The Quantum Mechanics of Minds and Worlds

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8

MANY HISTORIES

THERE is a sense in which simple interference effects are destroyed when a system's environment becomes correlated with its state. This phenomenon is called *decoherence*. I shall consider three approaches here. According to one, decoherence alone explains why we get determinate records. According to another, decoherence helps one to formulate a satisfactory interpretation of Everett by selecting a *globally* preferred basis that makes the right physical facts determinate in each Everett branch. And according to the third, decoherence selects a *locally* preferred basis for each observer that makes the right physical facts determinate *from the perspective of that particular observer.*[1]

There is a long tradition of arguing that although an observer will typically end up in an entangled superposition of recording mutually incompatible measurement results, decoherence effects will destroy the interference effects and thus provide the observer with an effectively determinate record. I shall argue, however, that environmental decoherence does not by itself explain our determinate records.

A more modest use for decoherence considerations is in determining an objectively preferred set of alternative histories in the context of a many-histories (or many-threads) formulation of quantum mechanics. If decoherence considerations select a single, objectively preferred set of possible histories and if observers typically have determinate measurement records in these histories and if the theory can explain why *we* should expect *our* history to exhibit the usual quantum statistics, then one would have a many-histories theory that was better than those discussed earlier since one would not have to choose a special preferred physical property

[1] See Zeh (1970) for an early discussion of decoherence in the context of Everett. We have already seen, however, that Everett did not think that decoherence effects were necessary to explain our determinate experience. Further, as far as I can tell, Everett never used decoherence effects to argue for a physically preferred basis; indeed, whenever he said anything about the subject, he always maintained that one's choice of basis was entirely arbitrary. But it seems to me that whether a decoherence formulation of Everett can be made to work is more important than whether such a formulation is a historically accurate reconstruction of what he wanted. Consequently, in this chapter we will sometimes find ourselves even farther from what Everett himself actually said than usual.

as always determinate. I shall argue, however, that one encounters similar problems in choosing a preferred set of histories as one does in choosing a preferred determinate property.

An even more modest use for decoherence considerations is in specifying a rule for what physical properties are determinate for each observer individually at a time given the current quantum-mechanical state. The hope here is that such considerations will select the observer's most immediately accessible records as determinate precisely when they need to be determinate in order to account for the observer's experiences and beliefs. One would then know what needs to be added to the usual quantum-mechanical description in order to describe the observer as having a particular determinate measurement record. I believe, however, that there are rather serious problems with each of the rules that have been cooked up so far. I shall briefly mention some characteristic problems at the end of the chapter.

8.1 *Interference effects and the environment*

Consider two interference experiments.

The first experiment is the two-slit interference experiment we started with in Chapter 1. A source emits one electron per second, these travel past a barrier with two slits A and B, and strike a phosphorescent screen. Suppose that the source and barrier are such that each electron ends up in a superposition of passing through slit A and passing through slit B. Suppose also that nothing in the environment becomes correlated to an electron's position until it hits the screen. In this case, each second there will be a small flash of light somewhere on the screen showing where the electron hit. If one marks each of these points, one will eventually observe an interference pattern on the screen. The pattern will be different from what one would get by randomly blocking one of the slits and thus forcing each electron to go through one slit or the other. As we saw, such interference behaviour led to the standard interpretation of states where one concludes that an electron did not go through slit A, it did not go through slit B, it did not go through both, and it did not go through neither; rather, it was in a *superposition* of going through slit A and going through slit B.

The second experiment is similar to the first except that a thin conducting loop is placed around slit A so that a current will be induced in the loop if and only if an electron passes through slit A. In this case one *will not* observe an interference pattern on the screen; rather, the pattern one gets on this experiment will be perfectly compatible with each electron either going through slit *A* or going through slit *B* (Fig. 8.1). This second experiment shows that there is a sense in which a system no longer exhibits quantum-mechanical behaviour when its environment becomes correlated with its state (when the current in the wire loop becomes correlated with the positions of the electrons). Given our observations of the electrons at the screen, one might feel more comfortable saying that each electron passed through one slit or the other in the second experiment than one would in the first—after all, one might argue, the pattern in the second experiment is precisely what one would expect to get if each electron did in fact go through one slit or the other.

This destruction of simple interference effects by environmental correlations is called *decoherence*. The basic argument that decoherence explains our determinate experience goes like this: just as the environmentally correlated superposition in the second experiment is empirically indistinguishable from a state where the electron passes through either one slit or the other, an environmentally correlated superposition of different measurement records is empirically indistinguishable from a particular measurement record. While there is something seductive about such an argument, I shall argue that it simply does not work. But we first need to

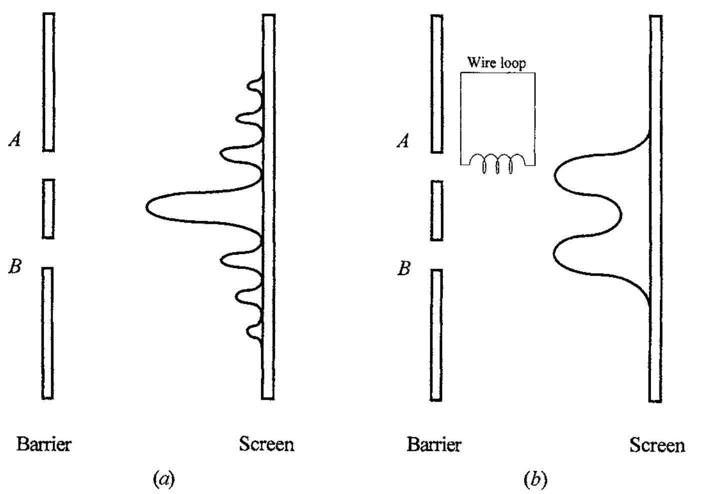

FIG. 8.1 How environmental correlations destroy simple interference effects. (*a*) The interference distribution. (*b*) With a wire loop at *A*.

consider the sense in which it would be very difficult to perform an experiment that would distinguish between a state where a measuring device recorded a superposition of results and a state where it recorded a single determinate measurement result.

8.2 *The sense in which it is difficult to distinguish pure states from mixtures*

Consider another Wigner's-friend story. Suppose that our friend F is ready to measure the x-spin of an electron S in an eigenstate of z-spin. Suppose further, as we have been supposing in such stories all along, that F's brain, where he records his measurement result, is perfectly isolated so that nothing in the environment of $F+S$ gets correlated with F's measurement record.[2] Since the electron is in a superposition of the x-spin states, and since F is a good observer, the usual linear dynamics predicts that F's brain will end up correlated with the x-spin of the electron: and S is initially in the state described by

$$\frac{1}{\sqrt{2}}(|\uparrow\rangle_F|\uparrow\rangle_S + |\downarrow\rangle_F|\downarrow\rangle_S). \tag{8.1}$$

Now consider an observable A of the composite system $F + S$ that has precisely this state as an eigenstate corresponding to eigenvalue $+1$ and everything orthogonal as eigenstates corresponding to eigenvalue -1. Since neither $|\uparrow\rangle_F|\uparrow\rangle_S$ nor $|\downarrow\rangle_F|\downarrow\rangle_S$ is an eigenstate of A, if one could make A-measurements of the composite system (or better, a collection of such systems), then one would be able to tell whether F's interaction with S was correctly described by the usual linear dynamics or the collapse dynamics: if a collapse occurred when F made its x-spin measurement, then an A-measurement might yield either $+1$ or -1, each with probability $1/2$; but if no collapse occurred, then an A-measurement would with certainty yield $+1$.

This suggests that one might simply go out and decide once and for all whether the friend's measurement of the electron leads to the superposition predicted by the linear dynamics or the statistical mixture predicted by the collapse postulate. In addition to the fact that F's brain is an

[2] This is, of course, obviously false. As Matthew Donald likes to say, real brains are warm and wet. See Donald (1996) for a rather more sophisticated model of mental states and processes than what I have been using here. Donald appeals to decoherence effects to formulate a version of quantum mechanics akin to a many-minds theory.

extraordinarily complex system, there is something else that would make it virtually impossible for us to make a measurement of $F + S$ that would distinguish between the superposition and the statistical mixture: if F is anything like a real observer, then F's environment will almost surely become correlated with F's brain state in one way or another and this will destroy the particular interference effect measured by A.

Suppose that the usual linear dynamics correctly describes the interaction between F and S but that the position of a single particle P becomes well-correlated with F's brain record of his x-spin measurement so that the final state is not

$$|\text{ideal}\rangle_{P+F+S} = |0\rangle_P \frac{1}{\sqrt{2}}(|\uparrow\rangle_F|\uparrow\rangle_S + |\downarrow\rangle_F|\downarrow\rangle_S), \tag{8.2}$$

a state where P can be ignored in the context of measurements of $F + S$ because it is correlated with the state of neither F nor S, but

$$|\text{real}\rangle_{P+F+S} = \frac{1}{\sqrt{2}}(|0\rangle_P|\uparrow\rangle_F|\uparrow\rangle_S + |1\rangle_P|\downarrow\rangle_F|\downarrow\rangle_S), \tag{8.3}$$

a state where $F + S$ no longer has a determinate state of its own and P can no longer be ignored when considering measurements involving F or S. The problem is that the A-measurement described above would fail to distinguish between the superposition of records represented by $|\text{real}\rangle_{P+F+S}$ and the statistical mixture of records represented by probability $1/2$ of $|0\rangle_P|\uparrow\rangle_F|\uparrow\rangle_S$ and probability $1/2$ of $|0\rangle_P|\downarrow\rangle_F|\downarrow\rangle_S$. In either case, there would be a probability of $1/2$ of getting $+1$ for the result of the A-measurement since the norm-squared of the projection of each of these three states on $|\text{ideal}\rangle_{P+F+S}$, which is the eigenstate corresponding to the eigenvalue $+1$ of the observer A for the composite system $P + F + M$, is $1/2$.

The upshot of this is that if F's brain record becomes perfectly correlated with any physical property of anything in its environment (and one would expect this to happen very quickly with any real brain record), then an A-measurement would tell one *nothing* about whether or not a collapse occurred—the stronger the correlation the less information one would get. That is, A would now be the *wrong observable* to measure to determine whether F's interaction with S was correctly described by the linear dynamics. Of course, there would be some other observable of the composite system $P + F + S$ that would in principle allow one to distinguish between $|\text{real}\rangle_{P+F+S}$ and $|0\rangle_P|\uparrow\rangle_F|\uparrow\rangle_S$ or $|1\rangle_P|\downarrow\rangle_M|\downarrow\rangle_S$ and there is always such an A-type observable (one that would distinguish between the superposition and the statistical mixture) regardless of how complicated the interaction between $F + S$ and its environment is. The problem is that one would have to know exactly what the interaction was in order to know which A-type observable to measure, but since brains are complex systems that interact with their environments in complex ways, it is extraordinarily unlikely that anyone will ever actually perform any such experiment. This is the sense in which it would be very difficult, perhaps impossible, to perform a measurement that would distinguish between a superposition of brain states and a statistical mixture of brain states.

So the argument that decoherence effects by themselves solve the measurement problem goes something like this: Since one would generally not know what systems are correlated with F's measurement record nor how they are correlated, one would generally not know which measurement to perform to distinguish between $|\text{real}\rangle_{P+F+S}$ and a state where F has recorded a determinate result. Further, any measurement that would distinguish between such states would presumably be extraordinarily difficult to perform even if one knew what it was. Consequently, it would be virtually impossible to distinguish between the superposition $|\text{real}\rangle_{P+F+S}$ and a state where F reports a single, determinate result. Thus (it is argued) for all intents and purposes $|\text{real}\rangle_{P+F+S}$ describes a state where F has recorded a single, determinate result.

But even if it were *impossible* for us to perform an experiment that would distinguish between $|\text{real}\rangle_{P+F+S}$ and a state where the observer has recorded a determinate result, it would not follow that F has recorded a single, determinate result when he is in the state $|\text{real}\rangle_{P+F+S}$, nor would it follow that there is a particular result that it somehow seems to F that he got.[3] Further, the difficulty of making appropriate A-type measurements is presumably not the sort of thing that *could* explain F's belief that he got a determinate result. An observer typically believes that he recorded a determinate result not because it is difficult to perform an A-type measurement that would distinguish between his brain being in a superposition of recording different results and being in a statistical mixture but *because he knows what result he in fact recorded.*[4]

[3] A proponent of the bare theory might argue that it would seem to F that he got *some* determinate result, but the bare theory predicts that F will in fact fail to have determinate measurement record, and this has nothing to do with decoherence anyway.

[4] That is, we do not believe that measurements yield determinate results because of the difficulty of A-type measurements; rather, as Albert put it, this 'is the kind of thing we learn by means of direct introspection, by merely knowing that there are matters of fact about what our beliefs are' (1992: 92 n.).

In order for the theory to predict that an observer typically records a determinate result *the theory must describe the observer as in some sense having a single determinate measurement result.* But a state like $|\text{real}\rangle_{P+F+S}$ cannot be understood as a complete description of an observer with a determinate measurement record because it does not tell us what the measurement record *is*. Moreover, if he does not have one to begin with, then further correlations between the observer and his environment will do nothing whatsoever to endow him with a determinate measurement record. One has a choice: either deny that the usual quantum-mechanical state is complete and add something to the theory to specify what result the observer in fact recorded or deny that the observer recorded a single, determinate measurement result.

While the problem with the basic decoherence argument is fairly straightforward, it remains popular. And it is sometimes proposed as the solution to the determinate-experience problem in Everett's theory.

8.3 *Decoherence and determinate perceptions*

W. H. Zurek (1991) argues that while Everett's formulation of quantum mechanics represents an attempt to do away with Bohr's boundary between the classical and the quantum worlds, it still provides no adequate explanation of where and how it is decided what an observer actually perceives. But such an explanation is possible, he believes, if one considers interactions between the observer and his environment. Observers are macroscopic systems, and as such are extremely difficult to isolate from their environments. Consequently, the state of an observer's environment will typically quickly become correlated with the state of the observer (and his measuring device), and one should no longer expect the observer (or his measuring device) to exhibit the indefinite quantum-mechanical behaviour that results from being in a pure coherent state. According to Zurek, then, the decoherence of the observer's state 'imposes, in effect, the required embargo on the potential outcomes by allowing the observer to maintain records of alternatives and to be aware of only one branch' (1991: 37). Zurek thus believes that decoherence explains why an observer would determinately perceive a single measurement result when he was in fact part of a composite system that was in a complicated superposition of states corresponding to having recorded mutually incompatible results.

Let's consider how this is supposed to work in more detail. Suppose the x-spin of a particle S is recorded in the state of a single detector particle M. If both particles are well-isolated from their environments, then Zurek concludes, in agreement with the standard interpretation of states (the eigenvalue–eigenstate link), that there is no determinate post-measurement record of the result; rather, the resulting pure state is just the correlated superposition:

$$|\psi\rangle = 1/\sqrt{2}(|S_\uparrow\rangle|M_\uparrow\rangle + |S_\downarrow\rangle|M_\downarrow\rangle). \tag{8.4}$$

The statistical properties of a system in this state are represented by the pure-state density matrix for S and M (see the end of Appendix A for a brief description of this notation):

$$\begin{aligned}\rho^p &= |\psi\rangle\langle\psi| \\ &= |\alpha|^2|S_\uparrow\rangle\langle S_\uparrow|M_\uparrow\rangle\langle M_\uparrow| + \alpha\beta^*|S_\uparrow\rangle\langle S_\downarrow|M_\uparrow\rangle\langle M_\downarrow| \\ &\quad +\alpha^*\beta|S_\downarrow\rangle\langle S_\uparrow|M_\downarrow\rangle\langle M_\uparrow| + |\beta|^2|S_\downarrow\rangle\langle S_\downarrow|M_\downarrow\rangle\langle M_\downarrow|\end{aligned} \tag{8.5}$$

But after a measurement one would like the statistical properties of S and M to be represented instead by the reduced density matrix ρ^r generated by simply cancelling the off-diagonal terms of ρ^p:

$$\rho^r = |\alpha|^2|S_\uparrow\rangle\langle S_\uparrow|M_\uparrow\rangle\langle M_\uparrow| + |\beta|^2|S_\downarrow\rangle\langle S_\downarrow|M_\downarrow\rangle\langle M_\downarrow| \tag{8.6}$$

since this describes S and M as being in a statistical mixture of S being up and M recording *up* and S being down and M recording *down*, with probabilities $|\alpha|^2$ and $|\beta|^2$ respectively. In the standard collapse theory, one can think of the collapse of the state on measurement as generating just such a statistical mixture of eigenstates of the measured observable. On a no-collapse theory, however, if we ignore possible environmental correlations, we are stuck with the pure state. Or, as Zurek puts it, 'Unitary evolution condemns every closed system to "purity" ' (1991: 39). His point, of course, is that this unfortunate consequence *only* applies to closed systems.

Consider what happens in such an experiment when the pointer on the measuring device M interacts with its environment E (as would almost certainly happen with any *pointer*—after all, measuring devices are intentionally designed so that their pointers can be easily read). Suppose that M's pointer becomes correlated to the x-spin of S as described earlier and that the environment E subsequently becomes perfectly correlated with the position of M's pointer. The state of the composite system $S + M + E$ might then be represented as

$$|\psi\rangle = 1/\sqrt{2}(|S_\uparrow\rangle|M_\uparrow\rangle|E_\uparrow\rangle + |S_\downarrow\rangle|M_\downarrow\rangle|E_\downarrow\rangle). \tag{8.7}$$

And if this ever happens, if the environment ever becomes perfectly correlated to the position of M's pointer, then, while the state of the composite system $S + M + E$ is still pure, the density matrix that describes the state of just $S + M$, the state one gets by tracing over the environment, $\mathrm{Tr}_E|\psi\rangle\langle\psi|$, is the reduced density matrix for $S + M$ (which is how the quantum formalism represents the physical fact that once the environment becomes correlated with M's pointer, one will not be able to observe interference effects involving just the system $S + M$). That is, if the environment becomes perfectly correlated with M's measurement record, then the density matrix that one would use to represent the state of just $S + M$ describes *this* composite system as being in a statistical mixture rather than a pure state. So (if one completely ignores the distinction between proper and improper mixtures) one might conclude that the environmental correlation puts $S + M$ in precisely the same statistical mixture as generated by the collapse postulate in the standard theory, and one might then conclude that one has solved the measurement problem and provided an account of our determinate experience (since, one might argue, the standard collapse theory accounts for our determinate experience, and we have deduced the same empirical predictions by considering environmental decoherence). But there is something seriously wrong with all this: unlike the post-measurement state in the standard collapse theory, the post-measurement state here *does not describe the observer as having recorded a particular determinate result.* That is, if one believes that the observer did in fact record a determinate result, then, since neither tells us what the result *is*, neither the pure state nor the improper statistical mixture one gets by tracing over the environment can be taken as providing a complete physical description.

Zurek argues that the quantum measurement problem is caused by pure states being *too informative*: 'if the outcomes of a measurement are to become independent, with consequences that can be explored separately, a way must be found to dispose of the excess information. This disposal can be caused by an interaction with the degrees of freedom external to the system, which we will summarily refer to as "the environment"' (1991: 39). He believes that one has solved the measurement problem if one can explain why the interference information contained in the complete pure state can be ignored when one restricts one's attention to a single physical system strongly correlated with its environment, and he believes that this is explained by showing that the state of such a system can be represented by a reduced density matrix. That is, one has solved the measurement problem if one can find some way to dispose of the information contained in the complete pure state that tells us that a typical post-measurement state cannot be thought of as a statistical mixture of independent records.

But this is puzzling. If one understands the measurement problem as one of explaining why the observer perceives a single, determinate measurement result, which is what Zurek himself set out to do (he wanted to answer the question 'Why do I, the observer, perceive only one of the outcomes?' (1991: 37)), then a pure state like (8.7) does not provide too much information; rather, it provides too little. It is certainly true that if the environment becomes strongly correlated to the state of M's pointer, it will destroy certain interference effects, but it will not add anything to the state that will describe M as having recorded a particular determinate measurement result. Or, put another way, there is nothing about the pure state of $S + M + E$ (8.7) nor the state of $S + M$ as represented by the reduced density matrix (8.6) that tells us *which* result M recorded; and if one wants to explain why an observer ends up recording the determinate result that he in fact ends up recording, then one's theory must describe the observer as determinately recording *that result.* Consequently, if one takes the usual quantum-mechanical state as complete and takes the linear dynamics to be universally true, then since nothing in the quantum-mechanical state describes the observer as having recorded a particular determinate result, decoherence considerations cannot by themselves explain why the observer records the single, determinate result that he in fact records—and if there is no determinate record of a result, then there can be no determinate experience of the result.[5]

Zurek notes that the origin of the quantum measurement problem was the clash between the predictions of the Schrödinger dynamics and our awareness of determinate outcomes. This is why he wants to explain why observers end up perceiving only one of the many possible quantum alternatives. Environmental interactions single out a preferred basis for any macroscopic recording device, a 'pointer basis', and consequently, 'An effective superselection rule has emerged—decoherence prevents superpositions of the preferred basis states from persisting' (Zurek 1991: 40).

[5] That decoherence destroys simple interference effects does not solve the measurement problem since it does not explain the determinateness of our measurement records. It is simply wrong to suppose that a superposition of records will 'look like' a single determinate record if the simple interference effects that one might observe between the possible record states has been destroyed. In order to observe a single determinate record, there must somewhere be a single determinate record.

He concludes: that

> We have seen how classical reality emerges from the substrate of quantum physics: Open quantum systems are forced into states described by localized wave packets. These essentially classical states obey classical equations of motion, although with damping and fluctuations of possibly quantum origin. What else is there to explain? (43)

Concerning appearances, he argues that since our brains are physical systems, awareness itself becomes susceptible to physical analysis.

> In particular, the process of decoherence is bound to affect states of the brain: Relevant observables of individual neurons, including chemical concentrations and electrical potentials, are macroscopic. They obey classical dissipative equations of motion. Thus any quantum superposition of the states of neurons will be destroyed far too quickly for us to become conscious of the quantum goings-on: Decoherence applies to our own 'state of mind.' (44)

Zurek thus concludes that an observer's determinate-belief property is the one selected as determinate by decoherence effects and that this explains the observer's determinate experiences and beliefs. But again if an explanation of the observer's determinate experiences and beliefs involves describing what those experiences and beliefs are, then the second half of this is simply false.

So what about the first half of Zurek's conclusion? After all, he does provide a rule for selecting a particular physical quantity as determinate for a system at a time: whatever physical property a system's environment is in fact correlated with is determinate *for that system*. But does this rule always select a determinate property for each observer that makes his most immediately accessible measurement records determinate? At first thought, at least, this seems unlikely. The problem is not that it will not select a property, the problem is that we do not know whether it will select the *right* property. Since the property that the rule selects as determinate for an observer depends on the interaction between the observer and his environment and since his environment is constantly changing, the property selected as determinate is constantly changing as well. The brain property that is determinate just before an observer enters an airport metal-detector, for example, is different from the brain property that is determinate when he is in it. Do all such environmentally selected brain properties make an observer's records, experiences, and beliefs determinate? We need a good argument that they do, and we need to be sure that it is not circular—that is, if one wants to explain why *this* *rule* selects a determinate physical property that guarantees determinate records, experiences, and beliefs, then presumably one cannot simply argue that it must because we do in fact make measurements that yield determinate records, experiences, and beliefs.

Moreover, Zurek's rule for selecting what physical properties are determinate for a system can only be applied to *open* systems, systems with environments, and he wants to be able to discuss the determinate properties of *closed* systems like the universe (those systems that he earlier claimed were in fact condemned to purity). Zurek refers us to Murray Gell-Mann and James Hartle's many-histories interpretation for a discussion of how decoherence considerations allow one to talk about determinate histories of the entire universe: 'The many-histories interpretation builds on the foundation of Everett's many-worlds interpretation, but with the addition of three crucial ingredients: the notion of a set of alternative coarse-grained *histories* of a quantum system, the decoherence of the histories in a set, and their approximate determinism near the effective classical limit' (Zurek 1991: 42). Since many interpretations of Everett seem to call for some natural way of characterizing a mutually exclusive and exhaustive set of alternative physically possible histories *for the entire universe*, it is certainly worth considering how Gell-Mann and Hartle select determinate properties of *closed* systems (if this is what they in fact do). I shall also briefly discuss why it has proven rather difficult to find a satisfactory rule for selecting determinate properties for even *open* systems at the end of the chapter.

8.4 *Gell-Mann and Hartle's many-histories approach*

While the notion of measurement and the distinction between observer and observed have played an important role in traditional interpretations, Murray Gell-Mann and James Hartle (henceforth GH) argue that such interpretations are inadequate for discussing cosmology.

> In a theory of the whole thing there can be no fundamental division into observer and observed. Measurements and observers cannot be fundamental notions in a theory that seeks to discuss the universe when neither existed. (GH 1990: 429–30)

Of course, finding a formulation of quantum mechanics that could be applied to cosmology was one of Everett's primary concerns.

> It was Everett who in 1957 first suggested how to generalize the Copenhagen framework so as to apply quantum mechanics to cosmology. His idea was to take

quantum mechanics seriously and apply it to the universe as a whole. He showed how an observer could be considered part of this system and how its activities—measuring, recording, and calculating probabilities—could be described in quantum mechanics.

Yet they believe that Everett's formulation of quantum mechanics was incomplete.

It did not adequately explain the origin of the classical domain or the meaning of 'branching' that replaced the notion of measurement. It was a theory of 'many worlds' (what we would rather call 'many histories'), but it did not sufficiently explain how these were defined or how they arose. Also Everett's discussion suggests that a probability formula is somehow not needed in quantum mechanics, even though a 'measure' is introduced that, in the end, amounts to the same thing. (430)

GH see their project, then, as 'an attempt at extension, clarification, and completion of the Everett interpretation' with the ultimate goal of finding a 'coherent formulation of quantum mechanics for science as a whole, including cosmology' (430). The first thing they do is discuss what it is that we ought to expect from scientific theories generally and from quantum mechanics in particular.

GH believe that 'All predictions in science are, most honestly and most generally, the probabilistic predictions of the *time histories* of particular events in the universe' (1990: 428). A satisfactory formulation of quantum mechanics for cosmology, then, would allow one to assign probabilities to alternative histories for the universe as a whole. They believe, however, that these probabilities may be approximate in the sense that they do not need to satisfy the standard axioms of probability theory precisely (how one ought to understand such 'approximate' probabilities is something I shall discuss later). Moreover, they do not require a satisfactory formulation of quantum mechanics to assign even an approximate probability to every possible alternative history since 'In quantum mechanics not every history can be assigned a probability' (428).

Their many-histories theory, then, provides two rules: one rule that tells us what sets of alternative histories of the universe can be assigned approximate probabilities and another rule that tells us what these probabilities are. GH describe these rules in the context of the Heisenberg picture.

The Heisenberg picture differs from the Schrödinger picture in that in the former one thinks of the quantum state as constant and the observables as evolving. On the many-histories formulation, the quantum state of the universe is represented by a density operator ρ, and the yes–no observables, those observables that represent facts that are either true or false, are represented by projection operators that evolve according to the Heisenberg dynamics. The complete Hamiltonian for the universe $\hat{H}$ determines how an operator $P(t)$ corresponding to a particular *yes–no* question evolves:

$$P(t) = e^{(iHt/\hbar)} P(0) e^{(-iHt/\hbar)}. \tag{8.8}$$

GH then add histories. A history is a particular time-sequence of facts (represented by a time-sequence of projection operators) $[P_\alpha] = (P^1_{\alpha_1}(t_1), P^2_{\alpha_2}(t_2), \ldots, P^n_{\alpha_n}(t_n))$. By specifying a set of alternative facts at each time (i.e. by specifying a particular time-sequence of exhaustive sets of projection operators), one determines a set of *alternative histories*.[6] Thus each history in a particular set of alternative histories describes a specific fact as being realized at each time, and the set of alternative histories describes every possible time-sequence of determinate facts *in the context of that particular set.*

On the many-histories formulation there is no single set of physically possible histories; rather, there is a different set of alternative histories for each time-sequence of alternatives one might specify. Instead of providing a single probability measure over a single set of possible alternative histories, then, as one might have expected (and as is provided by the distribution postulate in Bohm's theory, for example), the many-histories formulation provides many different *approximate* probability measures over many alternative *sets* of alternative mutually decohering histories (how one ought to understand these alternative sets of alternative histories is something we shall worry about later). The empirical content of the theory is given by the probability measure associated with each set of alternative histories; but not all sets of alternative histories can be associated with even an approximate probability measure.

In the standard two-slit experiment where an electron is in a superposition of passing through slit A and B and where nothing in its environment gets correlated to its position, there is no probability measure associated with a set of alternative histories where the electron determinately passes through A on some histories in the set and determinately passes through B on other histories in the set. If one assumes that the electron either determinately passes through A or determinately passes through B, then the probability of it striking the screen in region R is equal to the sum of the

[6] Where an alternative and exhaustive set of projection operators has the property $\sum_\alpha P^k_\alpha(t) = 1$, $P^k_\alpha P^k_\beta = \delta_{\alpha\beta} P^k_\alpha$. See GH (1990: 432).

probability of the electron determinately passing through A and striking R and the probability of the electron determinately passing through B and striking R. But, because of interference effects, the probability of finding the electron in R that one calculates by supposing that the electron either passed through A or passed through B is far from the observed quantum probability; that is, if ψ_A is an eigenstate of the electron having passed through A and ψ_B is an eigenstate of the electron having passed through B, then the probability density at the screen that one gets by supposing that it passed through either A or B $|\psi_A|^2 + |\psi_B|^2$ is not equal to the observed probability, the probability that one gets by supposing that it was in a superposition of passing through A and B $|\psi_A + \psi_B|^2$. This same point might be put another way: if one tries to assign probabilities to histories that describe the electron as determinately passing through either A or B in this experiment (using GH's rule for assigning probabilities), then these probabilities will not even approximately obey the standard axioms of probability theory. Hence GH conclude that such histories cannot be assigned probabilities at all.

There are, of course, experiments where one can assign *approximate* probabilities to alternative histories. In a two-slit experiment like that described above, if the state of even a single particle in the electron's environment becomes strongly correlated to the position of the electron (as happens in the second experiment at the beginning of this chapter), then one can assign approximate probabilities to histories that describe the electron as having passed through a particular slit if one is only interested in histories involving just the electron. That is, if the electron's environment becomes correlated to the slit that it passes through, then single-particle interference effects are destroyed and one can assign approximate probabilities to alternative histories for that particle; and the stronger the environmental correlation, the better behaved the probabilities. And while the electron is not in an eigenstate of passing through A and is not in an eigenstate of passing through B, if it is in an eigenstate of the *coarser-grained observable* of passing through A *or* B, then, even without supposing that it interacts with its environment, one can assign a probability, a probability of exactly one in this experiment, to a history where the electron particle passes through either A *or* B. The point here is that while the many-histories formulation does not assign probabilities to completely fine-grained sets of histories (histories that give the precise position of every particle, for example), one can always assign probabilities to sufficiently coarse-grained sets of histories. Indeed, coarse graining will always eventually yield a set containing a single (rather uninteresting) history represented by the identity operator (to which the theory assigns probability one) (GH 1990: 432–4).

In order to give the rule for when a set of coarse-grained histories can be assigned approximate probabilities and the rule that says what these probabilities are, GH define a decoherence functional $D[\text{history}_1, \text{history}_2]$ on pairs of histories in a particular set of alternative histories:

$$D([P_{\alpha'}], [P_\alpha]) = Tr\left[P^n_{\alpha_n'}(t_n)\cdots P^1_{\alpha_1'}(t_1)\rho P^1_{\alpha_1}(t_1)\cdots P^n_{\alpha_n}(t_n)\right], \tag{8.9}$$

where the projections are time-ordered with the earliest on the inside. A set of coarse-grained alternative histories, then, is said to *decohere* when the off-diagonal elements of D are sufficiently small (I shall discuss what *sufficiently small* means later) for every pair of histories in the set. This, then, gives us one of the two rules: probabilities can be assigned to *decoherent* sets of alternative coarse-grained histories. And the other rule is easily stated: for a decoherent set of histories, the probability for each history $p([P_\alpha])$ is given by the *diagonal* elements of D:

$$\begin{aligned} p([P_\alpha]) &= D([P_\alpha], [P_\alpha]) \\ &= Tr\left[P^n_{\alpha_n}(t_n)\cdots P^1_{\alpha_1}(t_1)\rho P^1_{\alpha_1}(t_1)\cdots P^n_{\alpha_n}(t_n)\right]. \end{aligned} \tag{8.10}$$

The approximate probabilities assigned to a set of decoherent histories typically fail to satisfy the standard axioms of probability theory, but the smaller the off-diagonal elements of D, the better behaved the approximate probabilities.

GH believe that decoherent sets of alternative histories give a definite meaning to Everett's talk of branches. For a given decoherent set of alternative histories, each element in the exhaustive set of projection operators at a particular time specifies an Everett branch at that time (GH 1990: 440). Suppose the density matrix ρ representing the complete quantum state of the universe is pure: $\rho = |\psi\rangle\langle\psi|$. The state $|\psi\rangle$ can be decomposed using the projection operators that define a particular set of alternative histories:

$$|\psi\rangle = \sum_{\alpha_1\cdots\alpha_n} P^n_{\alpha_n}(t_n)\cdots P^1_{\alpha_1}(t_1)|\psi\rangle. \tag{8.11}$$

The terms on the right-hand side of the equation are approximately orthogonal because the set of histories is decoherent. GH take these terms, one term for each possible history in the particular decoherent set, to represent Everett's branches (441). The many-histories formulation of quantum mechanics then is presented as an improved version of Everett's theory where Everett's branches are understood as alternative decohering histories. But this improved version of Everett's theory is itself rather puzzling.

8.5 *Some problems*

So how are we suppose to understand *approximate* probabilities? GH tell us that such probabilities are to be understood pragmatically, and along these lines they argue that probabilities 'need obey the rules of the probability calculus only up to some standard of accuracy sufficient for all practical purposes'. And they claim that one can achieve whatever standard of accuracy one needs in a particular situation in the many-histories formulation by considering sufficiently coarse-grained histories. But the sense in which these probabilities are approximate is curious: the approximate probability that the theory assigns to an alternative history is not approximate in the sense that it is approximately equal to the actual but unknown probability for that history; rather, it is that GH believe that the probabilities associated with alternative histories in quantum mechanics must typically fail to obey the standard axioms of probability theory. That is, GH believe that 'In quantum mechanics ... it is likely that only by this means [by violating the standard axioms of probabilities theory] can probabilities be assigned to interesting histories at all' (GH 1990: 428). In particular, the probabilities assigned by the many-histories theory violate the standard sum rule—the probabilities assigned to mutually exclusive and exhaustive alternative histories typically do not add to one.[7]

If the approximate probabilities are not to be understood as approximations to the probabilities that obey the standard axioms of probability theory, then we need some other way to understand approximate probabilities here. Further, a proponent of the many-histories theory would presumably want to explain why agents who accept the theory would not end up committed to irrational action or inconsistent beliefs.

[7] As I understand their position, in the two-slit experiment, for example, there simply can be no quantum probabilities associated with the histories where the particle determinately passes through *A* or determinately passes through *B* that satisfy the standard axioms of probability theory. Similarly, since decoherence is a matter of degree, the approximate probabilities associated with a set of alternative decoherent histories are not approximations to actual probabilities that satisfy the standard axioms (standard probabilities that are somehow 'out there' but unknown); rather, they must be approximate probabilities in the basic sense that they do not quite satisfy the standard axioms of probability theory. If this is right, then GH implicitly rule out formulations of quantum mechanics like Bohm's theory where there is a standard probability measure over alternative trajectories in configuration space.

GH themselves worry about the logical consistency of assigning probabilities in situations where the histories do not decohere. In the context of the standard two-slit experiment, for example, they argue that one cannot assign probabilities to histories where the electron determinately passes through A or determinately passes through B because 'It would be inconsistent to do so since the correct probability sum rules would not be satisfied' (1990: 428).[8] It is curious, then, that they do not worry that the many-histories theory ultimately makes predictions that violate precisely these rules. While their position is clear enough, they believe that large violations of the standard axioms are unacceptable but that small violations are typically necessary in order to assign probabilities at all. But logical consistency is usually not understood as a matter of degree.[9]

Another problem concerns how we are supposed to understand *histories* in the many-histories theory. Is only one history from a particular decoherent set actual? If so, then the usual quantum-mechanical state is descriptively incomplete because it does not tell us which history this is. Or do all histories somehow exist simultaneously? But if this is right, then why do we only experience one history? Is it because different histories describe events in different worlds and *we* only inhabit one world (as in the many-histories theories described earlier)?

[8] See Griffiths (1984) for the first discussion of consistency conditions in the context of this sort of formulation of quantum mechanics.

[9] Another way to put the problem is to note that if an agent assigns probabilities that fail to satisfy the standard axioms of probability theory, then he is committed to irrational action. More specifically, one can argue that the agent would be committed to accept a bet or series of bets where he would be guaranteed to lose money *regardless of what happens*. Such a bet is called a Dutch book. There are various ways of making a Dutch book against an agent who assigns probabilities to mutually exclusive and exhaustive alternatives that do not add to one. Suppose, for example, that an agent assigns probabilities $p(a) = 0.51$ and $p(\neg a) = 0.51$ to the mutually exclusive and exhaustive alternatives a and $\neg a$ respectively (as might happen in GH's many-histories theory). Suppose one then offered the agent the following deal: Pay \$100; then if a occurs, you get \$99, and if $\neg a$ occurs, you get \$99. The agent would presumably calculate his expected return as $\$[(0.51)(99) + (0.51)(99)] - \$100 = \$0.98$, so he would accept the offer expecting to win about \$1. He would, however, be guaranteed to lose exactly \$1 *regardless of which alternative is realized*. This is not an issue of making precise measurements; if the agent is in fact committed to the alternatives being mutually exclusive and exhaustive and having the probabilities predicted by the theory, then he is committed to pay \$1 even if no one ever looks to see what actually happened. And while one might argue that a real agent would never have sufficient information about the global quantum-mechanical state to reach the conclusion that he should accept an offer like this, it would be curious if the only thing that prevented an agent from irrational action was incomplete information.

Concerning how we are to understand histories, Gell-Mann and Hartle explain that

The problem with the 'local realism' that Einstein would have liked is not the locality but the *realism*. Quantum mechanics describes *alternative* decohering histories and one cannot assign 'reality' simultaneously to different alternatives because they are contradictory. Everett and others have described this situation, not incorrectly, but in a way that has confused some, by saying that the histories are all 'equally real' (meaning only that quantum mechanics prefers none over another except via probabilities) and by referring to 'many worlds' instead of 'many histories'. (GH 1990: 455)

It seems, then, that GH do not think of the alternative histories in a particular decoherent set as describing actual events in different worlds. On the other hand, it is not clear that they take precisely one history in a set of alternative histories to be actual either. But if they do not, then it is difficult to understand the significance of the probabilities assigned to the various histories in a set. Or perhaps they are critical of Einstein's realism because they believe that there is no single matter of fact about which history describes the world (which may suggest that they ultimately have something like the relative-fact theory in mind).

Since it is unclear what is meant by a history, it is also unclear how the theory is supposed to account for our determinate records, experiences, and beliefs. GH explain that 'The answer to Fermi's question to one of us of why we don't see Mars spread out in a quantum superposition of different positions in its orbit is that such a superposition would quickly decohere' (1990: 445). But how exactly is this supposed to work? Since Mars interacts strongly with its environment, although the usual linear dynamics tells us that it is most likely in a complicated superposition of being pretty much everywhere, there are decoherent sets of histories where each history in the set describes Mars as having an almost definite position right now. But how does the existence of such sets account for us seeing Mars where we do? After all, there are *also* other decoherent sets where none of the histories describe Mars as having a determinate position now.[10]

Further, even if one sorts out how to understand alternative histories *within a particular set of decohering histories*, there is another problem: the many-histories theory does not provide *just one* set of alternative decohering histories; rather, it provides many mutually incompatible *sets* of

[10] Consider the identity history, for example.

alternative decohering histories (each with its own approximate probability measure). And this makes the interpretation of histories in the theory all the more difficult.[11] Also, in so far as the many-histories theory fails to select a single objectively preferred set of decohering histories where observers typically have determinate measurement records, it presents us with something very much like the preferred-basis problem. Just as we were faced with the embarrassment of having to choose a special preferred physical quantity for the sort of many-histories theories discussed earlier, we are now apparently faced with the embarrassment of having to choose a special preferred set of decohering histories.

But given the initial state of the universe and its energy properties, perhaps there is some way of selecting a single objectively preferred set of decohering histories. Whatever their ultimate interpretation of histories, GH seem to want something very much like this:

> It would be a striking and deeply important fact of the universe if, among its maximal sets of decohering histories, there were one roughly equivalent group with much higher classicities than all the others. That would be *the* quasiclassical domain, completely independent of any subjective criterion, and realized within quantum mechanics by utilizing only the initial condition of the universe and the Hamiltonian of the elementary particles. (GH 1990: 454)

This would, in effect, provide us with a single, objectively privileged set of alternative histories. If we also had a clear interpretation of these histories and if the histories were such that they typically described observers as having determinate measurement records and if we had an explanation why one should expect to record the usual quantum statistics, then we would have an interesting theory.

There are, however, a couple of problems with this strategy. For one thing, we do not have an objective notion of what should count as a quasi-classical domain. GH define a quasi-classical domain to be a maximally refined decoherent set of almost classical histories, so in order to provide

[11] Consider what happens. Suppose we try taking exactly one history from each alternative set of alternative histories as descriptive of *our* world (the history randomly chosen from the set with the approximate probabilities given by the many-histories theory), and hope that this set of histories is analogous to different coarse-grained descriptions of the same trajectory in phase space in classical mechanics (this is similar to a suggestion made by Bob Griffiths in conversation). The problem with this, however, is that there is no reason to expect such randomly selected histories to mesh at all. That is, in order to take each such history to be genuinely descriptive of the same world one would have to take physical facts in that world to be contingent on the level of description in a striking way: whether the Eiffel Tower is in Paris or Pittsburgh might, for example, depend on whether one considers the foundations of the structure to be a part of the tower itself.

an objective standard for what it takes to be a quasi-classical domain, one must first provide objective standards for what it takes to be a *decoherent* set of histories and what it takes for a set of histories to be *almost classical* (GH 1990: 437, 445–6). But whether a particular set of alternative histories is *decoherent* or not is a matter of degree. The smaller the off-diagonal terms of D are for histories in the set, the more decoherent the set is and the better behaved the probabilities assigned to the histories by the theory. Consequently, there is no objective matter of fact about whether a particular set is or is not decoherent; rather, it is just a matter of convention that depends on what sort of histories one is interested in discussing given one's degree of tolerance for violations of the axioms of probability theory. Indeed, GH at first take precisely this line and argue that the standard of decoherence one adopts is a matter of choice given particular pragmatic considerations: 'if a standard for the probabilities is required by their use, it can be met by coarse graining until [the decoherence conditions] are satisfied at the requisite level' (437). And in this pragmatic spirit, they never try to specify a sharp criterion for when a set of histories is decoherent. If one allows for approximate probabilities at all (in GH's sense of approximate), then I cannot see how one *could* argue for any *objective* standard for decoherence since the choice of a particular standard would presumably never amount to anything more than better- or worse-behaved probabilities. Just as significant, it is unlikely that there is any objective standard for when a set of histories is *almost* classical. And if there is no objective standard for when a set of histories is decoherent and no objective standard for when a set of histories is almost classical, then there is no objective standard for when a set of histories is quasi-classical. And if there is no objective standard for when a set of histories is quasi-classical, then we ultimately have no objective standard for selecting a single, preferred set of alternative histories.

Another problem, and I think a much more serious one, concerns how the existence of one or many quasi-classical domain(s) is supposed to account for our determinate records, experiences, and beliefs. This problem is closely connected with the problem of interpreting histories in the theory.

GH think of observers as information-gathering and -utilizing systems (IGUSes), complex adaptive systems that have evolved to exploit the relative predictability of *a particular* quasi-classical domain (1990: 425–6, 454).

> The reason that such systems as IGUSes exist, functioning in such a fashion, is to be sought in their evolution within the universe. It seems likely that they evolved

> to make predictions because it is adaptive to do so. The reason, therefore, for their focus on decohering variables is that these are the *only* variables for which predictions can be made. (454)

And if there are many quasi-classical domains, then an IGUS would somehow 'choose' or 'exploit' just one of these.

> [w]e could adopt a subjective point of view, as in some traditional discussions of quantum mechanics, and say that the IGUS 'chooses' its coarse graining of histories and, therefore, 'chooses' a particular quasiclassical domain, or subset of such domains for further coarse graining. It would be better, however, to say that the IGUS evolves to exploit a particular quasiclassical domain or set of such domains. Then IGUSes, including human beings, occupy no special place and play no preferred role in the laws of physics. They merely utilize probabilities presented by quantum mechanics in the context of a quasiclassical domain. (454)

But this talk of observers *choosing* and *exploiting* alternative, almost classical sets of histories only serves to make the status of histories even more puzzling to me. What does it mean for an observer to choose or to exploit a particular set of histories? How is this choosing and exploiting supposed to explain our determinate measurement records? Or is it? Is my experience always in fact associated with only one history? If so, then why? If not, then why does it seem that it is? One would presumably want to be able to answer such questions before claiming that we have explained our determinate measurement records, experiences, or beliefs.

8.6 *Does the environment select the right determinate quantity?*

While it is difficult to see how decoherence considerations would select a single objectively privileged set of mutually exclusive and exhaustive alternative histories for *the entire universe*, perhaps such considerations allow one to give a rule for selecting determinate physical quantities for *a particular physical system* given the global state of the universe. We want a rule that we are convinced always makes determinate precisely what needs to be determinate in order to account for our determinate measurement records, and we want this rule to work in real physical situations given the imperfections and complex environmental interactions that real observers exhibit. But finding a rule that does precisely what we want it to do is difficult.

Some rules that initially look as if they ought to work may in fact make entirely the wrong properties determinate in slightly imperfect experimental situations.[12] But perhaps more puzzling is the fact that what physical quantity a decoherence rule selects as determinate will typically depend on precisely which physical system one specifies, and this can lead to incompatible determinate properties for *nested systems*: an observer might, for example, have a determinate measurement record when his brain is the specified system but not when his whole body is the specified system.[13] How would one explain an observer's determinate experience when there is no single matter of fact about what he recorded or whether he even had a determinate record? Further, even if we settle on a canonical specification of the observing system and if we find a rule that typically makes a physical quantity determinate that is close, in an appropriate sense, to a quantity that would provide an observer with determinate measurement records (which is typically the best that a decoherence rule will be able to do), then one must also somehow argue that close is good enough to explain the determinate experiences that observers in fact have.[14] And finally, one would expect that a decoherence rule would select different properties as determinate for a given observer in different environments, so even if one had precisely the right physical property determinate at a time (a property that would make the observer's mental state determinate),

[12] For the debate concerning this problem in the context of the so-called modal theories, see Albert and Loewer (1990), Dieks (1991), Albert (1992: 191–7), Ruetsche (1995, 1998), Bacciagaluppi and Hemmo (1996*a*), and Vermaas (1998). One would expect many of the same issues to arise in the context of trying to find a decoherence rule for selecting which quantities are determinate for a system.

[13] See Clifton (1995) for a discussion of one popular modal rule where the determinate quantities for nested systems do not mesh. My point here is that there is a similar meshing problem for the determinate properties selected by decoherence rules since what is determinate depends on one's perspective. Suppose that I measure the x-spin of a system initially in an eigenstate of z-spin, and suppose that a particular decoherence rule chooses my recording quantity (or something that would make my record determinate) as determinate for me (say, whatever property of me becomes strongly correlated with my environment); for me and the rest of California (whatever property of me and California becomes strongly correlated with *its* environment); for me, the rest of California, and the earth (whatever property becomes correlated with its environment); but not for me, California, the earth, and the solar system *because there has not been sufficient time for anything outside the solar system to get correlated with my record.* Is there any absolute sense in which there is a determinate record? Is there a preferred physical system for explaining the determinateness of my experience? What is it and why?

[14] It seems to me that whether or not such an explanation works depends on the details of one's theory of mind: in particular, it depends on the precise details concerning the relationship between physical and mental states. See Bacciagalupi and Hemmo (1996*a*) for an argument that is close enough.

one would expect that that property would *not* be the one selected by the rule as determinate at other times.

There is much to say about trying to explain our determinate experience by appealing to a rule that uses the global state to select determinate quantities for individual systems, but here I would just like to suggest that it may never be obvious that a particular rule does precisely what it needs to in order to account for our determinate records, experiences, or beliefs. And we certainly have no clear argument for such a rule right now.[15]

It should be clear that decoherence does not by itself solve the measurement problem. It also seems unlikely to me that a decoherence rule would select a naturally preferred basis for the entire universe that would provide a single set of mutually exclusive and exhaustive histories or worlds. And while a decoherence rule may select a physically preferred property for an *open* macroscopic system at a time, it is not clear to me that such a rule will select the right physical property for an observer—one that would make his beliefs and memories determinate. It is certainly the case that for physical systems like brains a decoherence rule would typically select a preferred property very quickly. It is also true that, for a fixed Hamiltonian describing the interaction between the brain and its environment, one would expect that the selected property would be quite stable over time. But since the Hamiltonian that describes the interaction between our brains and the world is not fixed, it seems that we do not even have stability of the selected property, let alone a guarantee of its appropriateness. Finally, even if we did have a decoherence rule that we were convinced made a physical property that was very close to the recording brain property determinate (and very close is the best that one can expect from a decoherence rule), it is not entirely clear that close is close enough. It is, then, not yet clear, at least not to me, how decoherence effects can be used to explain our determinate records, experience, or beliefs.

[15] For the state of the art, see Dieks and Vermaas (1998); especially Guido Bacciagaluppi's paper describing how the Bohm–Bell–Vink dynamics might be used to describe the evolution of the actually possessed properties on one version of the modal interpretation. See Frank Arntzenius's (1998) contribution to the same volume for a detailed discussion of some of the problems faced by the modal theories. See also, Meir Hemmo's (1996) thesis for more details on how one might try to use decoherence effects in the context of a many-worlds interpretation.