REVIEW ESSAY*


If nothing else had been published on the philosophy of quantum mechanics in 1989, these two books would have made it a good year. Hughes has written the best self-contained introduction to the foundations of quantum mechanics yet to appear, and the volume by Cushing and McMullin is an accessible but sophisticated snapshot of philosophical thinking on Bell's theorem that is likely to become a classic. For the reviewer, the two books make an appealing pair. Hughes has an essay in the Cushing and McMullin volume (henceforth C&M) that sketches his own position, but also amounts in some ways to a reply to various of the papers in the collection. I will begin by discussing a selection of the essays from C&M, all of which seem to me to raise issues that bear interestingly on Hughes's position. At that point, I will take up Hughes's view, weaving together his essay in C&M with the more developed position in his book.

Philosophical Consequences of Quantum Theory consists of thirteen papers on Bell's theorem, together with a background essay by James Cushing, a historical discussion of action at a distance by Ernan McMullin and an extensive bibliography. Some of the papers have appeared elsewhere (van Fraassen's contribution, and part of Mermin's) but most are new to this collection. I will single out three papers for special attention: Bas van Fraassen's, Arthur Fine's, and Paul Teller's. In the process, I will refer to several of the other essays. I will not mention every essay in C&M. In particular, I will not be discussing the contributions by Abner Shimony, Michael Redhead, Linda Wessels, Don Howard and Henry Folse, nor the introductory and concluding essays by Cushing and McMullin respectively. However, I don't think there are

any bad essays in the book, and the selection I have made is mostly a reflection of my idiosyncrasies.

Van Fraassen’s contribution is a somewhat expanded version of his 1982 ‘The Charybdis of Realism: Epistemological Implications of Bell’s Inequality’, together with an appendix based on his 1985 ‘EPR: When is a Correlation Not a Mystery?’. The thesis of “Charybdis” is that experimentally confirmed violations of Bell’s inequality provide a strong argument against what van Fraassen calls epistemic realism. This is the view that we can have reasonable expectations about future regularities, in particular, about the persistence of correlations, only if we have reasonable certainty that there is some causal mechanism that underwrites the regularities. In the case of the Bell-type experiments, the regularities are correlations between pairs of space-like separated events, and van Fraassen urges that a reasonable causal account would have to appeal to a common cause in Reichenbach’s sense – an event in the past that renders the correlated events stochastically independent. He gives an elegant demonstration that there are no such common causes.

Let us concede (as virtually everyone does) that there can be no common cause explanation for failures of Bell’s inequality. Van Fraassen thinks this carries an epistemological moral: so long as our expectations are based on well-supported theories, we are justified in expecting regularities to persist whether or not a causal account is available or even possible. In the end, this might be right, but not necessarily in a way that conflicts with the deep intuitions of epistemic realism. In “Charybdis”, van Fraassen depicts epistemic realists as insisting on causal accounts of correlations, but surely that is too strong. The strongest reasonable position would be that in order to have reasonable expectations about the persistence of correlations, we need reasonable certainty that there is some account of the correlations, whether it be causal or of some other form. And it might turn out to be a constraint on the confirmation of theories that they either embody or are consistent with some account of the correlations they predict.2

I think there is an interesting issue here, but let us waive the point for now. Van Fraassen’s appendix in C&M amounts to a concession that not all accounts of correlations need be causal. He lists five ways other than common cause in which correlations have traditionally been explained. The two of most interest to us are coordination and logical identity. Van Fraassen takes coordination – i.e., direct causal link – to
require matter or energy to travel from one location to another, and he thinks that this is incoherent if the two locations are space-like separated. Further, he adds, “Correlations between distant events happening to parts of the same extended entity are not ipso facto less mysterious” (p. 112). What remains, then, are explanations that appeal to “logical identity”. The full discussion of this matter is in the 1985 paper on which the “Appendix” is based. I would like to turn to that account, which I find quite curious.

Logical identity explanations proceed by showing that the correlated variables are aspects of a single variable. Here’s how van Fraassen suggests that this might seem to work for quantum systems. Suppose that A and B commute, have a set \( \{ \phi_i \} \) of common eigenstates such that \( A\phi_i = a_i \phi_i \), \( B\phi_i = b_i \phi_i \), and the \( a_i \)'s, as well as the \( b_i \)'s, are all distinct. Now suppose that \( \Phi = \Sigma_i c_i \phi_i \). In that case, if A and B are measured jointly, we can be certain that the results will be correlated: for some \( k \), the joint outcome will be \( \langle a_k, b_k \rangle \). The apparent explanation is that there is a maximal quantity C such that \( A = f(C) \) and \( B = g(C) \), and the measurements of A and B are just partial measurements of C. Van Fraassen goes on to tell us that this sort of explanation “is not embarrassed by spatial separation” (1985, p. 118). This is because observables AI and IB for systems X and Y (I have deleted the tensor product sign) are always compatible, and are both functions of AB. An appropriate choice of state will produce correlations of the sort discussed above, and once again, it will apparently be no mystery that the results of AI and IB measurement are correlated: in measuring AI and IB, we are really just making partial measurements of \( C = AB \).

Van Fraassen thinks that this account breaks down. Return to the general case. If C had a pre-existing value \( z \), then we could claim that the correlated values \( x = f(z) \) and \( y = g(z) \) simply reveal partial information about \( z \). But suppose C has no such pre-existing value. Then we can’t account for the x-result of the A-measurement by saying that if we had measured C, we would have observed \( z \), and \( x \) is just \( f(z) \), nor can we say any such thing about B. Worse, we can assume that quantum observables have ready-made values if we adopt a contextual hidden variable theory, but to accept such a theory is

\[ \ldots \text{to admit a radical and unverifiable difference between the relations among observables and the algebra of Hermitian operators} \ldots \] (1985, p 121)
I find much about this discussion puzzling. Consider the initial, general case of A, B, and C. At one point, van Fraassen suggests that there is something peculiar to quantum mechanics that gives the putative explanation its purchase. Whereas we might expect in general that co-measurability would not imply a logical connection among observables, “sub specie the quantum mechanical formalism, the world is much more tightly organized and internally connected than our general intuitions might suggest” (1985, p. 118). Nonetheless, there is nothing peculiarly quantum mechanical about the intuitions needed to make the present example run, and there is also no reason why pre-existing values should be required. Consider a thoroughly classical device: a tetrahedral die, with faces with one, two, three or four spots on them. Now consider two experiments. The first, A, consists of tossing the die and recording the number of spots mod 3 on the base of the die. The second, B, consists of tossing the die and recording the number of spots mod 2 on the base. Clearly, these experiments can be performed jointly. Now in general, knowing the result of A will not tell us the result of B nor vice-versa, as the reader can easily check. But suppose we have so contrived the state of things that even though the outcome of the roll is not completely determined, the side that ends up as the base will have either one or two spots on it. In that case, the results of A and B will be correlated: A will have result 1 iff B has result 1; A will have result 2 iff B has result 0. This is no mystery. When we perform A, we get partial information about the result of the experiment C, i.e., tossing the die and simply noting the number of spots on the base. Similarly, performing B gives us partial information about the result of C. If the state of things restricts the possible outcomes appropriately, A and B will be strictly correlated. But notice: there is absolutely no need to assume that there is a pre-existing fact about what the result of C was to be before the die was actually tossed, or that in tossing the die, we are discovering facts about the pre-existing value of some variable.

My point thus far is that nothing van Fraassen has said points to anything peculiarly quantum mechanical, and further, at least at the general level, the question of pre-existing values is a red herring. We can describe a classical case that is like the quantum one in relevant respects, and includes the feature of indeterminism, but for which a “logical identity” explanation of correlations is perfectly acceptable.
Why can’t we proceed in the same way in the quantum case? Why isn’t this a perfectly good way to account for the correlations?

An objection that won’t work is that in the quantum case, we are supposedly performing *measurements*, and this presupposes pre-existing values. We needn’t presuppose that quantum measurements merely reveal pre-existing information. For all we have said – for all we *know* – a quantum measurement is like a roll of a die, with information created in the process. Suppose a quantum measurement, like a roll of a die, is a process in which a system comes to be in a certain condition, and hence in which an event from a certain range of possible events will occur, though precisely which event is not, in general, fixed. The quantum state places restrictions on the range of possibilities open to the system, but quantum theory, like any other rich theory, provides resources for individuating events. In the case van Fraassen describes, the theory tells us that the event of the system coming to display $A = a$, is the same event as the event of the system coming to display $B = b$. In fact, we see that the reference to the third observable $C$ is not crucial; what is crucial is the identity of certain possible events.

It seems that we really can explain some correlations among chance outcomes, without hidden variables, by appealing to the identity of certain possible events. Without further argument, there is no reason why we can’t do this in the quantum case. However, I want to look more closely at van Fraassen’s discussion of contextual hidden variables. We will find that he is straining at a gnat while standing ready to swallow a camel.

Let us grant that the “radical and unverifiable difference” between the algebra of operators and the algebra of hidden variables is a serious epistemological objection. But suppose we could somehow know that the contextual structure really is correct. That leaves the real problem: the hidden variables will be non-local in a very peculiar way. There will be pairs $AB$, $AD$ such that $AB$ has a value associated with the pair of numbers $(a, b)$, $AD$ is associated with $(a’, d)$, and $a \neq a’$. Now to measure $AB$, I use an $A$-apparatus “here” and you use a $B$-apparatus “there”. To measure $AD$, I use my $A$-apparatus, and you use your $D$ apparatus. Thus, which maximal observable I am partially measuring depends on which apparatus you set up. And for the hidden state under consideration, this translates into a constraint on results: what result my apparatus will display depends on what apparatus you set up. But
how does my apparatus “know” what quantity is undergoing partial measurement?

The underlying point here is actually one that van Fraassen already made. AB and AD are non-local quantities. This suggests that the underlying system is a particular (and peculiar) sort of extended entity, and the displays of results in the two wings of the experiment are, in effect, “distant events happening to parts of the same extended entity” (C&M p. 112). But as van Fraassen pointed out, correlations in such a case are not automatically unmysterious. The measuring devices are, presumably, local entities. They are connected by virtue of being “in contact” with some aspect of a single entity, but there is no obvious local information to tell the apparatus which feature of the entity it should be looking at. So the real problem with contextual hidden variables is not their unverifiability; it is that they seem incapable of performing their explanatory task.3

We will return to contextual hidden variables later. I would like to move on to a discussion of Arthur Fine’s paper, ‘Do Correlations Need to Be Explained?’. Fine’s target is explanatory essentialism, which holds that correlations require explanation regardless of the scientific context in which they arise. Explanatory essentialists talk of “new and quasi-mysterious physical processes (maybe funny ‘influences’ or odd ‘passions’)” (p. 180) in response to the failure of the Bell inequalities. Fine believes there is no need to invoke such oddities.

The kernel of the argument is simple: quantum mechanics violates the Bell inequalities even when there is no interaction term in the Hamiltonian of the two particles. Fine thinks that philosophers should take their lead from the theory and stop positing influences, but he also offers a scheme for thinking about the experiments – a scheme according to which there are no cross-wing influences of any sort, and thus, one that satisfies what Fine calls strong locality (SLOC).

The model is simple enough. Think of individual measurement outcomes as coming about “in a perfectly random way” (p. 184). Suppose $A_i$ is measured in the A-wing, with result plus, and $B_i$ is measured in the B-wing. Fine assumes, sensibly, that neither choice of setting influences the other.

The possible dependency left over would be between the $B_i$ measurement itself and the outcome of the $A_i$ measurement. However, ... randomness implies that the outcome does not depend on which measurement is performed in the B wing ... Thus ... we ... have a framework in which [SLOC] is satisfied. (pp. 184–5)
Further, it is easy to describe random sequences of outcomes that link together to violate Bell’s inequality. So we apparently have a purely local model for the correlation experiments.

Indeterminacy is the key here. In an indeterministic world, there surely can be sequences of individually undetermined events that exhibit an overall statistical pattern. But, Fine adds,

Correlations are just probabilistic patterns between two sequences of events… Why, from an indeterminist perspective, should the fact that there is a pattern between random sequences require any more explaining than the fact that there is a pattern internal to the sequences themselves? (pp. 191–2)

Indeterminism forms part of the “ideal of natural order” (a phrase that Fine borrows from Toulmin) in terms of which quantum theory is framed. Thus conceived, Fine maintains, the correlations of quantum theory no more call for explanation than do uniform motions in Newtonian and post-Newtonian physics.

This may puzzle readers of Fine’s earlier work. In ‘Antinomies of Entanglement’ (1982) and ‘Correlations and Physical Locality’ (1981), Fine claimed that responsible indeterminism (see especially Fine 1981, pp. 544–8) does not encompass violations of Bell’s inequality. What’s different now?

Following Fine, call two runs of a Bell-type experiment adjacent if they differ in the setting in one wing. Let $A_1, A_2$ be the settings on the A-wing of the experiment, and similarly for $B_1, B_2$. Thus, an $A_1B_2$ run is adjacent to an $A_1B_2$ run. And call two adjacent runs entangled if the sequence of outcomes for the shared observable differ. Even if $A_1, A_2, B_1$ and $B_2$ were chosen to satisfy Bell’s inequality, it would be very unlikely that the four runs of the experiment would be untangled, but so long as Bell’s inequality stands, the statistics could be exhibited in untangled runs. However, if the inequality is violated, entanglement is required. In ‘Antinomies of Entanglement’, Fine found this genuinely puzzling (see especially p. 743). Two considerations seem to have helped change his mind.

The first is the fact, noted above, that, Bell’s inequality aside, the odds for finding untangled experiments are minute. Violations of the inequality simply make these tiny odds vanish (p. 189). Looked at in this way, failures of Bell’s inequality seem less impressive. The second consideration has to do with hypothetical questions about a single run. Suppose I performed a sequence of $A_1B_1$ measurements. Would the
A₁ sequence have been the same if I had measured A₁B₂ instead? It is tempting to think that locality says yes. One might think that if the A₁ sequence could have been different, this would amount to an influence of the distant setting on the local outcomes. Fine disagrees. If the outcomes are undetermined, there is no fact about what would have happened had things been different in any respect, local or remote. To insist otherwise seems to Fine to require determinism, or something like it; indeterminism blocks the derivation of a Bell inequality.

This suggests weakening the premise: say simply that the same A₁ result might have occurred if the B-setting had been different. Fine argues that this won’t do. True, the “might” statement doesn’t presuppose determinism and is a plausible consequence of locality. But it is too weak. In any actual run of an experiment, quantum mechanics might not be verified; indeed, for any finite experiment there is a probability given by the theory that the numbers will come out “wrong”. So counterfactual reasoning strong enough to derive Bell’s inequality requires overly strong premises; counterfactual reasoning from appropriately weak premises doesn’t imply the inequality.

I’m puzzled. Call the principle that the result here would have been the same if the only difference was in the setting there the “would” principle. Call the similar principle with “would” replaced by “might” the “might” principle. Appending the “would” principle to a statistical theory does not entail that the statistics of any particular experiment will come out right. The interesting question is whether the “would” principle is compatible with the statistics coming out right. For quantum mechanics, it isn’t. (For a very nice demonstration, see David Mermin’s essay, ‘Can You Help Your Team Tonight by Watching TV?’) Now the “might” principle doesn’t imply a Bell inequality if we allow a violation of the quantum statistics. But neither does the “would” principle. If we offer the “might” principle the same courtesy extended to its brasher brother, perhaps we will get the same result. In fact, Mermin’s concluding section strongly suggests that this is so. (See pp. 47–9, especially 47.) Further, Henry Stapp argues explicitly (‘Quantum Non-locality and the Description of Nature’) that if we require the quantum statistics to be verified, we can extract a contradiction from locality and the “might” principle.

I am not sure whether Stapp’s argument is correct. To convince me, he would have invoke an explicit semantics for counterfactuals and compare every argumentative move to it explicitly. But even though
I'm not certain that Stapp is right, I'm not certain that he's wrong. The counterfactual questions here are profoundly tricky, and I don't think Fine's treatment of them is sufficiently detailed to settle the matter. But rather than agonize over counterfactuals, I would like to approach the question of "influences" in another way.

Jon Jarrett has pointed out that the usual locality condition is the conjunction of a condition he calls *locality*, and another that he calls *completeness*. (See his 'Bell's Theorem: A Guide to the Implications' for a nice summary.) I will use Shimony's (1986) terms, *parameter independence* (PI) and *outcome independence* (OI). PI requires that the local outcome be stochastically independent of the distant setting. In the elegant notation that Mermin proposed for the volume,

\[
(\text{PI}) \quad p(x/i, j, \lambda) = p(x/i, \lambda) p(y/i, j, \lambda) = p(y/j, \lambda)
\]

\(x\) and \(y\) are, respectively, A-wing and B-wing outcomes, \(i\) and \(j\) are A-wing and B-wing settings, and \(\lambda\) is the state. Fine focuses on whether changes in the distant setting influence local results. However, quantum mechanics satisfies PI, and that may explain why Fine doesn't detect violations of locality. On the other hand, quantum states violate OI:

\[
(\text{OI}) \quad p(x, y/i, j, \lambda) = p(x/i, j, \lambda)p(y/i, j, \lambda).
\]

Thus a more fruitful question is whether violations of OI embody some sort of influence of events in one wing on events in the other. I think there are plausible reasons for answering yes. An important one goes back to Reichenbach's principle of the common cause. In 'A Space-Time Approach to the Bell Inequalities', Jeremy Butterfield gives the following, slightly weaker statement of Reichenbach's principle:

\[
\ldots \text{if } x \text{ and } y \text{ are correlated without direct causation, then there is in their common past an event } z \text{ that screens them off. (p. 115)}
\]

Although Butterfield is not willing simply to embrace this principle, he argues convincingly that it can be made plausible by associating the screeners-off with large regions of space-time. But from Reichenbach's principle, we have a corollary:

If \(x\) and \(y\) are correlated, and there is no event \(z\) in their common past that screens them off, then they are connected by a direct causal link.
In one sense, to avert to this principle is to beg the question against Fine. But there is a serious issue of where the burden of proof lies. The principle of the common cause is a highly general one that serves us well in a wide variety of contexts. When we apply it to Fine’s proposed model, we conclude that SLOC is violated. True, quantum mechanics does not posit an interaction term in the Hamiltonian, but that may just be to say that certain fundamental causal connections are part of the deep structure of the theory itself. Further, in a forthcoming article, Butterfield shows that on David Lewis’s analysis of causation, failures of OI do constitute a causal link between events in the two wings. Again, Butterfield is not prepared simply to embrace Lewis’s analysis, but that analysis is plausible, sophisticated and highly general. So I maintain that Fine has not made his case. Even though the jury may still be out, a prima facie case can be made for non-local influences.

There are, of course, reasons to doubt that the quantum correlations are causal connections. A bad one is that faster-than-light causation violates relativity. This is a bad reason because (I claim) no such conclusion follows from relativity. What relativity requires is that real physical facts be invariant under Lorentz transformations. For causes, this means that it must be impossible to reverse or transform away a causal connection by a Lorentz transformation. If it were part of the concept of a causal connection that the cause must unambiguously precede the effect in time, then relativity would rule out superluminal causes. But I am deeply suspicious of this supposed conceptual connection. What is more plausible is that for a connection to be causal, there must be an unambiguous distinction of some sort between cause and effect. If the distinction could be made on other than temporal grounds, this would leave room for superluminal causation. The more plausible relativistic difficulty is that failures of OI are symmetric and causation as usually understood isn’t. Even this is open to challenge, however. For example, Richard Healey conceives of the quantum correlations as “non-separable causal processes” (Healey 1989) and is attempting to work out the issues in detail.

So what’s the point? The point is that the quantum correlations are very much like causal connections – so much so that on some accounts they are causal. This suggests that when people talk of “passion at a distance” or the like, they are not being obscurantist; they are trying to walk the line between, on the one hand, acknowledging the important similarities between quantum correlations and more familiar causal
connections, and on the other, giving due weight to the differences between these phenomena and the ones on which traditional analyses of causation were based. Fine admits in a footnote (p. 190) that the matter of “influences” needs investigation, but he doesn’t give the point nearly enough weight in his discussion.

A related point about causation and indeterminism. Surely there is something right about Fine’s stress on indeterminism as an ideal of natural order. Coming to terms with quantum mechanics will require coming to terms with indeterminism. But there is an unresolved question of how to tell when it is appropriate simply to chalk up a surprising class of phenomena to the manifestations of the natural order. Suppose we discovered “sub-quantum” states that grounded superficial violations of OI in deep probabilistic violations of PI. This would strongly suggest that there is an influence exerted by settings in one wing on outcomes in the other. But someone might try to insist that we need not talk of influences; that there is merely a complicated pattern among “setting-events” and measurement outcomes, not unlike the pattern that Fine sees between outcome sequences in the two wings of a Bell-type experiment. I don’t think we would be content with this. Whether we are Humeans about causation or whether we adopt a thicker view, our overall notions of influence and causation would not cohere with this story. My point is not to propose looking for violations of PI. My point is not even that we explain the violations of Bell’s inequality by coming to see violations of OI as causal. It is that in trying to decide whether events are connected by a relation of influence, we need to look to our more general concepts of causation and influence. We may end up modifying these in the light of the theory. Or we may simply be able to use them to make clear what the theory has been saying all along.

Teller
Part of Fine’s aim in his presentation of indeterminism as an ideal of natural order was undoubtedly therapeutic; it was intended to put us at ease conceptually with the quantum correlations by helping us to overcome old deterministic habits of thought. Teller’s aims are also therapeutic in part, but his diagnosis suggests a different conceptual ailment.

Teller’s paper is called ‘Relativity, Relational Holism, and the Bell Inequalities’, and his immediate aim is to explain the nagging feeling
that somehow, violations of Bell's inequality are in conflict with quantum mechanics. He notes that there is an extremely simple argument that suggests such a conflict. If we combine the assumption of determinate values for quantum observables with the requirement that local outcomes not depend on distant settings, we get a Bell inequality. But on the face of it, relativity requires both the determinateness of values and the locality condition. Here's why.

The argument for determinate values is essentially the EPR argument. By the correlations of the singlet state, we know that a measurement here allows us to predict an outcome at a space-like there. But relativity is normally taken to forbid the superluminal transmission of information. So my measurement here could not have created the value there. Hence, the value must already have existed. Further, and again by relativity, this value must be independent of my choice of measurement setting. So we have determinateness and locality, and hence, Bell's inequality, from the correlations and relativity.

Teller does not think this is a valid argument. However, it is at least as plausible as the (surely very plausible) EPR-style argument on which it rests. Teller's project is to figure out what has gone wrong. To do that, he has to say something about relativity. Teller accepts Einstein's view that relativity is a principle theory; "a set of general constraints which any more detailed theory must satisfy" (p. 212). For special relativity, the most general constraint is the one mentioned earlier: "real" physical processes must be Lorentz invariant. But our working picture is more detailed. We think of relativistic theories as describing the world in terms of the values of physical quantities at space-time points. That is, we think of relativity as a space-time theory. Further, "classic" relativistic theories describe processes in terms of local action. That is, there is no "action at a distance" in space or time; direct causal connections are always between "infinitesimally close" points. And finally, causal chains are always time-like or light-like. Teller calls theories satisfying all of these constraints Relativistic Causal Theories.

Why are we so wed to relativistic causal theories? As Teller sees it, it is because we accept an even more basic metaphysical doctrine, which he calls particularism. Particularism is the view that

... the world is composed of individuals, that the individuals have nonrelational properties, and that all relations between individuals supervene on the nonrelational properties of the relata. (p. 213)
Teller doesn’t think that particularism literally entails that physical theories must satisfy contact action and no superluminal propagation, but he does believe that both of these theses are natural adjuncts to particularism.

The next question is whether quantum mechanics is a particularistic theory. Teller’s answer is no. States such as the singlet state – superpositions of tensor product states – ascribe relational properties to pairs and larger collections, but the relations do not supervene on non-relational facts about the individuals. Now relational holism is compatible with Lorentz invariance, as relativistic quantum theory makes clear. But relational holism clashes with the idea that all respectable theories must be relativistic causal theories, i.e., must incorporate local action and no superluminal propagation. This is not because relational holism posits action-at-a-distance or superluminal propagation; it is because particularism doesn’t allow for the possibility that there may be non-supervening relations between systems.

How does this help us shake off the grip of the EPR-style argument for Bell’s inequality? Merely to be told that there are nonsupervening relations is not to be told enough. Teller doesn’t address this question directly, but the answer seems to be a corollary of his treatment of the stochastic case. The standard locality condition for stochastic theories has been

\[ \text{Factorizability: } p(x, y/i, j, \lambda) = p(x/i, \lambda)p(y/j, \lambda). \]

A metaphysically naive argument from relativity to factorizability would go something like this: a correlation between \( x \) and \( y \) presupposes either a direct causal link or a common cause.6 But (recall Butterfield) if \( i, j, \) and \( \lambda \) are sufficiently inclusive, they encompass any possible common cause. And for Bell-type experiments, a direct causal link between \( x \) and \( y \) is ruled out by the relativistic No Superluminal Propagation principle. The metaphysical naïveté here resides in thinking that relativity requires correlations to be accounted for by positing either a common cause or a direct causal link. As Teller sees it, these are presuppositions of particularism; not of relativity.

In fact, we can be more precise. We know from Jarrett that factorizability is the conjunction of PI and OI. We also know that quantum mechanics respects PI and violates OI. And Teller argues that the failure of OI can be seen as a failure of particularism. To see this, we need two premises:
First Assumption: When particularism holds, nonaccidental correlations arise only through
(a) The action of a common cause, or
(b) The action of a direct causal chain.
Second Assumption: When particularism holds, relativistic causal theories exclude superluminal causal propagation. (p. 220)

This yields a simple argument:

The action of a common cause has been eliminated by making everything conditional on \( \lambda \), the list of all possible known and unknown common causes. The action of a direct causal link is ruled out by assumption 2. Thus, there are no nonaccidental correlations conditional on \( \lambda \). That is, outcome independence holds. (p. 221)

Hence, by contraposition, failures of outcome independence are failures of particularism. But the perfect correlations of the EPR thought experiment are special cases of the failure of outcome independence. Outcomes in one wing are perfectly correlated with appropriate outcomes in the other not because of a direct causal link, not through a common cause, and not by the obtaining of locally definite, correlated values, but because the conditional probabilities provided by quantum mechanics embody a nonsupervenient relation between the two particles.

Teller writes that when the grip of particularism on his thinking became clear to him, his resistance to the "random devices in harmony" of Fine's 1981 and 1982 melted away.\(^7\)

The correlation – as an objective property of the pair of objects taken together – is simply a fact about the pair. This fact . . . need not be . . . supervenient upon some more basic, nonrelational facts. There need be no mechanism into which the correlations can be analysed. (p. 222)

I find that the gestalt of Teller's view switches uncomfortably back and forth between a species of realism about relations and a deflationary view. Is the claim that there are real relations that ground and, in a sense, account for the quantum correlations, or is it simply that the
statistical patterns among events have formal features that our classical experience leads us not to expect? Teller spends much time talking the language of metaphysics: he talks of ontological locality of values vs. nonsupervenience or relational holism. He says that “relational holism takes there to be things in the world – nonsupervening relations” (p. 223) for which particularism makes no room. But think about the claim that failures of outcome independence just are failures of particularism. One would expect Fine to reply that failures of outcome independence are just particular statistical patterns among events and nothing more. If failures of OI are ipso facto instances of relational holism, then we have a grand name for a humble fact. A comparison: both Hume and his opponents use the term “necessity” in talking about causation. But for Hume, talk of necessity is simple talk of regularities, whereas for his opponent, it is a way of saying that something grounds the regularities. What I honestly can’t tell is whether nonsupervening relations are meant to be part of the glue of the universe or mere oil on troubled waters.

The analogy with accounts of causation raises other questions about the nature of relational holism. The salient contrast throughout Teller’s paper is between relativistic causal theories (RCTs) and nonsupervenient relations. The suggestion is that (a) RCTs are particularist theories, and (b) relational holism eschews causal accounts of the correlations (though it does not forsake relativity.) But I find the connection between the principles of RCTs and particularism quite obscure. The standard relativistic construal of events as the instancing of properties at place-times has a reasonably clear connection with particularism. But the aspects of RCTs that actually figure in Teller’s arguments are that causal action is (i) “local” and (ii) subluminal. And I don’t see that these have any connection with the reducibility of relations.

Begin with a simple question. Suppose we discovered a correlation that was not mediated by any intervening process, i.e., that acted “at a distance”, but was in all other respects just like a causal connection. Would this amount to a failure of particularism? Why should the fact that the correlated events are “at a distance” make the relation a nonsupervenient one? Why should the interpolation of a continuous series of “connecting” events be a step in the direction of supervenient relations – of particularism? The possibility of such an interpolation rests on the continuity of space-time. But what does that have to do
with particularism? Would particularism automatically fail if space-time were discrete? Or would particularism hold if neighboring events were connected, but fail when the connection skips a space-atom or two?

I am similarly confused about the connection between subluminal propagation and particularism. Teller concedes (p. 212) that tachyonic accounts of the Bell correlations can’t be ruled out. I therefore assume that he does not want to say that tachyonic connections would amount to nonsupervenient relations. But if not, I am still puzzled about the connection between the slower-than-light signals and particularism, which after all is a view about the reducibility of relations.

To all of this I add a related worry. Why are causal relations not cases of nonsupervenient relations? Since Teller believes that failures of OI are failures of particularism, the question seems especially acute for stochastic causal connections. Consider a correlation between two events $x$ and $y$ in which $x$ is the complete probabilistic cause of $y$. (My discussion here is indebted in part to Andrew Elby’s forthcoming ‘Should We Explain the EPR Correlations Causally?’) Then $x$ and $y$ will be stochastically dependent, and there will be no event $z$ that screens $y$ off from $x$. In other words, we will have a situation that is formally like the case of a failure of outcome independence. So why isn’t the relationship between $x$ and $y$ a nonsupervenient one? What in terms of relations distinguishes causal relations from the supposedly nonsupervenient relations that enter into failures of OI? True, failures of OI are symmetric, whereas, perhaps, causal relations are not. But what does symmetry have to do with whether a relation is reducible?

Perhaps there are answers to all these questions, but my diagnosis is that relational holism has been formulated at too high a level of generality. What we need to do is look more closely at the crannies and crotchets of quantum theory itself. And with that, we turn to Hughes.

*The Structure and Interpretation of Quantum Mechanics* is a clear, pedagogically sound introduction to the foundations of quantum mechanics. Part one (the first six chapters) treats the basic facts of quantum mechanics in Hilbert space and is well-suited for use with undergraduates. The examples are finite and manageable, and there are exercises sprinkled throughout the text. Part two (the remaining six chapters) covers more advanced material and is more self-consciously interpretive. It will probably work best with graduate students.

Hughes is clearly a gifted teacher, and a casual look at the book may suggest that it is primarily a textbook. In fact, a definite interpretive
perspective is developed throughout the book. Hughes’s essay in C&M, ‘Bell’s Theorem, Ideology, and Structural Explanation’, provides a summary of many of the most important themes, as well as a perspective on Bell’s theorem. I will structure the discussion by weaving back and forth between the book and the essay, with Bell’s theorem as the guiding focus. References will be to Hughes’s book unless otherwise indicated.

Pace Fine, van Fraassen, and Teller, Hughes thinks that quantum mechanics explains the correlations. What is crucial for the account is his conception of what is involved in providing an explanation. Two influences shape his general view: the “Western Ontario” quantum logical tradition and the semantic conception of theories. These come together in the way that Hughes develops the idea that quantum mechanics is a principle theory in Einstein’s sense. As Bub puts it, principle theories “introduce abstract structural constraints that events are held to satisfy” (Bub 1974, p. 143. Quoted in Hughes at p. 258). Much too briefly, Hughes believes that quantum mechanics provides structural explanations. A principle theory provides these ground-level explanations by “making explicit the structural features of the models the theory employs” (p. 258). In so doing

... it disassembles the black box, shows the working parts, and puts it back together again. (C&M, p. 198)

“Model” is used here in the sense of abstract mathematical structures such as those studied in model theory. But these same structures are also the focus of the semantic conception of theories. So the semantic conception provides the natural setting for expounding the notion of a principle theory, and for providing structural explanations. In the case of quantum mechanics, many important features of the models are embodied in the same structures that have been the focus of quantum logic: the lattice of subspaces of Hilbert space, generalized probability measures on these subspaces and the like.

What, then, are the components of a structural explanation of the quantum correlations? There are four, dubbed Representability, Non-Orthodoxy, Projectability and Entanglement. These terms appear only in the essay, but the ideas are worked out in detail in the book.

Representability is just the fact that the observables and probabilities of a quantum system can be represented on a single Hilbert space. This may sound unenlightening or even trivial, but chapters three and four
of Hughes’s book make clear that this is not so. The treatment of incompatible observables in chapter three is especially nice. Here Hughes makes an elementary but very important point. Consider any family \( \{ A_i \} \) of, e.g., two-valued physical observables and a single statistical state \( p_1 \). It will always be possible to represent the state and the observables on a single vector space. However, if we consider even a pair of two-valued observables \( A \) and \( B \) and a pair of states \( p_1 \) and \( p_2 \), then unless \( p_1 \) and \( p_2 \) are related in a highly constrained way, it will not be possible to represent these observables and states on a single vector space; representability is a highly non-trivial feature of quantum systems.

This theme is further developed in chapter four, ‘Spin and its Representations’. Here Hughes gives a clear elementary account of how very general symmetry and continuity assumptions determine the Hilbert space representation of electron spin. The emphasis on symmetry is, of course, old hat to physicists, but it has been neglected by philosophers for too long.\(^8\) Hughes goes on, both there and in C&M, to make the point that a system can possess many of these very general symmetries without being representable in Hilbert space. Mielnik (1968) has provided an example of a possible system that exhibits probabilistic behaviour satisfying most of the symmetry conditions that Hughes lists for the probabilities of electron spin measurements, (essentially continuity, isotropy of space, and rotational invariance) but the transition probabilities are not quantum mechanical. On the one hand, this emphasizes the point that Representability is a non-trivial matter. On the other hand, the additional constraint that is needed to force the quantum mechanical representation is very “natural”. It is that spin, like angular momentum, be a vector quantity.

By looking at the observables and their representations in terms of symmetry considerations, we can see the representation as intelligible and, we might say, “natural”. I think this is part of the answer to a question that we raised in connection with Fine, namely, when it is appropriate to declare that some phenomenon is just a manifestation of “nature”. Begin by recalling an example that Fine mentions, but that Teller also appeals to (pp. 222–3). This is the switch from requiring that uniform motion be explained to seeing it as “natural”. Galileo didn’t just declare uniform motion to be natural; in effect he offered symmetry arguments to support the posit. Now of course, symmetry principles can fail. Indeed, nature could be any consistent way at all;
God could have made the world any way He wanted to. But the guiding assumption of theorizing is that nature is intelligible; or, as Einstein put it, that the Old One may be subtle, but not malicious. Appeals to symmetry are one way of making nature intelligible. They also call into question the dichotomy “explainable/natural”. To explain is sometimes to render intelligible or unpuzzling. We may explain something precisely by showing how it can be regarded as “natural”.

Departing from Hughes’s order of presentation, we come to Entanglement. This is ultimately related to what Fine means by the same term, but the immediate sense is different. Entanglement has to do with the fact that if we have two quantum systems, each represented by a Hilbert space, then the algebraically natural way of combining the Hilbert spaces – taking the tensor product – also provides a representation of the observables for the two systems. Among the vectors in this space, we have the “non-factorizable” ones, the existence of which is responsible for some striking facts: (i) All pure states of the pair determine “reduced” states for the subsystems, but (ii) there are pure states of a pair of systems that determine mixed “reduced” states for the subsystems. On the other hand, (iii) assigning mixtures to the subsystems does not suffice to fix the state of the joint system. Finally, (iv) the peculiar pure states of (ii) do not render the two subsystems statistically independent (C&M, p. 201). These are the features that comprise Entanglement. Features (ii) and (iv) are strikingly non-classical. That is, pure states on a classical probability space never exhibit features (ii) and (iv). In one sense, this is a mere rehearsal of a formal fact, in another, it isn’t. The fact that single systems are Representable is not just a brute fact; as noted above, considerations of symmetry can give us insight into what makes the representation tick – into how the components of the black box fit together. But these same considerations can be extended to pairs and larger collections to make some sense of Entanglement.

Entanglement is a species of holism. But the way in which Hughes, or I in my (1984), appeal to it seems to me to have advantages over the general doctrine of relational holism. The emphasis here is on a very specific structure, and what does the work is not just the fact that the singlet state doesn’t supervene on the states of the subsystems; it is the way in which this state is related to the eigenstates of the observables to be measured, and the way in which, via Gleason’s theorem, the overall structure fixes the probabilities. We don’t need to decide
whether violations of OI per se, or, for that matter, causal relations are cases of relational holism. Rather, we get a very fine-grained picture of how it is that classical probability structures lead to Bell’s inequality, but quantum structures allow it to be violated.

Entangled states are an indication of a more general feature of quantum mechanics: Non-Orthodoxy. Non-Orthodoxy is the fact, exhibited especially clearly in results such as Bell’s theorem and Kochen and Specker’s theorem, that quantum probability functions are not classical. The underlying probability space – in effect, the lattice of subspaces of Hilbert space – has a very different structure. Hughes’s assessment of Non-Orthodoxy is important in distinguishing his view from more standard quantum-logical views. Putnam and, especially, the Western Ontario quantum logicians saw the lattice of subspaces as telling us how the properties of a quantum system are structured. But Hughes rejects talk of properties for quantum systems altogether. Instead he sees the real insight of the quantum logical program as lying precisely in its emphasis on the non-standard character of quantum probability – on Non-Orthodoxy. But associated with Non-Orthodoxy is a generalization of classical conditionalization to what has come to be known as the “Lüders Rule”.

In the book, Hughes spells this out in some detail. For any probability measure $\mu$ on the subspaces of Hilbert space, and any subspace $S$, there is exactly one measure $\mu_S = p(X/S)$ such that whenever $R$ is a subspace of $S$, $\mu_S(R) = p(R/S)$ is given by $\mu(R)/\mu(S)$ (p. 224). This new measure comes as close to conditionalization on the non-standard probability space as it is possible to come. Hughes sees the application of Lüders’s rule as telling us how the state changes on measurement. Hence, he uses the term Projectability to refer to the applicability of Lüders’s rule.

Let us now tie these strands together. Here I will both follow and (marginally) expand upon Hughes’s account. Representability, supplemented by the account of spin in terms of symmetry, provides insight into how the spin observables for a single spin-1/2 system are related. The story is continued by introducing the tensor product representation for the pair, and noting that the singlet state exhibits Entanglement. Entanglement is tied to a particular manifestation of Non-Orthodoxy; the way in which the individual systems are represented leads naturally, if not inevitably, to states that exhibit the peculiar statistics characteristic of the singlet state. Finally, given the result of a measurement in one wing, Projectability, the conditionalization rule for the non-orthodox probability space, provides a revised state that predicts the perfect
EPR correlations and the more general statistical correlations that are the subject of Bell’s inequality.

Note that this account (if it is successful) provides a response to van Fraassen’s attack on epistemic realism. It is not a causal account. But it is a realist account. Further, it bears on the matter of contextual hidden variables. Van Fraassen’s throwaway comment that “sub specie the quantum formalism, the world is much more tightly organized and internally connected than our general intuition suggests” might well serve as a motto for Hughes and for the more general quantum logical tradition into which his view fits. On the quantum logical view, what is wrong with contextual hidden variables is precisely that they ignore the tightly organized structure of the quantum observables. Further, Hughes in no way presupposes that observables have values prior to measurement. Measurement elicits certain events; it localizes the values of observables. But the pattern of localization is guided, so to speak, by the way in which the observables are knit together in the Hilbert space. The singlet state is orthogonal to – excludes – cases in which parallel spin measurements have the same value. And given the way in which the singlet state is related to the eigenstates of the other product observables, Gleason’s theorem completely determines the probabilities for the measurement outcomes.

It should be clear that I am broadly sympathetic to Hughes’s account of the correlations. This is not surprising, since (as Hughes points out) it is similar to views that I have offered in print already. I also think there is genuine and important novelty here. To mention just one thing, I think that Hughes has done more than anyone else to provide content to the important but elusive notion of a principle theory. Nonetheless, I have some complaints and some worries.

Hughes’s account of the correlations, and the account I would favor, is in the spirit of the discussion of the tosses of the die several pages back. It portrays measurements as creating information; as realizing possibilities. But this presupposes a particular view of the measurement problem; it presupposes that measurement is not a linear process. I think that something like this must be right, but saying more is notoriously difficult. Hughes is well aware of all the problems, and says some interesting things about quantum mechanics as a theory formulated against a “classical horizon” (see especially pp. 312 ff.) but the discussion leaves one thirsty for more. To be sure, this is not a problem peculiar to Hughes or the quantum logician, but it takes on a particular
aspect there. More needs to be said about the underlying notion of “event”, and I find Hughes much less informative than I would like on this score.

The objection I want to raise is not crucial in the context of Bell’s inequality, but in a full-scale evaluation of Hughes’s position it would be important. I am quite unconvinced by his claim that we should eschew talk of properties in dealing with quantum systems. The argument for the view seems to be that any attempt to ascribe properties to quantum systems will result in a change of logic with little accompanying gain, and in any case, the resulting properties will be “pallid, scarcely recognizable variants of these creatures” (p. 217). But it is hard to see how Hughes himself can avoid talk of properties. His is an event interpretation, and the most natural way to think of events is in terms of the instancing of properties. Further, part of the pallidity of quantum-logical “properties” is that they are frequently disjunctive. But disjunctive properties are not obviously worse than disjunctive events, and Hughes cannot avoid disjunctive events. This comes out very clearly in his discussion of the two-slit experiment. In order to apply Lüders’s rule, he must say that when both slits are open, the event $A \lor B$ occurs, which is “the event that the electron passes through either [slit] A or [slit] B” (p. 232). But Hughes also tells us that “we will no longer be able to describe the electron . . . as passing either through aperture A or through aperture B” (p. 237). In other words, a disjunctive event has occurred, a fact characterizable by means of a disjunctive statement. But neither of the disjoined events has occurred, and so neither of the disjoined statements is true.

I have no intrinsic objection to this story. But, to repeat, I don’t see why disjunctive events are less objectionable than disjunctive properties. Indeed, if it turns out that the best analysis of events analyzes them in terms of properties, then we are likely to be stuck with disjunctive properties anyway. So why not just leave them in from the start?

Hughes offers a substitute for properties, in the form of what he calls latencies (pp. 301–6). Latencies are probabilistic generalizations of properties. Whereas Hughes sees properties as grounding deterministic dispositions (e.g., the disposition of a green thing to appear green “under normal conditions”) latencies ground the probabilistic dispositions of systems. Hughes writes

\[
\ldots \text{we can never find a probability function } p \text{ such that, for}
\]
every event \( [E] \), either \( [p(E) = 1] \) or \( [p(E) = 0] \). That is to say, these latencies can never be reduced to properties. (p. 304)

However, this begs the question against the quantum logician, who denies that properties must have any such feature. Indeed, as near as I can tell, quantum logicians get everything from properties that Hughes gets from latencies, with the added interpretive bonus that we can say something about what the system is like – what is true of it – when it is not being measured.

In spite of the length of this essay, it has presented at best a selective sample of what these two volumes contain. A virtue that I haven’t stressed enough is the service that these two books will provide to instructors teaching courses in the foundations of quantum mechanics. Either, or both, could serve as the basis for an undergraduate course, but both contain plenty of material suitable for use with graduate students. Pedagogical virtue aside, I hope to have conveyed my own sense of just how stimulating the papers in Cushing and McMullin’s collection and Hughes’s book are. Both volumes deserve to become standard works in the foundations of quantum mechanics.

NOTES

* Research for this essay was partially supported by NSF Grant SES #8720600.
1 I have commented on this paper before (see my 1984), but I hope the reader will find what I have to say here sufficiently different from my previous remarks to justify returning to this paper.
2 This is not completely far-fetched. It is plausible that confirmable theories must be framed in terms of “natural kinds”, “genuine properties” or some such. And the naturalness of kinds or genuineness of properties might well have to do with their ability to figure in explanations.
3 This occurred to me as a result of thinking about a related critique of holism that Arthur Fine offered in a lecture about holism delivered at the 1990 Pacific Division of the APA.
4 Jarrett states these conditions in a somewhat different (and technically superior) way. However, the differences won’t matter here.
5 In particular, as Butterfield points out, apparent exceptions to it offered by van Fraassen, Salmon and Bell fail because they don’t take the potential screeners-off to encompass a large enough chunk of the common past (pp. 121–31).
6 This is, in effect, Reichenbach’s principle again. Teller notes (p. 220) that the statistician Lincoln Moses also subscribes to it.
7 “Random Devices in Harmony” were conceived of as ways of thinking about conser-
vation laws in an indeterministic world. They were pairs of particles whose individual responses were completely random, but somehow determined at the source to mirror one another (Fine 1981, p. 547).

\[\text{Van Fraassen is an exception to this rule.}\]

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