

## Quantum Logic and Meaning<sup>1</sup>

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### 1. Background

In concluding their major survey, "Quantum Logics," Greechie and Gudder (1975) listed four outstanding directions for future research, the last of which was, "Explain the meaning of the word 'logic' in the title of this paper." Broadly speaking, there are two radically different points of view on this. There is the view that, taken strictly, 'logic' is a misnomer: quantum logic studies certain algebraic structures naturally arising from the formal apparatus of quantum theory, structures which may afford insights into quantum physics on a number of levels, but which do not in any way involve giving up or replacing classical logical laws. (Cf., e.g., Jauch (1968, p. 77).) In contrast, there is the view that the non-Boolean structures arising from the Hilbert space formalism should be taken as giving rise to a genuine non-classical logic, in conflict with classical logic, but appropriate to the description of quantum mechanical reality. (This position is set forth most forcefully by Putnam (1969) and is also favored by Finkelstein (1969 and 1972), and Bub (1973) (but cf., Bub (1979), and others).) Associated with this is a general philosophy of logic according to which logical laws are subject to revision for cause in much the way deeply entrenched laws of physical geometry have been. In fact, Putnam has developed this analogy in some detail, arguing that all the reasons usually given for the absolutely a priori status of classical logic apply equally well to the principles of Euclidean geometry (of physical space), which our best current physical theory contravenes. On the positive side, it is argued that giving up some classical logical laws (in particular, the distributivity of conjunction over disjunction) leads to the most satisfactory overall understanding of quantum mechanics, blocking derivations of the paradoxes (2-slit, potential barrier, etc.) and allowing one to retain a principle of realism with respect to the quantum magnitudes (the so-called "precise values principle" according to which systems possess definite values of the quantum magnitudes independent of observation),<sup>2</sup> while at the same time avoiding

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any ad hoc split between different levels of the physical world (such as between atoms and measuring devices).

One rather standard objection to the latter "real logic" view of quantum logic alleges that the quantum logical connectives mean something different from the classical connectives, and hence nothing in classical logic is contradicted by the truths of quantum logic. In his original paper (1969), Putnam considered this "meaning change" objection and gave several persuasive reasons why it is not decisive. However, the opponent against whom Putnam argued was a rather dogmatic conventionalist who was rather prone to put more weight on the notion of "meaning" than scientific scrutiny should allow. What I want to do here is focus on a more precise "meaning-change" argument, one which makes absolutely minimal reliance on the problematic word, 'meaning', and which, as far as I can see, a proponent of Putnam's view can neither defeat nor bypass.

Before presenting the argument, two preliminary remarks are in order. First, quite apart from the issues I shall be discussing, as already noted in connection with realism, there are serious problems confronting the "positive side" of the "real-logic" view that need further attention.<sup>3</sup> (Let us abbreviate 'real logic' by 'reall', pronounced just like 'real'.) For example, the view, as I understand it, is not "revisionist" in that it respects classical logic on the levels of macroscopic physical reality and classical mathematics. (Cf., e.g., Finkelstein (1969, p. 212).) Thus, there is the problem of specifying just where classical logic takes over. In his analogy with geometry, Putnam wrote, "Quantum mechanics itself explains the approximate validity of classical logic 'in the large', just as non-Euclidean geometry explains the approximate validity of Euclidean geometry 'in the small'." (1969, p. 184). But, whereas it makes sense to speak of a continuous approach to flatness, sense has yet to be made of a continuous approach to distributivity! Closely related to this is a deeper point: while it may possibly be true that reall quantum logic allows one to retain something approximating the precise-values principle (avoiding  $\omega$ -inconsistency by modifying the quantifier rules -- e.g., exportation of ' $\exists$ ' across conjunction must go (1969, p. 183)), and to avoid a mysterious cut between system and observer, a new mysterious cut is introduced and cries out for explanation, namely the cut between those portions of reality to which classical logic applies and those to which it does not. If quantum logic is real logic, why does it not apply to abstract sets or to natural numbers, domains of exceptional purity, free of the vagaries of ordinary discourse? In the old days, when logic was true in virtue of linguistic conventions, an intelligible response was forthcoming to the question why certain truths were logical truths. How can the reall quantum logician explain why it is contradictory for an electron to have definite position  $q$  and definite momentum  $p$  at the same time?

Second, even if I am right and it turns out that quantum logic is not "real logic" at all, the question whether, in general, logic is quasi-empirical remains open. One of the advantages of the argument

below is that it does not rest on any a priori conception of the nature of logical truth. Furthermore, it is an open question how far the same sort of argument reaches over the field of "deviant logic" generally. It must not be assumed that the moves open to the quantum logician exhaust those open to, say, the intuitionist or the relevance logician. That must be pursued elsewhere.

## 2. Argument

We agree that talk of "meaning" is indeterminate in many ways and that many meaning-change arguments in the past cannot be supported by clear and acceptable principles of meaning. However, we are not utter skeptics either and are prepared to accept some constraints (in the spirit of (1975)). One would be, for example, that if two denoting expressions are genuine synonyms, then their denotations are identical. A parallel principle would seem to govern sentential connectives (occurrences thereof):

MDT: If  $\alpha$  and  $\beta$  are synonymous sentential connectives, then

(a) if one is a truth-functional connective, then so is the other,

and

(b) if  $\alpha$  and  $\beta$  are truth-functional, they have the same truth tables.

("MDT" for "meaning determines truth-tables".)<sup>4</sup>

Clearly, if MDT is accepted, then the QM logical connectives (corresponding to the meet, join, and orthocomplement operations on the lattice of closed linear subspaces of Hilbert space) cannot all be synonymous with the respective corresponding classical connectives, since they cannot have the same truth-tables. (This latter because, e.g., distribution fails for QM meet and join but is guaranteed by the classical truth-tables.)

Alternatively, one may argue as follows, making use of just clause (a) of MDT: it is trivial to show that both QM join and meet, in any quantum logic in which the partially ordered set of propositions is isomorphic to the lattice of closed subspace of a Hilbert space,<sup>5</sup> do not behave truth-functionally.<sup>6</sup> Since this point is disputed, and since it will prove important in what follows, it is worth spelling out in some detail here. In order to do so, it is first necessary to specify what rules are to govern the assignment of truth-values, T and F, to the subspaces. Only two conditions are needed, as follows:

(1) For any subspaces, M, N, if  $M \leq N$  and M is assigned T, then N is also assigned T;

(2) For any subspace M, M is assigned T iff  $M^\perp$  is assigned F.

It turns out that these are precisely the conditions arrived at by Friedman and Glymour (1972, pp. 19-20), constituting the only semantics they regarded as a reasonable framework "within which to realize Putnam's proposals." (p. 26). Condition (1) is indispensable, as it merely guarantees that QL implication be truth-preserving. Condition (2) is required if QL "negation" is to behave truth-functionally. Abstractly, we may take an admissible assignment of T, F to the subspaces  $\mathcal{M}(H)$  of a Hilbert space  $H$  to be a function  $\varphi: \mathcal{M}(H) \rightarrow 2$ , which meets conditions (1) and (2). (Here 2 is just the set containing 0 and 1—it could be any two-element set. We will informally think of 1 as the truth-value T.) Now let us say that an assignment  $\varphi$  violates the truth-functionality of a (binary) connective  $k$  just in case there are  $M, N, M',$  and  $N'$  in  $\mathcal{M}(H)$  such that  $\varphi(M) = \varphi(M')$  and  $\varphi(N) = \varphi(N')$  and  $\varphi(k(M, N)) \neq \varphi(k(M', N'))$ . Now we may prove the following little

Theorem: Let  $H$  be a Hilbert space of dimension  $\geq 2$ , and let  $\varphi$  be any admissible assignment from  $\mathcal{M}(H) \rightarrow 2$ , and let  $M$  be some 1-dimensional subspace of  $H$  such that  $\varphi(M) = 1$ . Then  $\varphi$  violates the truth-functionality of  $QM \vee$  (dually of  $QM \wedge$ ).

Proof. By hypothesis  $\varphi(M) = 1$ . (Intuitively, this just means that the system represented by  $H$  is in some state, not necessarily a unique state. I mention this here since part of Putnam's QL program denies that the instantaneous state of a system is represented by a unique Hilbert space vector.) By condition (2),  $\varphi(M^\perp) = 0$ . Let  $N$  and  $K$  be any two subspaces of  $M^\perp$  which span  $M^\perp$ .  $M^\perp = N \vee K$ . By condition (1),  $\varphi(N) = \varphi(K) = 0$ . (Recall that, for arbitrary  $M, N, M \leq N$  iff  $N^\perp \leq M^\perp$ , and  $\varphi$  is assumed to be defined on all of  $\mathcal{M}(H)$ .) Now let  $L$  be any 1-dimensional subspace of  $M^\perp$ .  $\varphi(L) = 0$ . Now by condition (1),  $\varphi(L \vee M) = 1$ . But we can find another 1-dimensional subspace  $P$  in the plane spanned by  $L$  and  $M$  such that  $\varphi(P) = 0$ . Let  $Q$  be any 1-dimensional subspace in that plane distinct from  $L$  and  $M$ . If  $\varphi(Q) = 0$ , set  $Q = P$  and observe that  $P \vee L = L \vee M$ . Thus we have  $\varphi(P \vee L) = 1$ ,  $\varphi(N \vee K) = 0$ , but  $\varphi(P) = \varphi(L) = \varphi(N) = \varphi(K) = 0$ , and  $\varphi$  violates the truth-functionality of  $\vee$ . If  $\varphi(Q) = 1$ , then  $\varphi(Q^\perp \wedge (L \vee M)) = 0$ , by successive applications of conditions (2) and (1). Then set  $P = Q^\perp \wedge (L \vee M)$ , and observe that, in exactly the same way,  $\varphi$  violates truth-functionality of  $QL \vee$ . An exactly dual argument shows that  $\varphi$  also violates the truth-functionality of  $QL \wedge$ .

Before entertaining possible objections to these arguments, we make two brief remarks. First, note that the non-truth-functionality argument is entirely distinct from the argument of Friedman and Glymour in (1972). There it was argued that QL could not satisfy the demands of realism, because, despite the fact that admissible truth-assignments (meeting conditions (1) and (2)) exist (which they proved for arbitrary Hilbert spaces), no such assignment could be a homomorphism from  $\mathcal{M}(H) \rightarrow 2$ , in light of the theorem of Kochen and Specker (1967), whereas realism was understood as requiring such a homomorphism (intuitively, an assignment of truth to one 1-dimensional subspace for each eigenbasis spanning the space). Our argument does not involve "realism" at all. And it makes no demand that there be a homomorphism from

$\mathcal{V}(H) \rightarrow 2$ , since conditions (1) and (2) on admissible truth-assignments are essentially weaker (a fact which follows from the Friedman-Glymour consistency proof just mentioned).<sup>7</sup>

Second, note that the above theorem belongs to pure mathematics. Its significance of course depends on connecting  $\mathcal{V}(H)$  with propositions about quantum systems and 2 with  $\{T, F\}$ , but the theorem itself says nothing about quantum systems. This is important because it means that any real QL program that respects classical mathematical truth must also respect this theorem. In other words, what we have called the preservationist QL program must be able to recognize the non-truth-functional behavior of the QL operations, join and meet.

### 3. Disputation

Objection 1 (Quine (1970, p. 84)): Circularity is involved in attempting to give the meanings of logical connectives by means of truth-tables. These are, anyway, just notational devices abbreviating truth definitions. In giving the latter, use must be made of logical connectives in the meta-language, and this begs the question.

Answer: It all depends on what the question is. The objection is right with respect to the attempt to explain the meaning of classical connectives (to one who claims not to understand!) in terms of truth-tables. (This is explicitly the situation Quine considered.) However, lack of understanding is not the real quantum logician's problem, since, as noted above, he is a preservationist with respect to "classical levels of reality". Furthermore, the MDT principle is not a TDM principle: it does not assert that truth-tables exhaust the meaning of sentential connectives. Nor is it even concerned with defining the connectives or with how they might be learned. It is merely a one-way connection between meaning and "reference" (in the wide, Fregean sense).

Objection 2<sup>8</sup>: The appeal to truth-tables, even in the limited way involved in MDT, is unacceptable because it is biased toward classical logic, building in distribution in the very set-up of the tables (i.e.,  $p$  is T or F and  $q$  is T or F, therefore either  $p$  is T and  $q$  is T or . . .). In fact, it is central to QL that the rows of the classical truth tables do not exhaust all the possibilities since, for some  $p$  and  $q$ , we do not have:  $(p \wedge q) \vee (p \wedge \bar{q}) \vee (\bar{p} \wedge q) \vee (\bar{p} \wedge \bar{q}) = 1$ .

Answer: This is ingenious, but, first, do we really need to argue, using distribution, that the rows of the truth-tables correspond 1-1 with all possible truth-assignments? Surely, in the cases of unary and binary connectives, we know this by inspection as well. Second, this use of distribution is on the macro-level where it is being respected by the real quantum logician. In fact, it is on the level of the pure theory of finite sets: to say the TT, TF, FT, and FF exhaust the possibilities is just to say that these are in one-one correspondence with all the functions assigning T or F to two elements. Third, the inference from the failure of  $(p \wedge q) \vee (p \wedge \bar{q}) \vee (\bar{p} \wedge q)$

$v(\bar{p} \wedge \bar{q}) = 1$ " to "the rows of the classical truth-tables don't exhaust the possibilities" loses any initial credibility it may have had once it is realized that the ' $\wedge$ ' and the ' $v$ ' of this formula do not behave truth-functionally. Moreover, this can be grasped by the quantum logician. Thus, the failure of the above formula in QL in no way cuts against the MDT principle. Truth tables are of course inappropriate for certain sentential connectives; the MDT then simply requires that they be recognized as non-synonymous with the classical connectives.

Objection 3 (Putnam (1969, pp. 189-190)): "But is the adoption of quantum logic a 'change of meaning'? The following principles:  $p$  implies  $p \vee q$  . . . all hold in quantum logic, and these seem to be the basic properties of 'or' . . . Thus a strong case could be made out for the view that adopting quantum logic is not changing the meaning of the logical connectives, but merely changing our minds about the law

$$p \cdot (q \vee r) \text{ is equivalent to } p \cdot q \vee p \cdot r."$$

Answer: Yes, some of the "essential rules" are obeyed by the QM connectives, but, if we follow MDT, not all. In fact, the fourth row of the truth-table for classical disjunction does not generally hold for QM join. One can easily have "system S is in A eigenstate  $\psi_1$ " and "system S is in A eigenstate  $\psi_2$ " both false but the QM disjunction true because the system is in the closed linear span of the 1-dimensional subspaces corresponding to  $\psi_1$  and  $\psi_2$ .

Objection 4<sup>B</sup>: Your last answer depends crucially on using the classical connectives in your metalanguage. If instead, in fairness to the QM logician, you stick with QM connectives in the metalanguage, the fourth row of the truth-table for QM 'or' will come out just the same as it does for classical 'or'. Notice that when we define falsity 'F' ( $\perp$ , in Quine's notation) as 'not T', we must use QM 'not', i.e., the operation corresponding to orthocomplement, ' $\perp$ '. That is, 'false' means  $T^\perp$ , pronounced "tee-perp". ( $\perp^\perp$  just means T, and must be pronounced "false-perp", not "perp-perp" which is nonsense!) This understood, the fourth row for QM join just says that the intersection of  $p$  perp and  $q$  perp is in the perp of the span of  $p$  and  $q$ , which is valid in the QM lattice. (It has the form of a de Morgan law, which holds in QM logic.)

Answer: "Ingenuity" is too mild a word for this kind of cleverness! There are, however, two counters to this that seem to me decisive. First, it suffices to point out that, if the QM logician is to maintain his preservationist stance, his metalanguage must also contain the machinery for expressing the classical connectives. In particular, elementary truths of the purely mathematical theory of vector spaces must be preserved, including indefinitely many of the form

(\*)  $f$  does not belong to  $S$  and  $f$  does not belong to  $S^\perp$ ,

$f$  a vector in the vector space of which  $S$  is a proper subspace. But

this means that the 'not' in such sentences must be classical, not QM 'not', since no such sentence comes out true under the QM interpretation. (QM 'and' behaves classically as ordinary set-theoretic intersection,<sup>9</sup> so that there is no room for play in cases of form  $(*)$ , unlike instances of the law of the excluded middle,  $p \vee p^\perp$ , where the two non-classical connectives compensate one another to preserve the truth of the whole sentence.) But then the quantum logician must be able to represent classical negation and can frame the classical definition of 'false'. Thus he can at least understand the MDT principle as it was intended, and can observe the divergence in behavior of the classical and QM connectives.<sup>10</sup>

The second point is that the argument for the truth-functionality of QM ' $\vee$ ' based on the use of QL connectives in the metalanguage is simply a mistake. No matter how 'F' is defined, bivalent assignments of T, F, satisfying the two minimal conditions of truth-preservation of ' $\leq$ ' and classical behavior of ' $\perp$ ' violate the truth-functionality of both ' $\vee$ ' and ' $\wedge$ ', as was proved at the outset. The argument of Objection 4 fails because it assumes truth-functionality of ' $\wedge$ '; otherwise, all it establishes is that  $p^\perp \wedge q^\perp$  and  $p \vee q$  must have opposite truth-values, far short of the desired conclusion.

Objection 5: You have placed much emphasis on the need to preserve classical mathematics. But it must not be assumed that this cannot be done on the basis of quantum logic. In fact, some formal work of Takeuti (unpublished manuscript) has established that there is a reasonable translation of classical set theory into "quantum set theory" preserving classical theorems. In effect, if one begins with an orthomodular lattice  $Q$ , one can define a hierarchy based on propositional functions with values in  $Q$ , and then show that classical mathematics will be reproduced in suitably rich Boolean subalgebras of  $Q$ . This brings out the quantum logician's central idea, which your above polemic seems to have missed, that classical mathematics and logic are to be seen as a special case of a more general situation, reflected in the wider mathematical universe of quantum set theory, and properly described by quantum logic. Classical logic works perfectly well within a domain of mutually compatible (commutable) propositions, but it breaks down elsewhere when we have to shift from one Boolean subalgebra to another.

Answer: This is intriguing. At least the preservationist position is clearly being recognized. Now there are some interesting formal questions here that certainly deserve investigation. For example, transfinite induction is used repeatedly in Takeuti's constructions. Does this mean that classical set theory is being presupposed in the metalanguage? In any case, I do not see how, even if fully non-circular and successful, Takeuti's constructions can make the slightest difference to the meaning-change argument. What will have been shown is that classical mathematics can be reproduced using just the resources of quantum logic.

It should be emphasized this has not been shown. Moreover a recent result of Dunn's (1981) suggests that it cannot be done. The result is that within the QL framework the axiom of extensionality for sets implies the distributive law (and ultimately all classical tautologies) for arbitrary propositions. Set theory based on quantum logic cannot countenance extensionality without generating classical logic. Now to say that extensionality fails for sets is absurd. The quantum mathematician must be regarded as treating of different objects. That in itself may be unobjectionable, but here it seems the QL program cannot take a pluralist stand. Rather, to get around Dunn's theorem, it must insist that, somehow, mathematics based on set theory really is wrong!

In any case, developing an optimal quantum mathematics (whatever that might be) would not support the view that some classical logical law fails for incompatible propositions. In fact, the Takeuti approach itself suggests a simple way in which the quantum logician can introduce new sentential connectives that behave classically on the domain of all propositions. All he need do is stipulate, for example, that " $\text{p}$  and  $\text{c}_1 \text{q}$ " is to have the same truth-value as a function of the truth-values of  $\text{p}$  and  $\text{q}$  that " $\text{p}' \wedge \text{q}'$ " has for any  $\text{p}'$ ,  $\text{q}'$ , propositions in a common Boolean subalgebra of  $\mathcal{Q}$ , such that the pairs  $\text{p}'$ ,  $\text{p}$  and  $\text{q}'$ ,  $\text{q}$  have the same truth values, respectively. Within a common Boolean subalgebra, the quantum connectives coincide with the classical connectives, so these stipulations have the effect of introducing the classical connectives, defined on  $\mathcal{Q} \times \mathcal{Q}$ . Now the quantum logician can find a non-compatible triple of propositions,  $\text{p}$ ,  $\text{q}$ ,  $\text{r}$ , such that

(i)  $\text{p} \wedge (\text{q} \vee \text{r})$  does not imply  $(\text{p} \wedge \text{q}) \vee (\text{p} \wedge \text{r})$

but such that

(ii)  $\text{p}$  and  $\text{c}_1 (\text{q} \text{ or } \text{c}_1 \text{r})$  does imply  $(\text{p} \text{ and } \text{c}_1 \text{q}) \text{ or } \text{c}_1 (\text{p} \text{ and } \text{c}_1 \text{r})$ .

Thus, if he refuses to subscript the connectives, he has a contradiction within his own framework. And it will not do at this point to contemplate rejecting (ii) on the grounds that TT, TF, FT, FF, do not "exhaust the possibilities". For, we have already seen that this rested entirely on the failure of a QL identity, a failure readily explained by the non-truth-functionality of ' $\wedge$ ' and ' $\vee$ ', which itself was established by reasoning that, by hypothesis, has been preserved!

Objection 6: What a predicament! As soon as QL succeeds in preserving classical mathematics, you insist that it will also have preserved the non-truth-functionality theorem, and that, therefore, two types of connectives must be distinguished from within the QL as well as from within the classical framework. But let us return to "meaning". It seems that if MDT is accepted, it must be granted that some of the  $\mathcal{Q}^M$  connectives are not synonymous with their classical counterparts. But all that this shows is that the real quantum logician must reject the MDT as an arbitrary constraint on "meaning". And are we not free to do so? For, as Putnam has said, "we simply do not possess a notion of 'change of meaning' refined enough to handle this issue." (1969, p. 190).



Answer: This simply won't do, because we don't need a very refined notion of "meaning" to support the MDT principle. For both clauses (a) and (b) are readily derived (without using distribution, though, again, this is reasoning on the "classical level" where it should be allowed!) from the more elementary principle of substitutivity salva veritate of synonyms in (extensional, eternal) sentences. For example, to derive (a) (for binary connectives, for simplicity), suppose  $\alpha$  and  $\beta$  synonymous, with  $\alpha$  truth-functional but  $\beta$  not. Then, by definition of truth-functionality, there exist two compound sentences,  $p_1 \beta q_1$ ,  $p_2 \beta q_2$ , such that  $p_1$  and  $p_2$  have the same truth-value and  $q_1$  and  $q_2$  have the same truth-value but  $p_1 \beta q_1$  is (say) T and  $p_2 \beta q_2$  is F (or any truth-value other than T). But then consider the sentences  $p_1 \alpha q_1$  and  $p_2 \alpha q_2$ . By the substitutivity principle, the first must be T but the second cannot be. This contradicts the truth-functionality of  $\alpha$ . An equally simple argument goes through for clause (b) of MDT.

Could it be that substitutivity salva veritate is an arbitrary constraint on 'meaning'? Is this meagre fragment of Fregean semantics also up for grabs? Here we run the risk of terminating debate since, if Putnam were to take this line, we would want to charge him with having changed not only the meaning of "The Meaning of 'Meaning'" (1975) but also the meaning of 'meaning'. One of the problems with philosophy of logic is that, when elementary logical laws are being challenged, it rapidly becomes unclear what counts as allowable argument. At the risk of inviting still further quibbling, let us simply state that 'synonymy' in the sense of 'sameness of truth-conditions' is the kind of synonymy relevant to logic, since logical rules aim to be truth-preserving. Agreed, in general we know how unhelpful such dicta are (what, in general, are "truth-conditions"?), but where, as in the present debate, the interpretation is clear (in terms of truth-functionality and truth-tables), it is hard to see anything arbitrary in applying such principles.

Objection 7: It seems that it must be granted that your meaning principle is not arbitrary and that the  $\text{QM}$  connectives are not all synonymous with their classical counterparts. Till now, we both have been arguing as though everything turned on that, but you realize that that isn't so. When we speak of "the classical connectives", so long as we are referring to operations of a certain kind of formal system there is no problem in knowing what we're talking about. But we cannot simply assume that classical formal systems properly represent the particles of actual languages in use, ordinary, scientific, or mathematical. The quantum logician, like the intuitionist and relevance logician, can maintain that classical formal systems systematically misrepresent informal logical reasoning . . .

Interruption: So that we don't waste time and trees, let me make sure that you don't mean to be giving up the preservationist standpoint. Surely, that would render the overall position bankrupt. It's one thing to challenge a small set of principles in a limited and otherwise problematic domain, but to say, for instance, that everyone has simply been mistaken about distribution in arguments involving sets or

natural numbers is to betray a dogmatism that not even an intuitionist could be accused of. But that is what you would be committed to if you were to maintain that such arguments are not properly represented by classical calculi. Elementary mathematical facts such as those of form (\*) above would have to be declared really not to be facts . . .

Objection 7, cont. Yes, of course. It is you who are wasting paper. If you had let me continue, I was about to say that the quantum logician merely insists that informal arguments concerning the quantum mechanical level are not to be represented by classical calculi. And here it is not speakers' intentions that decide, but rather all the complex theoretical factors that Putnam has emphasized. To insist that classical formalisms apply because, by convention, they were set up to apply to everything is to overlook the possibility that conventions may not be "compatible with the aims of inquiry." (1969, p. 188).

Answer: This is a formidable attempt to by-pass the meaning-change argument. With it belongs Putnam's remark that "even if we were to develop [a notion of 'change of meaning' refined enough to handle this issue], that would be of interest only to philosophy of linguistics and not the philosophy of logic." (1969, p. 190). But what does it mean to say that classical rules "do not apply" in the domain of quantum mechanics (assuming that such a domain can be successfully demarcated, a question already raised in section 1, above)? As we have already seen, it cannot mean that a classical law, such as distribution, fails to hold for sentences in the language of QM: if the connectives used are QM connectives, no classical law is contravened. On the other hand, if classical connectives are used, then the classical tautologies all hold. "Inapplicability" must be interpreted in some other way.

Perhaps it can be understood as a claim of a pragmatic sort: in order to avoid paradoxical (or unpalatable) results, we should adopt some new stipulations with regard to sentences about quantum phenomena--we should simply withhold the classical connectives and therewith certain classical patterns of inference.

However, this is highly unsatisfactory, as will emerge by considering the following dilemma: either the classical connectives can be used to form meaningful sentences in the language of QM or they cannot. (That is, either that language can be closed classically or it cannot.) In the former case, all the deductions leading to paradoxical conclusions can be written down: in a perfectly clear sense, they are there, side by side with their QL "resolutions", and nothing has been accomplished. Surely, it is irrelevant whether any human logician has actually written down the classical deductions. So imagine them, if you like, on the left side of a large page, with their QL "resolutions" off to the right. Then the above pragmatic interpretation amounts to nothing more than refusing to look to the left! That cannot change the logical facts, and is, by the way, completely at odds with Putnam's earlier logical realism.

We are left then with the second alternative: the language of QM is not closed under the classical connectives. Can this be understood "pragmatically", as the claim that our aims of inquiry will be better pursued by not using classical compounds of QM sentences? This cannot be sufficient because, if those compounds could be used meaningfully, that is, if they are meaningful, the very same travesty we just encountered can be presented again. Adhering to the pragmatic claim would merely mean that our "aims of inquiry" had taken on aspects of the ostrich. We must, if I am right, construe the claim as a semantic one: somehow, classical compounds of QM sentences are not meaningful.<sup>11</sup>

But how can this be? I claim that, if the component QM (atomic) sentences (of the form, "The value of magnitude A of system S in state  $\Psi_t$  lies in Borel set B") are meaningful, and are interpreted realistically as either true or false,<sup>12</sup> then the classical compounds obtained by applying the classical truth-functional connectives are also meaningful. This is simply because the truth-conditions for those compounds are determinate functions of the truth-conditions of the components. For mere cognitive significance, what more could one ask?

Objector: We can ask for operational significance. As Putnam, drawing on work of Finkelstein (1969), has argued, we must change our logic "if we seek to preserve the (approximate) 'operational meaning' that the logical connectives always had." (1969, pp. 196-7).

Answer: To this there are two replies, one technical, the other philosophical. The technical reply concerns Putnam's claim, "if there is any test at all (even 'idealizing' as we have been) which corresponds to the disjunction  $P \vee Q$ , it must have the property of being a least upper bound on  $T_p$  and  $T_q$ " (*ibid.*), where  $T_p$  and  $T_q$  are, respectively, tests for the propositions P and Q. Then "Finkelstein's result" is invoked to conclude that "the only tests which have this property are the ones which are equivalent to the test T corresponding to the subspace spanned by  $S_p$  and  $S_q$ " (*ibid.*), where  $S_p$  and  $S_q$  are, respectively, the subspaces of the Hilbert space corresponding to P and Q. Similar considerations for the other connectives lead "directly to quantum logic, not classical logic." (*ibid.*). However, Putnam's argument that any test for the disjunction  $P \vee Q$  must be a least upper bound on  $T_p$  and  $T_q$  is curious: it relies crucially on a classical interpretation of ' $\vee$ ' in moving from the assumption, for arbitrary test T', that "the things with property P are a subset of the things that pass T' and the things with property Q are a subset of the things that pass T'" to the conclusion, "the things with  $P \vee Q$  are a subset of the things which pass T'" (*ibid.*). If ' $P \vee Q$ ' is understood quantum mechanically--and the whole point of this passage (as we can now put it) is that it must be--this move is simply a non-sequitur. (Something may lie in the span of P, Q without having either property P or property Q--the distinguishing feature of QM 'or', as Piron notes (1976 p, 21).) But without this move, Putnam has not established the leastness of the bound, and therefore has not established the correspondence sought between the structure of operational tests and that of the subspaces of Hilbert space.<sup>13</sup>

Restating the matter thus brings us to the second and the main point. For we now can see, in light of the answer to Objection 7, that the demand for operational tests is really verificationism all over again.<sup>14</sup> Because "there is no test" for  $P \vee Q$  with 'v' interpreted classically, we must declare the sentence devoid of truth-value or significance entirely. On the other hand, if we interpret 'v' quantum mechanically, we do have an operational test, hence a meaningful sentence. But if we are prepared to be verificationists with respect to logic, why not (and must we not?) with respect to the elementary statements of quantum mechanics, as our friends from Denmark have been insisting for decades! It's cheaper (one can retain classical logic), and it's been around for years.

The applause from Copenhagen is now so loud that I can hear it from my study in Bloomington: in a courageous effort to save a realistic interpretation of QM, the real quantum logician has been driven to the same discredited position on meaning that he sought to escape and that formed the philosophical cornerstone of the anti-realist interpretation that Bohr bequeathed to physicists generations ago.

Objection 8: ?

#### Notes

<sup>1</sup>This material is in part based upon work supported by the National Science Foundation under Grant No. SES-7924874. I am grateful to Linda Wessels, J. Michael Dunn, J. Alberto Coffa, David Malament, and Richard Healey for helpful discussion on the issues of this paper, and to Bas van Fraassen, Nuel Belnap, and Hilary Putnam for thought-provoking correspondence.

<sup>2</sup>In fact, however, as Friedman and Glymour (1972) and Bub (1979, pp. 36-37), have pointed out, moving to a bivalent, non-distributive quantum logic in no way reconciles the conflict that, on rather plausible assumptions, exists between the precise-values principle and the main theorem of Kochen and Specker (1967). For a rigorous derivation of the conflict, cf., Healey (1977, Appendix 4).

<sup>3</sup>For discussion of some of these, especially treatment of the so-called "paradoxes," see Gardner (1971). For a recent argument that the quantum logical interpretation avoids the projection postulate, see Friedman and Putnam (1978); for criticism of this, see Hellman (Unpublished Manuscript).

<sup>4</sup>It should be pointed out that nothing in this principle prevents us from employing other notions of meaning according to which, for instance, distinct occurrences of an indexical expression such as 'I' share meaning but not reference. In fact, for our purposes here, "meaning" could in principle be dropped in favor of truth conditions and logical compatibility. In these terms, a slightly more refined version of MDT would require subscribing occurrences of sentential connectives and recognizing that there is no logical incompatibility

(on anyone's logic!) between e.g.,

(i)  $p \wedge_{c1} (q \vee_{c1} r)$  implies  $(p \wedge_{c1} q) \vee_{c1} (p \wedge_{c1} r)$

and

(ii)  $p \wedge_{qm} (q \vee_{qm} r)$  does not imply  $(p \wedge_{qm} q) \vee_{qm} (p \wedge_{qm} r)$ .

<sup>5</sup>In the present context, this is no restriction at all, since integral to the real-logic view in question is the procedure of reading off logical relations from the Hilbert space structure. (Cf., Putnam (1969) and McGrath (1978).) It should be mentioned that justifying the claim of isomorphism between propositions and subspaces is problematic. Some have simply stipulated it (Mackey (1963)), while others have gone to some lengths to prove it (Piron (1976)).

<sup>6</sup>This point and its connection with "meaning change" have been clearly recognized by Fine (1972, sec. 4). What is being added here is, first, to show how non-truth-functionality of both  $\mathcal{Q}M$  ' $\vee$ ' and ' $\wedge$ ' follows from the preferred minimal semantical rules for  $\mathcal{Q}L$  (of Friedman and Glymour (1972)), and, second, to emphasize that only purely mathematical reasoning that any preservationist  $\mathcal{Q}L$  program must respect is needed for this demonstration, so that, from within  $\mathcal{Q}L$ , two distinct types of connectives must be discerned.

<sup>7</sup>It follows from this consistency result that our argument on the non-truth-functionality of  $\mathcal{Q}M$  ' $\vee$ ' and ' $\wedge$ ' does not run afoul of the Kochen-Specker theorem or the theorem of Gleason (cf., Kochen and Specker (1967) and Bell (1966)). This is also immediate from the observation that our argument need involve only two orthogonal triples (working in  $H^3$ ) pertaining to the same or even different systems at two distinct times, whereas at least three pertaining to a single system at a time are required to generate a contradiction via Gleason's theorem. (Cf., Belinfante (1973), Appendix B.) (I am indebted to David Malament for pointing out the need to consider this aspect of the problem and to specify precise rules governing truth-assignments.)

In fact, as Johan van Benthem has pointed out, an admissible truth-assignment that assigns 1 to the whole space  $H$  and respects truth-functionality of ' $\wedge$ ' and ' $\vee$ ' is a homomorphism from  $\mathcal{A}(H) \rightarrow 2$ . For dimensions  $\geq 3$ , therefore, the non-truth-functionality theorem is a consequence of the Kochen-Specker theorem. Of course, the latter is stronger in that it shows the inexistence of full, classical truth-assignments respecting truth-functionality of ' $\wedge$ ' and ' $\vee$ ' with respect to just compounds of compatible proposition, whereas our proof relies on some compounds of incompatible propositions. Thus, even if the quantum logician holds such compounds to be meaningless or otherwise illegitimate, the non-truth-functionality of his connectives is still a mathematical fact.

For finite dimensional  $H$ , our assumption that an admissible truth-assignment assigns 1 to a 1-dimensional subspace can be dropped in

favor of the condition that  $H$  gets the value 1. Thus, for finite-dimensional  $H$ , we have in effect given a simple derivation of the original von Neumann no hidden-variable proof (which works for  $\dim(H)=2$ ). For the infinite dimensional case, we need the extra assumption that some finite-dimensional subspace gets assigned 1.

<sup>8</sup>Objections 2 and 4 were suggested in conversation by J. Michael Dunn. They do not, however, represent his final view.

<sup>9</sup>This must not be confused with truth-functionality, in which respect QM ' $\wedge$ ' is not "classical," as was shown above. What can be shown is that truth-functionality breaks down only in the first row of the truth-table for ' $\wedge$ ': since  $p \wedge q \leq p$  and  $p \wedge q \leq q$ , whenever one (or both) of  $p, q$  is F, so is  $p \wedge q$ .

<sup>10</sup>Throughout this section, I am being charitable in supposing that ' $T^1$ ' makes sense in general, and not just in cases where there is a nice 1-1 correspondence between propositions and subspaces of a vector space. Or perhaps with enough ingenuity we could devise such a correspondence for everything that we have to say!(?)

<sup>11</sup>This appears to be the direction in which Putnam has moved. In a recent article (with Michael Friedman, (1978), it is maintained that "incompatible [in the QM sense] propositions can be true simultaneously, but their propositional combinations simply don't exist." (p. 311). Although the distinction between classical and QM compounds is presumably not officially recognized, let us see what happens if this remark is understood to cover classical compounds. In one place, at least, the distinction between classical and QM connectives is clearly recognized--and it is not just typographical. (Cf., p. 312, where an inference is said to depend on 'or' being "classical".) In what sense do these compounds "not exist"? It is never made very clear. The authors do assure us, however, that they "will refrain from asserting propositional combinations of incompatible propositions" and that they also "will not simultaneously assert individual incompatible propositions." (p. 312). Now comes a curious appeal to Gleason's theorem, which purports to explain this "failure of simultaneous assertability": first, they say, "idealize assertion as the assignment of probability 1." (Presumably, the assignment need only be meaningful, not necessarily correct. But wait!) Next they remind us that Gleason's theorem states that all probability measures on the partial algebra of projectors are generated by the QM (pure and mixed) states. From this "it follows that [totally] incompatible propositions cannot simultaneously have probability 1." (*ibid.*). Here, however, "having probability 1" means "truly having"--no theorem that I know of prevents us from (perhaps inconsistently) assigning probability 1 to whatever we please. In fact, it is difficult to understand the import of Gleason's theorem to which attention is being drawn if it is not meaningful but false to assert of incompatible propositions that they both simultaneously have probability 1! If this is not equivocation, then there is a contradiction: for then "non-assertable" comes down to "not truly assertable", and certain compound propositions (including classical conjunctions)

are said "not to exist" in a new but innocent sense: they are demonstrably false. This, however, contradicts the claim with which we started, that, in some cases, as required by "realism", they are true.

Surely there is no basis in Gleason's theorem for denying classical closure of the QM language. Perhaps there is a basis for saying that (totally) incompatible propositions cannot be true simultaneously.

Finally, note that, implicitly, the non-truth-functionality of QM ' $\wedge$ ' has been granted in these quoted remarks: there can be incompatible  $P, Q$ , each true simultaneously, but such that  $P \wedge Q$  "does not exist."

<sup>12</sup>If such sentences are not realistically interpreted, then the most natural alternative would involve relativizing to experimental arrangements. The whole point of the QL interpretation was to get away from this. (Cf., Friedman and Putnam (1978).)

<sup>13</sup>If ' $v$ ' is understood classically in Putnam's argument, then his reasoning is correct. However, in that case we know there cannot be the correspondence sought between tests and subspaces, as a test for  $P \vee Q$  would then correspond with the union rather than the span of  $P$  and  $Q$ . None of this should come as a surprise. For how could one hope to prove in such a manner that no "physically possible" test (as opposed to "currently available" test) could filter out superpositions without filtering out their components?

<sup>14</sup>From here on, I am no longer relying on what Putnam (or anyone else) has said. Certainly he did not see his own position in this light. (Cf., (1969, pp. 192, 197).) This is not surprising, since he did not think he had to take the meaning-change argument very seriously. But I think what I am saying flows quite inevitably from the debate developed here. For the meaning-change argument drives a wedge between the classical and the QM connectives. That accomplished, the distinction between conflicting with classical laws and doing something else (using different connectives) is no longer blurred. At this point the question of classical closure of the QM language must be faced.

To put the matter another way: the real quantum logician, in contrast to the benign algebraist, contends that the lattice of projections on the given Hilbert space  $H$  represents all logically possible properties of the quantum system described by  $H$ . The meaning-change argument has the effect of forcing confrontation of the (embarrassing) question, "How could ' $P$  or  $Q$ ' not be logically possible given that both  $P$  and  $Q$  are, since whenever  $P$  is true of a system, so automatically is ' $P$  or  $Q$ ', any  $Q$ ?"

Note, by the way, that the form of verificationism to which our QM logician is naturally led is quite strong, due to the stringent notion of 'test' involved. In a weaker sense, we automatically verify ' $P \vee Q$ ' (classical ' $v$ ') whenever we verify  $P$  or  $Q$  separately. But this, as we have seen, is not enough for the QM logician, who must rule out classical semantic closure of the QM language. For that, the requisite

notion of test is one that never filters out too little as well as too much. This comes perilously close to early standards of "conclusive verification" that the positivists themselves were later to abandon. (This was pointed out to me by J. Alberto Coffa.)

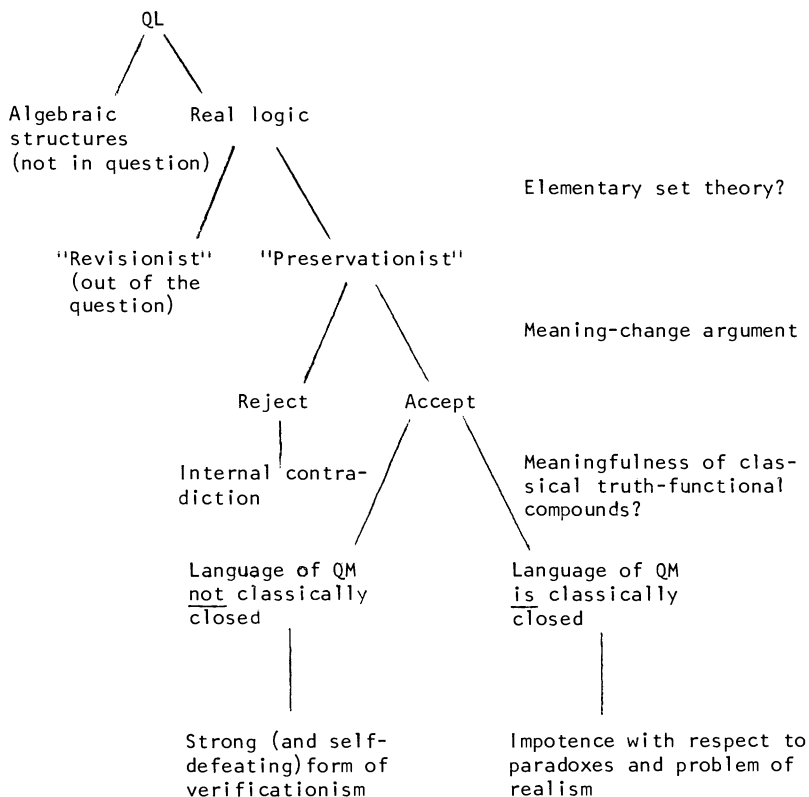
Finally, note that the situation is even worse: from a verificationist standpoint, the real quantum logic program can't even get off the ground. That is because, from such a perspective, there seems to be no satisfactory way of assigning meaning to the meet of an arbitrary set of questions (problems arise especially for infinite sets, even if all are pairwise compatible). But without arbitrary meets well-defined there is no way of establishing isomorphism with the lattice of subspaces of a Hilbert space and hence no basis for reading off QM logical relations from such a structure.

In sum, the possibility of real quantum logic requires a strong verification theory of meaning, and such a theory of meaning precludes real quantum logic. This completes the reductio.



Appendix

The argument of this paper may be diagrammed as follows:



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