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THE BARE THEORY AND HOW TO FIX IT

ABSTRACT

In this paper I respond to recent objections to the bare theory of quantum mechanics then explain what I take to be wrong with the theory. The main problem is that the bare theory's empirical predictions are too weak. Rather than predicting ordinary measurement results, the bare theory predicts that an observer would typically end up with what one might call disjunctive results, measurement results that fail to have any specific phenomenal content. Modal and hidden-variable theories can be thought of as alternative ways of fixing the bare theory. But they only succeed to the extent that they provide a satisfactory account of the actual experiences and beliefs of observers. I will discuss some of the problems faced by such theories.

THE BARE THEORY'S ACCOUNT OF EXPERIENCE

The bare theory is the standard von Neumann-Dirac formulation of quantum mechanics without the collapse postulate but with the eigenvalue-eigenstate link. Albert (1992, 116-125) presented the bare theory as one way of understanding Everett's relative-state interpretation. At first glance, it looks as if the bare theory cannot possibly account for our experience. After all, at the end of a measurement an observer will typically be in a superposition of having recorded mutually incompatible results, which on the standard interpretation of states (the interpretation provided by the eigenvalue-eigenstate link) is a state where the observer fails to have any determinate measurement record whatsoever.

Because of its simplicity, however, the bare theory is inherently interesting to anyone interested in no-collapse formulations of quantum mechanics. But further, it is surprising how far the bare theory can go in accounting for our experience; or, more precisely, how far the bare theory can go in explaining why we *falsely* believe that we have determinate measurement results. Rather than explain our determinate experience, one might say that the bare theory seeks to explain it away as a sort of illusion. The bare theory does this by telling us what a good observer would be disposed to believe about her own experience in various measurement situations if the theory were true (in the context of the bare theory it is crucial that one carefully distinguish between one's experiences and beliefs and one's beliefs about the determinateness of

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those experiences and beliefs). In this section I will describe just one of the bare theory's suggestive properties and explain how the theory uses this property in its account of experience. I believe that this account is ultimately unsatisfactory, but it is important to understand exactly what goes wrong in order to know how to fix the bare theory.[1]

Suppose that M is a perfect x-spin measuring device in the following sense: it is constructed so that the position of its pointer becomes perfectly correlated with the x-spin of a spin-1/2 system S without disturbing it. That is, M has the following two dispositions: (1) if S is initially in an x-spin up eigenstate and M begins in a ready-to-make-a-measurement state, then M will record that the result of its measurement is x-spin up and will leave S in the x-spin up state

$$|r\rangle_M|\uparrow\rangle \quad \rightarrow |\uparrow\rangle_M|\uparrow\rangle_S \qquad (1$$

and (2) if S is initially in an x-spin down eigenstate and M begins in a ready-to-make-a-measurement state, then M will record that the result of its measurement is x-spin down and will leave S in the x-spin down state

$$|r\rangle_M|\downarrow\rangle_S \rightarrow |\downarrow\rangle_M|\downarrow\rangle_S \qquad (2)$$

This is presumably precisely how one would want to construct a good x-spin measuring device.

According to the bare theory, the time-evolution of every physical system is always correctly described by the time-dependent Schrödinger equation. The dynamics then is both deterministic and linear. It follows from the linearity of the dynamics and from the two dispositions described above that if M begins in a ready-to-make-a-measurement state and S begins in a superposition of x-spin up and x-spin down

$$|r\rangle_M(\alpha \quad s + \beta|\downarrow\rangle_S) \qquad (3)$$

then the state of $M + S$ after M's x-spin measurement will be

$$_M|\uparrow\rangle_S + \beta|\downarrow\rangle_M|\downarrow\rangle_S \qquad (4$$

since the result of applying the linear dynamics to a linear superposition is the linear superposition of applying the dynamics to each term. If α and β are non-zero, then (4) is not an eigenstate of M recording any determinate x-spin result; rather, it is a state where M is entangled with S and in a superposition of recording mutually contradictory results. But while M would not be in an eigenstate of reporting any particular determinate result, one can argue that M would nonetheless report that it had recorded *some* determinate x-spin result, either x-spin up or x-spin down.

The description of the determinate result property in this section follows Barrett (1997)

Suppose that the measuring device has other dispositions – dispositions concerning how it makes reports about the determinateness of its own measurement records. Suppose that M is constructed in such a way that it has the disposition to answer the question "Did you get some determinate result to your x-spin measurement, either x-spin up or x-spin down?" with "Yes" if it recorded x-spin up (if $M+S$ ended up in the state $|\uparrow\rangle_M|\uparrow\rangle_S$) and with "Yes" if it recorded x-spin down (if $M+S$ ended up in the state $|\downarrow\rangle_M|\downarrow\rangle_S$). It follows immediately from the linearity of the dynamics that if M in fact recorded a superposition of the two possible x-spin results (if $M+S$ were in the state described by (4) above), then M would (incorrectly) respond to the question "Did you get some determinate result to your x-spin measurement, either x-spin up or x-spin down?" with "Yes." Moreover, M would report that its result was a "perfectly ordinary x-spin result" because this is what it would report in each of the two determinate record eigenstates $|\uparrow\rangle_M|\uparrow\rangle_S$ and $|\downarrow\rangle_M|\downarrow\rangle_S$. That is, given the way it is constructed, the linear dynamics requires that M would (falsely) report that it recorded a perfectly ordinary and determinate x-spin result.

Suppose then that the bare theory is true and that an observer begins a measurement in a situation like that described by (3) above. If the observer is competent at reporting her beliefs when she sees a pointer in an eigenstate of pointing at x-spin up and when she sees a pointer in an eigenstate of pointing at x-spin down, then, by the argument above, even when she ends up in a superposition of believing that she sees x-spin up and believing that she sees x-spin down, she will, like M, report that she got a determinate x-spin result; that is, she will answer the question "Did you get a perfectly ordinary determinate result of either x-spin up or x-spin down?" with "Yes," and, presumably, this is also what she would believe. This meta-belief would be false in the sense that the observer has not determinately recorded x-spin up and has not determinately recorded x-spin down (the state is not one like $|\uparrow\rangle_M|\uparrow\rangle_S$ or $|\downarrow\rangle_M|\downarrow\rangle_S$), but she would nonetheless believe that she just got a perfectly ordinary result to her measurement and that she knows what it is (if she would believe that she knew what her determinate result was in each of the two determinate belief eigenstates). That is, the observer would be under the illusion that she had recorded a determinate x-spin result when there was no such record anywhere. A proponent of the bare theory would argue that a significant portion of the experience of real observers might be explained by illusions of just this sort – situations where an observer (falsely) believes that she has ordinary determinate experiences and beliefs about those experiences.

In order to be clear about the way in which the bare theory seeks to account for experiences and beliefs, it is useful to distinguish between *ordinary* and *disjunctive* experiences, records, and beliefs. Suppose that an observer M measures the x-spin of a system in an eigenstate of z-spin and thus ends up in a superposition of recording x-spin up and x-spin down. It follows immediately from the standard eigenvalue-eigenstate link (and the assumption that we can talk about an observer having a belief the same way that we talk about a measuring device having a physical record) that in this state M does not believe x-spin up, does not believe x-spin down, does not believe both, and does not believe neither. Thus, a proponent of the bare theory cannot say that either M would believe x-spin up or that M would believe x-spin down after the measurement. If one insists that exactly one of these two ordinary beliefs is what M would in fact believe based on M's experience, then the bare theory cannot account for M's experience. A proponent of the bare theory, however, would deny that M would end up with either of these two ordinary determinate beliefs. A proponent of the bare theory would not say that M would believe that she recorded x-spin up or that she would believe that she recorded x-spin down, but rather that she would believe that she recorded x-spin up *or* x-spin down. One might call the experience leading to this disjunctive belief a disjunctive experience. This disjunctive experience would be phenomenally indistinguishable from either getting x-spin up *or* getting x-spin down (because this is precisely what M would be disposed to report). But it would be wrong to say that it would be phenomenally indistinguishable from getting x-spin up and it would be wrong to say that it would be phenomenally indistinguishable from getting x-spin down since the observer would not be in an eigenstate of making either of these reports. And, for the same reason, it would also be wrong to say that the disjunctive experience would be phenomenally distinguishable from getting x-spin up or that it would be phenomenally distinguishable from getting x-spin down. Again, the right thing to say is that the observer would be unable to distinguish the disjunctive result from x-spin up *or* x-spin down. The upshot is that if what one takes as standing in need of an explanation is the belief that one's experiences are perfectly ordinary and determinate, then the bare theory provides one in this sort of experimental situation.

It is important to note that the bare theory does not seek to account for the ordinary determinate experiences that we (naively) suppose ourselves to have. Rather, the bare theory denies that there typically are any such experiences, then seeks to explain why one might nonetheless believe that there were such experiences – why one would mistake disjunctive experiences for *generic* determinate experiences (*generic* because one would find a particular disjunctive experience neither distinguishable nor indistinguishable from a given *specific* associated determinate experience).

Perhaps the following experiment will help to make this distinction between ordinary and disjunctive experiences, records, and beliefs clearer. Suppose that an observer measures the x-spin of three object systems: the first is in an x-spin up eigenstate, the second is in an x-spin down eigenstate, and the third is in a superposition of x-spin up and x-spin down. The observer will believe that she has a determinate phenomenal result in all three cases. Moreover, she will believe that the result of her last measurement was phenomenally indistinguishable from the result of exactly one of her first two measurements. But the observer's disjunctive result will be neither distinguishable nor indistinguishable from getting x-spin up (the observer will not have a determinate belief concerning whether her first and third results agree) and it will be neither distinguishable nor indistinguishable from getting x-spin down (she will not have a determinate belief concerning whether her second and third results agree); rather, the disjunctive experience will be indistinguishable from either x-spin up *or* x-spin down (the observer *will* determinately believe that the result of the third measurement *is indistinguishable from exactly one of the first two measurement results*, but she will not have any determinate belief regarding *which* of the first two results it is indistinguishable from).

So if an observer can correctly identify those experiences that are perfectly ordinary and specific when she has them, then, by the linearity of the dynamics, she will report (and believe) that her disjunctive experiences are perfectly ordinary and specific. But what a disjunctive experience lacks is ordinary specific content. It is as if it were the shell of an ordinary experience but with nothing inside. It is, however, precisely this lack of ordinary specific content that an observer would be unable to detect through introspection.

The moral of all this is that if the bare theory were true, then an observer with dispositions like M's who started in a ready-to-make-a-measurement state would end up reporting and believing that her result was perfectly determinate, ordinary, and specific when it would typically be disjunctive and thus lacking any ordinary specific content. If the bare theory were true, then first-person authority concerning whether particular experiences and beliefs had ordinary specific content would be routinely violated in a striking way – an observer would typically believe that she had an ordinary determinate experience when there would in fact be no such experience that she believed that she had.

One can tell other stories in the context of the bare theory to show that ideal observers would believe that their determinate results agree (when they in fact had a no determinate results) and that each observer would, as she continues to make observations, approach a state where she would report that her determinate measurement results were randomly distributed with the usual quantum relative frequencies (when there were in fact no such determinate results.[2]

[2]Such results are suggested by Everett (1957). There have been several subsequent attempts to clarify and extend Everett's results and to determine their significance for various formulations of quantum mechanics: see Hartle (1968), DeWitt (1971), Everett (1973), Graham (1973), Albert and Loewer (1988), Albert (1992), and Barrett (1995) and (1996). The last four references are each concerned with the bare theory in one way or another.

2 OBJECTIONS TO THE BARE THEORY

Objection 1 (Weinstein 1996): The operator that is supposed to correspond to the question "Did you get some determinate result to your x-spin measurement?" in the standard sort of story one tells in the bare theory is the identity operator, but such a trivial operator cannot possibly represent asking such an interesting question.

It seems to me that this type of objection misses the point. The point of the determinate-result stories like that described in the last section is that an observer constructed like M would have the ***disposition*** to report that she got a determinate x-spin result when it was actually in an entangled superposition of recording mutually incompatible x-spin results. How one represents the observable corresponding to asking M whether she got a determinate result is irrelevant. What matters are ***the actual physical dispositions of the systems involved*** – what matters is what observers would in fact report and believe in experiments like that described in the last section if the bare theory were true. That an observer like M would report and believe that she got a determinate x-spin result follows directly from the dispositions that M would have on the bare theory if she were wired to report and believe that she recorded a determinate result in those situations where she did in fact record a determinate x-spin result.

The problem is that M is not wired in such a way that she can reliably tell us whether she got a determinate result when she is in fact in a superposition of having recorded mutually incompatible results. One might naturally wonder whether there is some way to rewire M so that she would *always* make reliable reports concerning the determinateness of her records of past measurements. It turns out that the answer is *no*. Indeed, the bare theory places a very strong constraint on the reliability of an observer: If an observer must answer a question the same way in two orthogonal states but differently in an superposition of the orthogonal states in order to answer the question correctly, then no observer can answer the question correctly in general since, by the linearity of the dynamics, she will always answer the question the same way in the superposition as she does in the orthogonal states.

Objection 2 (Bub, Clifton, and Monton 1996): Since one cannot design a universally reliable measuring device, the bare theory requires one to choose a preferred basis (the basis that tells us when a good measuring device or observer will be able to reliably answer a particular question about its own state), and this undermines one of the most compelling arguments for the bare theory, its simplicity.

There is clearly something right about this objection. In order to tell a measurement story in the bare theory, one must first decide what questions the measuring device or observer will be able to answer correctly (since it cannot answer every question correctly), and this, in effect, requires one to choose a preferred basis. But while Bub, Clifton, and Monton are right to insist that the mathematical formalism of quantum mechanics fails to tell us how to model observers and that it is conceivable that an observer might be wired differently than M (wired, for example, to reliably tell us only whether it ended up in a specific superposition of recording up and down), one should not suppose that the choice of how to model human observers is arbitrary. We tend to believe that if human observers are wired to answer any question correctly, then they are wired to answer the question "Did you get a determinate result to you measurement?" correctly *when they either determinately did or determinately did not record a result*. And we tend to believe that humans are not wired to perform the exotic sort of measurement that would reliably determine whether they are in a specific superposition of having recorded mutually incompatible results. That is, we tend to believe that real observers are wired like M and not in one of the subtle ways that they would have to be wired in order to reliably detect whether their brains were in a particular superposition of different belief eigenstates.

It also seems to me that the role played by the preferred basis in the bare theory is relatively modest. In other versions of quantum mechanics the preferred basis typically picks out one physical observable as ontologically privileged, the one and only physical property that, as a basic matter of how the physical world is put together, is always determinate. In the bare theory, however, the choice of a preferred basis has nothing to do with the fundamental nature of the physical world, but rather just represents a choice about how to correctly model observers.

Objection 3 (Bub, Clifton, and Monton 1996): Even granting the usual way that observers are modeled, the bare theory cannot account for our actual experience – we not only know that we get determinate measurement results, but we typically know what those results are, and the bare theory is flatly incompatible with this fact. That is, the bare theory simply fails to be empirically adequate since we know from our experience and direct introspection on the nature of that experience that observers typically do in fact get ordinary determinate results, not the disjunctive results predicted by the bare theory.

One might distinguish here between two possible objections: (i) the bare theory cannot account *for my belief* that I get determinate, perfectly ordinary results to my measurements and (ii) the bare theory cannot account *for the fact* that I do get determinate, perfectly ordinary results to my measurements. While I believe that objection (i) is serious in general, I think that there is good reason to suppose that it is simply false of the sort of idealized experiment considered in the last section. Objection (ii), on the other hand, is true of even the idealized experiment considered above, but I believe that a stalwart proponent of the bare theory would be undaunted by this objection. Let's first consider what a bare theory proponent would say about objection (ii) and then return to objection (i) at the end of the section.

The idea behind objection (ii) is that the bare theory's account of experience fails because it predicts that our experience typically has no ordinary specific content when we know *by direct introspection* that our experience does in fact have ordinary specific content – I do not just believe that I got a determinate result of either *up* or *down* but I believe that I got *up* and not *down* (or the other way around). The problem with this objection, from the perspective of a bare theory proponent, is that one can never know that an experience has ordinary specific content by introspection because introspection is typically an unreliable way of determining one's own mental state in the theory. While an observer might claim that she knows that her result is determinate by consideration of the determinate result itself, the bare theory proponent would deny that there is any determinate result to be considered and thus conclude that the observer's report of determinateness is based solely on her *meta-belief* that her experience is determinate, which he would take as unreliable evidence for the determinateness of the observer's result. And, of course, the bare theory proponent would deny that the observer ever reported to him a specific determinate result; rather, he would take both himself and the observer to be subject to a similar illusion: *not* an illusion that the observer reported *up*, but an illusion that the observer made *some* determinate x-spin report. The bare theory does not seek to explain the specific content of an experience as an illusion – the illusion is that the experience has specific content.

Let me put this another way. If what one takes as standing in need of an explanation is the ordinary specific content of one's experience, then the bare theory simply fails to provide a satisfactory account of experience; indeed, as we have discussed, it denies that there typically is any ordinary specific content to an observer's measurement record, belief, or experience. In some situations the bare theory can explain why one would mistakenly believe that there was determinate content, but that is all it can do. If one is firmly convinced that one's own experience actually does have ordinary specific content, then one will not like the bare theory; but one cannot argue that experience typically has ordinary specific content by appealing to direct introspection and expect to convince someone who takes the bare theory seriously to abandon it – if one allows for the possibility of a basic failure of first-person authority concerning whether or not an experience is disjunctive, which is something any serious proponent of the bare theory must allow, then the bare theory can go some way in explaining why an observer might falsely believe that her experience was typically ordinary and specific. Again, while a proponent of the bare theory can explain why an observer believes that she got a determinate result in an experiment like that described above, he typically cannot explain why she knows what the result is; rather, he will believe that the observer is mistaken – not mistaken because she believes that she got *up* when she did not (because she does not determinately believe *this*) but mistaken because she believes that she has got *some* determinate result with ordinary specific content when she did not. Again, if one wants an explanation for the specific content of experiences and beliefs, then the bare theory is inadequate, but a stalwart proponent does not require such an explanation.

While a proponent might be unswayed by objection (ii), there are, I believe, good reasons for one not taking the bare theory too seriously. In order to take the bare theory seriously, one must suppose that an observer might be mistaken concerning the basic nature of occurrent phenomenal experience (that she might believe that her experience had ordinary specific content when it did not); but if one is willing to allow for this extreme sort of skepticism (more extreme than Descartes' *First Mediation* doubt), then it is difficult to say how one could have any empirical knowledge whatsoever. So what grounds would one have for accepting the bare theory in the first place?[3]

A further problem, one that is at least as serious, is that the bare theory fails to predict even disjunctive results in *typical situations* (this, I believe, is the right way to put objection (i)). Suggestive stories like that told above only work if one assumes that the observer begins in an eigenstate of being sentient and ready to make a measurement on a system with a determinate quantum-mechanical state. If the bare theory were true, however, such just-right conditions would virtually never be met; rather, if the usual linear dynamics always correctly described the time-evolution of the global quantum state, then an observer would presumably almost always be in a complicated entangled superposition of being asleep, unconscious, part of a geranium, etc., and the observer (insofar as this expression even picks out a determinate entity) would almost never determinately have any beliefs or experiences, not even disjunctive ones.

3 HOW TO FIX THE BARE THEORY

The obvious strategy for fixing the bare theory is to drop the standard eigenvalue-eigenstate link, supplement the usual quantum-mechanical state description with something that would provide observers with determinate measurement results with specific content, then provide a dynamics for this extra part of the state. The most direct way to do this would be, as Albert and Loewer (1988) have suggested, to add the mental states of observers to the usual quantum-mechanical state then provide a dynamics for the mental states given the time-evolution of the wave function. There is, however, a

[3]See Barrett (1996) for a discussion of the problem of reliable empirical evidence in the bare theory and other formulations of quantum mechanics.

long tradition of trying to get by without including minds as entities in our physical theories. In this spirit, we will suppose here that what one wants to add to the bare theory is extra state information of a purely physical sort.

Modal and hidden-variable theories seek to guarantee determinate measurement results by supplementing the usual quantum state description with a determinate physical quantity at each time. There are essentially two ways of doing this: (1) one might stipulate a single preferred physical quantity as always determinate (like particle configuration in Bohm's theory) or (2) one might stipulate a rule (as in various modal theories) that picks out a determinate property for each physical system at each time given the current quantum-mechanical state (like the rules provided by some modal theories). In either case, the complete description of the physical state at a time is given only by the usual quantum-mechanical state *together with the value of the determinate physical quantities* (Bacciagaluppi and Dickson provide one example of how strategy (2) might work – see Bacciagaluppi (1998)). What the determinate quantities are and how their values change over time is supposed to explain why we get determinate results to our measurements and why we get the specific results that we do. In order for a modal or hidden-variable theory to account for our experience and belief, then one must be convinced that it is precisely those physical quantities that the particular theory makes determinate that determines our experiences and beliefs.

On Bohm's theory, perhaps the best known hidden-variable theory, the particle configuration is always determinate. The quantum-mechanical state ψ evolves in the usual linear, deterministic way, but particles always have determinate positions and move along continuous trajectories described by an auxiliary dynamics. It is this information about the positions of particles that is added to the usual quantum-mechanical state to complete the physical state. The velocity of particle P is given in Bohm's theory by the expression

$$\mathbf{v}_P = \frac{\mathrm{Im}\ \psi^*(\mathbf{x},t)(\partial/\partial\mathbf{x}_P)\psi(\mathbf{x},t)}{m_p|\psi(\mathbf{x},t)|^2}, \tag{5}$$

where m_P is the mass of the particle and $\mathbf{x}_P$ represents the three coordinates of configuration space that determine its position. In order to calculate the velocity of P at some time t_1, one must evaluate the right-hand side of this expression for each component of v_P using $\psi(\mathbf{x},t_1)$, then substitute the positions of every particle at time t_1, into the resulting expressions (see Bell (1987, 97, 112-114, 127-128) for more details concerning how the dynamics works).

Following Bell (1987, 176-177), Vink (1993) has described a natural way to extend Bohm's theory to physical quantities other than position. On Vink's formulation of quantum mechanics the quantum-mechanical state ψ evolves in the usual linear, deterministic way and the Bell-Vink dynamics (which is simply an extension of Bohm's dynamics to discrete quantities generally) describes the time-evolution of the determinate physical quantities. Suppose that the current value of some physical quantity is o_m. The probability that the value jumps to o_n in the time interval dt is $T_{mn}dt$, where T_{nm} is an element in a transition matrix that is completely determined by the evolution of the wave function. More specifically, the wave function evolves according to the time-dependent Schrödinger equation

$$i\hbar\partial_t|\psi(t)\rangle = H|\psi(t)\rangle \tag{6}$$

where H is the global Hamiltonian. The probability density P_n is defined by

$$P_n(t) = |\langle o_n|\psi(t)\rangle|^2 \tag{7}$$

and the source matrix J_{mn} is defined by

$$J_{mn} = 2 \text{ Im } (\langle\psi(t)|o_n\rangle\langle o_n|H|o_m\rangle\langle o_m|\psi(t)\rangle). \tag{8}$$

Finally, if $J_{nm} \geq 0$, then for $n \neq m$

$$T_{nm} = J_{nm}/\hbar P_m; \tag{9}$$

and if $J_{nm} < 0$, then $T_{nm} = 0$.

Bub's (1997) formulation of quantum mechanics uses this auxiliary dynamics (see also Bub (1998) in this volume). On Bub's theory one chooses a privileged observable R, which is always determinate and evolves according to the Bell-Vink dynamics. Bub shows how, given the wave function $\psi(t)$, to construct a set of other physical quantities that can also be taken to be determinate at time t. According to the Kochen-Specker theorem (1967) this set of determinate physical properties cannot include everything and still preserve functional relationships (see also Mermin 1990). The set described by Bub then is meant to be as large as possible while preserving functional relationships. As $\psi(t)$ changes, so does the set of determinate quantities (R is always in the set), and while determinate, they evolve according to the Bell-Vink dynamics.

For each different choice of the determinate physical quantity R, one ends up with a theory that describes a very different sort of physical world. So what is the *right* choice for R? The lesson we learned from the bare theory is that we ultimately need a formulation of quantum mechanics that allows us to account for the fact that we have determinate experiences and beliefs. We want to make determinate an R then that will guarantee determinate experiences and beliefs, but what physical quantity is this? Presumably what physical quantity would provide determinate experiences and beliefs depends on such things as brain physiology and the relationship between physical and mental states.

One might immediately object that it is crazy to worry about such things as brain physiology and the relationship between physical and mental states in the context of one's most basis physical theory, but in trying to find a satisfactory hidden-variable theory, the Kochen-Specker theorem seems to force us into such considerations. If one cannot make everything one is tempted to take as a genuine physical property determinate and if one nonetheless wants an account of determinate experiences and beliefs, then one must choose the right physical property to make determinate, one that makes experiences and beliefs determinate, and what physical property does this depends on such things as how we in fact record our experiences – not just any determinate physical property will work.

In Bohm's theory, for example, one must assume that determinate *positions* (together with the quantum-mechanical state) is sufficient to account for all our determinate experiences and beliefs. If this is wrong, then Bohm's theory is no better off than the bare theory in providing an adequate account of our experiences and beliefs. And similarly, in order for Bub's theory to provide an adequate account of our experiences and beliefs, one must choose R so that its being determinate (together with the quantum-mechanical state) is sufficient to make all our experiences and beliefs determinate. And more generally (and perhaps more obviously), we cannot be satisfied that any theory accounts for our experiences and beliefs if we are unconvinced that the set of facts that the theory makes determinate includes the experiences and beliefs we in fact take ourselves to have.

One strategy would be to simply postulate the existence of a physical quantity that would account for our experiences and beliefs, then stipulate that *that* quantity is in fact always determinate. Suppose that there is some quantity, call it Q, that, together with the quantum mechanical state, determines the experiences and beliefs of all sentient beings. By hypothesis, taking Q as always determinate would make determinate our experiences and beliefs, and if the quantum-mechanical state ψ evolves in its usual linear, deterministic way, and if Q evolves according to the Bell-Vink dynamics, then the experiences and beliefs of observers would exhibit the standard quantum-mechanical statistics, and the Q-theory would be empirically adequate. The main problem with this strategy, setting aside the fact that we do not know what Q is, is that choosing a single just-right physical property as the only always determinate physical quantity is clearly ad hoc.

Vink's own proposal was to make all the physical quantities that can be represented in the quantum formalism simultaneously determinate. This would avoid the problem of having to choose a single just-right physical quantity to make determinate. But if one makes all the physical quantities simultaneously determinate, then the values of these quantities must violate the usual functional relationships, and one might argue that nothing is gained by making such bizarre quantities determinate (they certainly aren't the sort of physical quantities that we set out to rescue). On the other hand, if one only had epistemic access to one of these bizarre quantities at a time, then one would never notice the lack of functional relationships.

Vink's theory might thought of as a richer version (perhaps too rich since functional relationships between the values of physical quantities are lost) of Bub's theory, and if R is chosen to make experiences and beliefs always determinate, then Bub's theory is a richer (perhaps too rich since the extra determinate properties would, by hypothesis, be unnecessary to account for our experience) version of the Q-theory, and the Q-theory is just Bohm's theory if it turns out that making positions determinate suffices to make the experiences and beliefs of all sentient beings determinate. In contrast, Albert and Loewer's single-mind and many-minds theories avoid the problem of trying to guess what physical quantities would guarantee determinate experiences and beliefs by directly postulating determinate mental states, which, while being a sure-fire solution, is in some sense the most directly ad hoc solution possible.

One might take the problems one encounters in stipulating a single, always determinate physical quantity for a hidden-variable theory as a good reason for instead stipulating a rule that picks out a determinate physical property for each physical system at each time given the current quantum-mechanical state (see Bacciagaluppi (1998) for a particularly nice example of such a theory that appeals to a Bohm-Bell-Vink-like auxiliary dynamics). It seems to me, however, that this strategy encounters similarly difficult problems. One must still, for example, make a choice and justify it: while one avoids having to justify choosing a particular just-right always determinate quantity, one must justify choosing a particular just-right rule for determining the currently determinate quantity for each system. This rule must (even in imperfect measurements), make determinate at the right time a physical quantity that by dint of its being determinate and having the value that it does accounts for our determinate experience and beliefs. Choosing such a rule and arguing that it does precisely what we want it to do has proven to be difficult.[4] And I doubt that it will ever be obvious that a particular rule does precisely what it needs to in order to account for our determinate experiences and beliefs.

There is another problem for any formulation of quantum mechanics that appeals to a Bohm-Bell-Vink-like auxiliary dynamics. Not only do we want

[4]There is much to say about this. Some rules that might initially look like they would work may not even get one close to the right observable in realistic experimental situations. See Albert (1992, 191-197) and Albert and Loewer (1990) and (1991) and the continuing debate described by Vermaas (1998) in this volume. Also, it is unclear, to me at least, that even if a given rule typically makes a physical quantity determinate that is *close* to a quantity that would provide an observer with a determinate mental state that this would explain why the observer has a determinate belief concerning what her result was. Yet another worry results from the fact that some rules pick out a determinate quantity that depends on which physical system one specifies. On such a rule, I might have a determinate brain record when *my brain* is the specified system but not have a determinate brain record when *the solar system* (which includes my brain) is the specified system. In such a situation would I have determinate beliefs? If so, then why? Since I would have a determinate belief state from some perspectives but not from others, why would those perspectives where I did end up as privileged? Etc.

to make determinate physical quantities that would explain our determinate experiences and beliefs but, if possible, we would like the determinate quantities to evolve in a Lorentz-covariant way so that quantum mechanics ends up being consistent with relativity (indeed, most physicists would presumably insist on this). While it is relatively easy to write the usual linear, deterministic quantum dynamics in a covariant form, the auxiliary dynamics in Bohm's theory is not covariant and neither is the Bell-Vink stochastic dynamics. In the case of Bohm's theory, the violation of covariance is fairly straightforward since the outcomes of EPR-type experiments typically *depend on the temporal order in which space-like separate observations are made.* Indeed, if one knew the precise positions of the two particles in an EPR experiment, then one could send superluminal signals (but unless one starts with such knowledge, the theory prevents one from ever getting it). We will take it to be a necessary condition for a modal or hidden-variable theory being *dynamically covariant* that there be no EPR-type experiment where, given the complete physical state (the usual quantum-mechanical state plus the current values of the determinate quantities), the measurement results depend (from a god's-eye view) on the order in which the measurements are preformed.

Consider an EPR-type experiment on two spin-1/2 particles where the initial quantum-mechanical state is

$$\sqrt{2}(|\uparrow\rangle_1|\downarrow\rangle_2 - |\downarrow\rangle_1|\uparrow\rangle_2)|R_1\rangle_1|R_2\rangle_2 \tag{10}$$

where $|\uparrow\rangle$ and $|\downarrow\rangle$ are eigenstates of x-spin and $|R_1\rangle_1$ and $|R_2\rangle_2$ are symmetric wave packets in spherically shaped regions R_1 (close to measuring device 1) and R_2 (close to measuring device 2), respectively, and zero elsewhere. Suppose that particle 1 is in the top half of region R_1 and particle 2 is in the top half of region R_2 (See Albert (1992, 159) for a picture of such an experiment and an explanation of how one could use such experiments to send superluminal signals with the right sort of prior information).

If a Stern-Gerlach device measures the x-spin of particle 1 first, the device will separate the wave function into a $|\uparrow\rangle_1|\downarrow\rangle_2$ part, which will be deflected up (in particle 1 coordinates), and a $|\downarrow\rangle_1|\uparrow\rangle_2$ part, which will be deflected down (in particle 1 coordinates), and particle 1 will be deflected up since it started in the top half of R_1 (in Bohm's theory one can think of the two-particle configuration as being carried around by the quantum probability current in configuration space). The two-particle configuration will then be influenced only by the $|\uparrow\rangle_1|\downarrow\rangle_2$ component of the wave function since the configuration will now be in a region of configuration space where $|\downarrow\rangle_1|\uparrow\rangle_2$ is zero. A subsequent measurement of particle 2 (by a Stern-Gerlach device whose field is oriented the same way as the first) will, therefore, surely deflect particle 2 down (just as one would expect with particles whose x-spins are anticorrelated). But if particle 2 is measured first, then it will be deflected up (because it is initially in the top half of R_2) and the particle configuration will then be influenced only by the $|\downarrow\rangle_1|\uparrow\rangle_2$ component, so a subsequent measurement of particle 1 will surely deflect it down. That is, if particle 1 is measured first, then particle 1 goes up and particle 2 goes down; and if particle 2 is measured first, then particle 1 goes down and particle 2 goes up. If the two measurements are space-like separate, then there is an inertial frame where measurement 1 occurs first and another inertial frame where measurement 2 occurs first. Since there is no absolute matter of fact about the temporal order of the measurements, there can be no absolute matter of fact, like what the results of the measurements are, that depends on their temporal order. But since the measurement results in Bohm's theory *do in fact* depend on the temporal order of the measurements, the dynamical laws that govern the motions of the particles must fail to be covariant.

It is important to note that this failure of covariance is not because Bohm's theory is deterministic. If we keep particle configuration as the privileged physical quantity, then we have precisely the same problem when we move to the stochastic Bell-Vink dynamics. Indeed, if we consider the above experiments in discrete configuration space and use the Bell-Vink dynamics to describe the motions of the particles, then the relevant details of the story are very similar.

Suppose that both particles begin in the top half of their respective wave packets. If particle 1 is measured first, then it does not move in the x-direction while the two wave packets are separating in configuration space until the transition probabilities for the x-position of particle 1 are determined by only one of the two components of the wave function (before then the probability currents associated with the two wave packets will precisely cancel each other in the x-direction, which on the Bell-Vink dynamics means zero transition probabilities). So if particle 1 is measured first and if it is initially in the top half of the wave packet, then the $|\uparrow\rangle_1|\downarrow\rangle_2$ component will ultimately determine all transition probabilities for the two-particle configuration. Since the probability current associated with this component will be in the up direction for particle 1 and zero outside the wave packet, particle 1 will be deflected up. Since the transition probabilities for the positions of both particles would now be determined solely by the $|\uparrow\rangle_1|\downarrow\rangle_2$ component of the wave function, a subsequent measurement of particle 2 would surely deflect it down. But for the same reason that particle 1 would be deflected up if measured first (it is initially in the top half of its wave packet), particle 2 would be deflected up if it were measured first, so again the results of the measurements depend on the order in which they are performed, and we have a failure of covariance.

One might imagine more complicated (and realistic) situations where the wave packets are neither symmetric nor confined to specific regions of configuration space so that there is a nonzero probability of particle 1 being deflected down *even when it begins in the top half of the wave packet and is measured first.* But if the *probabilities* for the outcomes of the two measurements depend on the temporal order of the measurements, which is what the

Bell-Vink dynamics would predict for a sufficiently fine-grained partition of configuration space, then we still get a violation of dynamical covariance. One might try to change the dynamics to avoid this problem, but any dynamics whose statistical predictions depend on the temporal order of the EPR measurements will fail to be covariant; and if it is the determinate property of the first measured particle that is supposed to explain the result of the second measurement and if we determine the property of the first particle by how we choose to measure it, then I cannot see how any such theory could be both empirically adequate and have a covariant auxiliary dynamics.[5]

Because of the difficulty in finding a Lorentz-covariant hidden-variable theory Bell (1987), Bohm and Hiley (1993), and others have suggested that it is perhaps not so bad giving up *dynamical* covariance if we can still have *empirical* covariance, where, as in Lorentz's own formulation of relativity, there would be a dynamically preferred inertial frame, but one would never be able to perform experiments that would determine what is was (see Bell (1987, 67-79) and Maudlin (1994)). It turns out that we can have empirical covariance on even Bohm's theory if the positions of particles are statistical distributed in the way that we believe that they are (see Dürr, Goldstein, and Zanghí 1992, for example). If we opt for a Bohm-like theory, then we may have to settle for this.

4 CONCLUSIONS

In an important but subtle sense the bare theory makes different empirical predictions than the standard collapse formulation of quantum mechanics for even the simplest observations. If an observer begins ready to make a measurement on a system in a determinate quantum-mechanical state, then the bare theory predicts that the observer will believe that she got an ordinary determinate result and that she knows what it is when there is in fact no determinate result that she believes she has. The standard collapse formulation of quantum mechanics (insofar as it makes coherent empirical predictions) predicts that an observer will believe that she got an ordinary determinate result and that she knows what it is, *and she will be right.*

But while the two theories make different empirical predictions, one cannot conclude from this that the bare theory fails to account for our experiences. The bare theory seeks to account for our experience not by making the same empirical predictions as the standard formulation of quantum mechanics but by telling us that the structure of experience is fundamentally different from what it is usually taken to be. It tells us that our experience is typically

[5]The fact that the Bell-Vink stochastic dynamics fails to be covariant is not at all surprising given that Bohm's dynamics fails to be covariant. After all, as Vink argues, if one chooses discrete configuration as the always determinate quantity, then his stochastic dynamics approximates Bohm's dynamics increasingly well as one considers finer-grained partitions of configuration space and shorter time intervals.

disjunctive, that it typically fails to have ordinary specific content even though it may seem to have it. One might argue on the basis of direct introspection that the bare theory is wrong, that our experience actually does have ordinary specific content, but it is precisely this sort of introspection that the bare theory tells us is unreliable.

While one can tell suggestive stories in the context of the bare theory, and while for all I know, it might be true, it presumably is not the sort of physical theory we want. In order to fix it, one might want to add something that provides observers with genuinely determinate experiences and beliefs with the right specific content, which is exactly what the modal and hidden-variables theories seek to do. Since not just any always determinate physical quantity (or rule for picking out a determinate quantity for each physical system given the quantum mechanical state) will account for our determinate measurement results, we need to convince ourselves that a particular choice is the *right* choice. If one also wants an auxiliary dynamics for the determinate quantities that is Lorentz-covariant, then this makes the task that much more difficult, perhaps impossible.[6]

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