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HOW SHOULD WE EXPLAIN REMOTE CORRELATIONS?

John Forge

Wesley Salmon ends his intellectual history of the theory of explanation (Salmon 1989, p.186) by quoting a passage from *Scientific Explanation and the Causal Structure of the World* to the effect that explanation in quantum mechanics "remains the premier challenge to contemporary philosophy of science". While it would be difficult to dispute this judgment today, it seems that quantum mechanics constitutes a stronger challenge to some philosophers than to others. In particular, it represents a very stern challenge to those such as Salmon and myself who subscribe to the *ontic* conception of explanation. Why this is the case will become clear in the first two parts of this paper when I give an exposition of the ontic conception. It will also be seen that when it comes to *constructing explanations* — this is the general theme that I will address in this discussion — the ontic conception directs us to find some 'real relation' in which the explanandum phenomenon stands. The problems posed by quantum mechanics are evident: where can we find real relations in the quantum domain?

There is a strand of the quantum logic approach which seeks to show that the structure of events in quantum mechanics is different from that of the classical domain and that this enables us to explain matters such as remote correlations. R.I.G. Hughes endorses this approach in his excellent book *Structure and Interpretation of Quantum Mechanics* (Hughes 1989a) and in a paper published in the same year as the book (Hughes 1989b). Hughes shows that the models which (the Hilbert space formulation of) quantum mechanics provides for computing the probabilities of various occurrences, such as correlations between measurement results at remote locations, differ from classical probability models. Moreover, he claims that these furnish *structural* explanations. Hughes does not, however, make any attempt to justify this claim: he does not show how it accords with, or is justified by, any mainstream theory of explanation.[1] This remark is not intended as a criticism; it is, rather, that Hughes' concerns are primarily those of a philosopher of physics. The question I wish to address, then, is whether Hughes' structural explanations can be incorporated within the ontic conception of explanation. This would, so to speak, benefit both camps: it would help the ontic conception come to grips with explanation in quantum mechanics and it would provide some justification for Hughes' claim that he has actually provided explanations. But let me make it clear from the outset that I do *not* believe that Hughes has actually succeeded in explaining remote correlations, although he may have taken a first step in that direction.

1. *The Ontic Conception of Explanation I.*

Salmon has classified theories of explanation under three headings or 'conceptions', namely, the modal, the epistemic and the ontic. This classification is by now fairly well-known so I will not say anything about the modal conception.[2] I do, however, want to indicate that we need not adhere to Salmon's own ideas about the ontic conception. I will suggest that we can distinguish an official or majority position with regard to the ontic conception, held by most people, and an unofficial or minority position held by at least one person (myself). The latter is broader than the former, allowing for different sorts of ontic explanations. Unless we allow for this broadening of the original ontic conception it will not be possible to construct explanations in the quantum domain (nor is it possible to construct explanations in certain classical domains), as we shall see in a moment.

According to the *epistemic* conception, an explanation is necessarily related to some epistemic concept. What is meant by this can be best understood in terms of an example. Thus, Hempel's theory asserts that an explanation is an answer to a why-question that provides 'nomic expectability' of the explanandum-phenomenon. What this means is that if a (rational) person is aware of the explanans of a D-N or an I-S explanation, then, had she not known that the phenomenon had appeared, she would have had good grounds for expecting it. What is provided is *nomic* expectability because the explanans for the models are required to contain law statements. For Hempel, understanding simply is nomic expectability. An explanation is in fact an argument which answers a why-question by providing understanding in this sense. An argument that does not provide understanding is not, however, merely a poor explanation, it is no explanation at all.[3] In general, what distinguishes a theory of explanation that falls under the epistemic conception is that it maintains that there is a logical relation between explanation and some epistemic notion. Or, to put the matter in terms of concepts, we can simply say that the epistemic conception holds that explanation is an epistemic concept. No doubt we could make this talk of 'epistemic concepts' more precise. But it is not really necessary to do so here, for enough has been said to differentiate this conception from the main focus of our attention, the ontic conception.

On the official version of the ontic conception, an explanation identifies the *causal mechanisms* responsible for the production of the events to be explained. Salmon has developed an ontology of causal processes which describes what these mechanisms are like.[4] Suppose we have a causal account of some event. This may not provide nomic expectability of the event if the probability of the event's taking place is low. Hence this account will allow some explanations that would not qualify as such on the Hempel theory. Furthermore, it is evidently not an epistemic theory because there is no necessary connection with any epistemic concept. Once a causal mechanism has been given and we have an ontic explanation, it does not follow that we *must* know why or understand why the event took place. In Salmon's words, a causal-mechanical explanations shows how an event "fits into the causal structure of the world". It points to a relation *in re* which holds independently of anyone's knowledge or understanding. This is why it is an ontic explanation.

There may well be some *contingent* relation between the causal account and our understanding of an event. Indeed, if in actual fact we very rarely understood events when presented with a causal account, or if there was no argument to the effect that we should understand events when given causal explanations, then it is hard to see how there could be any basis for a causal theory of explanation. One of the reasons why I like the ontic conception of explanation is that it does not make the relation between explanation and understanding a matter of definition. I firmly believe that explanations should provide understanding, but that understanding should not be a consequence of what we mean by explanation. The causal-mechanical account provides what we might call *filling-in-the-gaps* type understanding. A causal mechanism shows how an event was produced from given antecedents via spatio-temporally continuous processes that transmit energy and momentum from one location to another. It fills in the gaps between the events *in re* and hence it seems that it also fills in the gaps in our understanding of the explanandum. Again, the relation is contingent: the existence of a causal-mechanical explanation does not guarantee that the event is understood. Nevertheless, we very often do understand events when the gaps are filled in and it is this aspect of scientific explanation which is so well expressed by Salmon's theory.

Let us suppose that we have a scientific theory which we use to construct causal-mechanical explanations. This theory will be a *local realistic theory*. It will be local in that the production of structure and interactions, to use Salmon's terminology, will take place when causal processes come together. Causal processes do not act at a distance, which is why there are no gaps. The theory is realistic, not just in the sense that the processes exist *in re*, but in the sense that they are definite; they carry definite amounts of energy, momentum, structure, information, etc. It is well-known that the Bell-type inequalities which restrict the degree of remote correlation for certain pairs of events presuppose that the interactions are governed by local realistic theories. In fact we can see this by considering just the first two steps in Redhead's very nice derivation, although I will include the whole derivation here as it, or rather its violation, will exemplify what is meant by remote correlation (Redhead 1987, p. 84).

The correlations are to be produced by an experiment which comprises the following: (a) a device for preparing pairs of particles as composite systems having 0 spin (pure singlet state), (b) a means of separating the pairs and propelling them in opposite directions without in any way affecting whatever spins they may have individually, and (c) two detectors, A and B, able to measure spins (values $= \pm 1$) in any direction perpendicular to the line of flight of the particles.[5] The experiment is as follows: Two directions for each detector, a, a', b ,b', are chosen and 4 sets of results, $\mathbf{a}$, $\mathbf{a}'$, $\mathbf{b}$, $\mathbf{b}'$, are obtained, one for each possible combination of directions (a, etc., signify directions while $\mathbf{a}$, etc. signify values measured in those directions). The product of any 2 such values is ± 1. Now consider the sum

1. $s = \mathbf{ab} + \mathbf{ab'} + \mathbf{a'b} - \mathbf{a'b'}$

Write

2. $s = \mathbf{a}(\mathbf{b} + \mathbf{b'}) + \mathbf{a'}(\mathbf{b} - \mathbf{b'})$

Since **b** and **b′** either have the same sign or they do not

3. $s = \pm 2$

Now form

4. $\langle \mathbf{a},\mathbf{b} \rangle = 1/n\Sigma_i(\mathbf{ab})_i$

So for n cases the sum s_n is

5. $s_n = 1/n\Sigma_i s_i = \langle \mathbf{a},\mathbf{b} \rangle + \langle \mathbf{a},\mathbf{b'} \rangle + \langle \mathbf{a'},\mathbf{b} \rangle - \langle \mathbf{a'},\mathbf{b'} \rangle \leq 2$

(Think of s_n as the average of n sums of form 1.)

Define correlation coefficients

6. $(\mathbf{a},\mathbf{b}) = \lim(i \rightarrow \infty) 1/n\Sigma \mathbf{a}_i \mathbf{b}_i$

Then in the limit

7. $|(\mathbf{a},\mathbf{b}) + (\mathbf{a},\mathbf{b'}) + (\mathbf{a'},\mathbf{b}) - (\mathbf{a'},\mathbf{b'})| \leq 2$

Quantum mechanics gives

8. $(\mathbf{a},\mathbf{b}) = -\cos\Theta_{ab}$

where Θ_{ab} is the angle between the settings. Suppose the a-setting = b-setting and this bisects the angle 2Φ between a′ and b′. Then unless

9. $F(\Phi) = |1 + 2\cos\Phi - \cos 2\Phi| \leq 2$

7 will not be satisfied. But for $0° < \Phi < 90°$, $F(\Phi) > 2$, and hence the

inequality should be violated.

In order to obtain results to substitute in equation 1, it would be necessary to conduct 4 different experiments. A pair of particles can only be subject to two measurements, one at A and one at B. But in equation 2, **a** and **a**′ have been factored out and this is only justified if the **a**'s in the first 2 terms in equation 1 are the same, similarly for **a**′. Which is to say that the terms in 1 are constants, not variables. It is therefore necessary to make two assumptions, namely that the value at A(or at B) would be the same regardless of whether the B(A) setting was b or b'(a or a'), and that the spin of the particle has a definite value in the $a'(b')$ direction even though this value was not measured. Suppose the value in the a direction is $+1$ when the B-setting is b. What has come to be called parameter-independence is the assumption that had the setting at B been b', the measurement at A would still have been $+1$. This is clearly a locality assumption and it warrants the factoring of **a** in the first term of 2. If the particle did not have a definite spin in these circumstances in the a' direction, then there could be no second term in 2. This is evidently a realist assumption. Without these assumptions the derivation of the inequality cannot get going. Since the inequality does not hold, at least one of them must be rejected. This confounds the causal-mechanical account.

van Fraassen has claimed that no explanation of remote correlations is possible. In a nicely titled response ("Sailing into Charybdis"), Allen Stairs argues that this will be the case only if causal-mechanical explanations are the only acceptable explanations (Stairs 1984, p. 354). Stairs himself believes that "The thesis of realist quantum logic is that the world has a non-classical logical structure ... and that appeal to this logical structure can play a role in explanation" (Stairs 1984, p.356). I take this to be Hughes' position as well.[6] So, do we look for some other realist theory of explanation as a basis for a quantum logical explanation of remote correlations or do we try to adapt the ontic conception? I opt for the second alternative.

2. *The Ontic Conception of Explanation II*

What I have called the 'unofficial version' of the ontic conception holds that the matter to be explained, be it an event, phenomenon, state of affairs or whatever, is to be shown to stand in some appropriate real relation.[7] By a 'real relation' I simply mean one that holds independently of us and our investigations. The causal relation between events, as conceived by Salmon, is a real relation, as are, for example, spatio-temporal relations. But whereas many would agree that we can appeal to causal relations to give explanations, there is not much scope for explanation in terms of spatio-temporal relations. David Ruben refers to those real relations which can be appealed to in explanations as "structural relations"(yet another sense of that term!), but he concludes that causal relations are the only viable candidates for structural relations (see Ruben 1990, p.210 and pp. 209-232).[8] To broaden the ontic conception it is necessary to come up with more candidates for appropriate real relations.

The so-called instance view of explanation (e.g. Forge 1986) is based on the idea that events, etc. are explained if they are instances of laws of nature. This account identifies the relation between laws and their instances as appropriate for the purposes of explanation. This will only be acceptable if the knowledge that something is an instance of a law of nature constitutes understanding. Again, this need not necessarily happen, but a case must be made for saying that once it is seen that something is an instance of a law of nature, then the explanandum can be understood. An account that tries to base itself on spatio-temporal relations will fail this test. It seems that there is no way in which, say, the magnitude the current flowing in a circuit in one laboratory can enable us to understand why the density of sample being prepared in an oven in the next door laboratory is what it is, even though they bear definite spatio-temporal relations to one another.

Causal laws may well provide filling-in-the-gaps type understanding. But I do not think that all laws are causal laws, nor do I want the instance view to reduce to the causal-mechanical account. Thus the instance view posits a *systematic* type of understanding in which a state of affairs, event, etc., is understood when it is seen as systematised, as part of a pattern in nature which constitutes a law of nature.[9] One can embellish this notion if one is willing to indulge in a little speculative metaphysics: laws of nature can be interpreted as relations between universals (Armstrong 1983, *passim*) or as generalisations holding in a range of possible worlds (Bigelow and Pargetter 1990, pp.245-258). This is not the place to discuss these possibilities. However, the account we give of laws of nature will be of considerable significance when the adequacy of the instance view is assessed. Showing that something conforms to an accidental generalisation will systematise it but this will not, I think, enable us to understand it. What laws of nature have over and above accidental generalisations must be such that systematisation into lawful patterns does indeed provide understanding. The instance view must address these issues, but not here, for it does not seem as if we can account for remote correlations by appealing to laws of nature.

The problem with trying to use the instance view, or any other theory of explanation (such as Hempel's) that incorporates laws, as a basis for explaining remote correlations is that quantum mechanics does not appear to deal in laws. Of course, if Schrödinger's equation, the Heisenberg relations, etc., were stipulated to be laws of nature then quantum mechanics would deal in laws. But then we would have a highly amorphous collection. Laws in classical physics are, essentially, relations between quantities. What used to be called empirical laws, such as Boyle's law and Ohm's law, quite clearly are (or rather *designate*) relations between quantities. Theoretical laws such as Newton's laws and Maxwell's equations are usually thought of as describing the *states* of classical systems. This becomes more explicit when we consider, for instance, the analytical mechanics of Lagrange and Hamilton and the phase space representation of classical particles. There are certainly important and interesting differences between the 'empirical' and the theoretical laws of classical physics. For example, the latter (with the exception of thermodynamics) describe the ways in which states evolve while the former are often simple correspondences between quantities. But since the state of a classical system is just a collection of values of certain physical quantities, like position and momentum, the laws and principles which describe the evolution of states thereby determine relations between the values of quantities. In classical physics, systems possess definite values for a given range of quantities at all times, and the ways these values are inter-related are described by laws.

Quantum mechanics is not like this at all. States are not collections of quantities but closed linear subspaces S(**H**) on a Hilbert space **H**, or, more generally, they are density operators D on the Hilbert space. Perhaps we should admit that we don't really know what the states of quantum mechanical systems are, but that whatever they are quantum mechanics tells us they can be *represented* by subspaces and operators of the Hilbert space. Schrödinger's equation specifies the way in which this state evolves when the system in question does not undergo measurement. Moreover, unless a system is in an eigenstate with respect to a quantity or 'observable', then there are grounds for denying that it possesses a definite value for that quantity. All in all the differences between the quantum and classical domains are much greater than those between the empirical and theoretical laws of the latter.

If the only viable interpretation of quantum mechanics is as an instrument for predicting the results of measurements on systems that had been subject to given state-preparation procedures, then no ontic account of remote correlations will be forthcoming. It is, therefore, necessary to try to find some other candidates, apart from causal relations and lawful connections, for the real relations which hold between the correlated events. This is where the realist approach to quantum logic comes in. Before I turn to Hughes' discussion of this matter, it is worth remaining in the classical domain a little longer to see how the ontic approach can be developed beyond the instance view. For we cannot simply claim that Hughes' approach conforms to the ontic conception; some argument is necessary. The general argument, in outline, is as follows: it is possible to systematise events, etc., in several different ways. Showing them to be instances of laws of nature is one way, but there are others besides. Provided that these 'other ways' can be seen to involve appropriate real relations, then according to the ontic conception these methods of systematisation will lead to understanding. As an example let us consider the measurement of an extensive quantity, such as mass or length.

3. *Structural Explanation I: Extensive Measurement*

The work of the Structuralists takes its point of departure from Suppes informal style of axiomatisation, whereby a theory is axiomatised by defining a set-theoretical predicate.[10] This serves to display the models of the theory and leads naturally to the idea that theories just are various collections of models. The main exponents of this 'school' are Balzer, Moulines and Sneed, whose authoritative monograph *An Architectonic for Science* (BMS, 1987) develops this type of reconstruction to a high degree of sophistication. I shall refer to their axiomatisation of extensive measurement as a means for explaining the relationships between certain measurements.

The axiomatisation is as follows (BMS 1987, p.4):

M(EXT) x is an *extensive structure* iff there exist D, $\succeq$, $\oplus$, such that
(1) $x = \langle D, \succeq, \oplus \rangle$,
(2) D is a non-empty set,
(3) $\succeq$ is a binary, transitive relation, connected on D
(4) $\oplus$ is a binary operation on D
(5) for all a,b,c $\in$ D: $a \oplus (b \oplus c) \sim (a \oplus b) \oplus c$
(6) for all a,b $\in$ D: $a \oplus b \sim b \oplus a$
(7) for all a,b,c $\in$ D: $a \succeq b$ iff $a \oplus c \succeq b \oplus c$
(8) for all a,b,c,d $\in$ D, there is an n $\in$ N such that $a \succ b$ implies $na \oplus c \succeq nb \oplus d$.

where $a \succ b$ means "$a \succeq b$ and not $b \succeq a$", $a \sim b$ means "$a \succeq b$ and $b \succeq a$"and where N is the class of natural numbers. To find actual instances of **M(EXT)** it is necessary to interpret the symbols. That is to say, it is necessary to find a collection of objects with a suitable binary relation and operation defined on it.

This is to be done in two stages as follows: First we construe $\succeq$ as signifying a particular quantitative relation, such as "has at least the same mass as", and $\oplus$ as a composition operator which we signify by $\succeq_m$ and $\oplus_m$, where m stands for mass. Next we must 'operationalise' the quantitative relation and the composition operator, and this can be done with reference to a measurement technique such as the equal-arm balance technique for measuring mass. By operationalising $\succeq_m$ and $\oplus_m$ in other ways, further models for **M(EXT)** will be found, for in general different 'operationalisations' do not generate precisely the same collections of models. Moreover, by interpreting $\succeq$ and $\oplus$ in other ways, for example as signifying "at least the same length as"and as a concatenation operator on lengths, we find more models. But what can be *explained* by displaying models for **M(EXT)** in this way?

It seems that all sorts of things can be explained. For instance, suppose we have a number of small masses, such as can be placed on the pans of an equal-arm balance, and a number of rods that can be laid end to end and next to one another. We can establish a linear order among the masses, we can see how this order is modified by forming various composites, we can divide the masses into subclasses and see if there are ways to pair off elements of different subclasses so as to preserve the orderings in the subclasses and we can even set up a scale of measurement by using a given subclass as the class of standards. The same can be done with the lengths. Moreover, possible order-preserving pairings off between the masses and the lengths can be investigated. All of these revealed or empirical matters of fact can be explained by pointing out that the classes belong to **M(EXT)** under the interpretations given. That is to say, the empirical matters of fact are what they are because the objects really do constitute extensive structures. Or, to put the same point in a different way, the objects stand in the structural relations displayed by the axiomatisation of the theory of extensive structures. And there is obviously a great deal more that can be explained when we consider more objects, more measurement techniques and more interpretations of $\geq$ and $\oplus$.

The *reason* why there are explanations of this kind is that demonstrating that something is a model for **M(EXT)** systematises the objects in question. To express this in the terminology of the ontic conception, they are seen as an instance of a pattern or structure. If we continue to maintain the view that laws are relations between quantities, then we have here a different sort of pattern. It comprises a *quantity* as opposed to a relation between quantities. This raises the question as to what justifies the claim that this systematisation is such that it provides understanding. Why is it that the empirical relationships revealed by the measurement techniques are understood when they are seen to exemplify extensive structures associated with quantities? Just as we needed a fully-fledged theory of laws to answer the corresponding question raised in connection with the instance view, so we need here a theory of quantity. I am now inclined to the position that quantities are universals and that appeal to universals can resolve this issue. Again, this is not the place to pursue such a topic[11], for it is now time to examine Hughes' account of remote correlations and see whether this can be incorporated within the expanded ontic conception illustrated here.

4. *Quantum Mechanics and Remote Correlations*

Quantum mechanics predicts that there will be correlations between remote events such as were described above, in that it is possible to calculate that the degree of correlation will be greater than 2 using the quantum mechanical correlation coefficients — see expressions 8 and 9. Prediction is not in itself explanation, at least not according to the ontic conception. Thus if the calculation is to yield or to suggest an explanation for the remote correlation, or even to show that no explanation is called for, then some analysis or interpretation of the details is required. In his paper (Hughes 1989b) Hughes attempts to show that the calculation employs non-Boolean, i.e. non-Kolmogorov, structures for probability. This is much clearer in his book (Hughes' 1989a), where he has time to elaborate on some of the technicalities involved.

Let us begin by reminding ourselves of the way in which quantum mechanical predictions are made.[12] The states of systems in the quantum domain are supposed to 'contain' all that can be known about the properties and behaviour of the systems. So if we want to find out how a system will respond when a certain measurement is made, we must determine its state. States are represented by the closed subspaces S(**H**) or the density operators D on **H**. The state of a system depends on how it is 'prepared'. Preparation can be understood either as the process in which the system is actually created, for example, paired systems of photons can be prepared in this sense by the laser excitation of atoms such as calcium, or simply as measurements. A proton whose component in the z-direction has been measured has thereby been prepared. Then the appropriate statistical algorithm is used for making a prediction about the result of a measurement. These results can be thought of as *events*, for example, the result of a reading of +1 at the A-station of the setup described above can be thought of as the event "showing a value of +1".

There are three different cases that need to be distinguished, depending on whether a system *s* has been prepared in a pure state, and if so whether or not it is an eigenstate with respect to the observable — call it O — to be measured, or whether it is in a mixed state. If it is in an eigenstate of O, then quantum mechanics predicts that there will be no uncertainty or spread in the value obtained. So if 100 replicas of *s* in O-eigenstates were subject to an O-measurement, the same value would be found every time. This can be explained as follows: Observables are represented by operators on **H** and predictions are obtained by applying operators to states and seeing what happens. Operators representing observables have spectral decompositions which render them equal to classes of projection operators (projectors) which are one to one with the subspaces of **H** that represent the states of systems having some value for that observable. In other words, there is a subspace S_{oi} and a projector P_{oi} for each distinct value *i* of the observable O. Projectors take state vectors and project them onto 'their' subspaces. If the state vector is already in the subspace, then the corresponding projector will leave it there. There is another way of looking at this case. If we think of states as being in the first instance represented by projectors rather than subspaces, then it will be necessary to apply a projector to a projector to make a prediction. However, projectors are indempotent, hence $P_{oi}P_{oi} = P_{oi}$.

If *s* is in a pure state which is not an eigenstate of O, then it will be represented by some projector, say P_{qj}, for some other observable Q.[13] The effect of this projector on the state of *s* will yield an *expectation value* for the result of the measurement. This is a weighted average of the possible outcomes of the measurements. If there are just two possible outcomes, as there are for spin in a given direction, we can think of this as giving the distribution of +1's and -1's for, say, 100 replicas of *s*. It would appear that we might be able to retain some link with classical physics if this statistical interpretation of pure states is adopted. Thus for the eigenstates of observable O, *s* has a definite value of this quantity and moreover we know what it is before measurement. If s is in a pure state which we know is not an eigenstate of O, then we cannot know what the O-value is before measurement but maybe we can put this down to our ignorance. Unfortunately, when it comes to mixed states we lose touch with classical physics altogether.

Pure states are always representable by single projection operators. Mixed states, on the other hand, are 'convex combinations' of projectors: if $w_1 + w_2 = 1$, where w_1 and w_2 are real numbers neither equal to zero, then $D = w_1P_1 + w_2P_2$ represents a mixed state (there also are mixed states with denumerably many combined projectors). Mixed states are thus represented by weighted averages of pure states, which are known as density operators. Projectors are also density operators, namely those which reside on the 'surface' of the space of operators representing the states of *s*. The mixed states which give rise to the most marked departure from classical behaviour are those which are prepared by the physical separation of a system composed of two or more subsystems. This is precisely the state of affairs exemplified by the remote correlation experiments. Recall that the paired or composite systems were prepared in a singlet (pure) state. This means that they will exhibit no net spin in any direction and we might try to think of this as the result of the two particles individual spins cancelling out in every direction. Quantum mechanics requires that the space of states of this system is the tensor product $\mathbf{H}_a \otimes \mathbf{H}_b$ of the spaces of states of its components, where the two subsystems have been labelled *a* and *b*. Suppose D_{ab} is a density operator on $\mathbf{H}_a \otimes \mathbf{H}_b$, then this represents a possible state of the composite system. Now suppose the components are separated. What does quantum mechanics tell us about the states of the component particles?

There are in fact all sorts of possibilities here, depending on the way in which the composite system is put together (Beltrametti and Cassinelli 1980, p.66). In the particular case we are considering, of two spin-one particles forming a composite, it can be shown that the density matrices describing the component parts are such that these are in mixed states. Reverting for a moment to the state vector notation, given that the singlet state is

$$\Psi = 1/\sqrt{2}(\phi_+ \otimes \theta_- - \phi_- \otimes \theta_+)$$

then the density operators for the two particles (a and b) are

$$D_a = 1/2(P\phi_+ + P\phi_-),\ D_b = 1/2(P\theta_+ + P\theta_-)$$

D_a and D_b are convex combinations of projectors, and therefore they describe mixed states. Now it may seem that we simply do not *know* which pure state $P\phi_+$ or $P\phi_-$ the *a*-particle is in, and hence which value it possesses for some A-setting. In which case it would have some definite though unknown value for some direction. But suppose the 'decomposition' of D_{ab} is not unique in the sense that a whole range of different projectors for *a* and *b* are possible. This is indeed so. Hence the 'ignorance interpretation' must fail, because a particle cannot be in more than one pure state at once, even if we cannot know which pure state it resides in. It seems that quantum mechanics is telling us that the particles are completely *unpolarised*: it is not that their spins cancel out in every direction, it is rather that they have no spins in any direction. If we go back to the derivation of the Bell inequality we can see that the move from step 1 to step 2 is blocked: the particles have no spins in any direction before they are measured, so no inferences can be made about what the value would have been found in the direction *a* if only a measurement were made in direction *b'* at the B-station.

When the particles are separated, then they are described individually by the two operators, and these will allow us to make predictions about the probabilities of the events of +1's and -1's for various settings. It is evident,. however, that this will not lead to any predictions about correlations: D_a only refers to *a* and will not give any predictions about *b*. To make predictions about correlations we need to use D_{ab}. That is to say, even when *a* and *b* are physically separated, it must be assumed that they are in the same state as they were when they were physically associated, which implies that as far as their spins are concerned their state remains the same (recall condition (b) mentioned above for the experiments). We need not worry about how this is done (for details, see Beltrametti and Cassinelli 1980, pp. 69-72), so let us go straight to Hughes summing up (I have changed his terminology slightly)

> ... the *a-b* pair is treated as a whole even when the two components are spatially separated. The correlation is not predictable from the states D_a and D_b, but from the state D_{ab} of the composite system; the system a + b is therefore not reducible to the sum of its parts. Indeed it is a consequence of the way that quantum mechanics constructs the probability space $\mathbf{H}_a \otimes \mathbf{H}_b$ for a + b from the probability spaces $\mathbf{H}_a$ and $\mathbf{H}_b$ of the components that this is so. In this respect the product of two quantum mechanical probability spaces is radically different from the product of two classical probability spaces. (Hughes 1989, p. 250)

In the classical case, the product of two probability spaces is a 'Boolean product', which is such that the probabilities of the component events determine the probabilities of the whole (see Stairs 1983, pp. 49-50). Let us grant that the non-classical (non-Boolean) structure of probability in quantum mechanics is responsible for remote correlations and let us suppose that this has been 'displayed' in some appropriate way (ideally by some Structuralist style reconstruction). Now we must ask whether this explains the remote correlations.

5. *Structural Explanation II: Remote Correlations*

The *structure* of probability in quantum mechanics is different from that of classical physics because it is non-Boolean. As Hughes himself shows, the collection of closed subspaces **S(H)** of **H** does not have the structure of a Boolean algebra (Hughes 1989a, pp. 190-191). It follows that quantum mechanical probability is non-Boolean because it is **S(H)** which defines the probability functions. This is all explained very clearly in Hughes' book (see especially Section 3.3) and it is not controversial. What is controversial is its significance. Does it, for instance, mean that we can (or must) give up classical logic, in particular the law of the excluded middle, when it comes to sentences describing quantum events? This would not, in my view, be much help when it comes to explanation. On the other hand, does the non-Boolean structure of quantum mechanical probability mean that quantum mechanical events have a different structure from those of the classical physics? If this is the case, then there is some scope for explanations which conform to the ontic conception — maybe the structure determines some 'appropriate real relation' between events.

Probability functions in classical physics are defined on spaces of events which are sets of possible outcomes (Halmos 1974, pp. 184-191). The space of events has the structure of a Boolean algebra, in that, for instance, the distributive law for intersection over union and its dual holds. The probability assignments preserve this structure provided the 'compounded' events are independent. When two spaces of events are combined to form a product space, then the product will also have a Boolean structure. For example, if the two spaces are comprised of sets of particles having values for position and momentum, such as one finds in statistical mechanics, then the resultant space will be the product of the individuals spaces and the probabilities of the joint events of particles from the two spaces having certain position and momentum values will be determined by the probabilities of the individual events. Now the way these events combine together is not a *consequence* of our probability assignments. The former is given and the latter must express the relations which hold therein. This is what I understand by a realist account of the logic of events. It is not a matter of formal logic, it is a claim about the independence of the structure of the events which make up the field of our investigation.

We have seen that structural explanations are such that an event, state of affairs, etc., is explained because it is a part or an element of a structure — it stands in certain relations which constitute the structure. We saw this for mass and length measurements and the extensive structure **M(EXT)**. Boolean structures are very pervasive; all sorts of things have Boolean structures. So if it were possible in an explanation to invoke the fact that certain events were embedded in a Boolean structure, then this would result in the events being systematised into a very pervasive pattern and as such might appear to be a very good explanation. It seems that requests for explanation about the way probabilities combine together to give joint probabilities might well be answered in this way. If explanations can be constructed in this manner in classical physics for Boolean structures, it will also be possible in quantum mechanics for non-Boolean structures. Although the structures are different, the putative explanations are of the same type; if one is legitimate, so is the other. Alas, there are two problems. One concerns quantum mechanics alone, the other is more serious and applies to the whole project of referring to structures of events to give ontic explanations.

The first problem is that probability in quantum mechanics is defined on S(**H**) and not on a space of events. S(**H**) is the representation of possible states and to make the proposal before us work, it will be necessary to interpret these as events. The normal procedure is to take the states to represent possible results of measurement — what Howard Stein calls 'eventualities' (Stein 1972, p.373). In the example we have been considering, the eventualities comprise the possible results of spin measurements at the A and B stations. To carry through the ontic conception, these eventualities must be assumed to exist independently of us and our measurement practices.

The second problem is this: We decided that systematisation by itself was not enough. The pattern or structure into which the explanandum is to be fitted must be such that the systematisation does indeed provide understanding. One suggestion was to take laws of nature and quantities to be certain sorts of universals. Thus showing that some state of affairs is an instance of a law of nature would refer it to a pattern of universals. Just why this would provide understanding was not spelt out, but the suggestion was that patterns of universals have a special character and this is what does the trick. It is not at all clear to me how something similar might be done in the case of eventualities or of events. In classical probability, events are just sets of outcomes and the Boolean structure is a consequence of the fact that a field of sets has this structure. Although I once thought that systematisations in set-theoretical structures were enough for explanations, I now have considerable doubts about this.[14] The prospects in the quantum mechanical domain look even less promising. In addition to accepting the independent existence of collections of eventualities, we must also posit some relations between these which are strong enough to ensure that systematisations provide understanding. I confess that I do not know how to deal with this problem.

6. *Conclusion*

I believe that we *should* explain remote correlations in the way Hughes suggests, that is, by displaying the models which quantum mechanics uses for predicting the correlations. We should do this because it accords with the (unofficial version of the) ontic conception of explanation. The aim of this discussion has been to offer some justification of this approach. We have ended up with a metaphysical problem, which, as a matter of fact, I think is where we should end up. The problems which quantum mechanics poses for explanation are metaphysical problems. Explanations are given with reference to the way the world is and it is precisely because quantum mechanics does not provide a clear and uncontroversial image of the world that we are confronted with such problems. To return to Hughes' account, it is surely most implausible to think that these difficulties could be overcome just by displaying the models quantum mechanics uses for probability. The present discussion has, I hope, clarified the issue. Displaying the models is just a first step; now we must do some metaphysics.

Griffith University

NOTES

1. There is a very brief paragraph (p. 257) which refers to a 1980 essay of Cartwright in which she discusses both theoretical and causal explanations in physics. Hughes sees the explanations he has in mind as theoretical. However, I do not think that Cartwright herself had a well-worked out account of theoretical explanation to complement her well-known views on causal explanation. I would also mention

here that Hughes is *not* using "structural" in the sense of the Structuralist programme of Balzer, Moulines and Sneed. I have tried elsewhere to develop a Structuralist account of theoretical explanation, but although I will mention Structuralism again, I will not make any use here of that account of explanation.

2. Any reader who by chance is not familiar with these scheme could consult the works by Salmon mentioned above.
3. What makes something a good explanation is the depth and the breadth of the explanation it gives and this is a quality of a good theory. See Hempel 1966, p. 75.
4. Harré has put forward a similar view. He writes "In order to give a scientific explanation every happening must be looked on as due to the workings of some mechanism ..." (Harré 1970, p. 124).
5. The actual values of the spins are not important, as they depend on the choice of units. What is important is that the particles have just two values. Electrons and positrons are often mentioned in connection with these experiments. These are in fact spin $\pm$ ½ particles, that is, their intrinsic angular momentum is ½ in units of h bar.
6. I am not aware of any publication in which Stairs has actually given an explanation of remote correlations and that is why I use Hughes as my example. Hughes himself acknowledges Stairs' contributions to the topic.
7. The unofficial version also draws a sharp distinction between explanation as themselves being states of affairs in the world and the *presentation* of explanations. One might give or present an explanation to a given audience by formulating an argument which conforms to the D-N model, whereas to another audience a less formal presentation of the same explanation may be more satisfactory. Salmon sometimes writes as if he is inclined to this strong interpretation of the ontic conception; for example, when he refers to a causal explanation as the state of affairs of something fitting into the causal fabric of the world (Salmon 1984, p. 274).
8. Ruben describes his position with respect to explanation as realist. This raises the question as to what is the difference between a realist view of explanation and the ontic conception. One can argue that Hempel adopts a realist view of explanation because he requires the explanans to be true, and, since the explanans must contain law statements, this means that he adopts a realist account of laws. In this

way realist accounts cut across the epistemic/ontic distinction. There are, however, other features of the ontic conception which serve to distinguish it from realist accounts. These are not relevant to the present discussion, but the interested reader might consult Forge 1989.

9. Campbell agrees with this notion of understanding. After describing some commonplace examples of frozen water bursting pipes, etc., he writes "Why do we feel that when we have received them [answers to requests for explanation], the matter is better understood ...? The reason is that the events and changes have been shown to be examples [instances] of a general law. Water *always* expands when it freezes ... Laws explain our experience because they order [systematise] it by referring particular instances to general principles ..." (Campbell 1953, pp. 78-79).
10. I must stress that I am not using "structural explanation" to mean explanation in the sense of the Structuralist school.
11. I used to believe that quantities were sequences of ordered classes of objects, such as one gets at the first stage of the interpretation of **M(EXT)**. But it seems that this cannot account for all our measurement practices and, moreover, it does not leave us any means of justifying the claim that the systematisations described in the text provide understanding. See Forge forthcoming.
12. For more details, see the first seven chapters of Beltrametti and Cassinelli.
13. For example, if O is the operator Z for spin the z-direction, then if the particle is in a pure state that is not an eigenstate of Z, this means that is polarized in some other, i.e. not in the z-, direction.
14. As was mentioned above in note 10, I no longer think that the purely set-theoretical account of quantities will do.

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