LOGIC, QUANTUM LOGIC AND EMPIRICISM*

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This paper treats some of the issues raised by Putnam's discussion of, and claims for, quantum logic, specifically: that its proposal is a response to experimental difficulties; that it is a reasonable replacement for classical logic because its connectives retain their classical meanings, and because it can be derived as a logic of tests. We argue that the first claim is wrong (1), and that while conjunction and disjunction can be considered to retain their classical meanings, negation crucially *does not*. The argument is conducted *via* a thorough analysis of how the meet, join and complementation operations are defined in the relevant logical structures, respectively Boolean- and ortholattices (3). Since Putnam wishes to reinstate a realist interpretation of quantum mechanics, we ask how quantum logic can be a logic of realism. We show that it certainly cannot be a logic of *bivalence* realism (i.e., of truth and falsity), although it *is* consistent with some form of *ontological* realism (4). Finally, we show that while a reasonable explication of the idealized notion of test yields interesting mathematical structure, it by no means yields the rich ortholattice structure which Putnam (following Finkelstein) seeks.

In his (1969), Putnam argues that logic is 'in a certain sense a natural science' (p. 174), and that the natural logic for quantum mechanics is the so-called quantum logic. The aim of this paper is to examine the basis for these claims, and to show where they go wrong. Despite (or perhaps because of!) the huge literature on quantum logic, there is still a good deal of confusion about the subject, and it seems to us that many of the fundamental issues remain shrouded in obscurity. We take Putnam's paper as a starting point because it has the admirable merit of putting forward various bold theses. By showing where we think Putnam is mistaken we hope to shed light on at least some of the problem areas. We concentrate on four topics central to Putnam's paper: the question of in what sense logic can be said to be empirical; the relationship between quantum logic and classical logic; the adequacy of quantum logic as a logic of realism; and the Putnam-Finkelstein attempt to base quantum logic on an operationally determined semantics.

1. Empirical Logic.

What does it mean to say that logic is empirical? Quine in his (1951) proposed that logical laws might be given up as false

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as a direct result of a clash between a theoretical system and experiment. If this happened, one could surely say that observation of the world had directly affected our decisions about logical laws, thus showing logic to be empirical in a very strong sense.[1] The opening paragraph of Putnam's paper suggests that he believes that developments in quantum mechanics and the emergence of quantum logic show logic to be empirical *in precisely this sense*.

> I . . . want to raise the question: could some of the 'necessary truths' of logic turn out to be false *for empirical reasons*? I shall argue that the answer to this question is in the affirmative, . . . (Putnam 1969, p. 174; Putnam's italics).

The first point we wish to make in this paper is that, whatever his intention, and despite his emphasis on various experimental 'anomalies', Putnam establishes no such thing.

Putnam's position can be summed up as follows. Classical physics cannot satisfactorily account for the results of a number of experiments involving microphenomena. Accordingly, classical theory (C) was replaced by quantum theory (Q) which *does* explain these results. However, despite the empirical success of Q, it has proved very difficult to furnish it with a satisfactory metaphysical interpretation.[2] In particular, it has proved very difficult to reconcile Q with common sense or classical realism (M_C). Putnam sees this as a high price to pay for the shift from C to Q, since he regards classical realism as the natural position to hold with respect to physics. (He also regards as seriously flawed the attempt to rescue realism via hidden variable theories.) What he proposes therefore is this. By bringing logic into the framework in addition to physics and metaphysics, we shift from the schema $Q + M_Q + L_C$ (where M_Q stands for any of the unacceptable metaphysical interpretations of Q, and L_C is classical logic) to a new system $Q + M'_C + L_Q$, where something like classical realism (M'_C) is preserved, but L_C is replaced by *quantum logic*, L_Q. Where does quantum logic come from? Well, it is simply read off from the structure of the lattice of closed subspaces of Hilbert space employed in the usual formulation of quantum mechanics. (The suggestion goes back to Birkhoff and von Neumann 1936.) The overall system $Q + M'_C + L_Q$ is supposedly much more satisfactory than any $Q + M_Q + L_C$ because the change from L_C to L_Q is claimed to be relatively minor and therefore preferable to the adoption of any of the intuitively unpalatable metaphysical positions M_Q (or the equally unpalatable hidden vari-

[1]Though for some doubts about this scenario ever being realized, see Fine (1972, pp. 10–14).

[2]For a review of the various interpretations, see Putnam (1965).

ables assumption) (see specifically Putnam 1969, pp. 188–9 and 191–2). That the existence of incompatible propositions (or 'complementary' properties) is high on the list of things Putnam wishes to explain with quantum logic (see p. 358) without recourse to unacceptable metaphysics, illustrates perhaps that it is metaphysical or philosophical considerations which dominate here, and not the outcome of any particular experiments.

Nevertheless, one of Putnam's main arguments for rejecting classical logic *is* based on a clash between a theoretical system and experiment, in this case, the celebrated two-slit experiment. Putnam gives an analysis of the experiment based on classical probability theory (which of course yields an experimentally *incorrect* result) and points out that in this analysis the distributive law of classical logic plays a crucial role. This seems to suggest that a sufficiently careful inspection of the fallacious classical argument would already have cast doubt on distributivity even before the proposal of Q. But this really won't do. The theory Q (as Putnam points out, p. 180) *correctly* predicts the observed outcome of the experiment. And logic is simply not at stake because $Q + L_C$ accounts for the results perfectly well. Moreover, Gardner in his (1971) has argued that under the Putnam-Finkelstein interpretation of quantum logic one can show that distributivity does *not* actually fail in the two-slit case.

It therefore seems clear that the example Putnam gives does not instantiate the radical Quinean thesis of the empirical nature of logic. The *metaphysical difficulties* associated with the shift to Q could, it is true, lead to the rejection of L_C, but this is quite another matter from the claim that the results of an experiment lead *directly* to such a rejection.

This question of the failure of distributivity in quantum logic brings us to a point rightly raised and stressed by Dummett in his (1976), namely concerning the *meaning* of the quantum logical connectives. We have emphasized that at bottom Putnam's motivation for the shift to L_Q from L_C was dissatisfaction over the metaphysical support for Q, and could not be because of dissatisfaction over the empirical adequacy of Q.[3] It is not possible here to go into all the metaphysical difficulties encountered by quantum mechanics, nor is it necessary to do so. However, one of the issues that clearly does exercise Putnam a good deal is that of how to *interpret* certain classes of 'anomalous' statements of quantum mechan-

[3]For example, in his (1965), Putnam is clearly impressed by the empirical adequacy and success of quantum mechanics. It is, he says, too successful to be simply accidentally correct:

> Not only does the quantum mechanical formalism yield correct answers to too many decimal places, but it also yields too many predictions of whole classes of effects that would not have been anticipated on the basis of older theory, and these predictions are correct (p. 133).

ics, in other words the issue of what *meaning* is to be attached to these statements. Thus, if we have two statements

S: the position of electron e at time t is P

and

T: the momentum of electron e at time t is M.

Putnam points out that according to quantum logic $S \wedge T$ is a logical contradiction. This is taken to explain *why* we can never *in practice* assign the truth value 'true' to $S \wedge T$, something which upholders of classical logic are hard put to explain. Let us quote Putnam. Let S_r represent some position statement, and let $T_1, \ldots, T_R$ represent the statements expressing a possible momentum for the given particle. Then:

> The [unacceptable] idea that momentum measurement 'brings into being' the value found arises very naturally, if one does not appreciate the logic being employed in quantum mechanics. If I know that S_r is true, then I know that for *each* T_j the conjunction $S_r \cdot T_j$ is false. It is natural to conclude ('smuggling in' classical logic) that $S_r \cdot (T_1 \vee T_2 \vee \ldots \vee T_R)$ is false, and hence that we must reject $(T_1 \vee T_2 \vee \ldots \vee T_R)$—*i.e.*, we must say 'the particle has no momentum'. Then one measures momentum, and one gets a momentum—say, one finds that T_M. Clearly, the particle *now* has a momentum—so the measurement must have 'brought it into being'. However, the error was in passing from falsity of $S_r \cdot T_1 \vee S_r \cdot T_2 \vee \ldots \vee S_r \cdot T_R$ to the falsity of $S_r \cdot (T_1 \vee T_2 \vee \ldots \vee T_R)$. This latter statement is *true* (assuming S_r); so it is *true* that 'the particle has a momentum' (even if it is also true that 'the position is r_3'); and the momentum measurement merely *finds* this momentum (while disturbing the position); it does not create it, or disturb it in any way. It is as simple as that (Putnam 1969, p. 186; see also p. 180).

Such a simple solution to an old problem has obvious attractions. (Though, of course, the solution is perhaps not *quite* as simple as it seems at first sight. 'We cannot know $S_i \wedge T_j$,[4] because this is a contradiction and we cannot know a contradiction' loses some of its force if logic is indeed empirical as Putnam claims. For if it *is*, it will be fallible and therefore subject to flux. Hence the ascriptions 'logically true/false' lose their absolute status, and Putnam's explanation will lose much of its power.) But as Dummett has pointed out this solution is only really acceptable if one can explain *why* $(S_r \wedge T_1) \vee \ldots \vee (S_r \wedge T_R)$ is *logically*

[4]We shall use '$\wedge$' for conjunction, except in quotations from Putnam's paper, where '·' is used.

false, and yet $S_z \wedge (T_1 \vee \ldots \vee T_R)$ is *true*. This will involve explaining what the statements *mean*, and hence what the meaning of the (quantum logical) connectives is, and how the meaning differs from that of the classical connectives. As Dummett stresses,[5] if such an explanation is *not* given, then the 'simple' Putnam solution will be quite mysterious, indeed, one could say, just as mysterious as any that Putnam wishes to eschew. The point Dummett is making here is just that a change in the logical formalism can never by itself be enough. Whether the original motivation for the change is empirical or not the change itself will stand or fall on grounds of the meaning being assigned to the logical terms. And the ascription of meaning may or may *not* have anything to do with empirical considerations.

As we will see, Putnam is extremely sensitive to this point. Certainly, it seems to us that the issue of the meaning of the quantum-logical connectives is central to Putnam's paper. He attempts to deal with it in two (apparently disparate) ways. He first tries to sidestep the issue by claiming that there is no real *need* to explain the meaning of the quantum connectives because they are after all the same old, well-understood *classical* connectives in a slightly new guise. This is the so called 'invariance of meaning' argument, which we discuss in **2–3.** We argue that it is quite mistaken: there is a substantial and crucial variance of meaning between the two logics. Putnam's second way of tackling the meaning issue takes it somewhat more seriously. For, following Finkelstein, he has attempted to explain the meaning of the quantum logical connectives in 'operationalist' terms, by relying on what he and Finkelstein call the *logic of tests*. This certainly raises the possibility that Putnam can meet Dummett's challenge head on as an empiricist logician, and, in addition, rebut the charge that quantum logic is excessively dependent on (accidental) formal features of the current formulation of quantum theory. We discuss this attempt to provide 'operational semantics' in **5**, arguing that it just does not work. We start, however, with the 'invariance' argument, and why it fails. The debate here demands a clearer understanding of the relationship between quantum and classical logic. This is the area where, we feel, Putnam's analysis is most confused, and where clarification is most needed. We begin with Putnam's own account.

2. The Invariance Argument. As we have said in **1**, Putnam's claim for the necessity of espousing quantum logic arises out of the need to reconcile quantum-mechanical statements with 'common sense' realism. (The question of what constitutes 'common sense' realism is touched on *below;* see section **4.**) In doing this he claims that we are still using the

[5]See, for example, Dummett (1976, p. 281).

same (and not different or additional) connectives as in classical logic, *except* that they no longer satisfy all the laws that we previously thought they did. Specifically the distributive laws now fail.

There appear to be three closely interrelated reasons why Putnam should want to claim that the quantum-logical connectives are the old, familiar classical connectives. First, as we mentioned *above*, it neatly sidesteps the whole issue of assigning a meaning to the quantum-logical connectives, since it is asserted that the meaning has not essentially changed. Secondly, this move makes it unnecessary to argue for the acceptability or coherence of quantum logic: since classical logic is coherent, then, in so far as the new logic is not *essentially* different, it is equally coherent. Thirdly, and crucially linked with Putnam's interest in quantum logic, the minimality of the change involved in the shift from classical to quantum logic raises the hope that something like 'common sense' realism (with which classical logic is closely associated) can be retained for quantum mechanics. Certainly the fact that quantum logic preserves the distinctive classical laws of excluded middle and double negation may strengthen the impression that quantum logic is consonant with realism. However, it is by no means obvious that quantum negation, conjunction and disjunction are actually the old familiar, well-understood connectives. Why should they be? Putnam freely admits that he is asking for a conceptual revolution: how do we know that this revolution didn't liquidate the old connectives and replace them with radically new ones? Putnam tries to establish that there is *substantial* (and presumably sufficient) continuity between the two logics by providing *an invariance of meaning argument* backed up by an *analogy* drawn from the Einsteinian revolution in geometro-physics.

Let us take the analogy first. Consider the notion 'straight line'. Evidently, this term obeys different laws in the new theory. But, says Putnam, we have not simply dragged in a new notion, 'Riemannian straight line', and thrown out an old one, 'Euclidean straight line'; or in other words, we have not just shifted the label 'straight line' from one set of paths (Euclidean straight lines) to another set (Riemannian geodesics). For, says Putnam:

> We can say that the geodesics are straight, because they at least obey what were always recognised to be the operational constraints on the notion of straight line; but they do not obey the old geometry. In short, either we say that the geodesics are what we always meant by 'straight line', or we say that there is nothing clear that we used to mean by that expression (Putnam 1969, p. 177).

In other words, the only viable candidates for being straight lines in the new geometry are Riemannian geodesics, and these obey the old operational constraints on the term 'straight line'. That is to say, a fundamental core of meaning has been preserved, enough for us to be able to overlook the failure of some of the old Euclidean laws governing 'straight line'. Now, crucially, Putnam regards

> . . . the analogy between the epistemological situation in logic and the epistemological situation in geometry as a perfect one (1969, p. 190).

By this he means that in this case also the quantum connectives *obey the key laws characterising the classical connectives*, and that distributivity (which is portrayed as the main casualty here) cannot be classed as one of these key laws. The following passage (pp. 189–90) contains the heart of this argument:

> The following principles:
>
> p implies $p \vee q$ (1)
>
> q implies $p \vee q$ (2)
>
> if p implies r and q implies r then
>
> $p \vee q$ implies r (3)
>
> all *hold* in quantum logic, and these seem to be like the basic properties of 'or'. Similarly
>
> p, q together imply $p \cdot q$ (4)
>
> (Moreover, *$p \cdot q$ is the unique proposition that is implied by every proposition that implies both p and q.*)
>
> $p \cdot q$ implies p (5)
>
> $p \cdot q$ implies q (6)
>
> all *hold* in quantum logic. And for negation we have that p and $-p$ never both hold. (*$p \cdot -p$ is a contradiction*) (7)
>
> $(p \vee -p)$ holds (8)
>
> $--p$ is equivalent to p (9)
>
> Thus, a strong case could be made for the view that adopting quantum logic is *not* changing the meaning of the logical connectives but merely changing our minds about the law
>
> $p \cdot (q \vee r)$ is equivalent to

$$p \cdot q \vee p \cdot r \text{ (which fails in quantum logic)} \qquad (10)$$

Only if it can be made out that (10) is 'part of the meaning' of 'or' and/or 'and' (which and how does one decide?) can it be maintained that quantum mechanics involves a 'change in the meaning' of one or both of these connectives.

The intended conclusion is clear: the quantum connectives differ only marginally from the classical ones; as in the geometry case, a fundamental core of meaning has been preserved, enough for us to be able to overlook the failure of distributivity. The analogy with geometry is supposedly completed with Putnam's (unsubstantiated) claim that "quantum mechanics explains the approximate validity of *classical logic* 'in the large' " (p. 184).

We argue here that the invariance of meaning claim is *false:* indeed, we show that the meaning of the negation operation in (abstract) quantum logic is quite different from the meaning of classical negation. This is sufficient to destroy the 'marginal change' contention, and thus the suggestion that quantum logic is *automatically* a satisfactory replacement for classical logic. Moreover, we argue that, in any case, it is not clear how quantum logic can be a logic of realism.

3. . . . and its Failure. It seems to us quite wrong when considering the meaning of the quantum-logical connectives to focus merely on the formal rules which they obey. *If* one chooses to define a logic in a purely formal way by specifying various axioms and/or rules of inference, then one might properly claim that any notion of meaning appropriate for the connectives must stem solely from their formal properties. But this is not the way that quantum logic is arrived at. Rather the procedure is to specify a certain structure, in this case, the lattice of closed subspaces of a Hilbert space (a certain kind of ortholattice, see *below*) and to interpret the connectives as the corresponding operations in this lattice. It has been justly observed by Fine (1972) that this fact *already* reveals a difference in meaning between the quantum-logical and classical connectives, because in the quantum case we are concerned with a lattice of *subspaces* while in the classical case we are concerned with a Boolean algebra of *subsets*, and the operations corresponding to negation and disjunction are manifestly different in the two cases (see particularly Fine 1972, pp. 16–19).

All well and good. However, it seems to us that the discussion has to be taken further. For attempts have been made to free quantum logic from its origins in Hilbert space and to base it instead on the more abstract foundation of *general ortholattices*. This seems like an insistence on the 'formal rules' approach. For here the quantum-logical connectives are to be interpreted just as the designated operations in certain algebraic structures—ortholattices—satisfying certain *formal* axioms. This approach may be regarded as an attempt to build a semantic framework in which to analyze the formal rules satisfied by the quantum-logical connectives; for ortholattices are the mathematical structures which act as *models* for these rules, and hence furnish the most general class of interpretations for the connectives.

But now Fine's argument does not apply, because one is not now dealing with a *specific* structure in which both the classical and quantum-logical operations (or their natural interpretations) exist side-by-side, waiting to be differentiated. More importantly, this more general algebraic approach seems to reinstate Putnam's contention about the invariance of meaning. After all, ortholattices satisfy the same *core* of defining axioms as do Boolean algebras (which constitute the most general class of models for the classical logical rules). Hence Putnam's argument can now be stated like this: the meanings of the connectives, classical and quantum, are given by the basic rules expressed in the core axioms for ortholattices (which do not assert distributivity); therefore in the shift from classical to quantum logic one has not changed the *meaning* of the connectives, but only shifted to a broader class of interpretations, i.e., general ortholattices, where in general the distributive laws fail, rather than just Boolean algebras where they hold.

We argue that this position is untenable because it ignores the relationships between the connectives (or the corresponding lattice operations) within the relevant structures. We contend that if these are carefully analyzed it appears obvious that there must have been a shift in meaning, in fact, a shift in the meaning of negation. We base the argument on an examination of the way the lattice structures are given, and a consideration of the definability relations between the various lattice operations. The position we adopt is that if two terms t and t' are defined in terms of the primitives $a, b, \ldots$ etc. in non-equivalent ways, or if one is so definable and the other not, then they have different meanings relative to $a, b, \ldots$. More particularly, if two structures L and L' both have the primitives $a, b, \ldots$ etc. and t is definable in terms of $a, b, \ldots$ in one and not in the other than we will assume that t has shifted its meaning in the passage from one to the other. The point here is a simple one: if a term was once explicable in terms of $a, b, \ldots$ and now is *not* (or *vice versa*), then relative to $a, b, \ldots$ it must have changed its meaning. This seems to us a quite clear sufficient condition for there being a variance of meaning, a condition which we can use without getting caught up in the thorny issue of what meaning is. If one goes further and takes an extensional view of meaning by stating that two terms have the same meaning relative to $a, b, \ldots$ if they have *equivalent* definitions in terms of $a, b, \ldots$ then a case can be made out that the meanings of conjunction and disjunction do *not* vary in the passage from classical to quantum logic. However, we argue that there must nevertheless be a significant shift in the meaning of *negation*.

To put the argument clearly we have to consider the relevant algebraic structures in some detail. In particular we consider the way the lattice operations meet, join and (where applicable) complementation are defined, with the use of some minimal set-theoretic machinery in terms of a partial ordering $\leq$ on an underlying set L.[6] Since we assume $(L, \leq)$ to be a lattice, any pair x, y of elements of L has both a supremum and an infimum with respect to $\leq$. These are denoted respectively by $x \vee y$ and $x \wedge y$, thus giving rise to the operations $\vee$ ('join') and $\wedge$ ('meet'). If we think of the elements of a lattice as 'propositions' of some kind, then the relation $\leq$ corresponds to *entailment* or *inclusion* of propositions, while the operations $\wedge$ and $\vee$ correspond to *conjunction* and *disjunction* of propositions. For a lattice L to be an *ortholattice*, it must have *greatest* and *least* elements 1 and 0 with respect to $\leq$ and an operation $x \mapsto x^*$ called *orthocomplementation* subject to the conditions:

$$x \vee x^* = 1, \quad x \wedge x^* = 0, \quad x^{**} = x, \quad (x \wedge y)^* = x^* \vee y^*.$$

(*Cf.* Putnam's rules (7)–(9) mentioned *above*, p. 363.) The element x^* is called the *orthocomplement* of x (in this connection, an element y such that $x \vee y = 1$, $x \wedge y = 0$ is called a *complement* for x). The operation * corresponds to *negation* of propositions. A *Boolean algebra* is a *distributive* ortholattice, i.e., one satisfying the (mutually equivalent) distributive laws

$$x \wedge (y \vee z) = (x \wedge y) \vee (x \wedge z), \quad x \vee (y \wedge z) = (x \vee y) \wedge (x \vee z).$$

Boolean algebras provide the algebraic realization of classical logical systems. However the ortholattices which serve as models for quantum logic (e.g., the lattice of closed subspaces of Hilbert space) are, in general, *non-Boolean* (i.e., do not satisfy the distributive laws *above*).

Our point about conjunction and disjunction can now be simply made. In any ortholattice, the operations $\wedge$ and $\vee$ are set-theoretically definable in terms of $\leq$:

$$a \wedge b = c \leftrightarrow \{x : x \leq c\} = \{x : x \leq a\} \cap \{x : x \leq b\}$$
$$a \vee b = c \leftrightarrow \{x : c \leq x\} = \{x : a \leq x\} \cap \{x : b \leq x\}.$$

In view of these equivalences, the meanings of $\wedge$ and $\vee$ are completely

[6] This is the normal way lattices are given and certainly the way Putnam treats them in his paper (1969, pp. 193–4).

determined by $\leq$ and are therefore *invariant* under the transition from Boolean algebras to general ortholattices; that is, the meanings of conjunction and disjunction are invariant in the transition from classical to quantum logic.

Let us now turn to the orthocomplementation operation *. In a *Boolean algebra*, this operation is definable in terms of $\wedge$ (and hence $\leq$) by the prescription:

$$(*) \quad a = b^* \leftrightarrow \{x : x \leq a\} = \{x : b \wedge x = 0\}.$$

Conversely, any ortholattice satisfying (*) *must* be a Boolean algebra. It follows that, in the case of classical logic, negation is (set-theoretically) definable in terms of the basic relation $\leq$ via equivalence (*), while in quantum logic negation is *not* so definable. Therefore, passing from classical to quantum logic *destroys* the possibility of explicating negation in terms of the basic relation $\leq$ by means of (*), and *must accordingly induce a change of meaning in this operation relative to* $\leq$. Putnam assumes (1969, p. 194) that quantum negation, like conjunction and disjunction, is 'characterized in terms of the implication lattice', i.e., in terms of $\leq$. But he doesn't say how. It *could* be set-theoretically definable from $\leq$ in some manner. The crucial point, though, is that it cannot be defined by equivalence (*).

The fact that, in a non-Boolean ortholattice, orthocomplementation is generally not definable in terms of $\leq$ prompts one to ask: what additional structure is needed in order to render it definable? Although this question does not have a unique answer, the most natural way of achieving the desired result is to specify an extra relation $\perp$, called an *orthogonality* relation, satisfying certain conditions which need not detain us here. The relation $a \perp b$ corresponds to mutual inconsistency of propositions: 'a and b do not both hold'. (In the lattice of subspaces of Hilbert space, $\perp$ is just the usual relation of perpendicularity between subspaces while in a Boolean algebra, $\perp$ is the 'disjointness' relation $x \wedge y = 0$.) Once this is given, the orthocomplement of an element b can be defined as the largest element a such that $a \perp b$, that is,

$$a = b^* \leftrightarrow \{x : x \leq a\} = \{x : x \perp b\}.$$

And conversely, of course, $\perp$ can be defined in terms of *:

$$a \perp b \leftrightarrow a \leq b^*.$$

So an ortholattice may alternatively be construed as a structure of the form $(L, \leq, \perp)$ satisfying certain conditions.

In the case of a Boolean algebra the orthogonality relation *coincides* with the disjointness relation:

$$a \perp b \leftrightarrow a \wedge b = 0.$$

Conversely, *any ortholattice satisfying this condition must be a Boolean algebra*. Therefore, in the non-Boolean case, in order to define orthocomplementation, and hence the interpretation of negation, one *must* be given the orthogonality relation in addition to the partial ordering. We see, then, that the semantic fundamentals are *necessarily* more elaborate in the quantum case $(L, \leq, \perp)$ than in the classical case $(L, \leq)$ and they are so precisely for the purpose of defining negation. Moreover, in the passage from classical to quantum logic negation changes its meaning with respect to the basic partial ordering $\leq$ in direct proportion to the deviation of the orthogonality relation from its classical counterpart—disjointness.

We may conclude from this that Putnam's invariance thesis fails even in this abstract setting, for the apparently minor 'failure of distributivity' of quantum logic is inextricably bound up with a shift in the meaning of negation. The fact that quantum negation satisfies the rules (8) and (9) mentioned by Putnam does not prevent its meaning from differing from that of its classical counterpart.

What has been said so far does not at all challenge the legitimacy of, or interest in, the structures $(L, \leq, \perp)$ as the basis for a semantics for a logic. Indeed, there is a sense in which experimental setups in quantum mechanics yield natural definitions of $\leq$ and $\perp$ relations, making study of these in combination a natural starting point for investigations of quantum logic (see *below*, **5**). However, it *does* show that the change from classical to quantum logic is not a minor affair.

4. The Logic of Truth and Falsity. There is a natural, indeed compelling, sense in which classical (propositional) logic is the logic of *truth* and *falsity*, or the logic of realism, more specifically *bivalence* realism, which asserts that every proposition has exactly one of two truth values *true* and *false*, independently of our capacity to know what that value is. This arises from the fact that the *deducibility relation* $\vdash$ of a classical logical system can be *completely* explicated by means of assignments of the two classical truth values T (truth) and F (falsity) to the propositions of the system. In fact, let P and Q be two (molecular) propositions involving proposition letters $p_1, \ldots, p_n$. For any assignment of T and F to $p_1, \ldots p_n$, the propositions P and Q are also assigned T and F in accordance with the customary two-valued truth tables: such an assignment will be called a *truth valuation*. Then $P \vdash Q$ if, and only if, whenever P receives value T, so does Q: this is, of course, the *completeness theorem* for classical propositional logic.

If we think of a classical propositional system as a Boolean algebra in the usual way, then the completeness theorem translates into the well-known fact that

(*) for any distinct elements a, b of a Boolean algebra B there is a 2-valued homomorphism h on B such that $h(a) \neq h(b)$.

Here a 2-valued homomorphism is a homomorphism to the Boolean algebra $2 = \{0,1\}$, which, of course, can be construed as the truth value algebra, $\{T, F\}$. The statement (*) implies (and indeed, for Boolean algebras, is equivalent to)

(**) for any Boolean algebra B and any element $a \neq 0$ in B, there is a 2-valued homomorphism h such that $h(a) = 1$.

What (**) says is that for any proposition which is not *automatically* carried to F, we can find an interpretation which renders it T.

Like Dummett, we have stressed that Putnam is out to reinstate realism in quantum mechanics (see 1 *above*). But what does 'reinstating realism' amount to in this context? One need not be a realist with respect to classical physics. But certainly classical physics is *consistent* with bivalence realism. One way, then, of defending realism in quantum mechanics would be to show that quantum mechanics is also *consistent* with bivalence realism. This would be a natural way of asserting that there is continuity between classical and quantum physics. If Putnam's program is to accord with this aim, then it is clear that the change of logic must not block this consistency. Now what do we mean by 'consistency' here? With respect to classical physics it means just that a realist can quite consistently assert that *all* the propositions of classical physics have exactly one of the values T and F. This is indeed the case because classical physics is based on classical logic, so the set of propositions can be interpreted as a Boolean algebra, and (**) therefore applies. The key question is now: does the proposed change of logic still allow this?

This is an interesting test for a logic proposed to replace classical logic. Consider, for example, intuitionistic logic, which some have proposed should replace classical logic as *the* logic of mathematics. (A better, though still a somewhat crude, way to put the proposal is: mathematics should be rebuilt in accordance with intuitionistic logic.) Intuitionistic logic is naturally and correctly described as the logic of constructions. More pertinently it is based on strong anti-realist principles; and crucially it is not at all a logic of (classical) truth and falsity. Indeed, if we accept the view that the *mathematical structure* of intuitionistic logic is faithfully embodied in the notion of a Heyting algebra, just as we accept that the structure of classical logic is embodied in the notion of a Boolean algebra, then the deducibility relation of the system cannot be explicated by means of truth-valuations, for the analogue of (*) *fails*. Nevertheless, intuitionistic logic and systems based on it are still *consistent* with bivalence realism in our sense, because the analogue of (**) for Heyting algebras *continues to hold*. In other words, there will be a suitable truth-valuation under which any given irrefutable proposition is mapped to T, thereby 'restoring' bivalence realism. Thus, despite the fact that intuitionistic logic is inherently the logic of constructive *anti-realism*, and *not* a logic of truth and falsity, the bivalence realist is quite able to construe it in a realist way.

However, the situation is quite different for quantum logic. For in the case of an ortholattice which is not a Boolean algebra even the analogue of (**) may fail. In fact, it is easily seen that this situation obtains for the lattice of closed subspaces of Hilbert space which is normally taken to embody the structure of the set of quantum propositions. Thus, in contradistinction with the intuitionist case, it is not in general possible to construe quantum logic, or a physics based on it, in a bivalent realist manner.[7]

So our first attempt to show that quantum logic is compatible with realism fails. It is important to notice that this failure is not trivial; it is profound and profoundly disruptive. Whatever view one takes about the propositions of the given language there always has to be some point at which bivalence takes over. For example, the intuitionist rejects bivalence realism *in general;* but since he attributes the falsity of bivalence to the presence of statements about arbitrary infinite collections, it is natural to expect bivalence to operate (as is indeed the case) at the level of statements about finite, or decidable, collections. What causes the analogue of (**), and thus bivalence, to fail in the quantum case is the existence of pairs of *incompatible* propositions, that is to say, of pairs of propositions which taken separately may be true but which taken in conjunction must always be false.[8] (Thus, incidentally, the normal two-valued truth-tables fail for quantum logic. They continue to hold, however, for intuitionistic logic if 'proved' and 'refuted' are construed as 'true' and 'false' respectively.[9]) So it is natural to ask if classical bivalence will hold for subsets of *mutually compatible* propositions, for if the classical notions of truth and falsity are to be relevant at all it will be for these subsets. Of course, mutually compatible propositions form themselves naturally into Boolean sub-algebras of the lattice of quantum propositions, and for each of these sub-algebras there will be bivalence-homomorphisms to **2**. But bivalence about the world concerns simultaneous assignment of truth-values; so the natural question to ask is therefore:

[7]Dummett has argued in his (1976, pp. 271–6), that Putnam's interpretation of quantum propositions is inconsistent with bivalence realism. We regard our arguments here as reinforcing his.

[8]The existence of such pairs is at the heart of Dummett's criticism of Putnam mentioned in the previous note. See also pp. 358–359 *above*.

[9]See Dummett (1977, p. 16).

given an orthocomplemented lattice L, is there a map h (representing, if you like, a simultaneous valuation) which is a *weak homomorphism* of L to **2**, that is to say, such that the restriction of h to any Boolean subalgebra of L is a homomorphism? The celebrated results of Kochen and Specker (1967) show, in effect, that for the relevant quantum lattices *there can be no such map*.

Kochen and Specker showed that the lattice of subspaces of Euclidean 3-space E_3 has no weak homomorphisms. It follows from this that the lattice L_ω of all closed subspaces of Hilbert space (i.e., the usual lattice of quantum propositions) has no weak 2-valued homomorphisms which map at least one line (i.e., one-dimensional subspace) to 1.[10] Now under the usual assignment of elements of L_ω to quantum propositions, Boolean subalgebras of L_ω correspond to sets of *compatible* propositions, and lines in L_ω to *atomic* propositions of the form: 'the value of such-and-such a non-degenerate observable is so-and-so'. Thus the Kochen-Specker result means that there will be no way of assigning T and F across the whole system of quantum propositions so that compatible subsets are mapped homomorphically, and at least one atomic proposition is assigned the value T. Since this latter assertion is verified by experiment, the Kochen-Specker result seems to block finally any attempt to reconcile classical truth and falsity with quantum logic and the set of quantum propositions based on it.

This shows that quantum logic is quite inappropriate for one of Putnam's central purposes (*above*, p. 357); for it shows quantum logic to be irreconcilable with realism as usually understood. Does this mean that quantum logic is *absolutely* irreconcilable with realism? No, for as we shall see, (abstract) quantum logic can be reconciled with what we shall call for convenience *ontological* (or set-theoretic) realism, that is to say the view that the (mathematical) world is a realm of independently existing real objects.[11]

First let us observe that both classical logic and intuitionistic logic can be conceived as logics of realism in this sense. Classical logic can be conceived as a logic of properties of *real* (unchanging) individuals. This we know from the Stone Representation Theorem which says that every Boolean algebra is isomorphic to an algebra of subsets (which may be taken as properties in extension) of some underlying set of individuals. Moreover, recent work in topos theory (cf. MacLane 1975 or Bell forthcoming) shows that intuitionistic logic admits a similar description as a logic of properties, only now the individuals must be conceived of as *varying* in some manner (e.g., in time or space). In other words (and

[10]This does not seem to exclude the possibility that there exist weak 2-valued homomorphisms on L_ω which send every line to 0. We do not know if such maps exist.

[11]We stress that this claim is being made only for quantum *logic*, not quantum *physics*.

somewhat surprisingly, perhaps), intuitionistic logic too may be regarded as a logic of ontological realism, provided that the essential *variability* of the world is taken into account.

In fact there is a weak sense in which *any lattice* can be construed as a lattice of properties of a set of individuals. For if $(L, \wedge, \vee, \leq)$ is a lattice it is easy to find a set I and a one–one mapping h of L onto a family of subsets of I.[12] Moreover, h can be chosen to preserve $\wedge$ (i.e., to satisfy $h(x \wedge y) = h(x) \wedge h(y)$), and hence to preserve $\leq$. So now we can identify each $x \in L$ with its image $h(x)$ and think of the latter as a property of elements of I. In this way L becomes identified as a lattice of properties of the elements of I in which $\leq$ is *inclusion* and $\wedge$ is *classical conjunction*. Note, however, that since in general we do *not* have $h(x \vee y) = h(x) \cup h(y)$, we *cannot* (in this representation) construe $\vee$ as disjunction in the usual classical sense. We can, of course, guarantee to preserve classical disjunction simply by modifying the constructions.[13] But now in general conjunction will be necessarily different from its classical counterpart.

If L is an *ortholattice*, then one can represent L as an ortholattice of properties of a set that carries *some additional structure*. Specifically, let us define a *proximity space* to be a pair $(X, \approx)$ in which X is a set and $\approx$ is a *proximity relation* on X, i.e., a reflexive, symmetric binary relation.[14] For each $x \in X$ the *basic $\approx$-neighbourhood* of x is the set $U_x = \{y \in X : x \approx y\}$. For each subset $Y \subseteq X$, *the $\approx$-interior of* Y is the set $Y^0 = \{x \in X : \exists y \in Y\, [x \in U_y \subseteq Y]\}$, and the *$\approx$-complement* of Y the set $Y^* = \{x \in X : \exists y \notin Y\, [x \in U_y]\}$. A subset $Y \subseteq X$ is *$\approx$-open* if $Y = Y^0$. The class $\tilde{X}$ of all $\approx$-open subsets of X is then an ortholattice under inclusion, with $\cup$ as lattice join and $*$ as orthocomplementation.[15] It can now be shown that each ortholattice is isomorphic to a subortholattice of $\tilde{X}$ for some proximity space $(X, \approx)$. Thus any ortholattice may be represented as an algebra of properties of a set of individuals linked by a proximity relation. If the ortholattice is not a Boolean algebra, then in the corresponding algebra of properties, although disjunction is just classical disjunction, negation and conjunction are *necessarily* different from their

[12] Let I be the set of lattice filters in L, i.e., subsets G of L such that (*i*) $G \neq L$, (*ii*) $x \in G$, $x \leq y \rightarrow y \in G$, (*iii*) $x, y \in G \rightarrow x \wedge y \in G$. Define $h : L \rightarrow P(I)$ by $h(x) = \{G \in I : x \in G\}$.

[13] Instead of defining I as in n. 2, p. 356 *above*, take I as the set of lattice *ideals*, and define h by $h(x) = \{G \in I : x \notin G\}$. We then get $h(x \vee y) = h(x) \cup h(y)$.

[14] Our construction here is essentially a dualization of that given by Goldblatt (1975) for orthogonality spaces. Note that the notion of a proximity space is, in effect, dual to that of an orthogonality space.

[15] $\tilde{X}$ is in fact a *complete* ortholattice, because like a topology it is closed under arbitrary unions. It fails to *be* a topology because the intersection of two $\approx$-open sets may not be $\approx$-open.

classical counterparts. Again, we can if we choose preserve *conjunction* by modifying the construction[16]; but now, of course, classical disjunction and negation will not in general be preserved.

All this shows again just how different quantum logic (or the theory of non-Boolean ortholattices) is from classical logic. Nonetheless, we see that in principle quantum logic is compatible with the assumed existence of a world of real, independent objects, *provided* that not all of the logical operations on properties of these objects are given their classical interpretations. However, two points should be made immediately. First, this resolution becomes otiose if one insists that realism or a realist theory of properties (or their extensions) is *necessarily* bound up with the *classical* connectives. Secondly, it still falls far short of attaining Putnam's goal of showing that quantum *physics* (and not just abstract quantum logic) can be reconciled with realism. (For example, what are the real objects of the underlying space to be? Isn't this just one of the old insoluble fundamental problems come back to haunt us?) Furthermore, instead of standing out as a clear and relatively simple realist approach to the conceptual difficulties faced by quantum physics, it seems to be one among many *unclear* and *unstraightforward* approaches. Quantum logic is by no means an obvious replacement for classical logic, by no means obviously compatible with a realist approach to physics, and, so far in our account, by no means reveals how or why logic is empirical. In short, Putnam's 'sidestepping move' (see p. 361, *above*) fails to provide the kind of justification for quantum logic that he is looking for. However, as we pointed out, Putnam does have another approach to the quantum connectives which takes the issue of their meaning more seriously. This approach, although it seems to shift away from realism towards verificationism, at least attempts to put some body back into the claim that logic is empirical. We turn to this second approach now.

5. The Operational Determination Argument. Dummett has proposed that what is the correct logic for a given class of statements will be decided by *first* determining the correct model of meaning for those statements. In other words one develops a logic by developing a semantics which itself is guided by certain fundamental principles about meaning.[17] According to Dummett, in this respect 'there is an evident generic similarity between' the quantum case and the intuitionistic case:

> Both employ a notion of meaning that relates to the means available to us for knowing the truth of statements of the relevant class:

[16]That is, by following the original argument given by Goldblatt in his (1975).
[17]Dummett has argued this in various papers; the best reference in the present context is (1976, pp. 287–9).

in the quantum case, in terms of measurements of physical quantities; in the intuitionistic case, in terms of proofs of mathematical propositions (1976, pp. 288–9).

In other words, Dummett sees the way to look for the correct logic for quantum mechanics as building up the semantics from the notion of verification appropriate for elementary quantum mechanical propositions, just as intuitionistic logic is founded on the notion of verification appropriate for mathematical propositions (i.e., proof). There are two points that should be noted in the context of the present discussion. Firstly, this approach is fundamentally antirealist in spirit. Secondly, the Putnam-Finkelstein approach which we want to consider here *appears* to harmonize well with the program Dummett outlines.

The aim of the approach is to show that one can set up a natural operational semantics for quantum mechanical propositions based on the notion of a *test*, which is supposed to be an idealization of quantum-mechanical experimental practice. The verificationist tendency in this proposal is obvious, since the underlying assumption is that an elementary proposition P about a system S is said to be true just in case S passes a certain empirical test T_P. What Putnam and Finkelstein claim[18] is that the logic associated with this empiricist/verificationist semantical approach is precisely what we know as quantum logic. We argue here that this is correct only if one makes very strong *ad hoc* assumptions about the available collection of tests, assumptions for which neither Putnam nor Finkelstein provide any convincing arguments, and which in any case take us far away from the background of 'empirical practice' which is supposedly the starting point.

In order to give the notion of a *test* some substance we formulate it within a (simplified and idealized) framework of *filters* and *beams*, building on an idea suggested by Mielnik (1968) and (1969). This framework yields an axiomatisation of the empirical notions involved which is not only intuitively convincing, but has a quite clear and elegant algebraic structure. However, while the notion of test so formulated is entirely in harmony with that of Putnam and Finkelstein, the mathematical structure by no means stretches naturally to that of a lattice, which is what they are seeking.

We assume that we are supplied with various *streams* or *populations* of objects (to be thought of as *beams* of particles) which are to be *tested* to determine whether they have a given property or not. For a given property P, testing for P amounts to interposing some kind of *screen* or *absorber* which allows only those objects having property P to pass through it, all other objects being 'absorbed'. (For example, the stream of objects

[18] See Putnam (1969, pp. 192–7) and Finkelstein (1963), (1969) and (1972).

could be a beam of light photons, the property P, that of 'redness' and the screen a piece of red glass.) We shall assume that these screens are equipped with two 'windows' through which the objects can enter and leave, and that *either* of these windows can serve as entrance or exit. That is, each screen s can be 'reversed' to yield a new screen $\bar{s}$—its *transpose*—in which the entrance and exit windows have been interchanged. Given a pair of screens s, t we can construct a new screen st—the *product* of s and t—by juxtaposing t and s. Thus the effect of the screen st on a stream of objects is the same as that produced by first passing through t, and then through s. Clearly we then have: $(st)^{-} = t\bar{s}$ and $(\bar{s})^{-} = s$.

The notion of a screen sketched here is rather general, so we must now determine what properties a screen should possess if it is to correspond to a test. To begin with, since a test is supposed to select and pass only those objects in a stream having a given property, a subsequent application of the same test should have no further effect on the stream, i.e., should allow every object already selected to pass through unchanged. This means that a screen s which corresponds to a test must be *idempotent:*

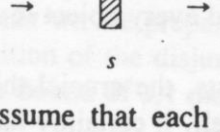
$$s^2 = ss = s. \tag{5.1}$$

We shall also suppose that a screen corresponding to a test is a *purely* selective device which is indifferent to the 'direction' that an object travels through it, i.e., which of its two windows the objects enter and leave through. In other words, a screen s corresponding to a test must be equal to its transpose:

$$\bar{s} = s. \tag{5.2}$$

A screen s satisfying (5.1) and (5.2) is called a *filter:* this is the notion which, in this framework, corresponds to the idea of a test.

After this heuristic account, let us now proceed more formally. We assume that we are given two sets S, the set of *screens* and B, the set of *beams* (of particles). Each screen s acts on each beam x to yield to a new beam sx (to be interpreted as the beam that emerges when x passes through s).



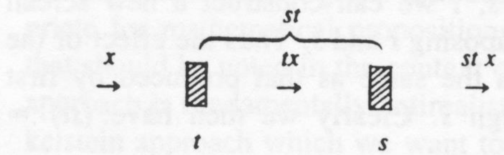
We assume that each screen is *uniquely* determined by its action on beams: thus for $s, t \in S$

$$\forall x \in B\ [sx = tx] \rightarrow s = t.$$

Any pair $s, t \in S$ is assumed to have a *product* $st \in S$ satisfying

$$\forall x \in B\, [(st)x = s(tx)].$$

So st may be regarded as the screen obtained by juxtaposing t and s:



We shall assume that the product operation is *associative:* $s(tu) = (st)u$ for all $s, t, u \in S$.

We also suppose that B contains a unique 'empty' beam $\bigcirc$ satisfying $s\bigcirc = \bigcirc$ for all $s \in S$, and that S contains two special elements 0 and 1 satisfying $0x = \bigcirc$ and $1x = x$ for all $x \in B$. Thus 0 is a screen which 'absorbs' every beam and 1 is a screen which passes every beam unchanged. It follows that $0s = s0 = 0$ and $1s = s1 = s$ for all $s \in S$. Thus, S may be described in mathematical jargon as a *monoid* acting on B. Finally, we assume that for each $s \in S$ there is an element $\tilde{s} \in S$ called its *transpose* such that

$$(st)^{\sim} = \tilde{t}\tilde{s}, \quad (\tilde{s})^{\sim} = s$$

for all $s, t \in S$. It is easily shown that $\tilde{0} = 0$, $\tilde{1} = 1$. (Thus, S is a monoid with involution.)

An element $s \in S$ is called a *filter* if $s^2\, (=_{df} ss) = s = \tilde{s}$. This is clearly equivalent to the condition: $s\tilde{s} = s$. We write F for the set of all filters. Observe that $0, 1 \in F$. Also note the important fact that, for filters s, t $\in F$,

$$st \in F \leftrightarrow st = ts. \tag{5.3}$$

A screen s is said to be *transparent* to a beam x, and x is said to *pass* s, if $sx = x$, i.e., if passage through s has no effect on x. And s is said to be *opaque* to x, or to *block* x, if $sx = \bigcirc$, i.e., if x is completely absorbed by s. Thus, still thinking of a beam x as a stream of objects, and a filter s as a *test* for a property P, then every object in x passes the test corresponding to s iff s is transparent to x, and every object in x fails the test iff s blocks x.

Given this set F as the collection of filters/tests, the crucial thing to ask is: does it have a natural *lattice* structure? Well, it certainly has naturally defined ordering and orthogonality relations. These relations are characterised as follows. For $s, t \in F$, let

$$s \leq t \leftrightarrow_{df} \forall x \in B\, [sx = x \rightarrow tx = x].$$

This says that every beam that passes s also passes t, and for the corresponding tests, every stream of objects each of which passes the test s also passes the test t. Thus $\leq$ corresponds to Putnam's and Finkelstein's natural ordering of tests (see Putnam 1969, p. 195). Next, we define $\perp$: for $s, t \in F$,

$$s \perp t \leftrightarrow_{df} \forall x \in B\, [sx = x \rightarrow tx = \bigcirc \quad \text{and} \quad tx = x \rightarrow sx = \bigcirc].$$

This says that no beam that passes s passes t, and vice-versa, i.e., *nothing* passes both the test s and the test t. Thus $\perp$ corresponds to Finkelstein's notion of *exclusion* (1972, p. 146). Now the following key results hold (for proofs, see the *Appendix*): for $s, t, u \in F$

$$\begin{aligned} &\text{(i)} \; s \leq t \leftrightarrow st = ts = s \\ &\text{(ii)} \; s \perp t \leftrightarrow st = ts = 0 \qquad (5.4) \\ &\text{(iii)} \; s \leq t \,\&\, t \perp u \rightarrow s \perp u \end{aligned}$$

And from these it follows that

$\leq$ is a partial ordering on F with $0, 1$
as smallest and largest elements and
$\perp$ is an orthogonality relation on F, (5.5)
i.e., $\perp$ is irreflexive and symmetric
on $F - \{0\}$ and $0 \perp s$ for all $s \in F$.

Let us write $P(x,s)$ for the statement 'the beam x passes the filter s'. Then each $s \in F$ is uniquely correlated with the set $\{x \in B : P(x, s)\}$ or equivalently with the 'property' $P(\cdot, s)$. Thus we may regard F as a set of *properties* of beams. Moreover, as we have seen *above*, we have natural relations $\leq$ and $\perp$ defined for these properties. The Putnam-Finkelstein claim may now be construed to assert that $(F, \leq, \perp)$ gives rise to the quantum-logical operations because it is actually an *ortholattice*.

It seems to us that this claim is open to doubt. To begin with, we certainly cannot *prove* it to be true within our framework, because it is easy to find an example of a monoid S for which the corresponding F is not a lattice at all.[19] Moreover, if we think of F as a structure given empirically, then whether F is *actually* a lattice would appear to depend on what we are prepared to accept as a filter or 'test'. For example, the definition of the disjunction $s \vee t$ of two elements $s, t \in F$ as the least upper bound of s, t means that it must satisfy

$$\forall x \in B\, [P(x, s \vee t) \leftrightarrow \forall u\, [s \leq u \text{ and } t \leq u \rightarrow P(x, u)]].$$

[19] Take S to be a lower semi-lattice (i.e., a partially ordered set having meets, but not joins) which has 0 and 1, but which is not a lattice. Let $st =_{df} s \wedge t$; then F (which is now just S) is not a lattice.

That is, in order to test whether a beam passes $s \vee t$ we are in principle obliged to test whether it passes *all* filters u such that $s \leq u$ and $t \leq u$. Is there any reason to suppose that we can always find a filter v which has the property that any beam passes v if and only if it passes every filter u such that $s \leq u$ and $t \leq u$? To put it another way, is the 'higher-order' property of beams $\forall u[s \leq u \text{ and } t \leq u \rightarrow P(x, u)]$ equivalent to an 'elementary' property of the form $P(x, v)$ for some filter v? The answer to this question is by no means clear cut, for Mielnik (1969) actually supplies examples of filters s and t for which the existence of the least upper bound filter $s \vee t$ only results from an appeal to a particular physical theory, in this case classical linear electrodynamics. It seems, therefore, that there is nothing *inherent* in the nature of F as a collection of empirically given entities which guarantees that it even carries a lattice structure.

Is there any way of ensuring that F is an ortholattice of the sort Putnam and Finkelstein have in mind? Of course there is: just assume that B is a Hilbert space,[20] S is the set of bounded linear operators on B, and F is the set of projection operators on B (which is naturally isomorphic to the lattice of subspaces of B). All the axioms are then satisfied. But how can *this* assumption be justified? Apparently only by invoking, as Putnam does (cf. 1969, p. 165), the 'truth of quantum mechanics'. But this invocation really begs the question, since the 'truth of quantum mechanics' is virtually *equivalent* to the assumption which is to be justified, and in any case carries us right away from the straightforward empirical considerations that served as our starting point. As far as we can see, the only convincing way of justifying the assumption would be to show by further analysis of the nature of the structure (B, S, F) that F satisfies conditions which ensure that it is isomorphic to a lattice of subspaces of a Hilbert space. Such conditions have been formulated,[21] but they are of such complexity as to make them look entirely *ad hoc* in the present context.

The plain fact of the matter is that although S, rather strikingly, carries a natural algebraic structure (that of a monoid with involution) the derived structure $(F, \leq, \perp)$ is *too weak* to guarantee that it carries unique, or indeed any, lattice operations. In this connection, it is worth observing that, irrespective of any lattice structure that F may carry—quantum-logical or otherwise—the structure $(F, \leq, \perp)$ is *always* embeddable in a Boolean algebra. For we may correlate each $s \in F$ with the class B_s of non-zero beams that pass it, and the B_s are, of course, members of the Boolean algebra of subsets of the class B of beams. This embedding preserves

[20]For simplicity we are assuming that the elements of B are 'pure' beams which correspond to the 'pure' states of orthodox quantum mechanics.
[21]See e.g., Varadarajan (1968, ch. 6).

$\leq$ and $\perp$ in the sense that $s \leq t \leftrightarrow B_s \subseteq B_t$ and $s \perp t \leftrightarrow B_s \cap B_t = \emptyset$. (In fact, as we show in the *Appendix* very weak conditions on a structure $(X, \leq, \perp)$ of the type under consideration suffice to ensure such embeddability.) In other words, the classical logical operations can be *formally* defined on F; what quantum mechanics implies is that F is *not closed* under *these* operations, but rather under the *quantum-logical* operations which are then *necessarily distinct* from the former.

The observation that quantum mechanics implies that F is not closed under the classical logical operations, but only under the quantum-logical ones, also exposes the error in Gardner's claim (1971, p. 519) that the 'logic of tests' is faulty because of Kochen and Specker's result (see *above*, p. 370). What Gardner's argument shows, in essence, is that this would be the case if the (quantum) conjunction and disjunction filters $s \wedge t$ and $s \vee t$ were such that (for compatible s and t)

$$\forall x \in B[P(x, s \wedge t) \leftrightarrow P(x,s) \text{ and } P(x,t)]$$

$$\forall x \in B[P(x, s \vee t) \leftrightarrow P(x,s) \text{ or } P(x,t)].$$

But these equivalences are another way of asserting that the quantum-logical operations on filters are just the classical ones, and we know this to be false (provided quantum mechanics is true). The 'logic of tests' may be not *justified*, but it is not *inconsistent*.

To summarize, then, in our view Putnam and Finkelstein's 'logic of tests', although consistent, falls far short of the requirements imposed on a semantic framework by Dummett's program. The structure $(F, \leq, \perp)$, albeit quite natural, is much too weak to yield the quantum-logical operations unaided. Moreover, the additional data required to yield these operations comes not from a deeper analysis of the intrinsic structure of F, but from what in this context is a quite factitious assumption about its isomorphism to a subspace lattice. And as long as this remains the case, the claim that quantum logic embodies the fundamental laws of empirical testing and is itself empirically determined must remain in doubt.

APPENDIX

Proof of (5.4) Let $s, t, u \in F$.

(i) If $s \leq t$, then $sx = x \rightarrow tx = x$ for all $x \in B$. But $s \cdot sx = s^2x = sx$, so $tsx = sx$ for all $x \in B$ whence $ts = s$. Therefore $ts \in F$, whence $st = ts = s$. Conversely, suppose $ts = s$; then if $sx = x$, we have $x = sx = tsx = tx$, so that $s \leq t$.

(ii) If $s \perp t$, then $sx = x \rightarrow tx = \bigcirc$ for every $x \in B$. But $ssx = s^2x = sx$, so $tsx = \bigcirc$ for all $x \in B$, whence $ts = 0$. Hence $ts \in F$, so that $st = ts = 0$. Conversely, suppose $st = ts = 0$. Then if $sx = x$, we have $tx = tsx = 0$ and if $tx = x$, then $sx = stx = 0$. Hence $s \perp t$.

(iii) If $s \leq t$ and $t \perp u$, then $st = ts = s$ and $tu = ut = 0$. Hence $su = stu = 0$ and $us = uts = 0$, so that $s \perp u$. ■

Finally, we prove the

Proposition Let $(X, \leq)$ be a partially ordered set with least element 0, and let $\perp$ be an orthogonality relation on X, i.e., $\perp$ is irreflexive and symmetric on $X - \{0\}$ and $0 \perp x$ for all $x \in X$. Then the following are equivalent:

(i) for all $x, y, z \in X$,

$$x \leq y \quad \text{and} \quad y \perp z \rightarrow x \perp z;$$

(ii) there is a set I and a one–one map $f : X \rightarrow P(I)$ (the set of all subsets of I) such that $f(0) = \emptyset$, and for all $x, y \in X$, $x \leq y \leftrightarrow f(x) \subseteq f(y)$, and $x \perp y \leftrightarrow f(x) \cap f(y) = \emptyset$.

Proof (Sketch) (ii) → (i) is obvious. For (i) → (ii), let I be the set of all subsets $J \subseteq X$ such that $x \in J$, $y \geq x \rightarrow y \in J$ and $x, y \in J \rightarrow x \not\perp y$. Define $f : X \rightarrow P(I)$ by $f(x) = \{J \in I : x \in J\}$. It is now easy to verify that I and f meet the requirements of (ii). ■

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