
Three Measurement Problems

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ABSTRACT. The aim of this essay is to distinguish and analyze several difficulties confronting attempts to reconcile the fundamental quantum mechanical dynamics with Born's rule. It is shown that many of the proposed accounts of measurement fail at least one of the problems. In particular, only collapse theories and hidden variables theories have a chance of succeeding, and, of the latter, the modal interpretations fail. Any real solution demands new physics.

Richard Feynman once described his attitude toward quantum theory as follows:

[We] always have had (secret, secret, close the doors!) we always have had a great deal of difficulty in understanding the world view that quantum mechanics represents. At least I do, because I'm an old enough man that I haven't got to the point that this stuff is obvious to me. Okay, I still get nervous with it . . . you know how it always is, every new idea, it takes a generation or two until it becomes obvious that there is no real problem. It has not yet become obvious to me that there's no real problem. I cannot define the real problem, therefore I suspect there's no real problem, but I'm not sure there's no real problem. (Feynman, 1982, p. 471)

What is remarkable about this quotation is the uncertainty that Feynman expresses. At least in the philosophical literature, there seems to be general agreement that there is a central interpretational problem in quantum theory, namely the measurement problem. But on closer examination, this seeming agreement dissolves into radical disagreement about just what the problem is, and what would constitute a satisfactory solution of it.

There are, in fact, several construals of the problem, each of which has a different focus. In this paper, I would like to lay out clearly three of these construals, and to argue that other, popular, statements of the problem omit the central puzzle altogether. I do not claim any of these observations to be particularly original,¹ and will be delighted if the reader finds this account to be boringly obvious and familiar. But a

cursory survey of the literature reveals that many distinguished authors have missed the point, and I do not know of any discussion which uses just the taxonomy I will employ. As we will see by the end, if the analysis presented in paper is accepted, many proposed solutions to the measurement problem will fall by the wayside.

PROBLEM 1: The problem of outcomes

The following three claims are mutually inconsistent.

1.A The wave-function of a system is *complete*, i.e. the wave-function specifies (directly or indirectly) all of the physical properties of a system.

1.B The wave-function always evolves in accord with a linear dynamical equation (e.g. the Schrödinger equation).

1.C Measurements of, e.g., the spin of an electron always (or at least usually) have determinate outcomes, i.e., at the end of the measurement the measuring device is either in a state which indicates spin up (and not down) or spin down (and not up).

The proof of the inconsistency of these three claims is familiar. For example, a good *z*-spin measuring device must be a device which has a ready state and two indicator states (call them "UP" and "DOWN"), e.g. the state of a pointer pointing to the right ("UP") and to the left ("DOWN"). Further, the device must be so constructed that if it is in its ready state and a *z*-spin up electron is fed in, it will evolve, with certainty, into the "UP" state, and if a *z*-spin down electron is fed in it will evolve, with certainty, into the "DOWN" state. Using obvious notation:

$$\begin{aligned} |z\text{-up}\rangle_e \otimes |\text{ready}\rangle_d &\rightarrow |z\text{-up}\rangle_e \otimes |\text{"UP"}\rangle_d, \text{ and} \\ |z\text{-down}\rangle_e \otimes |\text{ready}\rangle_d &\rightarrow |z\text{-down}\rangle_e \otimes |\text{"DOWN"}\rangle_d \end{aligned}$$

What happens to this device if we feed in an electron in an eigenstate of *x*-spin rather than *z*-spin? Since

$|x\text{-up}\rangle_e = 1/\sqrt{2} |z\text{-up}\rangle_e + 1/\sqrt{2} |z\text{-down}\rangle_e$, the question amounts to what the initial wave-function

$$(1/\sqrt{2} |z\text{-up}\rangle_e + 1/\sqrt{2} |z\text{-down}\rangle_e) \otimes |\text{ready}\rangle_d$$

will evolve into. If 1.B is correct, and the evolution is linear, then this initial state must evolve into

$$\text{State } S^*: 1/\sqrt{2} |z\text{-up}\rangle_e \otimes |\text{"UP"}\rangle_d + 1/\sqrt{2} |z\text{-down}\rangle_e \otimes |\text{"DOWN"}\rangle_d$$

and the question is what kind of state of the measuring device this represents.

If 1.A is correct, and the wave-function is *complete*, then this wave-function must specify, directly or indirectly, every physical fact about the measuring device. But, simply by symmetry, it seems that this wave-function cannot possibly describe a measuring device in the "UP" but not "DOWN" state or in the "DOWN" but not "UP" state. Since "UP" and "DOWN" enter symmetrically into the final state, by what argument could one attempt to show that this device is, in fact, in exactly one of the two indicator states?

So if 1.A and 1.B are correct, 1.C must be wrong. If 1.A and 1.B are correct, z -spin measurements carried out on electrons in x -spin eigenstates will simply fail to have determinate outcomes. This seems to fly in the face of Born's rule, which says that such measurements should have a 50% chance of coming out "UP" and a 50% chance of coming out "DOWN". But that is an additional problem, which we still take up later. In any case, the three postulates have been shown to be inconsistent.

A few comments are in order. When, in 1.A, we say that the wave-function is *complete*, we mean simply that all physical properties of a system are reflected, somehow, in the wave-function. It follows that two systems described by identical wave-functions would be physically identical in all respects. We do not assert in any deep sense that the wave-function is *real*, nor do we assert that physically identical systems must have mathematically identical wave-functions. As an analogy, in classical electromagnetic theory, the scalar and vector potentials are not, in some sense, *real*. Physical reality belongs to the electric and magnetic fields, and the gauge freedom in the vector and scalar potentials was not understood as a real degree of physical freedom. Different potentials which give rise to the same fields were taken to be physically equivalent. But even though the classical potentials are not real, they are complete: two systems described by the same potentials are

electromagnetically identical. There is nothing *more* to the state of the fields than is represented in the potentials. So we are not here concerned with the physical *reality* (in some sense) of the wave-function but with its representational completeness.

Since the problem of outcomes derives from the incompatibility of three claims, proposed solutions to it can be taxonomized by which claim or claims they abandon. Theories which abandon 1.A are generally called *hidden-variables* theories, since they postulate more to physical reality than is represented in the wave-function. The name is, however, quite tendentious and misleading. In Bohm's theory, for example, the extra variables (particle positions) far from being hidden are quite manifest. The positions of particles is what we can easily see. So to avoid the connotation buried in "hidden," let us call these *additional* variables theories.

Theories which abandon 1.B are generally called *collapse* theories. Since they deny that the evolution of the wave-function is always linear, they must assert that it is, at least sometimes, non-linear. If the evolution is generally linear and only occasionally non-linear (as in the original Spontaneous Localization theory of Ghirardi, Rimini, and Weber, 1986), then the brief non-linear episodes can reasonably be called collapses. But the theory might not take such a form. The Continuous Spontaneous Localization Theory of Perle (1990), for example, has an evolution which is *always* non-linear. So a less tendentious name for these theories is *non-linear* theories.

Theories which retain 1.A and 1.B while abandoning 1.C are not common. The salient example is the so-called Many-Worlds theory, which in some sense arises from Everett's Relative State interpretation. In Many-Worlds talk, at the end of the measurement, the measuring device indicates *both* outcomes, one in one world and one in another. We could reasonably call any such theory a *multiverse* theory. Of course, one could also deny 1.C in another way, by claiming that the measuring device doesn't indicate anything at all at the end of the measurement. We might call this a *nulliverse* theory. Everett's original paper (1957), when read carefully, seems to contain a nulliverse theory, although he seems unaware of it.²

A solution of this first measurement problem, then, must of necessity be either an additional variables theory, a non-linear theory, or a multiverse theory (or some combination of these). Each of these options

carries with it an obligation, the discharging of which demands the postulation of *new physics*. The measurement problem is sometimes portrayed as merely philosophical, or of no interest to physics proper. This is quite untrue.

If one opts for an additional variables theory, one must specify *what the additional variables are* and *what laws govern them*. If one says there is more to a physical object than is captured in the wave-function, the immediate physical problem is to state what more (or what else) there is and how it behaves. In Bohmian mechanics, the additional variables are particle positions and the dynamics of these variables is provided by Bohm's equation. This is new physics. In the modal interpretations, the additional variables are what van Fraassen (1991) calls the value states. We will return to this later.

If one opts for a non-linear theory, then one must specify exactly when and in exactly what way the dynamical evolution fails to be linear. In the GRW theory, this is given by the (stochastic) collapse equation. This is again new physics. The theory of Perle gives another example.

The "traditional" interpretation of quantum theory, if it is to be coherent, must be a non-linear theory. Bohr and the other founders explicitly rejected Einstein, Podolsky and Rosen's argument that quantum theory is incomplete (thus asserting 1.A), and they also insisted that macroscopic measuring devices be described by the language of classical physics (thus asserting 1.C, since in classical terms the pointer must point exactly one direction). So they must deny 1.B. It is no accident that von Neumann's (1955) classic presentation of the theory explicitly postulates collapses.

What the traditional theory did *not* do is state, in clear physical terms, the conditions under which the non-linear evolution takes place. There were, of course, theorems that if one puts in collapses *somewhere* between the microscopic and the macroscopic, then, for all practical purposes, it doesn't much matter *where* they are put in. But if the linear evolution which governs the development of the fundamental object in one's physical theory occasionally *breaks down* or *suspends itself* in favor of a radically different evolution, then it is a physical question of the first order exactly under what circumstances, and in what way, the breakdown occurs.

The traditional theory papered over this defect by describing the collapses in terms of imprecise notions

such as "observation" or "measurement".³ This is not much better than saying that the evolution is linear except when it is cloudy, and saying no more about how many, or what kind, of clouds precipitate this radical shift in the operation of fundamental physical law.

Denial of 1.C entails other difficulties. At the least, one must explain why it *seems* that z-spin measurements made on x-spin-up particles have determinate results, or perhaps why it *seems* that it seems that way. We will return to this in the next section.

I hope that the problem of outcomes looks familiar. It is just the usual understanding of the Schrödinger cat problem laid out in a formally exact way. Oddly enough, when so laid out, it becomes immediately evident that a fair amount of work in the foundations of quantum theory misses the mark.

The most widespread misunderstanding arises from the claim that the measurement problem has to do with *superpositions* versus *mixed states*. The state S^* is a superposition of the states $|z\text{-up}\rangle_e \otimes |[\text{"UP"}]\rangle_d$ and $|z\text{-down}\rangle_e \otimes |[\text{"DOWN"}]\rangle_d$. There is another state (which one can construct using statistical operators) which is called a *mixed* state, and which we can write $50\%[|z\text{-up}\rangle_e \otimes |[\text{"UP"}]\rangle_d] + 50\%[|z\text{-down}\rangle_e \otimes |[\text{"DOWN"}]\rangle_d]$. Let us call this state M^* . This state has slightly different mathematical properties from S^* , in that the so-called interference terms are eliminated. It is also the state we would use to make predictions if we knew that the whole system was *either* in $|z\text{-up}\rangle_e \otimes |[\text{"UP"}]\rangle_d$ *or* in $|z\text{-down}\rangle_e \otimes |[\text{"DOWN"}]\rangle_d$, and ascribed a 50% likelihood to each. It has often been claimed that the measurement problem is just the problem of explaining how the measuring device gets from the state S^* to the state M^* (see, e.g., Redhead, 1987, p. 56).

This understanding of the measurement problem has inspired several attempts to show how such an evolution might occur according to the linear dynamics (e.g. Bub, 1989; Hepp, 1972). Since the interaction of the electron with the measuring device does not lead to M^* , these attempts commonly take the form of adding in other interactions with the environment, sometimes involving very complex mathematical considerations. But even if these were to succeed, it should be obvious that the problem has not been touched. M^* , just as much as S^* , is symmetric between a state in which the device is "UP" and one in which it is "DOWN". M^* does not represent exactly one of the outcomes as occurring. The argument goes through unscathed.

Given that the state M^* is the one we would use to do calculations if there really were an outcome, but we were just unsure which it is, one may wonder why deriving M^* would not solve the measurement problem. That is, if we get M^* , why can't we use the so-called *ignorance interpretation* and say that the system is *really* in either $|z\text{-up}\rangle_e \otimes |{\text{"UP"}}\rangle_d$ or in $|z\text{-down}\rangle_e \otimes |{\text{"DOWN"}}\rangle_d$, with a 50% chance of each? The short answer is that this is affirming the consequent. Just because being ignorant justifies the use of M^* , it doesn't follow that if M^* is the state of the system, we can regard ourselves as ignorant of anything (i.e. of the real state). More bluntly, in order to use the ignorance interpretation of mixtures, *there must be something of which we are ignorant*. If 1.B holds, then even if linear evolution can lead to M^* , still we will not be *ignorant* of what the wave-function really is: it will be M^* . And if 1.A holds, then there is nothing (beside what is represented in the wave-function) for us to be ignorant of. So if we continue to hold to 1.A and 1.B we *cannot* invoke the ignorance interpretation of mixtures, even if the linear evolution leads to a mixture.⁴

A similar non-solution of the problem comes under the heading of *superselection rules* (e.g. Beltrametti and Cassinelli, 1981). The idea here is to argue that even though the wave-function never actually gets to the state M^* , still, given certain restrictions on what can actually be observed or measured, S^* may be *observationally indistinguishable* from M^* . Sometimes this is stated as the result that, given the superselection rules, certain superpositions do not exist. This way of stating the result is clearly tendentious: since observational indistinguishability is a *symmetric* relation, one could just as well say that the *mixtures* don't exist! In any case, having shown that even getting a true mixture does not solve the problem unless one *also* denies either 1.A or 1.B, this approach cannot, on its own, work. And if one *does* deny 1.A or 1.B, then it is superfluous.

One final approach deserves some notice here. When I discuss these matters with physicists, someone invariably objects that the *ensemble interpretation* of the wave-function avoids this trilemma completely. According to that interpretation, the wave-function is not intended to describe *individual systems* but only collections of systems (see Ballentine, 1970). Thus the state S^* does not describe any *individual* detector and electron in some weird state, it describes an (ideally infinite) collection of detectors and electrons, each of which are in decidedly non-weird states. In fact, one

uses something like Born's rule to interpret S^* as an ensemble of detectors half of which are indicating "UP" and half "DOWN".

But this ensemble interpretation does not avoid the trilemma – it simply directly denies 1.A. According to this approach, the wave-function is not a complete physical description of any individual detector or cat or electron. And since we are interested in individual cats and detectors and electrons, since it is a plain *physical fact* that some individual cats are alive and some dead, some individual detectors point to "UP" and some to "DOWN", a *complete* physics, which is able at least to *describe* and *represent* these physical facts, must have more to it than ensemble wave-functions. If the wave-function does not completely describe the physical states of individual cats we should seek a new physics which does.

PROBLEM 2: The problem of statistics

The three propositions in the problem of outcomes are not, *strictu sensu*, incompatible. We used a symmetry argument to show that S^* could not, if it is a complete physical description, represent a detector which is indicating "UP" but not "DOWN" or vice versa. But symmetry arguments are not a matter of logic. Since we have not discussed any constraints on *how* the wave-function represents physical states, we could adopt a purely brute force solution: simply stipulate that the state S^* represents a detector indicating, say, "UP". Then 1.A, 1.B and 1.C could all be simultaneously true.

If this seems a bit too crude, perhaps we could argue that while S^* doesn't represent a detector in a definite state, it is a practically unrealizable state. Suppose we claim that in any state of the form

$$\alpha(|z\text{-up}\rangle_e \otimes |{\text{"UP"}}\rangle_d) + \beta(|z\text{-down}\rangle_e \otimes |{\text{"DOWN"}}\rangle_d)$$

the detector indicates "UP" if $\alpha > \beta$ and "DOWN" if $\alpha < \beta$. Then the special case of $\alpha = \beta$, which yields S^* , is still problematic, but it is in some sense a set of measure zero. In any real system, perhaps, the two coefficients will be at least slightly different, defeating the symmetry argument and yielding a determinate outcome.

The ploy just described would defeat the problem of outcomes, but immediately falls prey to a second objection. Suppose that instead of feeding into the z -

spin detector an electron with x-spin up, we feed in an electron with definite spin up in a direction which differs from the z-direction by 45°. The spin state of the incoming electron, that is, is

$$\sqrt{3}/2 |z\text{-up}\rangle_e + 1/2 |z\text{-down}\rangle_e$$

The whole system would evolve linearly into

$$\text{State } S^{**}: \sqrt{3}/2 (|z\text{-up}\rangle_e \otimes | \text{"UP"} \rangle_d) + 1/2 (|z\text{-down}\rangle_e \otimes | \text{"DOWN"} \rangle_d).$$

Now by the interpretation rule suggested above, since $\alpha > \beta$, S^{**} would represent a state in which the detector definitely points to "UP". So this interpretation would imply that the experiment described should always have the same result: "UP". But the usual application of Born's rule says that the outcome of this experiment is not so determined, and that there is, in fact, a 3/4 chance of getting "UP" and a 1/4 chance of "DOWN".

Formally, the following three claims are mutually inconsistent:

2.A The wave-function of a system is *complete*, i.e. the wave-function specifies (directly or indirectly) all of the physical properties of a system.

2.B The wave-function always evolves in accord with a deterministic dynamical equation (e.g. the Schrödinger equation).

2.C Measurement situations which are described by identical initial wave-functions sometimes have different outcomes, and the probability of each possible outcome is given (at least approximately) by Born's rule.

The inconsistency of 2.A, 2.B and 2.C is patent: If the wave-function always evolves deterministically (2.B) then two systems which begin with identical wave-functions will end with identical wave-functions. But if the wave-function is complete (2.A), then systems with identical wave-functions are identical in all respects. In particular, they cannot contain detectors which are indicating different outcomes, *contra* 2.C.

Additional variables theories can solve the problem by denying 2.A. If there is more to the physical state of a system than is reflected in the wave-function, then systems with identical initial wave-functions may be physically different (with respect to the values of the additional variables), and, more importantly, systems with identical *final* wave-functions may also be physically different. Thus, the two detectors may indeed indicate different outcomes at the end. Note that this only works if the direction a pointer is pointing is

determined by the additional variables, that is, if the additional variables are *manifest* rather than *hidden*. Detectors that differ only in hidden (i.e. unobservable) physical respects would not help us at all.

Non-linear theories can solve the problem by also denying that the evolution of the wave-function is deterministic. The collapses in the traditional interpretation, as well as the GRW theory, are postulated to be irreducibly stochastic. Identical initial wave-functions evolve into different outcomes because they evolve differently.

Denying 2.C, however, is a more difficult matter. Born's rule is a central part of the quantum mechanical formalism; it is the means by which predictions are actually made. To deny 2.C is to deny the empirical heart of the theory. This is the deep reason for the unsatisfactory nature of the Many-Worlds approach. For even if we can find a way to understand the denial of 1.C, even if we could comprehend how a simple spin measurement could have many results, or none, still no sense could be made of Born's rule. Born's rule gives the probability for a spin measurement to come out "UP" rather than "DOWN". If all such measurements have no result, or if all such measurements have both results, then there is no probability at all for such an outcome.

Compare again S^* and S^{**} . On the usual interpretations, if Schrödinger evolution leads to S^* then there is a 50% chance of getting "UP" and 50% chance of "DOWN". If the evolution leads to S^{**} , there is a 75% chance of "UP" and 25% chance of "DOWN". But on the Many-Worlds picture, what marks the difference in S^* and S^{**} ? In both cases one has two outcomes, one "UP" and one "DOWN". What could Born's rule be telling us about these two cases? What do the numbers 0.75, 0.5 and 0.25 derived from the rule *mean*? Not that one world is more *likely* than another, since all will be created. Do the numbers indicate that some worlds are more *real*?

The usual approach to answering this question is as follows. First, imagine an infinite sequence of, say, z-spin measurements made on electrons. At each measurement, the world bifurcates, and every possible sequence of results appears on some branch. Now collect together the branches in which the observed long-term frequency of results matches the predictions derived from Born's rule. That is, if the measurements lead to state S^* , collect together the branches in which the frequency of "UP" results limits to 0.5; if

the experiments lead to S^{**} , collect the branches where the frequency limits to 0.75. In each case, the number assigned by Born's rule to that set of branches (the branches where quantum mechanical predictions are borne out) approaches 1. This is supposed to be important.

But to suppose that this result is of any use at all is to commit a manifest *petitio principii*. What is the *significance* of the fact that this set of branches is assigned a number that approaches 1? That they (or one of them?) are certain to *occur* while the remainder of the branches do not? Certainly not, for *every one* of the branches is equally certain to occur. That it is probabilistically certain that *I* will "go down" a branch with good frequencies? Certainly not, for "*I*" don't go down any particular branch at all; rather, *I* myself bifurcate at every branch, with one of my descendants going down each. There is, in the Many Worlds picture, simply nothing for the numbers generated by Born's rule to be probabilities *of*, and this problem is not ameliorated if those numbers approach 1 or 0. The denial of 2.C (and correlatively of 1.C) cannot be reconciled with the quantum theory as it is used to make predictions. Without also employing either additional variables or a non-linear, stochastic evolution of the wave-function, the multiverse (or nulliverse) views cannot solve our problems, and if they do invoke either of these, then the postulation of the many worlds is sheer extravagance. From here on, then, we will confine our attention solely to additional variables and non-linear (stochastic) theories.

Only the postulation of additional variables or the invocation of non-linear dynamics can solve our first two measurement problems. As J. S. Bell succinctly put it, "either the wave-function, as given by the Schrödinger equation, is not everything, or it is not right" (1987, p. 201). But simply adding some additional variable or some non-linear dynamics is not sufficient to resolve our puzzles. Putting together the two problems, we can say that whatever new physics we invent to solve the measurement problem, it must be so constructed that (a) measurements typically have outcomes and (b) probabilities are assigned to those outcomes which at least approximate the probabilities derived by use of Born's rule. These conditions supply the standard by which one can evaluate new theories.

As an example, Bohmian mechanics answers the first challenge by taking particle positions as the additional variables and by asserting that measurement outcomes

are typically determined by the positions of particles (such as those in a pointer). Since the particles have determinate positions, the measurements have determinate outcomes. Meeting the second point is a much more subtle business. It demands a close analysis of the wave-functions that are actually used in making quantum mechanical predictions (the so-called effective wave-function) and showing that, given a particular probability distribution over initial conditions of the universe, the frequencies observed typically match those derived via Born's rule from the effective wave-functions (see Dürr, Goldstein and Zanghi, 1992, for details). Since the dynamics is deterministic, Bohm's theory must ultimately rely on probability distributions over initial conditions; additional variables governed by stochastic laws could proceed differently.

Theories which retain the completeness of the wave-function face a different challenge. To solve the first problem, one must specify the conditions under which the wave-function represents an experiment with a determinate outcome (as S^* doesn't) and show that the new dynamics will typically lead to such states. To solve the second, one must further show that the probabilities for the various outcomes at least nearly approximate those derived using Born's rule.

The traditional interpretation holds that a system has a physical property exactly when its wave-function is an eigenstate of the operator associated with that property. The dynamics must therefore lead to eigenstates of the pointer observable. They do so, in the traditional story, by stipulation: measurements collapse the wave-function to one of the appropriate eigenstates. Further, they do so with the right probabilities, again simply by stipulation.

But an interpretation need not embrace this so-called eigenstate-eigenvalue rule. If we use Born's rule without any emendation, the rule looks strong in one direction: if a system is in an eigenstate of an observable, then any measurement of that observable is probabilistically certain to have a particular result. If any measurement of (say) z -spin on a given electron is certain to have the outcome "UP", then this seems quite sufficient to assert that the electron *has* z -spin up. The other direction, though, is more shaky. If statistical thermodynamics is correct, no state of a liquid is *certain* to result in a particular reading on a thermometer, since the liquid might interact with the thermometer so that heat flows from the cooler thermometer to the warmer liquid. Such behavior is highly unlikely, but possible. So one

might well assert that a system has a value for a physical magnitude even though measurements are not certain to reveal that value.

This is the route taken by GRW. The non-linear evolution of the wave-function in that theory will not lead to eigenstates of pointer position, although it will lead to states that are, in a precise sense, nearly eigenstates. GRW must insist that such near eigenstates count as outcomes, and then show that the outcomes have the right probabilities of occurrence.

PROBLEM 3: The problem of effect

The quantum mechanics of von Neumann, with collapses to eigenstates governed by probabilities derived from Born's rule, has spawned the most accurate predictions in the history of physics. The problem with it, as we have seen, is its fundamental vagueness: the notion of a measurement must be taken as an unexplicated primitive which ineliminably appears in the collapse postulate. We have been laying out the requirements for a fundamental theory to get by labor what the traditional approach secures by theft: an exact dynamics which, by virtue of the interaction of systems, yields outcomes with at least approximately the Born probabilities. So far we have mentioned two approaches which succeed: that of Bohm and that of GRW/Perle. A third class of proposals exist which might also solve the first two measurement problems, yet which run afoul of another.

I will denominate the approaches I have in mind *modal* interpretations. The leading example is the theory of van Fraassen (1991). It would be hard to give a precise characterization of the class, and perhaps not every modal interpretation falls prey to the third measurement problem, but the example will serve to illustrate how the third problem extends beyond the first two.

The modal interpretations all postulate additional variables. The wave-function (which van Fraassen calls the "dynamic" state) does not collapse, and so systems must be ascribed an additional state (the "value" state) which indicates further properties of the system. Since the wave-function does not collapse, the outcomes of measurements must be reflected solely in the value state. Two successive runs of our spin experiment may both terminate with the wave-function in state S^* while in one run the electron is found to have spin up

and in the other spin down. The difference is found in the value state. Hence we solve the problem of outcomes.

The problem is statistics as solved as follows. First, a physical characterization of a measurement interaction is offered (e.g. *ibid.*, p. 225), and then it is postulated that at the end of a measurement, the probabilities for various value states are given by Born's rule. *No other detailed dynamics for the value states are offered.* The difference between the modal interpretation and Bohm's theory lies in just this: Bohm's theory offers a universal dynamics for the additional variables, and then demonstrates that Born's rule will be (approximately) satisfied in measurements, while the modal interpretations fix probabilities for the additional variables only after measurements, and fix them by direct appeal to Born's rule.

Given these stipulations, and assuming that the characterization of measurement situations is tenable (i.e. that it picks out most of what we take to be measurements⁵) this approach obviously clears the first two hurdles. What further problems could there be?

In the traditional collapse interpretation, wave collapses serve to solve the problem of outcomes (by collapsing to states with definite outcomes) and the problem of statistics (by collapsing with the right probabilities). But the collapses play a third function: they change the state of a system and so influence its future development. The x -spin up electron which enters the z -spin apparatus could trigger either "UP" or "DOWN". But the electron which emerges does so in an eigenstate of z -spin. If the result of the first measurement is "UP", a second measurement will certainly yield another "UP", and similarly for "DOWN". The result of a measurement therefore has predictive power for the future: after the first measurement is completed we are in a position to know more about the outcome of the second than we could before the first measurement was made. Any theory which seeks to replicate the empirical content of the traditional theory should have this feature. Let us call this the *problem of effect*, to indicate the effect of the first measurement on the particle (or at least on our knowledge of the particle).

The GRW theory does not fall prey to this problem since the result of the first measurement is secured by wave-collapse. The dynamics of the wave-function then propagates the effect into the future: the wave-function of the particle when it reaches the second apparatus bears the marks of the first measurement. Bohm's theory

also avoids the problem of effect, although by a completely different route. Since the wave-function never collapses, the universal wave-function at the moment of the second measurement does not indicate the result of the first.⁶ The result of the first measurement is reflected solely in the additional variables, the particle positions. But the dynamics of those variables are so constructed that the information is propagated into the future: the particle found to be z-spin up at the first detector will certainly so be found at the second (see Albert, 1992, p. 147 ff.).

But van Fraassen's interpretation has no resources to solve the problem of effect. The result of the first measurement is not codified in the subsequent wave-function since the wave-function never collapses. What of the value state?

It may be noted that IN [the value state of the incoming particle], on this picture, plays no predictive role. The character of the initial values of the observables could at best be a symptom or clue to what the initial [dynamic] state is. The expectation and indeed character of the future is determined, to the partial extent that it is determined at all, by the dynamic state W (of the *whole* system) alone. . . . What then is the empirical significance of actual values of observables? They do not increase predictive power if added to a description of the concurrent dynamic state. In that sense they are 'empirically superfluous'. (van Fraassen, 1991, p. 277)

At the end of the first measurement, the dynamic state of the whole system is S^* . On the basis of *that* state, one could not predict whether the second measurement will result in "UP" or "DOWN". Further suppose that at the end of the first measurement, the value state of the first detector is "UP", i.e. the first measurement had an up outcome. How will this new information improve our predictive power? According to the passage just cited, not at all. Since the second measurement is not affected by the incoming values, it is not affected by the outcome of the first measurement. The problem of effect cannot be solved.⁷

Richard Healey's (1989) interpretation of quantum mechanics, although unlike van Fraassen's in many ways, suffers from the same defect. The only probabilities that appear in Healey's theory ultimately derive from the universal wave-function, which undergoes no collapses (ibid., p. 78). When a system interacts with the environment, its additional variables (which Healey calls the "dynamical state") evolve stochastically in some unspecified way, but such that at the end of the interaction the probabilities for its final state are derived

from the universal wave-function *at that time* (ibid., p. 82). But since the universal wave-function never collapses, *it* bears no marks of the history of the additional variables, and hence of the history of measurement outcomes. The only way to get quantum mechanical probabilities into the picture is for such stochastic evolution to occur, but if it does, all influence of past measurement outcomes will be destroyed.

A theory without wave collapse can only solve the problem of effect if the dynamics of the additional variables force the additional variables to carry information about the results of measurements through time. This will be an intrinsically more difficult task for a theory in which those dynamics are stochastic. It is no accident that Bohm's theory, the most successful hidden variables theory, has a deterministic dynamics, and is thereby able to derive an effective counterpart to classical collapse.

Between the three of them, the problems of measurement have fairly laid waste to the countryside. Approaches based on superselection rules or environmental interactions (à la Hepp) fail at the first, as does the ensemble interpretation. The Many-Worlds theory cannot survive the second; the modal interpretation (as expounded by van Fraassen) and Healey's interpretation fall at the third.

We are not left empty-handed. Bohm's interpretation and the GRW theory still stand, and there are others that can survive the test. But at least we can be clear about the questions that must be asked of an interpretation. Is it an additional variables interpretation whose dynamics guarantee solutions to the problem of statistics and the problem of effect? Is it a collapse theory that leads to appropriate outcome states with the right probabilities, and whose fundamental terms all have clear physical significance? If the answer in each case is "no", then commit it to the flames, for it can contain nothing but sophistry and illusion.

Notes

¹ Much of this paper is really a group effort, derived from a discussion group held in Princeton in 1992. Bas van Fraassen and Ned Hall particularly drew our attention to the second of the three problems. Needless to say, none of the participants should be taken as endorsing this account.

² Everett defines in a mathematically impeccable way the *relative* state of one system with respect to an *arbitrarily chosen* state of a second system, given the joint wave-function of the pair. If the

electron and device are in state S^* , the relative state of the device with respect to the (arbitrarily chosen) $|z\text{-up}\rangle$ state of the electron is $|“UP”\rangle$ and the relative state with respect to $|z\text{-down}\rangle$ is $|“DOWN”\rangle$, and the relative state relative to $|x\text{-up}\rangle$ is a *superposition* of $|“UP”\rangle$ and $|“DOWN”\rangle$. But since (in reality) the electron is in *none* of those states, it seems quite irrelevant what these relative states are.

³ See Bell (1990) for the definitive criticism of traditional formulations.

⁴ Bell puts this in his usual trenchant way: “The idea that elimination of coherence, in some way or other, implies the replacement of ‘and’ by ‘or’, is a very common one among solvers of the ‘measurement problem’. It has always puzzled me.” (1990, p. 25)

⁵ This is non-trivial. See Albert (1992), p. 191ff. for a criticism of, the use of the polar decomposition theorem. It is also not clear that van Fraassen’s approach to this works.

⁶ This is true of the universal wave-function. The *effective* wave-function of the particle is influenced by the first result: see Dürr *et al.* (1992).

⁷ See Maudlin (1994) for further discussion.

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