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ARTHUR FINE

Measurement and Quantum Silence

Nowadays, we have stripped Maxwell of his philosophy and retained only his equations. Perhaps we should do a similar job on quantum mechanics.

(H.R. Post 1974, p. 14.).

I. THE PROBLEM

The central problem in the interpretation of the quantum theory is how to understand the superposition of the eigenstates of an observable. To a considerable extent scientific practice here, especially as codified in versions of Bohr's Copenhagen interpretation, follows an interpretive principle that I have elsewhere called the *Rule of Silence* (Fine 1987). That rule admonishes us not to talk about the values of an observable unless the state of the system is an eigenstate, or a mixture of eigenstates, of the observable in question. With regard to the Rule of Silence, as in other matters bearing on the interpretation of the quantum theory, Einstein was one of the first to realize that there can be difficulties. They appear as soon as we look at something like an explosion; i.e., the interaction between a micro- and a macrosystem that involves the amplification of a microphenomenon to macroscopic scale (Fine 1988 Chap. 5, esp. p. 78ff.). John Bell describes the difficulty over the Rule of Silence this way.

> The 'Problem' then is this: how exactly is the world to be divided into speakable apparatus . . . that we can talk about . . . and unspeakable quantum system that we cannot talk about? (Bell 1987, p. 171)

The "Problem", of course, is the quantum measurement problem. It is set by a series of results that make up the *insolubility theorem* (Wigner 1963; Fine 1970; Shimony 1974). According to that theorem no unitary evolution of states corresponding to a measurement yields a mixed object-apparatus state in which the indicator variable on the apparatus shows definite results, even under minimal restrictions on what counts as a measurement interaction.

S. French and H. Kamminga (eds.), Correspondence, Invariance and Heuristics, 279–294.

The measurement problem poses an obstacle to what some regard as a necessary condition for an acceptable physical theory; namely, that it stand in a correspondence relation to its predecessors.

> Roughly speaking, this is the requirement that any acceptable new theory L should account for the success of its predecessor S by 'degenerating' into that theory under those conditions under which S has been well confirmed by tests. (Post 1971, p. 228)

Such a correspondence with classical physics was one of the touchstones that Einstein employed in constructing relativity, and in judging the plausibility of various proposals for new physical theories (Fine 1988, Chap. 2). Einstein's rejection of Bohm's (1952) hidden variables approach to the quantum theory, for example, was based in part on his contention that the Bohm theory did not enable one to retrieve the classical and well-confirmed account of a ball rebounding elastically between two walls. According to Einstein this violated

> . . . the well-founded requirement that in the case of a macro-system the motion should agree, approximately, with the motion following from classical mechanics. (Born 1953, p.39)

In his response to the criticism Bohm rejected the methodology of correspondence principles, allowing it some value in guiding the search for new theories, but urging that no such general considerations can provide a good basis for rejecting an existing and well-confirmed theory (*op. cit.*, pp. 18–19). In their correspondence over this issue in 1951, Bohm reminded Einstein that the quantum theory never issues in accounts of how objects are likely to behave, but rather only in accounts of what we are likely to *observe* regarding their behavior. In the Bohm theory, moreover, objects have initial values, and measurements of those objects, while they may disturb the initial values, always issue in results. In fact the Bohm theory actually satisfies a modified correspondence principle: where the classical account itself is well-confirmed, the Bohm theory 'degenerates' into the classical account of *what we are expected to observe* under well-defined conditions of observation. Given the fundamental role of measurement in the quantum theory, this 'observational' principle would seem to be the proper version of correspondence there. Unless we simply ignore the measurement problem, however, the quantum theory does not satisfy even this modified correspondence principle. For the insolubility theorem makes highly problematic indeed just "what we are expected to observe". According to the Rule of Silence, it may be nothing.

Despite the failure of general correspondence between classical mechanics and the quantum theory, Heinz Post does not want to regard the development of the quantum theory as running counter to his correspondence-driven heuristic. Instead, regardless of Bohm's advice, he would blame the failure of correspondence on the quantum theory itself, which (like Einstein) he finds unacceptable on roughly realist grounds. Post looks forward to a more satisfactory and realist theory that would yield a general correspondence with classical mechanics. In the sequel I explore a more pragmatic and less visionary goal; namely, the prospects for reconciling the existing and well-confirmed quantum theory with what I referred to above as "observational correspondence". This requires a constructive response to the insolubility theorem.

Responses to the insolubility theorem constitute so-called 'solutions' to the measurement problem. Generally these responses sacrifice the Rule of Silence by allowing talk of definite values in certain special, superposed states. In giving voice to the unspeakable these responses constitute hidden variables theories. Among them are the radical de Broglie-Bohm pilot wave theory (de Broglie 1956; Bohm 1952), as well as more conservative solutions that 'approximate' the final superposed state in a measurement by an appropriate mixture, often achieved only in the limit. Other responses seek to respect the Rule of Silence by sacrificing the unitary dynamics instead. Below I will look briefly at both kinds of solutions. First I want to discuss the problem itself, in order to frame it in just the right way. I hope that way will prepare us for a rather different kind of approach. It is an approach that satisfies the desire that "fundamental theory permit exact mathematical formulation" (Bell *op. cit.*, p. 171). Bell recommends this objective as an antidote to the loose pragmatism of the quantum theory, and if one's reservations are actually about looseness, then my approach may help. If the reservations are about pragmatism, however, then because my approach is also pragmatic it may not help enough. In that case we will still have a problem, although I am not sure whether this problem would concern physics or the philosophy of physics. Perhaps it does not matter.

I frame my discussion in terms of the most familiar example, that of a Stern-Gerlach measurement of a component of spin.

II. THE REFORMULATION

In a Stern-Gerlach measurement a spin-1/2 particle passes through a magnetic field inhomogeneous, say, in the z-direction. The action of the field correlates the microscopic position of the particle with the spin in the z-direction, spatially separating the state into spin up and spin down components that move toward two separate luminescent screens; say, U and D. When a particle strikes a screen (U or D) it puts electrons in the screen into an excited state. The electrons quickly wind down, and as they return to their ground state they emit photons. This produces a flash of light that marks a visible spot on the screen. The relative frequency with which flashes occur in the U and D screens, for a beam of particles prepared in the same initial state ψ, yields the probability in ψ for spin-up and spin-down (respectively) in the z-direction.

A single spin measurement produces a flash on either the U or the D screen as its result. That result conveys virtually no information about the spin of the particle in the initial state. All we can conclude if, say, U flashes is that the initial state was not a z-spin-down eigenstate. In particular, nothing follows from the U flash about initial spin values. Instead of 'revealing' spin values, the measurement transfers the whole initial probability distribution for spin up or down in the z-direction to the probability for flashes in U or D. It achieves this transfer by way of an amplification that leaves a thermodynamically irreversible and accessible record. If we write the initial spin state ψ as

$$\psi = a\,|\uparrow> + b|\downarrow> \tag{1}$$

then the relative frequency of the flashes determines $|a|^2$ and $|b|^2$. The flashes do not distinguish between a beam of particles initially in the pure state ψ and a beam initially in the corresponding mixed state ρ given by

$$\rho = |a|^2\,|\uparrow><\uparrow| + |b|^2\,|\downarrow><\downarrow| \tag{2}$$

Because measurements produce results that are macroscopically accessible, the inability to distinguish between an initial pure state and the corresponding mixture is a characteristic feature of quantum measurement procedures. On the macroscopic scale there seems to be no distinction between pure states and mixtures, and hence no way of using the results of macroscopic measurements (of a single variable) to tell the difference.

One can turn this characteristic feature of quantum measurements around. That is, we can ask whether the end product of a typical measurement interaction would differ, depending on whether the initial object state were the pure state ψ or the mixture ρ. In the Stern-Gerlach experiment sketched above the end product is a series of flashes and, as we have noted, the statistics do not differ for these pure and mixed starting states. If we describe the measurement quantum mechanically, however, then there ought to be a difference. In the case where we start with ρ, the linear Schrödinger evolution produces a transition to a final mixed object-apparatus state:

$$\begin{aligned}\rho \otimes \alpha \Rightarrow \rho_F = {} & |a|^2\,|\uparrow><\uparrow| \otimes |u><u| + \\ & |b|^2\,|\downarrow><\downarrow| \otimes |d><d|\end{aligned} \tag{3}$$

where $|u>$ and $|d>$ represent states of the U and D screens in which a suitable number of the electrons in the screen glow, and α is the density operator for the specially tuned starting state of the whole measurement apparatus. In case ψ were the starting state, there would be a transition to a pure object-apparatus state

$$|\psi><\psi| \otimes \alpha \Rightarrow \rho_F + \rho_I \tag{4}$$

where

$$\rho_I = ab^*\,|\downarrow><\uparrow| \otimes |d><u| + a^*b\,|\uparrow><\downarrow| \otimes |u><d| \tag{5}$$

The term ρ_I arises from the 'interference' between the up and the down terms present in the initial pure state and absent in the mixed one. As we have seen, it does not show up in the Stern-Gerlach measurement, nor would we

expect it in any interaction that produces macroscopically accessible results. That is, whether the starting state is pure or not, what we observe in practice is the transition to the mixed state ρ_F. This is exactly what we would expect to observe from a physical point of view. This expectation is not satisfied by the quantum theory, however, which is one way of reformulating the quantum measurement problem.

In its usual formulation the measurement problem asks us to account for the fact that measurements have definite results. This suggests that something is missing from the usual story; namely, the actual registration of a result. The 'solution' then seems to require some addition to the theory, an addition that (somehow) puts results in. The reformulation above emphasizes just the opposite. Instead of suggesting that something is missing from the measurement story, that formulation emphasizes that the usual story is actually too full. If we want to accomplish a transition from an initial pure case to the right mixture, it seems that we have to lose something, not gain it. What we need to lose is the possibility of distinguishing between an initial pure case, like ψ, and the corresponding mixture, like ρ. Measurements proceed just *as if* the interactions always start out from an initial mixture, regardless of whether the initial state is pure.

III. TWO PROPOSALS

Proposed solutions to the measurement problem can be graded on how well they succeed in explaining this *as if* feature of measurements. Why do measurements proceed *as if* the initial state of the measured system were mixed? There are two kinds of proposals that seem popular in the recent literature: replacement solutions, which modify the Rule of Silence while retaining the Schrödinger evolution, and collapse ones that change the unitary dynamics.

Replacement proposals (e.g., Machida and Namiki 1988; Fukuda 1987a, 1987b; Kobayashi and Ohmomo 1990) work roughly like this. They replace (4) with

$$|\psi><\psi| \otimes \alpha \Rightarrow \rho_F + \tilde{\rho}_I \tag{4R}$$

where $\tilde{\rho}_I \to 0$ in the infinite limit of some suitable parameter (like relative time, size, or degrees of freedom – or some combination of these). The adjusted interference term $\tilde{\rho}_I$ usually results from eliminating apparatus variables that seem to do no work as indicators of the quantity being measured. (Here I follow Kobayashi and Ohmomo *op. cit.*) For example, in the Stern-Gerlach experiment the spot on the scintillation screen results from a loss of energy by the excited electrons as they make the transition to the ground state. The particular position coordinates of the electrons in the screen are not important. So, in coordinate representation, if we take the evolved state function and simply integrate over the positions of all the electrons, we can still track the energy shift. This effects the replacement of ρ_I by $\tilde{\rho}_I$. Although the argument is not usually stated this way, the fact seems to be that the neglected variables (here the position coordinates of the electrons in the screen) actually do too much work. For they couple to the interference terms in the initial pure state ψ in a way that distinguishes between interactions starting from pure states and interactions starting from mixtures. These terms need to be dropped (or traced out) in order to make sure that the measurement does not carry too much information about the initial state. Thus, the very fact that needs explaining, that information distinguishing pure cases from mixtures is lost in transit, is used to adjust the penultimate state so that it comes out the right mixture in the limit. A common criticism of replacement theories is directed at this final limiting operation (see Bell *op. cit.*, p.45). My point here is more basic. From the perspective of providing an acceptable explanation, replacement theories are circular. They use the fact they are supposed to explain (that information distinguishing pure cases from mixtures is lost in transit) to make the replacement that does the explaining. Do collapse theories fare better?

According to collapse theories (e.g., Ghirardi, Rimini and Weber 1986; Gisin 1984; Pearle 1986; Shimony, Ghirardi and Pearle 1991), there is an initial strong coupling between object and apparatus corresponding to the transition represented by (4). That interaction results in the following state

$$a\,|\uparrow\,> \otimes |\,u > +\, b\,|\downarrow\,> \otimes |\,d > \tag{6}$$

which persists for a time that depends on the size or complexity of the composing partial systems. Since one of the systems (the luminescent screen) is macroscopic (as judged by size, degrees of freedom, or some other well-defined parameter), a collapse mechanism takes over in short order. Repeated applications of the collapse single out one of the two branches of the superposition in (6), renormalized – at least approximately. In the GRW theory (Ghirardi, Rimini and Weber *op. cit.*), for instance, the collapse mechanism multiplies one branch by a Gaussian that becomes sharply peaked over time. The collapse concentrates on that branch with a probability that approaches the norm-squared of the branch itself as collapses repeat and time goes on. In the limit, then, the overall transition produces the mixture ρ_F, or a close approximation. The physical story that goes with collapse is this. In the beginning the coupling entangles the spin components of the deflected particle with the fluorescent electrons in the screen, according to the usual dynamics. There is a time limit, however, on the life of the excited electron states and when that limit is reached they spontaneously collapse to a near-point which glows, due to the photons emitted during collapse. Because of the initial entanglement, that collapse is also centered on one of the spin components and occurs at a rate that depends on its norm.

Despite the story, this account has a number of unsatisfactory features. First off, it is really not so clear that the collapse need be to a small region that coincides with a particular spin component. For, as we saw above in connection with the replacement theory, the collapse is actually a change in the energy state, which need not be localized at all. The GRW theory is open

to criticism on just these grounds (Albert and Vaidman 1988). Moreover, the concurrence of collapse probabilities with those derived from the coefficients in (5) has no physical foundation. It is simply put it in by hand in order to get the right probability distribution. Even worse, if the collapse only approximates the eigenstates $(|\uparrow\,> \otimes |\,u >)$ or $(|\downarrow\,> \otimes |\,d >)$ and hence the desired final mixture ρ_F, then in interpreting the actual superposed state of the object-apparatus system as yielding the registration of a definite value we breach the Rule of Silence. (See Albert and Loewer 1990, for a related criticism.) Thus collapse theories (like GRW) that only approximate the desired mixture are a kind of replacement theory, where the replacement mechanism consists of an on-going stochastic process.

On the other hand, 'exact' collapse theories (i.e., ones that actually achieve the right mixture ρ_F and not merely an approximation) may seem able to explain why interference between the terms in the initial pure state does not show up in the final mixture. On each measured system only one term survives, so eventually there is nothing with which to interfere. If we recall the explanatory task, however, this answer may not seem very responsive. For we wanted to understand why measurements proceed just *as if* the interactions always start out from an initial mixture, regardless of whether the initial state is pure. Information that would enable one to differentiate between initial pure states and mixtures is scattered among the various terms of an evolving superposition. Exact collapse theories propose a series of spontaneous transitions that, in time, lop off all but one of those terms. Thus exact collapse theories simply postulate the loss of information that needed explaining. In terms of providing a satisfactory explanation, this account of the loss of information, even were it strictly correct, would not differ very much from that provided by a replacement theory.

The combination of strong coupling followed by collapse amounts to the transition

$$|\psi>< \psi| \otimes \alpha \Rightarrow (\rho_F + \rho_I) \Rightarrow \tilde{\rho}_t, \tag{4C}$$

where $\tilde{\rho}_t$ is the collapsed state at time t, and $\tilde{\rho}_t \rightarrow \rho_F$ as $t \rightarrow \infty$. Once again there may be a problem over the limit. More important, however, is the replacement of the entangled state $(\rho_F + \rho_I)$ by the collapsed one $\tilde{\rho}_t$, for that is where much of the excess information is discarded. Let me put the problem here in physical terms. We know from experience that (typically) quantum systems decay. There is no way to obtain the decay from basic quantum theory without somewhere invoking the collapse of the wave packet. Collapse theories do this systematically. In providing a systematic rule for discarding information, collapse theories make a virtue of necessity. In doing so, however, they thereby forego the possibility of explaining why the information is lost. According to the details of the particular assumed stochastic process, it just is. As in the rule provided by replacement theories, this codifies the problem of loss of information without providing further physical insight into its occurrence.

Perhaps we are asking too much, however, for quantum phenomena teach humility. They teach us to look critically at the sources of our puzzlement and at our needs for explanation. They suggest the wisdom of aligning our demands for insight with the character of the phenomena themselves. In the quantum theory we learn to be rigorous in our thinking but pragmatic in our expectations. This is the lesson many have taken from the investigations of the Bell theorem, where the 'puzzling' correlations between measurement results on separated systems can be seen as basic to the physics, not necessarily in conflict with relativity or local causality, and not in need of further explanation (Fine 1989). Maybe the lesson to learn from the measurement problem is that it is better to view the loss of information as a basic feature of interactions that do in fact produce definite results, than to treat it as a phenomenon for which we require an explanation. The correct response to the issue, then, would not be to 'explain' this feature but simply to give a general and reasonably precise account of it. Perhaps improved collapse theories, or replacement theories could eventually do just that. But I doubt it.

I doubt that we are ever likely to have a really clean and general account. To be sure, one might produce a general template that characterizes the form of a measurement interaction, and then fill in particular features to suit the circumstances of special applications, using an open catalogue of options. For collapse theories this would mean adjusting the relaxation times between collapses, the variable being collapsed upon (e.g., position or energy), the exact state that emerges from the collapse, and perhaps even the probability for collapse – all depending on the circumstances (Gisin 1989). For replacement theories the open catalogue would involve specific ways of eliminating extra variables, depending on the type of detector; e.g., depending on whether the registration of a result is internally induced or the product of external fields (Kobayashi and Ohmomo *op. cit.*). If we bear in mind that even the standard dynamics is not algorithmic (in the Schrödinger evolution a specific Hamiltonian has to be supplied for each separate case) and that the correspondence rules that associate physical quantities with operators are also open-ended, it seems to me that amendments to the dynamics or alterations in the rules, however general, are unlikely to be closed and context-free. That said, however, I do have a reasonably general and fairly definite proposal to make.

IV. A PROPOSED SOLUTION

My proposal (Fine 1987 and 1992; Stairs 1992) also makes a virtue of necessity. The necessity is set by the fact that the object-apparatus system evolves during a measurement just *as if* the initial state were mixed rather than pure. My solution is to suggest that this is so because, from the perspective of the measuring instrument, the initial object state really is mixed; there is no 'as if' about it. Thus I suggest that we rethink how to apply the interaction formalism initially, rather than look for replacements or collapses further down the line. The fundamental starting principle is that if system I is in (pure) state ψ and system II in (pure) state $\underline{\alpha}$, then the composite system evolves from state $\psi \otimes \underline{\alpha}$. We know, however, that if the evolution is unitary (actually linear, or even deterministic will do; see Gisin *op. cit.*) then the two systems immediately become entangled, and we will be faced with inventing strategies for discarding information down the line in order to achieve the disentanglement necessary to produce a definite result.

Instead of worrying over how to discard information later, I suggest that we do it sooner, replacing ψ by the corresponding mixture ρ (from equation (2)). We can then let the interaction run according to the Schrödinger equation to produce the desired mixture ρ_F, as in (3). There is a physical rationale for this procedure. It is that in making a measurement we do not interact with all the variables of the measured object. We only observe the particular aspect of the object that corresponds to the variable being measured, say spin in the z-direction. If the initial object state is a superposition over eigenstates of that spin variable (e.g., an eigenstate of spin-up in the x-direction), then there is no initial 'value' of spin in the z-direction to observe, at least if we accept the Rule of Silence. That initial superposed state, however, does carry information about the z-spin; namely, its probability distribution. As emphasized in section I, determining that distribution is what counts as a spin measurement. So, I suggest that what the apparatus 'sees' in coupling to the object is only the probability distribution for the measured variable, as represented by the mixed state, and not the whole (pure) state of the object. So far as the measurement interaction is concerned, starting states that have the same distribution function for spin in the z-direction are identical. Thus the measuring instrument really couples to what is common to a whole class of equivalent states and not to a particular one (where two states are equivalent with respect to an observable if they have the same probability distribution for values of that observable). If this is a plausible story, then we make a mistake in applying the interaction formalism in a fine-grained way. We need to coarse-grain in order to respect the discriminatory capacities of the measuring instrument.

A z-spin measurement is characterized by the fact that the initial object z-spin distribution is transferred to the final apparatus indicator distribution, and by the fact that each measurement registers some one result. This characterization is purely physical. It makes no reference to observers, whether conscious or not. It is also perfectly general: any interaction that transfers probability and gets results is a measurement. To represent the interaction formally we need to take both conditions into account. We represent an interaction that transfers probability by means of a dynamical group generated from a suitable joint Hamiltonian. We represent the fact that a result is produced by the procedure of course graining; specifically, we replace the initial pure object state by the corresponding mixture, and start the Schrödinger evolution with that.

This is an 'exact mathematical formulation'. What it formulates is the idea of an interaction with just part of a quantum system, the part (or 'aspect') represented by the probability distribution for a particular observable. The usual way of deploying the interaction formalism enforces a quite unreasonable holism. By entangling the interacting state functions, that formalism makes virtually any interaction capable of reflecting every aspect of the whole system. As in the case of measurement, much less may be true, and when it is we need a way of representing the nonholistic interaction formally. My way represents probability distributions by mixed states. It retains the usual dynamics and it respects the Rule of Silence. It may be desirable to have a more general way of treating nonholistic interactions, in terms of a fuller account of what constitutes a part or aspect of a system, and how that is to be represented formally (see Fine 1987). For measurement interactions, however, the relatively simple scheme sketched above seems to suffice.

V. COLLAPSE OF THE WAVE PACKET

My reformulation of the measurement problem asks why the measurement proceeds as if the initial object state were mixed rather than pure. The answer provided above is that the measuring instrument in fact only interacts with part of the object system, a part which is adequately represented, formally, by the mixed state. So the interaction that actually occurs in a measurement is with the mixed state, and not with the whole pure one – which is why it seems to be that way. My reformulation also emphasizes that information is lost in a measurement. My account of the loss is that it occurs because the interaction is nonholistic: the apparatus only couples to a particular aspect of the object, not to the whole. What is lost pertains to aspects of the object to which the measuring device does not respond. The usual formulation of the measurement problems asks how we can account for the fact that measurements have results. My answer is that "having a result" is part of what we demand of a probability-transfering interaction in order to count it as a measurement. We represent this formally by adjusting the starting states of interactions differently, according to whether they do or do not produce 'results'. Given the right deployment of the interaction formalism, it is then trivial to show (as in equation (3)) that measurements produce results. Thus the answer to the usual formulation of the measurement problem does not lie in deriving that measurements have results (using special approximations or non-standard dynamics). The answer is contained in understanding how to use the interaction formalism.

There is a third way of formulating the measurement problem, one that John Bell has emphasized:

> [S]o long as the wave packet reduction is an essential component, and so long as we do not know exactly when and how it takes over from the Schrödinger equation, we do not have an exact and unambiguous formulation of our most fundamental physical theory. (Bell 1987, p. 51)

This third concern asks when, exactly, does the state function collapse. The answer contained in the account of nonholistic interactions is, exactly, never; there is no collapse. The Schrödinger equation always applies. A collapse seemed to be required in order to destroy interference and lose information. Reversing the paradox of classical statistical mechanics, where sensitivity to initial conditions is equivalent to forgetfulness of them (see Bell *ibid.*, p. 103), in quantum mechanics sensitivity entails complete recall. Thus to achieve forgetfulness, which is to say the appearance of a collapse, requires loss of sensitivity to the initial conditions. Loss of sensitivity is exactly what the aspect-sensitive deployment of the interaction formalism achieves.

Formulations of quantum mechanics without collapse include the pilot wave theory and the many worlds interpretation. (See Bell *ibid.*, p.117, for an exposition and comparison.) The program I advocate here has little in common with either. It is not a hidden variables account, for it strictly respects the Rule of Silence. Unlike the pilot wave, it does not privilege position variables, or any other. It does not invoke any quantum potential, as a multi-dimensional guiding field for real particles, nor does it entail non-local effects that propagate across the field, but below the level of observation, with superluminal velocities. Unlike the many worlds interpretation, measurements analyzed as above entail no splitting of universes and hence there is no need to worry about transworld communications. Bell has characterized the many worlds conception as giving an account of present correlations with present phenomena, and hence as renouncing the association of a particular present with a particular past (*ibid.*, pp. 134–5; see also Geroch 1984). My proposal for nonholistic interactions is not like that at all. For according to my proposal, just as in the usual quantum theory of measurement, the significance of a measurement is that it tells us what the probability distribution was for the observable being measured in the initial state of the object. Thus measurements are inherently backward-looking. Although they are not sensitive to the whole past, they do look back to and reflect a particular aspect of it.

VI. PUZZLE CASES

My suggestion for tailoring the interaction formalism according to the anticipated result may seem to encounter problems in cases where we can change our decision about the measurement after it is in progress. Two such puzzle cases come to mind; namely, a delayed choice double slit experiment and a Stern-Gerlach experiment where we recombine the two beams. These may seem problematic on my account since they both allow for an interference pattern to be displayed which, one might think, would have been precluded by the switch from pure case to mixture that I recommend. That thought, however, is not correct.

Consider a delayed choice experiment. A low intensity beam of particles falls on a barrier with two suitable small and separated slits. Behind the barrier, at a respectable distance, is a detecting screen. In between the barrier and the detecting screen are particle counters capable of registering whether a particle passes through the top slit or the bottom. We can turn these counters on or off at will. The decision to turn the counters on or off is made only after the particle (assuming that only one at a time enters the apparatus) has passed the slits. With the counters off the particles build up an interference pattern on the detecting screen. The usual puzzle here arises from the conception that the interference pattern requires each particle (somehow) to go through both slits. By delaying the choice of whether to switch on the counters, on this conception, we seem to be able to make the particle go through one slit, or both, after the fact. Puzzle indeed.

My treatment of the experiment goes like this. We can expand the state function of a typical particle as a superposition of eigenstates corresponding to 'passage through the top slit' and 'passage through the bottom slit'. In runs where the counters are on, we need to replace this evolved pure state (at the time just after the particle passes the barrier) by the corresponding mixture over eigenstates of passage through one slit or the other. This will yield the observed counting rate. In those runs where the counters are turned off (after passing the barrier) a result is obtained on the detecting screen. Hence on those runs we need to analyze the interaction between the particle and the screen by replacing the particle pure state, at the time it encounters the screen, by the right mixture. The mixture we want is obtained by expanding the pure state, at the time the particle encounters the screen, in approximate eigenstates of position on the detecting screen (i.e., as corresponding to a coarse-grained position operator). This yields the result that, in such runs, the particle is detected on the screen and that, overall, the interference pattern builds up there. This treatment nicely illustrates how the replacement of pure states by mixtures is tailored to the specific sensitivity of the instrument. The counters are sensitive only to position at one time (near the time when the barrier is crossed). With the counters off the detecting screen is sensitive only to the later position of the particle. That sensitivity is enough to produce the interference pattern.

What then of the puzzle? In the preceding analysis nothing is made to happen after the fact. We do not make the particle go through the slits either singly or doubly. We only measure position at one time or another. The appearance of making things to have happened arises from a decision to treat the occurrence of the interference pattern as a sign that particles reaching the detecting screen have gone through both slits. This is one way of breaking the Rule of Silence, and the delayed choice experiment shows that it is not a very satisfactory way. The Rule of Silence says that we should not ask about passage through the slits in a run where we measure the position of a particle on the detecting screen. The approach to the measurement problem that I am sketching here respects that rule. Accordingly, it has nothing to say about passage through the slits in runs where the counters are off. This is orthodox Copenhagen non-speak. It too is unsatisfactory, but not because there is a problem about measurements and their results. With regard to solving problems in the quantum domain it is useful to treat one problem at a time, or at least to try. Here again, holism may not be the best basis on which to proceed.

The delayed choice experiment, however, can be given a further twist; namely, into the 'quantum eraser' (Scully and Drühl 1982). In this version, the counters are left on but after recording a particle's position on the detecting screen we erase the information contained in the counters concerning which slit the particle has passed through. With this erasure the interference pattern is observed on the screen, although not without it. Again we seem to have made something happen (the interference pattern) after the fact (i.e., after the particles have landed on the detecting screen). More importantly, in terms of my suggested analysis of measurement interactions, in an erasure experiment the measurement interaction may seem to be exactly the same as that in an experiment with the counters on but without erasure; namely, sensitive to position around the slits. But if that were the case, according to the preceding analysis, interference should not show up on the detecting screen. Consider, however, a time-reversed erasure. That is, suppose we first set the counters on, then we immediately erase that information, then we finally record the particle on the screen. The combination of turning the counters on and immediately erasing the count information amounts to an interaction sensitive only to position around the detecting screen, and hence in this case (according to the delayed choice analysis) we can demonstrate that the resulting pattern should show interference. If this is correct, then what difference does it make when the information is erased? When the whole measurement interaction is taken into account the net result is the same.

In an erasure experiment, just as in a time reversed erasure, we get a composite interaction with the measured object that is sensitive only to its position on the detecting screen. Thus the quantum erasure does not pose a difficulty for our analysis of measurement in terms of restricted sensitivity. It merely serves to highlight that in determining the range of sensitivity of an interaction we need to take into account the whole experimental arrangement. This is another feature of scientific practice in the quantum domain that Copenhagen has emphasized.

It is now straightforward to deal with a Stern-Gerlach experiment where the beams are combined after passage through the magnets but before a record is produced on the luminescent screens, for this experiment is similar to a double slit experiment with the counters turned off. We display interference between the two beams after recombination by measuring, say, spin in the x-direction. That measurement requires an interaction sensitive only to the x-spin distribution, and hence it is to be treated as starting from a state mixed over x-spin eigenstates. That mixture displays the interference between z-spin components that we get from the recombination of the beams, assuming that no z-spin has already been recorded. Nothing in the results of this treatment differs from the quantum theory, so nothing can go wrong with the analysis – that is, unless something is wrong with the quantum theory itself.

VII. CONCLUDING REMARKS

My focus has been the measurement problem. As suggested in section I, it is usually thought that the problem arises out of a conflict between linear dynamics and the Rule of Silence. Linearity entangles the object and apparatus states, and the Rule of Silence applied to such entangled states forbids us from attributing a definite result to the interaction. I have shown that there is a significant third player in the genesis of the problem; namely, the application of the interaction formalism itself. Revising the rules for using that formalism provides a way out of the conflict, a way that respects the usual dynamics and the usual interpretive practices.

My way out trades on the idea that some interactions are sensitive only to certain aspects of a system, not to the whole thing. This nonholistic conception calls for a way of treating interactions with only part of a system. Where the part corresponds to the probability distribution for an observable, I suggest we represent it by a density operator over the eigenstates of the observable, one whose coefficients yield the probability distribution in question. This gives an objective, non-ignorance interpretation to mixed states for a single system. They represent 'parts' or 'aspects' of the system. This way of interpreting mixtures (in the context of an interaction with a system part), and a more general investigation of parts or aspects of a system, seems to me worth pursuing independently of its utility in reconceptualizing measurement interactions.

The basic scheme I have pursued is this: a measurement of an observable on a system in state ψ is an interaction with the part of the system corresponding to the probability distribution for the observable that is given by state ψ. This conception of measurement is purely physical. It involves no 'observers'. When the interacting parts are represented by mixed states, this conception of measurement uses only the language of elementary quantum theory, where it can be given a general and precise mathematical treatment.

There is a long tradition that deplores the introduction of the concept of measurement as fundamental in the quantum theory. Einstein belongs to this tradition, as does Bell. The sticking point over measurement seems to relate to realism, and especially to concerns over objectivity. If the concept of measurement is what I make of it here, however, then I think that we need not worry about objectivity. No observers are required to make individual results definite. Interaction with part of a system makes results definite, although no particular one. Observers are not needed to collapse the wave

packet, either; for the packet never collapses. The results of measurement, taken collectively, are not created by the measurement; for what measurements reveal (collectively) are aspects of the object already present in the initial, undisturbed state (namely, probability distributions). In all of these ways, the quantum theory is objective. My treatment of measurement helps to bring these objective features out in the open. Of course the quantum theory, like any other, has to be understood and applied by human beings. In focusing the treatment of measurement on the way the interaction formalism is applied, we highlight a pragmatic element present in all theories. This does not implicate special features of the quantum theory, with regard to objectivity, that need worry us.

It seems, then, that we can reconcile the quantum theory with the sort of observational correspondence discussed in section I; i.e., that the insolubility theorem need not stand in the way of a correspondence with the confirmed observational predictions of classical mechanics. As Heinz Post used to remind me, however, there is a conservation law for problems with the quantum theory. When we seem to dispose of one, another pops up; just as sweeping an object under the rug merely moves it from one place to another. Thus one may not be content with observational correspondence, requiring a more general and realist version instead. Such realist concerns relate to the Rule of Silence which, as we have seen, need not stand or fall with a solution to the measurement problem – our concern here. I will only say to Heinz that realism is a topic for another time (e.g. Fine 1988, Chap. 9).

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SIMON SAUNDERS

To What Physics Corresponds*

I

In what follows I wish to reconsider certain ideas to be found in Post's (1971) defence of the 'retentionist' or 'accumulativist' view of science. In particular I shall focus on heuristics and methodology and will confine the discussion to physics, specifically to theories of *dynamics* (this, I hazard, is to be counted a constraint in principle: it seems unlikely that similar considerations will apply to any other branch of empirical science). Post's thesis (what he calls the "generalized principle of correspondence") is both historical and methodological; it may be simply put as the claim that what is taken over from preceding theories is not only those laws and experimental facts which are well-confirmed, but also 'patterns' and 'internal connections', that in this way the successor theory accounts for whatever success its precursor enjoyed, for it ". . . will in fact embody a good deal of the (lower) theoretical structure of the [precursor] theory." (1971, p. 229).

By 'dynamics' I mean to include statics and kinematics, as well as mechanics and field theory. The 'constraint in principle', as I understand it, is that in no other field does one see so powerful an interplay between mathematics and phenomenology, and only in mathematics has one the resources to elaborate a notion of 'patterns' and 'internal connections' that is something more than the generic concept of metaphor. For these reasons I shall further consider only those dynamical theories that achieved an internally consistent, systematic, and highly mathematical formulation, with a substantive and well-confirmed body of quantitative applications (with the exception of astronomy and statics, we are therefore limited to the Modern period). My principal target is the 'anti-accumulativist' (or 'anti-retentionist') consensus that has, by and large, replaced the traditional reductive account of inter-theory relationships that we owe to the positivists. This consensus appears a haphazard and perhaps temporary convergence of a number of themes in contemporary metaphysics and epistemology, ranging from social constructivism and his-

* Dedicated to Heinz Post on the occasion of his 75th birthday.

S. French and H. Kamminga (eds.), Correspondence, Invariance and Heuristics, 295–325.