

What is the Problem of Measurement?

By Simon Saunders

QUANTUM MECHANICS, TOGETHER WITH RELATIVITY, are the basis of most of what we know of the very large, the very small, and the very fast. And in all of these areas our knowledge is extensive. It would be hard to say how good these theories are; arithmetic, one might say, is good mathematics.

For all that, there is a problem, a strange and amorphous difficulty: it is the *problem of measurement*.

This problem has become much more interesting, and much more pressing, as a result of some recent developments. Hitherto the difficulty has been so peculiar, and so formless, that physicists have on the whole thought it a matter of philosophy. But that is not quite right (or that is what I shall argue). It is certainly philosophical, but it is also a matter of methodology, a question of what physics is to do; and now the matter has come to seem quite pressing. In this sense the measurement problem has become a *problem for physics*.

But what is the problem of measurement? As a first stab, we can say this: on the basis of quantum mechanics, there is a difficulty in accounting for the

fact that experiments have any outcomes at all. In Heisenberg's words: "it is the 'factual' character of an event describable in terms of the concepts of daily life which is not without further comment contained in the mathematical formalism of quantum theory, and which appears in the Copenhagen interpretation by the introduction of the observer."¹ The interpretation to which Heisenberg refers is mainly the work of Niels Bohr, one of the founders of quantum mechanics. It was the orthodoxy for several decades, but here too the situation is quickly changing, or it has already changed; little is left of it today.

It is necessary to make some preliminary remarks on the structure of quantum mechanics. A fundamental concept is the *state*, in many ways the analog of the classical state, which is an exhaustive specifica-

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tion of all the properties which a system has, which then changes in time according to the dynamical equations of motion. For the quantum case too there are equations of motion, called the *unitary* dynamics, which are linear, deterministic, local and covariant. "Covariance" means that the dynamics respects the space-time symmetries; "local" means, roughly speaking, that there is no action-at-a-distance, unless that is put in explicitly with the force-laws, just as in classical mechanics. "Linearity" means that given two quite different states, each a solution of the equations of motion, the sum of the two states (with arbitrary coefficients) is also a solution. From a mathematical point of view, this is at the heart of the measurement problem; it is called "the superposition principle." In this respect quantum mechanics resembles a classical wave theory.

This dynamics is the central object of study throughout quantum physics; for example, it is what particle physics or grand-unified theory is concerned with. In these respects the situation is not so different from classical physics.

But when it comes to the statement of how the quantum state is related to anything that can be observed, there is an important difference; here the so-called "measurement postulates" must be used, according to which the state is to be written as a list of numbers (hence as a vector), each of which gives the probability of a particular experimental outcome. The same list can be rewritten in a number of ways, corresponding to the different types of experiments that can be performed, so that "type of experiment" resembles the choice of a coordinate system in elementary vector calculus. This is the only parallel that we have with anything in classical physics. Further, the state itself must be "prepared"; it is associated with a single experiment, called a "state-preparation device." Putting the two together, we say that the state can be considered as a list of "transition amplitudes" from a particular preparation device to sets of experiment outcomes.

In summary, although the state seems to describe the properties of atoms and electrons and so on, in a way not so different from classical physics; and although it undergoes a complicated and interesting dynamical development in time, again similar to the classical state; it nevertheless also seems to make reference to *experiments* that we do or might perform, and the probabilities of the various outcomes of these experiments.

What I have said so far is entirely uncontroversial. So far we have a kind of "minimal" interpretation of the formalism, necessary for any application of quantum mechanics whatsoever. The state is on the one hand bound up with the microscopic system, and on the other with probabilities of measurement outcomes. At this point the problem of measurement is foreshadowed by the simple query as to how the two are related.

Instrumentalism

ONE RESPONSE IS TO ELIMINATE ALTOGETHER THE REFERENCE to the microscopic system. The state is *only* a list of numbers relating a preparation device with the probabilities of measurement outcomes, the particular list depending on the kind of final measurement considered.

This is clearly a form of instrumentalism, particularly on the understanding that, as goes the rest of the formalism, there are only mathematical statements concerning certain sorts of symmetries for these transition amplitudes. The theory has the character of a "symbolic calculus," a phrase often used by Bohr, for defining probabilistic relations between the instrument settings of experiments. Some physicists have advocated such a view, usually as a kind of fall-back position, in the face of difficulties that arise on any richer conception of what quantum mechanics is about.² We should understand Bohr's remark in this vein: "there is no quantum mechanical reality." The position is, however, quite different from Bohr's.

On this instrumentalist interpretation, we do not also have to deny that there is any substructure to matter at all. We might acknowledge that there is some sort of microscopic reality, but that we cannot say what it is. The state only amounts to a summary of the statistical relations between pairs of experi-

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— Werner Heisenberg

ments (preparation and detection), so if there is any sense in which these statistical relations are themselves a guide to what goes on in between, it will have to be made out in some other way. In this framework the problem of measurement can hardly be expressed at all,

or rather I have already stated it: what is this other way? Failing a response to this, it becomes the methodological query: why not simply describe the probabilistic experimental events explicitly, in terms of some sort of random stochastic process? Better still, why not reformulate quantum mechanics as a theory about the *macroscopic*, regarding "experiments" as simply one sort of (probabilistic) dynamical process among others?

With this the instrumentalism is unmasked as something much closer to a form of idealism; for the measurement postulates do not say "given such-and-such a macroscopic process, the probability of" but only "given an experiment to measure —, the probability of..." where "—" is the name of a purely formal mathematical construction, or else designates other objects of the form "an experiment to measure —." If we did know how to characterize an experiment as one sort of dynamical process among others (where the other processes might not be experiments at all), it would not be very hard to reformulate quantum mechanics as a theory of the macroscopic *tout court*. And of

course, if we had this, it would be very interesting to see how the question of the microscopic realm plays out, of “how far down” the description would extend, and why (as presumably it must) it would fall short of a microphysical theory as well.

But now we must recognize not only that there is no such reformulation, but that there can be no such reformulation, not that preserves the unitary dynamics. For suppose that the state is only an elliptical way of talking about the statistics of possible experiments. It is true that in that case there is no problem with indeterminism, at the level of outcomes, and determinism at the level of the state. But what about the preparation of the state? Here we have no option but to suppose that different singular events, at the level of the state-preparation device, lead to different states. But the indeterministic outcome of an experiment can determine the way that a future state is prepared;³ the state too must change indeterministically.

It follows that even when the state only describes the statistics of experimental outcomes, the unitary (deterministic) dynamics cannot possibly be the whole story. If we were to try to reformulate quantum mechanics in this way, as a universal theory, it would no longer be quantum mechanics. This is the measurement problem as it applies to this strategy.

The alternative is the instrumentalism-cum-idealism already reviewed. It is, I think, quite clear that physics cannot really proceed on this basis. To put the point simply, there is no room for heuristics; there is no notion of what physics is to do, of how it is to proceed, or of how we are to think about the microscopic *or* the macroscopic (we only know how to correlate our *procedures*). It is in any case not really consistent with what physicists *have* to do. After all, they must work with the formalism, and in particular make use of the state; there is no option but to engage in a certain practice. And practice, or use, tends to give rise to meaning, or familiarity, or let us say understanding; and since the state *appears* to describe atoms and electrons and so on, in that its mathematical form depends on quantities like mass, charge, and spin, and on how many systems are involved (and so on and so forth), it is inevitable that this understanding is *about* the microscopic. Since further the state undoubtedly depends on individual state-preparation events — for without this we have no basis to apply quantum mechanics to any experiment — it is inevitable that the state is associated with the individual microscopic system as well.

The Copenhagen Interpretation

ON OCCASIONS BOHR HINTED AT THE INSTRUMENTAL-ism just reviewed, but this was not the picture on offer in the early and critical period, when quantum mechanics was newly created, and when the question of whether or not it could be considered fundamental was a matter of urgency (a question that had to be *settled*).

The most important difference is that indeed the quantum mechanical state describes the individual system. It must be referred to a context of experiment, but it is not defined in terms of the statistics of preparation and detection events.

This is the absolutely crucial move: with this, there is a clear notion of “the

microscopic system" in place, the proper object of physical inquiry, no matter that there will be a number of constraints on what can meaningfully be said of it, and of how quantum mechanics is to be applied. With this we know what physics is about, and how it is to go on.

Now for the measurement problem on this strategy. A first version is this: microscopic systems (and hence the macroscopic) are in some sense probabilistic. If the state says all there is to say about the microscopic, so that it is a "complete" description of the microscopic, then just so far as there are random microscopic events, then there will be random changes in the state. But if the state obeys the unitary equations of motion, its change is deterministic. It follows that these, the laws of quantum mechanics, cannot be the whole story; either the measurement postulates cannot be separated off from the dynamics, or the state is incomplete.⁴

Bohr's way out of this dilemma was to confine the probabilistic change to the immediate interface between experiment and microscopic system, whilst taking the notion of "experiment" outside of the bounds of quantum physics altogether. He further held that there must always be a context of experiment in place. The upshot is that a certain constraint is put in place: *quantum mechanics cannot be applied to closed systems*. Here "closed" means "free of any external influence"; these external influences (reflecting the experimental context) were not to be described in quantum mechanics at all.

But Bohr did not formulate the Copenhagen interpretation as a response to the measurement problem. Rather, the constraint just mentioned, what I shall call "Bohr's constraint," was supposed to follow from a more fundamental epistemological principle, namely that *an objective phenomenon is only defined relative to an observation*. In this the notion of "observation" brings with it the notion of "an observer"; concerning this, Bohr held that *the observer must be described in terms of classical concepts*. I shall call the conjunction of these claims "Bohr's principle of significance" (or "Bohr's principle" for short). We shall shortly see how it is related both to his "principle of complementarity," and to what he called "the quantum postulate."⁵

For Bohr it was therefore more than satisfactory that the concept of observation should enter into the measurement postulates, although he had no particular inclination to bring in subjective aspects of observation or anything mentalistic. Chiming with this, it was a matter of indifference as to how far the classical description extended. Another key feature of his interpretation was what was left as a brute fact. We first need Bohr's definition of "the quantum postulate": this "attributes to any atomic process an essential discontinuity, or rather individuality, completely foreign to the classical theories and symbolized by Planck's constant of action." With this we can see how these ideas combine, and where Bohr draws the line at what can reasonably be explained:

Now, the quantum postulate implies that any observation of atomic phenomena will involve an interaction with the agency of observation not to be neglected. Accordingly, an independent reality in the ordinary physical sense can neither be ascribed to the phenomena nor to the agencies of observation. After all, the concept of observation is so far arbitrary as it depends upon which objects are included in the sys-

tem to be observed. Ultimately, every observation can, of course, be reduced to our sense perceptions. The circumstance, however, that in interpreting observations use has always to be made of the theoretical notions entails that for every particular case it is a question of convenience at which point the concept of observation involving the quantum postulate with its inherent "irrationality" is brought in. (*op cit* p.54).

There is clearly an equivocation as to whence this arbitrariness or "question of convenience" derives; it is partly because we must use classical notions, and partly because the concept of observation is somewhat vague. But the key point is that there is an "irrational" element to the quantum postulate (a phrase repeated again and again in his writings), which is not to be explained, and which it is not the business of physics to understand.

But why is it not? It might be argued that it is because mentalistic notions are brought in with the concept of observation, outside of the scope of physics;⁶ or it may be held that *because* the boundary between classical theory and quantum mechanics is a "question of convenience," there can be no fact of the matter as to what their connection is (this is perhaps Bohr's position). But both positions are clearly dogmatic. A third alternative is on the face of it not of this type: the "irrational element" in quantum mechanics has also been called the "quantum jump," or the "collapse of the wave packet," or "state-reduction"; towards the end of his life, Heisenberg favored the view that this is purely epistemic, reflecting only our *knowledge* of the system. But this is the same position as Einstein; it amounts to the supposition that there is some underlying reality, not described by the state, so that the state is incomplete. Given this, to insist that quantum mechanics is nevertheless a "fundamental" theory, and that we should not look to a deeper level of description (particularly in the sub-atomic or ultra-relativistic regime), is again purely dogmatic.

As it happens, Heisenberg also maintained that there is a transition from "potentiality" to "actuality" in the world, and that this transition "takes place during the act of observation," inventing a sort of mirror- image to state-reduction. But in that case — unless mentality *per se* is to play a role — the epistemic interpretation of state-reduction is a red- herring (and Heisenberg may as well go for completeness after all). It is to no avail to suppose that the state only describes "potentia" (Heisenberg borrowed the Aristotelian term), for on "actualization," a new system of "potentia" is set up, hence a new state. So long as this "actualization" is not described by the unitary dynamics, neither is the evolution of the state.⁷

To repeat the central point: in Copenhagen philosophy the state is associated with the individual system. Given this, it seems there is no alternative but to say the state is subject to a mysterious process of change, quite different from the usual dynamical evolution, something bound up with observation. The measurement problem is then this: how is it that this state-reduction is not the business of physics? The question appears particularly pertinent insofar as "observation" is understood to be a question of the micro-macro interface, rather than mentality *per se*. And Bohr's own position pointed in that direction; his "principle of significance" was supposed to be an epistemological prin-

ciple, whereby we are forced to consider the state in relation to classical concepts, rather than subjective aspects of observation. It is natural to back this up with an analysis of the physics of the macroscopic. After all, under the Copenhagen interpretation, modulo certain constraints, for physics it is business as usual. Bohr has set in motion a program of inquiry that his framework of interpretation cannot properly contain.

The Macroscopic Quantum State

BUSINESS AS USUAL INCLUDES THE THOUGHT THAT there are macroscopic events. Pursuit of this thought, understanding that the state describes individual microscopic states of affairs, led to the application of quantum mechanics to individual macroscopic states of affairs as well. This started quite innocuously, with some exciting experimental physics (superfluidity, superconductivity, the Ising model, phase transitions). The area continued to flourish; nowadays condensed matter physics is much more active than particle physics, and by some is thought the more fundamental. This is the first stage in the unraveling of the Copenhagen orthodoxy.

At this level it is only a shift of emphasis. Nothing in the Copenhagen interpretation prohibited the application of quantum mechanics to large systems. But proceeding in this way — always respecting Bohr's principle, and with it his constraint on the application of quantum mechanics to closed systems — we learn that classical notions are after all not so incompatible with quantum mechanics.

What was at issue with the alleged necessity of classical concepts? According to Bohr this is forced, no matter that they must ultimately be given up, because:

The recognition of the limitation of our forms of perception by no means implies that we can dispense with our customary ideas or their direct verbal expressions when reducing our sense impressions to order. No more is it likely that the fundamental concepts of the classical theories will ever become superfluous for the description of physical experience. (*op cit*, p.16).

The stated argument seems quite reasonable, and it was widely accepted. Heisenberg puts the matter like this:

Our actual situation in science is such that we do use the classical concepts for the description of the experiments, and it was the problem of quantum theory to find theoretical interpretation of the experiments on this basis. There is no use in discussing what could be done if we were other beings than we are. At this point we have to realize, as von Weizsäcker has put it, that "Nature is earlier than man, but man is earlier than natural science." The first part of the sentence justifies classical physics, with its ideal of complete objectivity. The second part tells us why we cannot escape the paradox of quantum theory, namely, the necessity of using the classical concepts. (*op cit*, p.55-6).

But with increasing experience in the treatment of large systems we learn that there is no real difficulty in the treatment of macroscopic systems either. Using only quantum mechanics, we can see that the system has such-and-such properties, where those properties are the familiar classical notions of everyday things. For this we do not need the measurement postulates; it is a consequence of what is sometimes called the "eigenvalue-eigenvector" link, the same principle that is at work when we understand the microscopic state as encoding certain microscopic properties of individual systems, entirely in the framework of pure quantum mechanics.

Of course doing this we will be constrained by the uncertainty relations; we will not arrive at classical properties which involve absolutely precise position *and* momentum (or velocity). But at the everyday level these imply no new constraints at all on what we see, or even what we (classically) judge to be the case. The uncertainties are minute for ordinary objects; for example, they are already swamped by thermal fluctuations. More prosaically, philosophers have long argued that all our everyday thing-words are vague, along with most of our everyday notions. To suppose that we cannot so much as conceive of objects which do not have absolutely precise properties meets the instant objection that it is hard to see how we can know of any objects which do.

But it does not follow that we can dispense with the measurement postulates, or that we can apply quantum mechanics to closed systems. If we tried to do this, we would have a deterministic development of a macroscopic state. Using the eigenvalue-eigenvector link, we can set this up so that it initially describes (and we can see that it describes) a macroscopic state of affairs, but under the unitary dynamics this description, using the same eigenvalue-eigenvector link, eventually becomes completely unrecognizable.

The problem of measurement takes the strengthened form: macroscopic states of affairs are probabilistic; if there is a quantum mechanical state which is the correlate of a macroscopic state of affairs, then it cannot be subject to deterministic equations of motion.

But does this not reinforce Bohr's point? Indeed, we must take into account "outside observation"; we must take into account his principle of significance. But its function is not quite what it was. Let us grant that the phenomenon is only defined relative to observation. Let us also grant that the observer must be described using classical notions. It no longer follows that that we cannot analyze the latter in quantum mechanical terms; the official argument no longer implies Bohr's constraint. It does not follow that we cannot apply quantum mechanics to closed systems.

We can see the lacuna in the argument in its very first appearance:

On one hand, the definition of the state of a physical system, as ordinarily understood, claims the elimination of all external disturbances. *But in that case, according to the quantum postulate, any observation will be impossible*, and, above all, the concepts of space and time lose their immediate sense. On the other hand, if in order to make observation possible we permit certain interactions with suitable agencies of measurement, not belonging to the system, an unambiguous definition of the state of the system is no longer possible, and there can be

no question of causality in the ordinary sense of the word. The very nature of the quantum theory thus forces us to regard the space-time co-ordination and the claim of causality, the union of which characterizes the classical theories, as complementary but exclusive features of the description, symbolizing the idealization of observation and definition respectively. (*op cit*, p.54, emphasis mine.)

The fallacy occurs in the statement italicized, for there is a missing premise: that observation is always *external* to the system, not from *within* the system. But that premise appears quite arbitrary; we can perfectly well incorporate Bohr's principle by considering the observer as internal to the system modeled, to be described in purely quantum mechanical terms. It follows that there is no *epistemological* reason to rule out the application to closed systems.

There is, of course, another reason: the problem of measurement. The logic of Bohr's position is then quite different from what it appears. It is *because of the problem of measurement* that we cannot apply quantum mechanics to closed systems.

Decoherence Theory

THERE IS AN IMPORTANT INSIGHT UNDERLYING the statement of Bohr's just reviewed, that requires a little experience with the equations to see; it is bound up with the sense in which the coupling of a quantum-mechanical system with another makes for a difference to the application of the measurement postulates, which is completely independent of the strength of the coupling.

This "structural difference" concerns interference effects; the way in which phase relationships between components of the state are relevant to ordinary predictions, using the measurement postulates. In terms of the state as a list of numbers, each corresponding to a different probability for the associated experimental outcome, they are essential to the way the same state is written down with a different kind of experiment in mind. If we think of the choice of the kind of experiment as similar to the choice of a coordinate system, or to use the proper term, a choice of "basis," the transformation from one basis to another is only possible by virtue of these phase relationships.

The analogy can be extended. How is it that relativistic space-time ("Minkowski space") is something more than a heterogeneous collection of 3-dimensional worlds? But Minkowski space is a metric space; within limits, we can arbitrarily choose what is to count as "space" and what as "time," for this is only a matter of how we set up coordinates on the manifold; there will be equations (coordinate transformations) connecting any such decomposition, with the invariant structure given by the metrical relations. But picking out a *privileged* decomposition into space and time breaks this symmetry.

It is because of the phase relationships between the components of the state, referred to different bases, that we have a similar symmetry in quantum mechanics. Were these eliminated, this symmetry too would be broken.

The application of quantum mechanics to large systems brought with it new interest in quantitative calculations of these phase relationships, among macroscopically distinguishable states. This has become the business of a new

branch of quantum physics, called “decoherence theory,” a vigorous and fast growing field. The literature is now enormous; what has become clear is that interference effects between such states become completely negligible in vanishingly short times, for all but the lightest and most weakly interacting systems. More to the point, this is so only with respect to the “correct” choice of states, essentially, those in which the center-of-mass variables have definite values. Equivalently, only with respect to a certain choice of “coordinate system” or basis, do the interference effects become irrelevant. The ultimate origin of this lies in the role that mass plays in the equations, and in the fact that the unitary dynamics is local.

The choice of basis, recall, is what in the instrumentalist interpretation, and what in Copenhagen philosophy, was bound up with the choice of experiment. But here it is a matter of how the macroscopic world is to be described. The macroscopic world is treated as a whole; quantum mechanics is here applied to closed systems.

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There is, however, an important qualification. What we obtain is a *superposition of macroscopic states*, albeit that the interference effects between them are small. Because they are small, we can if we wish replace this description by a different sort of description, called a “mixed” or “impure” state, where these phase relationships are eliminated altogether. With this the symmetries of the state-space of quantum mechanics are also eliminated. But by a formal application of the measurement postulates — ‘measuring the macroscopic world,’ so to speak — we obtain probabilities which are approximately the same as those obtained using the original pure state. In this sense we can say that the impure state is an “approximation” to the pure state; equally, we can now dispense with the measurement postulates altogether, and *interpret* the mixture as meaning that one or another of the components “happens,” to the exclusion of all others. That is that one or another macroscopic world is “actualized.”

We see something similar in relativity. How is it that there is a fact of the matter as to what is “now”? If this fact is objective, and hence intersubjective, it is a fact which breaks the symmetries of Minkowski space. With this we bring in a preferred definition of simultaneity; exactly what is prohibited by relativity.

It seems that this is just what is required if we are to make out a notion of "passage" through time, the sense that there is all that is now, that will shortly be changed into all that was; and we along with it.

There is another way of arriving at a similar result. We can consider the total system as divided into two, one of which involves a small number of massive particles, and the other a large number of much lighter particles. We call them *A* and *B* respectively. There is a theorem which says that there is a unique choice of basis, for the total state, such that each component state of *A* is correlated with one and only one component state of *B*; the superposition of all these correlated states returns the total state. The new result is this: given that the dynamics is local, and that the two systems are in approximate thermal equilibrium, it follows that each component in that unique choice of basis, at each instant, describes all the particles of *A* as well-localized, all the way down to molecular levels.⁸

I have of course glossed over a number of important difficulties. With respect to the first kind of result, the so-called "decoherent histories" approach, we do not have any proof of uniqueness. In particular, the mere stipulation that we must choose that basis for which the interference effects are vanishingly small is not enough: we can always choose the basis with the universal state (the "wave function of the universe") as one of its members. In that case we obtain a unique and deterministic history, unrecognizable to us. In the second case, the "dynamical decoherence" approach, the argument for uniqueness is much more convincing, but the details of the "preferred basis" thus obtained depend on how the division into subsystems *A* and *B* is made out.

In both cases Bohr's principle of significance can *also* be put in place; as a matter of course, we can suppose that the macroscopic includes "the observer." In the second case it is natural to suppose the observer is a fragment of the subsystem *A*, with *B* as the radiation field. But whether the notion of "the observer" is relevant to the details of the distinction between the two subsystems, and whether, in the decoherent histories approach, it is relevant to any additional constraints on how one kind of global decoherence rather than another is defined, remain open questions. In effect the issue is whether the macroscopic world in which we are imbedded is in part defined by our own physical make-up. Of course, what "physical" means here is no longer quite so clear.

The Problem of Measurement.

IT MIGHT NOW BE THOUGHT THAT THE PROBLEM of measurement has been solved; certainly the decoherence theory has transformed the situation out of all recognition. On the contrary, I suggest that only with this has it become *a problem for physics*.

What remains of the problem of measurement? Only this: it is by means of an "approximation" that we have passed from a pure state to an incoherent mixture, eliminating all phase relationships. The latter we can *interpret* as meaning that one or another component of the state has been "actualized," with the elimination of all others. But both steps violate the fundamental quantum mechanical dynamics. The first step involves a "small violation," a sort of scrambling of phase relationships; the latter a massive modification to

the state.

The last is our familiar friend, the reduction of state, but the state now extended to the entire universe, a closed system. The measurement problem becomes: is the process of state reduction a physical process? If so, what is its relationship to the unitary dynamics with which we began?

In effect the situation where “there can be no question of causality in the ordinary sense of the world,” to use Bohr’s words, has or can be formulated as the fundamental dynamics. This strategy has been taken by a small but growing number of physicists: it is called the “GRW proposal.” The unitary dynamics is abandoned; certain parameters, that in the decoherence theory vary somewhat with the details of the model, are now to be viewed as new and fundamental constants of nature.⁹ What replaces the unitary dynamics is a precisely defined stochastic process (“continuous state reduction”), with eminently useful and quite general applications to experimental physics. It predicts exactly what we see: stochastic laboratory events, with frequencies in perfect accord with predictions.

But this has only made the dilemma more acute. For all the models of GRW type, obtained from the decoherence theory, violate relativity and

“If we hold relativity and quantum mechanics to be fundamental — and we can scarcely do otherwise — then we must hold the universal state to be the fundamental object of physics. If we hold the world that we do see to be the fundamental object of physics — and we can scarcely do otherwise — then we must hold the stochastic and non-local dynamics to be fundamental.”

energy conservation.¹⁰ None of them, viewed as fundamental theories, have been successfully extended to a covariant system of equations. The question becomes: what is the status of relativistic quantum physics? If relativity is to be abandoned: what of general relativity? Given the continuous state reduction: what difference does this make to the physics of the early universe?

The dilemma is all the more bewildering when we recall that the decoherent histories approach has been most extensively developed by quantum cosmologists. The very success of their enterprise appears to call into question the foundations of their discipline.¹¹ Reflecting on the history of the problem of measurement, we see that the key developments all revolve around the notion that the quantum mechanical state is the correlate of individual states of affairs. By a natural and perhaps inevitable progression, this leads to the supposition that there is a quantum mechanical correlate to macroscopic states of affairs. And now it seems that that must be correct; the decoherence theory turns on this, its success cannot be gainsaid. We cannot turn our back on the results

that we have obtained in consequence. But we cannot stop with these results either, for we do not understand how the “approximation” involved can be justified, or even what it means.

But what we do know is that given this supposition, the macroscopic state *must* change indeterministically, in whatever sense macroscopic states of affairs change indeterministically. But the quantum mechanical evolution is unitary, hence deterministic. The entire structure of quantum mechanics revolves around this. Either the world that we see is not all that there is, or else the equations of quantum mechanics do not govern the world that we see. Given further that the macroscopic world that we see changes in a non-local and non-covariant way — as follows from all the models presently available — a similar disjunction follows¹²: either the world that we see is not all that there is, or else relativity does not hold for the world that we see.

The problem of measurement has finally emerged in its definitive and most virulent form: there is no escape from these dilemmas. But they *are* dilemmas, and in each case one disjunct is the same: from the point of view of the decoherence theory, based on the unitary laws, we have unitarity, locality and covariance. They hold at the level of the universal state, the superposition of macroscopic states, only one of which is recognizably the world that we see;¹³ it is the supposition that one of these is “realized,” or “actualized,” which breaks the symmetries of quantum mechanics and relativity both at once, and requires a stochastic dynamics in place of the unitary dynamics. This is the motivation for retaining the grand superposition as the fundamental object of physics, the approach that was proposed by Everett.

Philosophy and Physics

IF WE HOLD RELATIVITY AND QUANTUM MECHANICS to be fundamental — and we can scarcely do otherwise — then we must hold the universal state to be the fundamental object of physics. If we hold the world that we see to be the fundamental object of physics — and we can scarcely do otherwise — then we must hold the stochastic and non-local dynamics to be fundamental.

The dilemma is inescapable, and in a certain sense irresoluble. I doubt that we can return to the instrumentalist interpretation with which we started, and anything less is in a situation of unstable equilibrium. In effect we answer the problem of measurement if we acknowledge that quantum mechanics is not the whole story; if we say there is only a heterogeneous mix of prescriptions. But not only did physics seek for a unified framework (applicable to closed systems), it was also successful in the search: hence the problem of measurement.

It should be clear that the difficulty began as a question of methodology. Einstein’s debate with Bohr was not about the nature of “reality,” but a question of how physics was to proceed. As we have seen, Bohr’s principal contribution was to make out a sense in which quantum mechanics could be regarded as fundamental, and hence the basis for subsequent research, no matter that on the face of it it could not be the whole story. To do this, it was necessary to insist that the theory cannot be applied to closed systems; in effect, that we may not seek for a systematic and unified theory. But this was presented as an epis-

temological constraint, as a matter of philosophy; if he had been correct in this, then no such unified system could have been found. In fact the real force of his argument was that we can avoid the problem of measurement only if we do not apply quantum mechanics to closed systems. Now that we have applied quantum mechanics to closed systems, we can no longer avoid the problem of measurement.

And it *remains* a question of methodology. For let us suppose that quantum gravity were a success story; that Hawking were to be proved right, and that the end of physics were to have been achieved. In this we suppose that the fundamental principles of relativity and quantum mechanics are preserved, and that standard quantum mechanics results in a suitable low-energy regime. We then use the decoherence theory to determine macroscopic histories of the universe, some of which are recognizably in accordance with our everyday grasp of the world, again in an appropriate regime. But now from the point of view of physics it makes no difference whether there is any precise energy conservation along any one of these histories, or whether the selection of any one is in violation of relativity and unitarity. For were we to formulate a stochastic set of equations, in place of the pure quantum mechanics, that would only amount to another way of proffering the same sequences of states, with probabilities attached; the first way we obtain them all at once, and the second way we generate them one by one, making a particular choice, using e.g. a random number generator. It is true that according to the latter procedure, we could deny that other histories, those eliminated by the stochastic state reduction, have any existence; whereas according to the former, it seems there is only the superposition of them all. And it is true that we could then take the operation of the random number generator as a representation of "actualization" and "movement through time," with all that these notions involve. But it would only be a matter of the way in which the same data is presented; one might say according to taste. "All that these notions involve" — but it is all, all of it, metaphysics. One would like to ask: but is energy *really* conserved, or does relativity *really* hold, or is there *really* only the world that we see? But the sense of the word "really" is now wholly philosophical. These questions would have no bearing on how physics is to proceed, for by hypothesis its business would be already done.

But this is not our position. It is because we do not have a successful theory of quantum gravity that the situation is a difficulty *for physics*. The crucial point is this: there is no reason to suppose that a "correct" theory will be arrived at in the manner just formulated, leaving it as a "matter of taste" as to how the theory is presented, *unless it could only be a matter of taste*. That is, we cannot *presume* that there should be two such "empirically equivalent systems of the world," unless we are prepared to grant that nothing of importance could ride on the matter.

There is no *fait accompli*; we have to assess these questions as they stand. They are certainly philosophical; to suppose, as has long been the tradition in physics, that it is "only" a matter of philosophy, is to accept now, in advance of the success of otherwise of the program, that it "could only be a matter of taste." And that in turn means that one is prepared to accept an interpretation

of Everett-type. That something hangs on the matter is quite obvious: for one who is not prepared to accept such an interpretation, there is no alternative but to hold that however useful the basic concepts of relativity and quantum mechanics, they cannot be taken as fundamental. But then there is good reason to suppose that it is exactly by giving them up, that it is exactly by invoking continuous state reduction, that we may hope to make progress in quantum gravity or grand unification.¹⁴

What is required, of physicists, in a purely pragmatic sense, is a kind of metaphysical decision. It plunges physics into a style of metaphysics, and a style of philosophical reflection, that we have not seen since the time of Descartes. I say that the problem of measurement is a matter of methodology: what is extraordinary is the philosophical character of the issues at stake.

It is helpful to get a glimpse of what is involved. We are to suppose that there is only the superposition of all histories; it may be, following Everett, that we are to suppose that the "existence" of the world that we see is somehow relativized to our own "existence." This is familiar from Kant, but now the meta-

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physics takes an unfamiliar turn; how it is that our history could be merely one of these (so that the others in some sense exist) is certainly a kind of modal realism, but it is like nothing contemplated by philosophers:

for all these histories are integrated into a single dynamical object, accommodating probability and time as well. We do not deal with a heterogeneous collection of "worlds," as envisaged by Lewis ("possible worlds semantics"), no more than we deal with a heterogeneous collection of three-dimensional worlds in space-time theory. Probability is defined in purely extensional terms, as a network of relations. Along with time, there are only structural or purely formal desiderata of adequacy: not only is there no picture of how it is that we or our environment can pass from one time to the next, in accordance with these probabilities, but in a certain sense there can be no such picture. It is more than a world *sub specie aeternitatus*, for it includes all possibility as well; and even here — given that the symmetries of quantum mechanics are *not* violated — how the various possibilities are to be classified can only depend on how we are ourselves characterized; on what we are or could be.

Many of these notions are strange to philosophy, although they are all clearly philosophical. But there is one important fragment which is familiar: we must make do with only a formal or structural notion of time and personal identity. And it is noteworthy that philosophers have only made tacit appeal to the concept of substance in their treatment of personal identity. Whether

because the concept has proved idle, or for some other reason, philosophers have inclined to view personal identity in a more structural sense (e.g. in terms of memory or “psychological continuity”).¹⁵ And in connection with tense, we have a familiar metaphysical difficulty every bit as fundamental as modality: the question of whether what is “now” is an objective fact, of how and in what sense there is “passage” through time, appears no less important than the question of whether what “happens” is an objective fact, of how and in what sense there is state-reduction.¹⁶

But I have said enough to make clear the difficulty. It seems we have a God’s Eye View fit only for Gods. The scenario is so alien that one is inclined to dismiss it in sheer self-defense. Typically it is also stated that it is unintelligible, but in view of its evident empirical applicability that too is a philosophical thesis.

I have said that this is for physicists to decide; it is unlikely that they will do more than that. Their decision will amount to a choice of strategy. If they continue to work with quantum mechanics and relativity, something like the metaphysics just sketched will be given over to philosophy. If they do not, it will be for no other reason than that this metaphysics is judged untenable. That would in itself be a remarkable development; on philosophical grounds alone the basic concepts of the two most fundamental theories of physics will have been abandoned. In either case this metaphysics becomes the business of philosophy. The problem of measurement has long been ignored by physicists, rightly; however they now respond, it can no longer be ignored by philosophy.

Historical Note

THE CONCEPT OF DECOHERENCE HAS LONG BEEN FAMILIAR; it already figured in Mott’s analysis of the cloud chamber in 1929. It resurfaced in the so-called DLP theory of measurement in the early ‘60s (after Daneri, Prosperi, and Loingers). More than one champion of the Copenhagen interpretation reconsidered in consequence (Leon Rosenfeld, Bohr’s faithful disciple, is an example). A much more general (and rigorous) development was due to K. Hepp, who supposed that the approximations involved were as “natural [here] as elsewhere in microphysics.” In this form it was implicit in a variety of techniques used in the definition of thermal equilibrium in quantum statistical mechanics, particularly using the algebraic tools developed in quantum field theory in the late ‘50s and ‘60s. Together with Hepp’s work, this inspired a number of more specific models, among them studies by B. Whitten-Wolfe, G. Emch, D. Lewis, L. Thomas, and A. Frigerio, but the most important upshot was the general theory of quantum semigroups as systematically formulated by E. B. Davies (*The Quantum Mechanics of Open Systems*, Academic Press, 1976). This was a major stimulus for the work of G. Ghirardi, A. Rimini and T. Weber in 1986 (the GRW proposal), for whom the phenomenological model was to be considered fundamental. Others who contributed to this program included L. Diosi, P. Pearle, A. Barchielli, N. Gisin, and I. Percival. References to these papers can be found in the works cited below.

The same ideas, but understood in purely phenomenological terms (i.e.

consistent with quantum mechanics), were applied in a variety of concrete physical models throughout the '80s (as is to be expected, there was a corresponding loss of rigor). Here results of W. Unruh, W. Zurek, E. Joos, A. Caldera, and A. Leggett were particularly influential. The notion then in vogue was that of "environmentally-induced superselection rules," no matter that it was quite clear that the rigorous notion of superselection sectors only distinguished infinite-volume systems or the infinite-time limit. The debate over this has only played out recently; for an instructive and non-technical commentary, I refer to the exchange between Zurek and others in the pages of *Physics Today* (October 1991, April 1993).

All of this concerned dynamical decoherence; the decoherent histories approach came about quite independently, beginning with the work of R. Griffiths in 1984. Since then it has been extensively developed by R. Omnès, M. Gell-Mann, and J. Hartle; unlike the dynamical decoherence theory it is well-suited to the needs of quantum cosmology. Since its very early days the latter made appeal to Everett's ideas, particularly as championed by Wheeler and DeWitt (B. DeWitt and N. Graham, eds., *The Many-Worlds Interpretation of Quantum Mechanics*, Princeton, 1972); Everett made it quite clear that he was motivated by the *inadequacy* of the Copenhagen interpretation to the needs of quantum cosmology (Wheeler and Zurek, *op cit*). D. Zeh, who made important contributions to the dynamical decoherence theory, has also long advocated the Everett approach; a useful exchange involving Zeh, Gisin and Percival can be found in *Physics Letters A*, 1992-3 (Vols. 167, p.315, 172, p.189, 175, p.144).

A statement of the role of "the observer" in the definition of decoherent histories is first found in M. Gell-Mann and J. Hartle, 'Quantum Mechanics in the Light of Quantum Cosmology' (in W. Zurek, ed., *Complexity, Entropy, and the Physics of Information*, Addison-Wesley, 1991). It is developed in my 'Decoherence, Relative States, and Evolutionary Adaptation,' (*Found. Phys.*, 23, 1993, p.1553). For related ideas see W. Zurek, 'Preferred States, Predictability, Classicality, and the Environment-Induced Decoherence' (in J. Halliwell, ed., *The Physical Origins of Time Asymmetry*, Cambridge, 1993). For a study of the connection between the Everett approach and the philosophy of time, see my 'Time, Quantum Mechanics, and Decoherence,' *Synthese*, forthcoming.

Acknowledgment: I would like to thank Burton Dreben for some decisive comments on a first draft of this paper. ϕ

ENDNOTES

¹W. Heisenberg, *Physics and Philosophy*, Allen and Unwin, 1959, p. 121.

²A milder version of this view arose in connection with the "S-matrix" theory in hadron physics in the late '50s and early to mid '60s. There it was a reaction to the difficulties of formulating a relativistic space-time dynamics, in the quantum mechanical sense (quantum field

theory). The problem with this was that the only principles at hand (analyticity and unitarity) were too weak; even so, the theory was much richer than the instrumentalism just canvassed. For a detailed study see J. Cushing, *Theory Construction and Selection in Modern Physics: The S Matrix*, Cambridge, 1990.

³In “non-destructive measurements,” the measurement and preparation event are one and the same. In “repeatable measurements,” a sub-set of these, repetition of the experiment yields the same result as recorded at the first measurement outcome. The latter motivates the so-called “projection postulate.”

⁴The last option is so natural that one might wonder why Bohr did not take it. It was certainly Einstein’s position. But then Einstein argued strongly that in that case, physics should seek a deeper level of description, on the analogy of the relation between thermodynamics and classical statistical mechanics. One suspects that a good many physicists *did*, in their private thoughts, suppose the state was incomplete. That would account for the enormous interest raised by the experimental violation of Bell’s inequality, which shows that any ‘more detailed’ description would in some sense have to be non-local (the connection between this and relativity is a little vague; there are precise definitions of this non-locality, but they can take several forms and I shall not try to summarize them). On this strategy the ‘collapse of the wave function’ (see below) can be thought of as purely epistemic, reflecting a ‘change in knowledge’ of this deeper level of description. That would explain the obvious non-local character of the projection postulate. But following Bell, the deeper level of description would also have to be non-local. All the more reason, then, to seek this deeper description, and perhaps abandon relativity too (along with quantum mechanics). Or else abandon the strategy. (For a historical review of these issues, see A. Fine, *The Shakey Game*, Chicago, 1986; for a self-contained introduction, see T. Maudlin, *Quantum Non-Locality and Relativity*, Blackwell, 1994.)

⁵Bohr later formulated this principle explicitly, independent of these other ideas, but with the phrase “experimental conditions” in place of “observation.” The difference is probably marginal; his biographer Abraham Pais writes, “he sharpened his own language, one might say, *by defining the term ‘phenomenon’ to include both the object of study and the mode of observation*” (*Niels Bohr’s Times*, Oxford, 1991, p.432). Compare the physicist J. A. Wheeler: “In today’s words Bohr’s point — and the central point of quantum theory — can be put into a single, simple sentence. ‘No elementary phenomenon is a phenomenon until it is a registered (observed) phenomenon.’” (in J. A. Wheeler and W. Zurek, eds., *Quantum Theory and Measurement*, Princeton, 1981). The statements which follow are all taken from the Como lecture of 1927, reprinted in N. Bohr, *Atomic Theory and the Description of Nature*, Cambridge, 1934.

⁶J. von Neumann, F. London, and E. Wigner are the most important examples (see Wheeler and Zurek, *op cit*). Their latter-day followers include D. Albert, B. Loewer, M. Lockwood, and E. Squires; see e.g. E. Squires, *Conscious Mind in the Physical World*, Adam Hilger, 1990.

⁷The remarks are taken from Heisenberg, *op cit*; see in particular p.53-4. Heisenberg’s position is deeply compromised (cf. footnote 4), as a little reflection on the following makes clear: following a measurement interaction, “...the equation of motion for the probability function ... now contain[s] the influence of the interaction with the measuring device. This influence introduces a new element of uncertainty, since the measuring device is necessarily described in the terms of classical physics; such a description contains all the uncertainties concerning the microscopic structure of the device which we know from thermodynamics...It contains in fact the uncertainties of the microscopic structure of the whole world...It is for this reason that the results of the measurement cannot be predicted with certainty” (*ibid*).

⁸The precise connection of this with the previous notion of decoherence is a topic of great interest in decoherence theory. But there may be no good analog in relativity; it would correspond to a “natural” relation of simultaneity which is symmetric. The one situation where this occurs is given a family of co-moving frames. For some philosophical background to the latter notion, see H. Stein, *Phil. Sci.*, 58, 1991, p. 159.

⁹Only in this respect might the GRW proposal, in its present form, be ruled out on the basis of experiment. An experimental demonstration that these parameters vary, depending on the environment, would be significant, but nothing like this appears doable in the foreseeable future. Neither would it be decisive: a more complex approach, which builds in a dependence of these parameters on the state, may well be feasible.

¹⁰What must be given up is relativity as first formulated by Einstein and as developed by Minkowski. With the continuous state reduction taken as the fundamental physics, it does not respect the space-time symmetries. But since it has been obtained from a phenomenological model of a theory which does — there the distinction between space and time is quite arbitrary — there will be no superluminal signaling, and the contraction and dilation effects remain in place. The situation as goes energy conservation is a little more complicated, but from the point of view of the decoherence theory it is likewise an artifact of the phenomenological model.

¹¹The reason that quantum cosmologists nevertheless pursued such an approach is that the background interpretation was that of Everett. I remark on this below, and in the historical note.

¹²As remarked, we see this in all the GRW models. Attempts have been made to find some weakened notion of “stochastic covariance,” but they have not been very successful. That these models are also non-local is a direct consequence of Bell’s theorem.

¹³This is not quite correct. What we see is the superposition of those histories all of which agree as to what is presently actual, in the relativized sense of Everett. This is similar to the ‘supervaluational’ version of anti-realism about the past sketched by Dummett (*Truth and other Enigmas*, Harvard, 1978, p. 367), although here it has nothing to do with anti-realism.

¹⁴More realistically, we do not know if adopting the Bohm mechanics — another method for defining a unique history within the universal state, which leaves rather more of quantum mechanics intact, but once more abandons relativity — is the right strategy for making progress with quantum gravity. But as yet neither the Bohm theory nor the GRW theory can accommodate particle physics; in both cases a monolithic program of reconstruction would have to come first. The difficulties in the Bohm case are perhaps marginally less severe; it exploits a certain residue to classical mechanics present in non-relativistic quantum theory, so the problem is to import this into field theory. As follows from Bell’s theorem, this mechanism (for the definition of a unique history) is non-covariant; equivalently, this history, viewed from another reference frame, does not transform as a space-time object. In terms of the various formulations of the measurement problem that I have given in the text, we can include this option with the modification: if there is *something* which is the correlate of macroscopic states of affairs, then its dynamics is not given by the linear equations. (In fact the Bohm theory is deterministic.)

¹⁵The relevance of split-brain scenarios, and particularly the work of Parfit, should be quite obvious (D. Parfit, *Reasons and Persons*, Oxford, 1984).

¹⁶It is noteworthy that physicists have been completely indifferent to the question of ‘passage.’ It surely seems that they have no qualms with giving up the notion of personal identity (so that the problem can be given over to philosophy); it is a moot point as to whether it also indicates a flaw in the analogy.