Körner (New York: Dover Publications, 1957), p. 41.

49. "Is There a Problem . . . ?" p. 93; and cf. sec. I above.

50. Although the remarks that follow are elementary, I am not aware of their having been made previously in discussions of the quantum-mechanical measurement problem.

51. "Is There a Problem . . . ?" p. 102.

52. The last point is not immediately obvious, but is an easy consequence of the linearity of quantum dynamics.

53. E. P. Wigner, "Die Messung quantenmechanischer Operatoren," *Zeitschrift für Physik*, 133 (1952), pp. 101–08; Huzihiro Araki and Mutsuo Yanase,

"Measurement of Quantum Mechanical Operators," *Physical Review*, 120 (1960), pp. 622–26; H. Stein and A. Shimony, "Limitations of Measurement," *Proceedings of the International School of Physics ≪Enrico Fermi≫*, Course: "Foundations of Quantum Mechanics" (29th June–11th July, 1970), forthcoming.

54. Cf. the words of Einstein, who refers to quantum mechanics as "die erfolgreichste physikalische Theorie unserer Zeit": the most successful physical theory of our time (Paul Arthur Schilpp, ed., *Albert Einstein: Philosopher-Scientist* [Evanston, Illinois: The Library of Living Philosophers, 1949], p. 80).
55. The view that processes affecting a sentient observer, or "consciousness," have a peculiar importance for quantum mechanics, also often takes the form of a statement about the *subject matter* of quantum mechanics. Thus Wigner has recently written: "The basic principles of physics, embodied in quantum-mechanical theory, are dealing with connections between observations, that is, contents of consciousness. . . . Classical physics, of course, also can be formulated in terms of (deterministic) connections between perceptions, and the true positivist may prefer such a formulation. However, it can also be formulated in terms of absolute reality; the *necessity* of the formulation in terms of perceptions, and hence the reference to consciousness, is characteristic only of quantum mechanics" (Eugene P. Wigner, "Physics and the Explanation of Life," *Foundations of Physics*, 1 [1970], p. 38). It seems to me, on the contrary, that essential reference to "contents of consciousness" is no more required in quantum mechanics than in classical mechanics. The enormous difficulties that face any attempt to construct physics on such a strictly phenomenalistic basis are well known; they are certainly not less for quantum mechanics than for classical physics. Indeed, the difficulties for quantum mechanics are greater, in so far as the mathematical abstractions, or theoretical concepts, employed in it are more remote from ordinary experience than those of the classical theory; and we have seen that the basic notion of eventuality or observable, generally taken for the characteristically "empirical" feature of the concept system of quantum mechanics, is in anything but direct relation to experience. Therefore, *even if the problem of the reduction of the wave packet should turn out to be a matter of essentially psychophysical interaction*, it would remain the case (a) that quantum mechanics itself does not require reference to contents of consciousness, and (b) that the introduction of such contents as the proper subject matter of quantum mechanics would not clarify the theory, but would make it extraordinarily hard—if not impossible—to formulate.
56. Hugh Everett, III, " 'Relative State' Formulation of Quantum Mechanics," *Reviews of Modern Physics*, 29 (1957), pp. 454–62; John A. Wheeler, "Assessment of Everett's 'Relative State' Formulation of Quantum Theory," *Reviews of Modern Physics*, 29 (1957), pp. 463–65.
57. This situation was pointed out to me by Abner Shimony.
58. According to von Neumann's original analysis, a measurement should be instantaneous, and the double-screen process would have to be analyzed into two successive measurements. But no interactions are instantaneous; and many kinds of measurements cannot, in principle, be made arbitrarily short; so this requirement of von Neumann's is untenable. I consider it one of the merits of the present account that it explicitly rejects such a requirement.
59. See my paper, "On the Notion of Field" (cf. n. 41 above), pp. 281–82.

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