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enough guidance, it is the intuition of the researcher which comes to his aid.

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BUB ON QUANTUM LOGIC AND CONTINUOUS GEOMETRY

Until recently, Jeffrey Bub has been one of the more prominent defenders of the realist quantum logical programme set fourth in Hilary Putnam's 'Is Logic Empirical?'. Lately, in this journal and elsewhere, he has retreated from the confident position of *The Interpretation of Quantum Mechanics*. He has also suggested that whatever hope there is for quantum logic may rest in von Neumann's programme of continuous geometry. I will argue that abandoning standard quantum logic in favour of a version based on continuous geometry is unlikely to give us any new tools for solving the problems that Bub raises, but I will also suggest that many of Bub's worries may be needless.²

Readers who would like an elementary review of the ideas of continuous geometry may consult Holland's 'The Current Interest in Orthomodular Lattices' or Bub's 'What Does Quantum Logic Explain?'. For purposes of

¹ The views expressed are those of the author and not necessarily of his company.

² I would stress that even though I am not convinced by Bub's suggestions about the direction quantum logic should take, this paper still owes a considerable debt to Bub's work on quantum logic and to his lucid portrayal of the difficulties faced both by phase space interpretations of quantum mechanics *and* by the standard realist programme in quantum logic.

this paper, the following very brief account should suffice. Continuous geometry grew out of the theory of certain algebras of operators on separable Hilbert spaces called *von Neumann algebras*. (A familiar example is the set of all bounded everywhere-defined linear operators.) A von Neumann algebra is determined by its projection operators, which in turn determine an orthomodular lattice. If A is a von Neumann algebra and the only elements of A that commute with all elements are the complex multiples of the identity operator, then A is a *factor*. There are five types of factors, each associated with a particular sort of dimension function. The two most familiar are the sets of all bounded, everywhere-defined operators on, respectively, a finite-dimensional Hilbert space (type I_n) and an infinite-dimensional Hilbert space (type I_∞). The lattice associated with type- I_n factors is *modular*: ($a \leq b \Rightarrow (a \vee c) \wedge b = a \vee (c \wedge b)$) as well as *orthomodular*: ($a \leq b \Rightarrow b = a \vee (b \wedge a^\perp)$). The lattice of a type- I_∞ factor is orthomodular but non-modular. The dimension function of a type- I_n factor can take on (normalised) values $\{0, 1/n, 2/n, \dots, 1\}$, where n is the dimension of the Hilbert space. For type- I_∞ factors, the possible values of the dimension function are $\{0, 1, 2, \dots\}$. The case that interests us most is the type- II_1 factor. This gives rise to a lattice with no atoms (*i.e.*, no minimal non-zero elements) and its dimension function takes all possible values in the interval $[0, 1]$. This lattice is modular, but not orthomodular. Both lattices associated with type- I_n and type- II_1 factors are continuous geometries (as are various lattices having no special connection with Hilbert space, *including* arbitrary Boolean algebras), but we will use the term ‘continuous geometry’ to refer to lattices associated with type- II_1 factors unless otherwise specified. With these preliminaries, we can begin our discussion.

It must be noted that Bub’s views on this subject have not remained static, and the role that he envisaged for continuous geometry in this journal (‘Some Reflections on Quantum Logic and Schrödinger’s Cat’ henceforth just ‘Schrödinger’s Cat’) almost certainly does not represent his current view. Therefore, we will consider a number of papers. Nonetheless, I believe it will still turn out that continuous geometry is unlikely to add much that is new to the quantum logical interpretation.

‘Schrödinger’s Cat’ provides a useful starting point. In that paper, Bub is concerned with what he perceives as a need to establish the *reasonableness* of the quantum logical programme, and he offers a particularly clear argument for taking quantum logic seriously: the *EPR* problem and the difficulties that fall under the heading of ‘The Measurement Problem’ and/or ‘Schrödinger’s Cat Paradox’ both suggest that a satisfactory realist interpretation for quantum mechanics must preserve the idea that every quantum mechanical magnitude always has a value. Furthermore, reflections on *EPR* suggest that we must not represent ideal measurement as involving a disturbance, because the disturbance would have to be non-local. The hidden variable programme is one way of trying to uphold the value-

definiteness hypothesis, but a hidden variable theory would have to invoke a non-local disturbance to account for the *EPR*-type cases. Quantum logic, on the other hand, suggests a way of maintaining value-definiteness while denying that measurement involves a disturbance. Value-definiteness is suggested by the fact that if M is any discrete quantum mechanical magnitude and $\{m_i\}$ its spectrum, then the following exclusive disjunction is true

$$M = m_1 \quad \text{or} \quad M = m_2 \quad \text{or} \dots$$

and this seems to say that M takes one of the values m_i . The idea that the discontinuous change of state brought about by measurement does not involve disturbance comes from the fact that, at least in the ideal case, this change of state can be represented as the form of *conditionalisation* of the initial probability measure appropriate to the non-boolean structure of propositions that quantum logic ascribes to physical systems. In other words, the projection postulate (in the form of Lüders' rule) is simply the natural generalisation of *classical* conditionalisation to take account of the non-classical structure that quantum logic associates with quantum events.

There is no doubt that if all this could be defended, it would provide the basis for a very attractive interpretation of quantum theory, but Bub is well aware that there are problems. The most notorious is the absence of two-valued homomorphisms on the quantum logical algebra of propositions, leaving no consistent assignment of values to the magnitudes. Thus, he notes, even with quantum logic, it is very difficult to maintain the following four theses.

- (a) For any system S , at every time t , every magnitude has a value.
- (b) The values of the magnitudes of S preserve the characteristic algebraic structure of the magnitudes of a quantum mechanical system.
- (c) Ideal measurements merely reveal the values of these magnitudes . . . without disturbing S .
- (d) The statistical states of quantum mechanics represent all possible probability distributions over the values of the magnitudes for S , and transformations of probability distributions on measurement are derived by conditionalising the initial probability distribution in accordance with the measurement result. (Bub [1979a], pp. 36–37).

As Bub saw it at the time, 'The programme for quantum logic must be to maintain (a), (b), (c) and (d) by a non-Boolean analysis of the semantic notion of a property "obtaining"' (Bub [1979a], p. 38). His suggestion was that this might lead to a theory of 'sets without points' analogous to von Neumann's continuous geometry, which is a *geometry* without points. However, 'Schrödinger's Cat' was mainly suggestive. For more concrete proposals, we turn to 'The Measurement Problem of Quantum Mechanics'.

In that paper, Bub quotes with approval the following remarks from von Neumann.

A complete derivation of quantum mechanics is only possible if the calculus of logics is extended, so as to include probabilities, in harmony with the ideas of J. M. Keynes. In the quantum-mechanical terminology: the notion of 'transition probability' from a to b , to be denoted by $P(a, b)$ must be introduced. $P(a, b)$ is the probability of b , if a is known to be true. $P(a, b)$ can be used to define $a \leq b$ and $-a$: $P(a, b) = 1$ means $a \leq b$, $P(a, b) = 0$ means $a \leq -b$. But $P(a, b) = \phi$, with a $\phi > 0, < 1$ is a new 'sui generis' statement, only understandable in terms of probabilities (Bub [1979b], p. 120).

That is, as Bub points out elsewhere (Bub [1981a]), von Neumann was interested in an approach to quantum mechanics on which probability is *logical*. We will have something to say about this later. More important for our present purposes are the remarks that Bub makes about continuous geometry. He notes that von Neumann credits J. W. Alexander with the suggestion for the idea, since continuous geometry, as a geometry without points, is similar to Alexander's conception of *sets* without points. Bub comments:

Now this notion of a 'set without points' seems to be just the right sort of thing one would want to represent a property in a non-Boolean possibility structure, in which distributivity fails in such a way as to exclude the existence of 2-valued homomorphisms on the structure. I suggest that von Neumann is pointing to a generalized theory of properties for quantum-mechanical systems, in which properties are associated with 'sets', but no two-valued homomorphisms exist on the structure of properties, *i.e.*, the 'sets' have no points ([1979b], p. 121).

In fact, it is difficult to know how congenial von Neumann's own intentions were to realism. In the paper with Birkhoff, the overall tone is realistic. (They are careful to point out that different *experimental propositions* may be associated with the same 'physical quality'.) In 'Quantum Logics (Strict and Probability Logics)' von Neumann adopts a more operational approach. He characterises implication in terms of measurement, stating that ' $a \leq b$ ' means that if a measurement has shown a to be true, then an immediately subsequent measurement will show b to be true, with similarly measurement-oriented remarks on orthocomplementation and transition probability. Whatever von Neumann *might* have meant, however, the question is whether we can extract something from his remarks that will aid Bub's cause. And if we take that cause to be the one of 'Schrödinger's Cat', the answer seems to be no.

We begin with a technical comment. Bub suggests a connection between the absence of two-valued homomorphisms and sets without points, but in fact there is no obvious connection. To see this, recall that in *classical* contexts, there is an analogue of the notion of a set without points, *viz.*, an atomless Boolean algebra. But it is a theorem (*e.g.*, Sikorski [1960]) that *every* Boolean algebra is homomorphic to the two-element Boolean algebra. Further, standard quantum cases provide examples of structures with atoms, but no two-

valued homomorphisms. Thus, on at least one obvious construal of being 'without points', the fact that a structure has no points is neither necessary nor sufficient for the absence of two-valued homomorphisms.

The more important question, however, is whether non-atomic proposition structures might help us defend value-definiteness. Unfortunately, there is good reason to doubt that they will. For one thing, consider the fact that in standard quantum mechanics, there is *already* a place for non-atomic Boolean algebras. Both the position and momentum observables of a standard quantum mechanical system determine non-atomic Boolean algebras. This is associated with the fact that neither the position nor the momentum operators have eigenvectors in Hilbert space. But rather than helping us get a grip on the idea of definite values, this makes it *problematic* to claim that quantum systems have *either* a definite position *or* momentum, at least if we hold that all possible physical states are represented by vectors in Hilbert space. In fact, Paul Teller has argued (Teller [1979]) that the way these quantities are represented should lead us to the view that the position and momentum of quantum systems is always more or less *indefinite*. But if this is correct, continuous geometry will take us further away from definite values, rather than giving them a foundation.

This bears on a more general reason for doubting that value-definiteness has a place in a quantum-logical interpretation. As I have argued elsewhere (Stairs [1983a]), the *fundamental* thesis of realist quantum logic is that the algebra of quantum logical propositions represents the possibilities open to a quantum mechanical system and their logical relations. Furthermore, the emphasis on *logical structure* is very clear in Bub's own work, and this is reinforced in the opening paragraph of 'Schrödinger's Cat':

The claim that logic is empirical . . . is a thesis about the way the world is put together. The proposal that the logic of the world is non-Boolean concerns the actual possibility structure of events ([1979a], p. 29).

However, if quantum logic is concerned with 'the actual possibility structure of events', then if a certain event or state of affairs is *possible*, it ought to be represented in the quantum logical algebra of propositions. Restricting ourselves to standard Hilbert space quantum logic, we note a peculiar tension between the idea of value-definiteness and the 'fundamental thesis' of quantum logic. Let P and Q be two discrete magnitudes with no eigenvectors in common. If P and Q both have values, one of the possibilities open to the system is a state of affairs in which P takes one of its values, say p , and Q takes one of its values, say q . If the quantum logical structure represents the states of affairs into which the system can enter, then this one ought to be among them. But there is no such element in the logical space. In fact, if the logic is conceived as a lattice, ' $P = p$ and $Q = q$ ' is false for *every* choice of p and q . On the partial Boolean algebra approach, such conjunctions will not be well-defined, and so the possibilities that the

value definiteness thesis suggests ought to be represented are still missing. And while it *could* be claimed that this merely reflects some sort of necessary *ignorance* on our part, this would be to suggest that quantum logic is predominantly of *epistemological* rather than ontological interest, and hence to deny the fundamental thesis (Stairs [1983a]).

We can now see why continuous geometry will not help the quantum logician defend value-definiteness. The feature of quantum logic that underlies the difficulties we have been noting is the failure of the distributive law, or put in another way, the fact that the structure contains *perspectivities*; collections of elements a, b, c such that a implies $(b$ or $c)$ but the greatest lower bound of every pair of elements ab, ac, bc is the absurd proposition. This characteristic feature of projective geometries is retained in their generalisation to continuous geometries. In the non-atomic continuous geometry associated with type-II₁ factors, we will have elements a, b such that $(a \vee a^\perp) = 1$, $(b \vee b^\perp) = 1$, but $(a \wedge b) = 0 = (a \wedge b^\perp) = \text{etc.}$ Bub, of course, is well aware of this. However, my point is not that some technical fact has been ignored. It is, rather, that the tension described above between value-definiteness and the ontological ambitions of quantum logic will still be present if we opt for continuous geometry.

We should also recall that continuous geometries are not always atomless, and that all finite-dimensional quantum mechanical cases already involve continuous geometries of the atomic variety (factors of type I_n). Again, Bub is quite aware of this. But the relevance for our discussion is this: the *EPR* problem, which is important in the motivating argument that Bub offers for quantum logic, can be (and these days, typically is) raised in finite-dimensional Hilbert space. Furthermore, as both Heywood and Redhead ([1983]) and I ([1983a])¹ have noted, there is a version of the *EPR* problem involving two spin-one particles in which a Kochen-and-Specker-style argument is available, showing that the strict correlations alone, together with appropriate locality principles, rule out assignments of values. Since this case is one in which the lattice has atoms, it is difficult to see how atomless continuous geometries will help. It *could* be maintained that, contrary to all appearances, the spin components really are continuous observables, but there is no reason to think this is what Bub has in mind.

I conclude, then, that continuous geometry will not help us find a non-Boolean account of value-definiteness. However, Bub's more recent suggestions appear not to concentrate on value-definiteness. One of these has to do with locality. Continuous geometries are *modular*, whereas the lattice of all closed subspaces of an infinite dimensional Hilbert space is orthomodular but not modular. Further, the non-atomic character of continuous ge-

¹ Of course, quantum logicians have maintained that the logic really does contain an element representing the fact that every magnitude has a value, namely the conjunction of all the disjunctions ' $M = m_1$ or $M = m_2 \dots$ ' noted above. But as the above considerations should suggest, and as I have argued in detail elsewhere (see my [1983b]), it is by no means clear that this disjunction really tells us that every magnitude has a value.

ometries is intimately bound up with their modularity. Now there is a standard argument against modularity, namely that modularity is incompatible with localisability. Bub, however, suggests that we explore the possibility 'of von Neumann's continuous geometry as a generalised theory of the micro-domain, in which systems are to be thought of as inherently wave-like, *i.e.*, non-localisable' ([1981a], p. 291) and it might seem to be that if we were to adopt this course, the *EPR* problem would take on a quite different complexion. The other suggestion that Bub offers is that continuous geometry might help us resolve the measurement problem. Here we turn to 'What does Quantum Logic Explain?' There Bub stresses the fact that the measurement problem is associated with the difference between the pure state that the dynamics of quantum theory provides and the mixture that definite measurement results seem to require. Bub writes

The problem may well be characteristic of any quantum logic that admits pure states as well as mixtures. A crucial feature of von Neumann's quantum logic is that the lattice has no *atoms* in the general case, *i.e.*, there are no pure states, only mixtures for a system with an infinite number of degrees of freedom. A quantum logical solution to the semantic problem of measurement may lie in the application of von Neumann's atomless quantum logic to the macroscopic measuring instrument as a system with an infinite number of degrees of freedom (Bub [1981b], p. 99).

Let us begin with the role of pure states versus mixtures in the measurement problem, in particular, with what Bub calls the *semantic problem of measurement*. This is the question of how we can characterise the totality of properties possessed by a quantum system so as to avoid the apparent conflict between the measurement result and the superposition provided by the dynamics. Unfortunately, it is quite unclear that continuous geometry will help. What makes the measurement problem difficult is not the fact that the state provided by the dynamics is pure while the state apparently required by what we observe is mixed. To see this, we need only note that if the quantum-system-plus-measuring apparatus starts out in a mixed state, the dynamics will provide us with a mixed state at the end of the measurement. The problem is that this state will not be the 'right' mixture. It will not be a mixture of the measurement outcomes with appropriate weights, but of *superposition* of these outcomes. The fact that on the standard approach, we may be able to think of the system as 'really' being characterised by a single vector, and hence by an atom of the proposition system is irrelevant. What is crucial is not the existence of atoms, but, in lattice terms, the existence of perspectivities. And these still abound in continuous geometry.

This point is worth more time. Suppose the system to be measured and the measuring system are both represented by a type- II_1 factor, and suppose we want to measure a quantity A with two possible values, associated with propositions a and a' . (There will always be such quantities. Any proposition and its orthocomplement will define one.) Then we will need to set

up a correlation between this observable and a three-valued observable of the measuring apparatus, where a and a' are associated with propositions m and m' , and m_0 is the proposition associated with the ground state of the measuring device. Suppose that before the measurement, b describes the system to be measured, where $(b \& a) = 0 = (b \& a')$, and m_0 is true of the apparatus. Then $(b \& m_0)$ is true of the joint system. In order to find the value associated with a , $(a \& m)$ must be true at the end of the measurement. Given our characterisation of b , we have

$$\{(b \& m_0) \& (a \& m_0)\} = 0. \quad (1)$$

In standard quantum mechanics, dynamical transformations induce automorphisms on the logic. And since Bub does not call this feature of the theory into question, we may assume that it will be present in the hypothetical continuous geometry version of quantum mechanics. Let U stand for the automorphism characterising the measurement of A . As in the familiar case, it presumably will have the feature that $U(a \& m_0) = (a \& m)$. Furthermore, given (1), we must have

$$\{U(b \& m_0) \& U(a \& m_0)\} = 0, \text{ i.e.} \quad (2a)$$

$$\{U(b \& m_0) \& (a \& m)\} = 0, \quad (2b)$$

and since similar remarks apply to a' , we *still* have the result that the proposition provided by the dynamics for the characterisation of the system after the measurement (*i.e.*, $U(b \& m_0)$) is incompatible with the proposition characterising the observed result, *even though the logic does not admit pure states*. The problem is with the dynamics; not the presence of atoms in the logic. The only way Bub could avoid this result is to posit a dynamical law which does not induce automorphisms on the logic. But then the motivation for introducing continuous geometry would be obscure, since one could adopt the same approach in the usual setting.

On the question of locality, the extreme sketchiness of Bub's suggestions presents a problem for their evaluation, and at this point, I am not so much criticising Bub's programme as speculating on what his suggestions might mean. To begin with, the connection between modularity and non-localisability is not entirely clear. As Jauch notes, in the standard Hilbert space presentation of quantum mechanics, the lattice generated by the projections in the spectral representations of the position and momentum operators is non-modular. But he adds that 'the question of whether localisability can be formulated within a modular lattice of propositions in a more general setting remains open' ([1968], p. 221). Bub does not give us any real idea of what quantum mechanics would look like in the setting of continuous geometry. Presumably, we would avail ourselves of a factor of type II_1 rather than of type I_∞ . And since type II_1 factors are defined over a Hilbert space, this might be taken as evidence that the failure of localisability will be operative. But without a more concrete account of what the

new theory would look like, one can't be sure. Let us leave this issue aside, however, and assume that localisability would indeed fail. What seems clear is that we have moved from the problem of *interpreting* quantum mechanics to the problem of replacing it with some new and, presumably, significantly different theory, and this seems to amount to admitting that quantum mechanics, as it stands, does not admit of a satisfactory interpretation. The problem is no longer one of understanding a quantum-theoretical world, but of understanding some other world, described by a related but different theory. Two sorts of questions arise concerning this (incompletely specified) theory. One is whether it is physically acceptable, and (aside from the fact that the theory doesn't really exist) this is not a question that could be answered here. The other question, however, is whether the *philosophical* motivation for considering it is sound. That is, in this context, we must ask whether the non-localisability that Bub takes to be part of this new theory is likely to provide us with a useful approach to understanding currently-known puzzling phenomena, in particular, the *EPR*-type cases.

My guess is that the answer will be no. The mere fact that Bub's proposal would render quantum systems essentially field-like does not get us around problems of locality. Field propagation is normally thought to be subject to relativistic restrictions, and it is precisely these restrictions that lie at the root of many people's worries about locality in quantum mechanics. More specifically, a common worry is that in order to account for the violation of Bell's inequalities involved in the *EPR* phenomena, it will be necessary to posit the existence of faster-than-light causal signals, and such things are widely held to be incompatible with relativity. If Bub were to propose that the fields associated with continuous geometry do not satisfy the causality requirements of relativity, this would certainly give us a way of dealing with the *EPR* problem, but since the crucial part of the suggestion would be that quantum systems involve superluminal causal signals, the relevance of continuous geometry would be dubious.

Even though fields may be non-localisable, classical fields are *spatial* (or spatio-temporal) entities, and the values of field quantities vary spatially. It *could* be that Bub is suggesting that quantum mechanics should be reformulated in a version in which spatial concepts are simply absent. However, this would be an extremely drastic solution, and there is no serious reason to think this is what he intends. For the moment, then, we can say that without considerable elaboration, and in particular, a discussion of the issues raised here, Bub's remarks do not seem to offer much help on the problem of locality.

I conclude, then, that Bub has not given us good reason to believe that continuous geometry will help develop a realist version of quantum logic of the sort that interests him, nor is it likely to provide us with any new tools for treating the measurement problem or the problem of locality. This may suggest to the reader that I am sceptical about the overall promise of quantum logic, but in fact this is not so at all. What I believe, rather, is that

we need to consider a new approach. Since I have made this case at much greater length in my [1983a], I will confine my remarks here to pointing out a dubious methodological assumption, explicit in Bub and present in the work of other quantum logicians, and to suggesting that Bub, without intending to do so, has pointed out the direction in which quantum logic ought to move.

Let us begin with the second point, for which we return to the discussion in 'The Measurement Problem of Quantum Mechanics'. There, Bub drew our attention to von Neumann's suggestion that probability in quantum mechanics is *sui generis* and spoke of a theory 'in which properties are associated with "sets", but no two-valued homomorphisms exist on the structure of properties, *i.e.*, the "sets" have no points'. Even though we probably cannot have what Bub would have liked, there is something we *can* have that comes very close to satisfying the letter, if not the spirit of this quote, and in a way that gives a role to the idea that probabilities are *sui generis* and, in fact, *logical*.

As Bub notes, von Neumann's aim apparently was to develop an account of probability in which the logical structure completely determines the character of the probabilities. Because of Gleason's theorem, we know that even in the usual Hilbert space case, the structure of the logic completely determines the probability measures over the logic, and in particular, we know that if probability 1 is assigned to an atom, all other probabilities are thereby determined, even though in general, these will be strictly between 0 and 1. Thus, type II_1 factors aside, there is plenty of room for von Neumann's thought that ' $P(a, b) = \phi$, with a $\phi < 0, > 1$ is a new "sui generis" statement, only understandable in terms of probabilities'. Furthermore, by reflecting on our earlier discussion of value-definiteness, we will find a natural approach to the question of what sort of theory of properties this yields. As we have seen, value-definiteness does not cohere well with other aspects of the quantum logical position. Furthermore, the case of continuous magnitudes such as position and momentum suggests that there is *already* a place for the idea of magnitudes with *indefinite* values. So we can adopt the idea that the magnitudes of quantum systems take *indefinite* rather than definite values. This provides a very natural way of accounting for the fact that we can have two true quantum logical disjunctions (a or b or . . . n) and (a' or b' or . . . n'), but the distributed form (a & a' or a & b' or . . . n & n') is either false or (on the partial Boolean algebra approach) undefined. In fact, the simplest way of working out this suggestion is simply to take the view that the set of true propositions describing a quantum system is the set of all propositions above the atomic proposition P corresponding to the state vector, *i.e.*, the set of all propositions in the 'Kochen ultrafilter' determined by the state. The false propositions are all those orthogonal to P . Other propositions are neither true nor false, but have a certain *probability*, which is determined, in effect, by their logical relationship to P . What we have in other words, is something

very like 'sets without points'—true disjunctions for which none of the disjuncts are true—in a setting in which the logical structure determines probabilities.

As I argue in my [1983a], there is no conflict between realism and the denial of the value-definiteness thesis, suggestions by Bub and many others notwithstanding. Nonetheless, the reader will certainly have noticed that what I have described is a version of what Bub refers to as the 'orthodox interpretation' of the state vector, as Bub describes it, and which he rejects on the grounds that it leads directly to Schrödinger's cat paradox and the measurement problem. In fact, I think that part of the difficulty here is simply an unwillingness to consider certain sorts of solutions to these problems, but that is another story. Lying behind these worries of Bub's is a methodological assumption about quantum logic that I believe ought to be challenged. The assumption is that the only reason one might have for taking quantum logic seriously is the belief that it will help us dissolve the paradoxes of quantum mechanics. There are at least two reasons to challenge this assumption. The first is that even if, somehow, the quantum logical value-definiteness thesis could be defended, we still would not thereby obtain a solution to the measurement problem and other related difficulties. (For details, the reader should see my [1983a].) More important, however, I question the idea that the whole motivation for taking quantum logic seriously is its alleged ability to solve the paradoxes of quantum mechanics. I suggest that the real reasons for considering quantum logic are quite a bit simpler. First, if one is interested in finding a realist interpretation of quantum mechanics, then the thesis that the properties of a quantum system 'hang together' in the way quantum logic suggests is an extremely natural one given the structure of quantum theory itself. It begins with the straightforward realist assumption that the fundamental structures of the theory represent something about the structure of the *world*. Quantum logic takes more or less at face value the algebraic structure ascribed by the theory to the magnitudes. This structure gives rise to a set of relations of equivalence, exclusion, *etc.* among possible events, and these relations are the basis of quantum logic. However, there is room for disagreement on the question of just how, in detail, this structure should be understood. The important point to bear in mind is that both quantum logic with the value definiteness hypothesis *and* the alternative proposed here *agree* about the relations of exclusion, equivalence, *etc.* that define the structure. Further, both versions agree that this structure is a crucial part of what lies behind quantum phenomena and will function in explanations of those phenomena. What is not yet clear is exactly what form those explanations should take. In particular, it is not yet clear to the quantum logician or to any one else how the measurement problem is to be solved. But since quantum logic describes only the 'background structure', the demand that it solved all the paradoxes singlehandedly before being taken seriously is not reasonable. And this leads to the second point. The thesis that the world *has* a logical structure is a

striking one that we by no means fully understand yet. In particular, the question of what a quantum logical world would be like is an extremely interesting one even if it turns out that by asking this question, we will not be led inexorably to a solution to all the puzzles that quantum mechanics presents.

This is not to suggest that quantum logic could survive as an interpretive tool in foundations of quantum mechanics if it proved utterly irrelevant to the standard puzzles. In fact, I believe that quantum logic may well prove useful in a variety of cases, including the problem of locality and the understanding of probability in quantum theory. (On the topic of locality, the reader may consult my [1983c] and [1983d]. For some preliminary thoughts on probability, see my [1983a].) On the other hand, the problems of interpreting quantum mechanics are sufficiently vexing that I will be very surprised if any *one* idea, be it quantum logic or something else, will dissolve them. Thus, even though I am sceptical of Bub's suggestions, I applaud his willingness to consider alternative approaches, and am willing to believe that something may yet come of the programme of continuous geometry. However, I have tried to indicate that there may be alternative uses for the tools we already have at hand. The 'standard' realist programme may well have considerable internal difficulties, but this need not reflect on the overall programme of seeing quantum mechanics as a theory that posits novel logical relations among possibilities.

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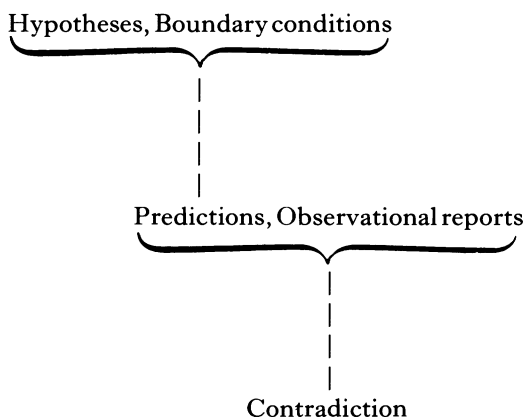
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**MINIMAL LOGIC IS ADEQUATE
 FOR POPPERIAN SCIENCE***

Assume that Popper's 'no counterexample' interpretation of scientific laws is correct ([1972], pp. 68–69):

The theories of natural science, and especially what we call natural laws, have the logical form of strictly universal statements; thus they can be expressed in the form of negations of strictly existential statements or, as we may say, in the form of *non-existence statements* (or 'there-is-not' statements). . . . In this formulation we see that natural laws . . . insist on the non-existence of certain things or states of affairs, proscribing or prohibiting, as it were, these things or states of affairs: they rule them out. And it is precisely because they do this that they are *falsifiable*. If we accept as true one singular statement which, as it were, infringes the prohibition by asserting the existence of a thing (or the occurrence of an event) ruled out by the law, then the law is refuted.

Assume also that Popper's analysis of scientific method, as set out in that book, is correct. Assume, that is, that the experimental refutation of scientific hypotheses has the following deductive structure, which I shall call *Schema P*:



Logic is needed for the downward passages indicated within this schema. As Popper himself puts it elsewhere ([1970], at p. 18):

. . . in the empirical sciences (logic) is almost exclusively used critically—for the re-transmission of falsity. . . . in the empirical sciences logic is mainly used for criticism; that is, for refutation.

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