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The logic of quantum mechanics*

I want to begin by considering a case in which 'necessary' truths (or rather 'truths') turned out to be falsehoods: the case of Euclidean geometry. I then want to raise the question: could some of the 'necessary truths' of logic ever turn out to be false *for empirical reasons*? I shall argue that the answer to this question is in the affirmative, and that logic is, in a certain sense, a natural science.

I. The overthrow of Euclidean geometry

Consider the following assertion (see Figure 1): two straight lines AB and CD are alleged to come in from 'left infinity' in such a way that, to the left of EF, their distance apart is constant (or, at any rate, 'constant on the average'), while after crossing EF they begin to converge – i.e. their distance apart diminishes – without its being the case that they bend at E or F (i.e. they are really straight, not just 'piecewise straight').

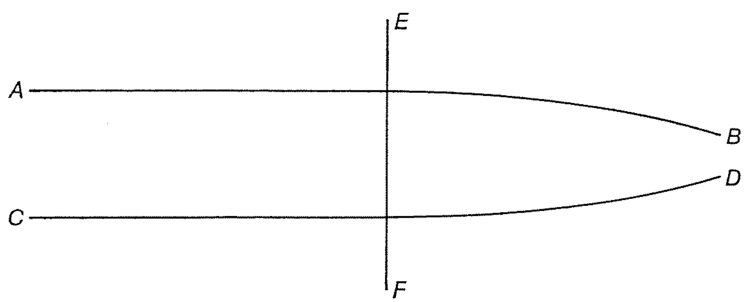

Figure 1

Is it not 'intuitively clear' that this is a contradiction? If one did not know anything about non-Euclidean geometry, relativity, etc., would the intuitive evidence that this is a contradiction, an impossibility, a complete absurdity, etc., be any less than the intuitive evidence that no

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surface can be scarlet (all over) and bright green at the same time? Or that no bachelor is married? Is the intuitive 'feeling' of contradiction really different in the following three cases?

(1) Someone claims that AB and CD are both *straight lines*, notwithstanding their anomalous behavior.

(2) Someone claims that he has a sheet of paper which is scarlet (all over, on both sides) and green (all over, on both sides) at once.

(3) Someone claims that some men are married (legally, at the present time) and nonetheless still *bachelors*.

It seems to me that it is not. Of course, (1) does not involve a 'contradiction' in the technical sense of formal logic (e.g. '$p \cdot -p$'); but then neither does (2), nor, unless we stipulate the definition 'Bachelor = man who has never married', does (3). But then 'contradiction' is often employed in a wide sense, in which not all contradictions are of the form '$p \cdot -p$', or reducible to that form by logic alone.

The important thing, for present purposes, is this: according to General Relativity theory, the claim mentioned in (1) is *possible*, not *impossible*. AB and CD could both be *straight paths* in the strict sense: that is, for no P, P' on AB (or on CD) is there a shorter way to travel from P to P' than by sticking to the path AB (respectively, CD). If we are willing to take 'shortest distance between two points' as the defining property of straight lines, then AB and CD could both be straight lines (in technical language: *geodesics*), notwithstanding their anomalous behavior.

To see this, assuming only the barest smattering of relativity: assume space is Euclidean 'in the large' – i.e. the average curvature of space is zero. (This is consistent with the General Theory of Relativity.) Then two geodesics could well come in from 'left infinity' a constant distance apart on the average ('on the the average' mind you! – and I am speaking about straight lines!). Suppose these two geodesics – they might be the paths of two light rays approaching the sun on opposite sides† – enter the gravitational field of the sun as they cross EF. Then, according to GTR, the geodesics – not just the light, but the very geodesics, whether light is actually travelling along them or not – would behave as shown in Figure 1.

Conclusion: what was yesterday's 'evident' impossibility is today's possibility (and even *actuality* – things as 'bad' as this actually happen,

† The physics of this example is deliberately oversimplified. In the GTR it is the *four-dimensional* 'path' of the light-ray that is a geodesic. To speak of (local) 'three' dimensional space' presupposes that a local reference system has been chosen. But even the geodesics in three-dimensional space exhibit non-Euclidean behavior of the kind described.

according to the GTR – indeed, if the average curvature of space is *not* zero, then 'worse' things happen!)

If this is right, and I believe it *is* right, then this is surely a matter of some philosophical importance. The whole category of 'necessary truth' is called into question. Perhaps it is for this reason that philosophers have been driven to such peculiar accounts of the foundations of the GTR: that they could not believe that the obvious account, the account according to which 'conceptual revolutions' can overthrow even 'necessary truth', could possibly be correct. But it *is* correct, and it is high time we began to live with this fact.

II. Some unsuccessful attempts to dismiss the foregoing

One way to dismiss the foregoing is to deny that 'straight line' ever meant 'geodesic'. But then one is forced to give up 'a straight line is the shortest path', which was surely a 'necessary truth' too, in its day. Moreover, if the geodesics are not the 'straight' paths in space, then which paths in space are 'straight'? Or will one abandon the principle that there are straight paths?

Again, one might try the claim that 'distance' (and hence shortest path') has 'changed its meaning'. But then what is the 'distance' between, say, A and B *in the old sense*? And what path between A and B is *shorter* than AB *even in the old sense*? No matter what path one may select (other than AB) as the 'really' straight one (i.e. the one that was 'straight' before the alleged 'meaning change'), it will turn out that one's path AGB will not *look* straight *when one is on it*, will not *feel* straight, as one travels along it, and will measure *longer*, not shorter, than AB by any conventional method of measurement. Moreover, there will be no nonarbitrary ground for preferring AGB over $AG'B$, $AG''B$ (Figure 2) ... as the 'really straight' path from A to B.

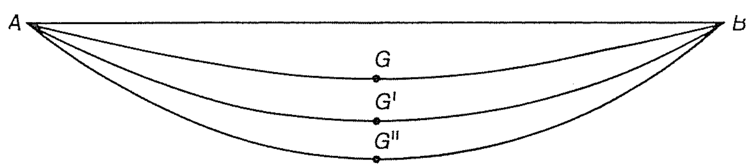

Figure 2

This brings out, indeed, one of the important facts about a conceptual revolution: such a revolution cannot successfully be dismissed as a mere case of *relabeling*. It is easy to see why not. The old scientific use of the

term 'straight line' rested on a large number of laws. Some of these laws were laws of pure geometry, namely, Euclid's. Others were principles from physics, e.g. 'light travels in straight lines', 'a stretched thread will lie in a straight line', etc. What happened, in a nutshell, was that this cluster of laws came apart. If there are any paths that obey the pure geometrical laws (call them 'E-paths'), they do not obey the principles from physics, and the paths that do obey the principles from physics – the geodesics – do not obey the old principles of pure geometry. In such a situation, it scarcely makes sense to ask 'what paths are straight in the old sense'? We cannot say that the E-paths are straight, because they are not *unique*; there is no *one* way of picking out paths in space and calling them 'straight' which preserves Euclidean geometry: either there is *no* way of doing this, or there are infinitely many. We can say that the geodesics are straight, because they at least obey what were always recognized to be the operational constraints on the notion of a 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. (Or, perhaps, that what answered to the expression were not-too-long *pieces* of geodesics; that the old notion of 'straight line' cannot successfully be applied 'in the large'.) But in neither case is it correct to say: it is just a matter of shifting the 'label' 'straight line' from one set of paths to another. A good maxim in this connection would be: *Do not seek to have a revolution and minimize it too.*

On the other hand, one is not committed by the foregoing to denying that 'straight line' changes its meaning as one goes over to the GTR. Perhaps one wants to say it does, perhaps one does not want to say this. That is a question about the best way to use the expression 'change of meaning' in a very difficult class of cases. The important thing is that it does not 'change its meaning' in the trivial way that one might at first suspect. Once one appreciates that something that was formerly literally unimaginable has indeed happened, then one also appreciates that the usual 'linguistic' moves only help to distort the nature of the discovery, and not to clarify it.

III. The logic of quantum mechanics

In classical physics, the state of a system S consisting of N particles $o_1, \ldots, o_N$ is specified by giving the $3N$ position coordinates and the $3N$ momentum coordinates. Any reasonably well-behaved mathematical function of these $6N$ quantities represents a possible physical 'magnitude' $m(s)$. Statements of the form $m(s) = r$ – 'the magnitude m has the value r in the system S' – are the sorts of statements we shall call *basic physical propositions* here.

The basic mathematical idea of *quantum mechanics* is this: a certain infinite dimensional vector space $H(s)$ is coordinated to each physical system S, and each basic physical proposition is coordinated to a subspace of the vector space. In the case of what is called a 'nondegenerate' magnitude $m(s)$, the subspace corresponding to the basic physical proposition $m(s) = r$ is a one-dimensional one, say V_r, and the one-dimensional subspaces V_r 'span' the whole space. The reader may easily picture this situation as follows: pretend the space $H(s)$ is a finite dimensional space. (Indeed, nothing is lost if we pretend for now that all physical magnitudes have finitely many values, instead of continuously many, and in such a world the space $H(s)$ would be just an ordinary finite dimensional space.) Then to each number r which is a possible value of such a physical magnitude as position (remember that we are pretending that there are only *finitely* many such!), there corresponds a single *dimension* of the space $H(s)$ – i.e. V_r is simply a *straight line* in the space – and the lines V_r corresponding to all the possible values of, say, position form a possible coordinate system for the space. (In fact, they meet at right angles, as good coordinates should.) If we change from one physical magnitude $m_1(s)$ to a different magnitude $m_2(s)$ (say, from *position* to *momentum*) then the new coordinates V'_r will be inclined at an angle to the old, and will not coincide with the old. But each possible momentum r will correspond to a straight line V'_r, though not to a straight line which coincides with any one of the lines V_r corresponding to a possible *position* r.

So far we have said that there exists a 'prescription' for coordinating basic physical propositions to subspaces of the space $H(s)$. This mapping can be extended to compound statements by the following rules (here, let $S(p)$ be the space corresponding to a proposition p):

$$S(p \vee q) = \text{the } \textit{span} \text{ of the spaces } S(p) \text{ and } S(q), \quad (1)$$
$$S(pq) = \text{the } \textit{intersection} \text{ of the spaces } S(p) \text{ and } S(q), \quad (2)$$
$$S(-p) = \text{the orthocomplement of } S(p). \quad (3)$$

These rules can also be extended to quantifiers, since, as is well known, the existential quantifier works like an extended disjunction and the universal quantifier works like an extended conjunction.

These rules, however, *conflict with classical logic*. To see this, let $r_1, r_2, \ldots, r_n$ be all the possible values of some 'non-degenerate' magnitude. Then the straight lines $V_{r_1}, \ldots, V_{r_n}$ 'span' the whole space $H(s)$ – i.e. the number n is also the number of dimensions of $H(s)$, and any vector in the space $H(s)$ can be expressed as a combination of vectors

in these directions V_{r_i}. Since the 'span' of $V_{r_1}, \ldots, V_{r_n}$ is the whole space, the statement:

$$m(s) = r_1 \vee m(s) = r_2 \vee \cdots \vee m(s) = r_n \quad (1)$$

(where m is the magnitude in question) is an always-true statement.

Now let m' be any magnitude such that the straight line V'_r representing the statement $m'(s) = r$ (where r is some real number) does not coincide with any of the n lines $V_{r_1}, V_{r_2}, \ldots, V_{r_n}$ (such a magnitude can always be found). The statement:

$$m'(s) = r \cdot [m(s) = r_1 \vee \cdots \vee m(s) = r_n] \quad (2)$$

corresponds to the intersection of V'_r with the whole space, and this is just V'_r. Thus (2) is equivalent to:

$$m'(s) = r. \quad (3)$$

On the other hand, consider the disjunction

$$[m'(s) = r \cdot m(s) = r_1] \vee [m'(s) = r \cdot m(s) = r_2] \vee \cdots \vee [m'(s) = r \cdot m(s) = r_n] \quad (4)$$

Each term in this disjunction corresponds to the 0-dimensional subspace (the origin), which we consider to represent the always-false proposition. For a typical term $[m'(s) = r \cdot m(s) = r_i]$ corresponds to the intersection of the two one-dimensional subspaces V'_r and V_{r_i}, and this is just the origin. Thus (4) is the space spanned by n 0-dimensional subspaces, and this is just the 0-dimensional subspace. So two propositions which are *equivalent* according to classical logic, viz. (2) and (4), are mapped onto different subspaces of the space $H(s)$ representing all possible physical propositions about S. *Conclusion:* the mapping is nonsense – *or, we must change our logic.*

Suppose we are willing to adopt the heroic course of changing our logic. What then?

It turns out that there is a very natural way of doing this. Namely, *just read the logic off from the Hilbert space $H(S)$.* Two propositions are to be considered equivalent just in case they are mapped onto the same subspace of $H(S)$, and a proposition P_1 is to be considered as 'implying' a proposition P_2 just in case S_{P_1} is a subspace of S_{P_2}. This idea was first advanced by Birkhoff and von Neumann some years ago, and has been recently revived by a number of physicists. Perhaps the first physicist to fully appreciate that *all* so-called 'anomalies' in quantum mechanics come down to the *non-standardness of the logic* is David Finkelstein of Yeshiva University, who has defended this interpretation in a number of lectures.

IV. The peculiarities of quantum mechanics

It is instructive to examine some of the peculiarities of quantum mechanics in the light of the foregoing proposal. Consider, first of all, *complementarity*. Let S be a system consisting of a single electron with a definite position r (strictly speaking, position is a *vector* and not a real number, but we may consider a one-dimensional world). The subspace of $H(S)$ representing the statement 'the position of S is r' is, as already remarked, a one-dimensional subspace V_r. Let V'_r represent the statement 'the momentum of S is r'', where r' is any real number. Then the intersection $V_r \cap V'_r$ is o-dimensional; thus we have:

For all r, r', the statement: (*the electron has position r. the electron has momentum r'*) is logically false. (1)

Or, in ordinary language 'an electron cannot have an exact position and an exact momentum at the same time'.

All cases of 'complementarity' are of this kind: they correspond to *logical incompatibilities* in quantum logic.

Next, consider the famous 'two-slit' experiment. Let A_1 be the statement 'the photon passes through slit 1' and let A_2 be the statement 'the photon passes through slit 2'. Let the probability that the photon hits a tiny region R on the photographic plate on the assumption A_1 be $P(A_1, R)$ and let the probability of hitting R on the assumption A_2 be $P(A_{,2}\, R)$. These probabilities may be computed from quantum mechanics or classical mechanics (they are the same, as long as only one slit is open), and checked experimentally by closing one slit, and leaving only A_1 (respectively A_2) open in the case of $P(A_1, R)$ (respectively, $P(A_2, R)$).

Now, it is often said that if both slits are open the probability that the particle hits R 'should be' $\frac{1}{2}P(A_1, R)+\frac{1}{2}P(A_2, R)$. This is the probability predicted by *classical* mechanics. However, it is *not* the *observed* probability (which is *correctly* predicted by *quantum* mechanics, and *not* by classical mechanics). How was this 'classical' prediction arrived at?

First of all, the probability that the photon hits the slit A_1 = the probability that it hits A_2. This can be tested experimentally, and also insured theoretically by symmetrizing the apparatus, rotating the apparatus periodically, etc. Since we count only experiments in which the photon gets through the barrier, and hence in which the disjunction $A_1 \vee A_2$ is true, we have

$$\begin{aligned} P(A_1 \vee A_2, R) &= P[(A_1 \vee A_2)\cdot R]/P(A_1 \vee A_2) \\ &= P(A_1\cdot R \vee A_2\cdot R)/P(A_1 \vee A_2) \\ &= P(A_1\cdot R)/P(A_1 \vee A_2)+P(A_2\cdot R)/P(A_1 \vee A_2) \end{aligned}$$

(here '$P[(A_1 \vee A_2)\cdot R]$' means 'the probability of $(A_1 \vee A_2)\cdot R$', and similarly for '$P(A_1 \vee A_2)$', etc.

Since $$P(A_1) = P(A_2),$$

we have $$P(A_1 \vee A_2) = 2P(A_1) = 2P(A_2).$$

Thus $$P(A_1\cdot R)/P(A_1 \vee A_2) = P(A_1\cdot R)/2P(A_1) = \tfrac{1}{2}P(A_1, R)$$

and similarly

$$P(A_2\cdot R)/P(A_1 \vee A_2) = P(A_2\cdot R)/2P(A_2) = \tfrac{1}{2}P(A_2, R).$$

Substituting these expressions in the above equation yields:

$$P(A_1 \vee A_2, R) = \tfrac{1}{2}P(A_1, R)+\tfrac{1}{2}P(A_2, R). \qquad (2)$$

Now a crucial step in this derivation was the expansion of $(A_1 \vee A_2)\cdot R$ into $A_1\cdot R \vee A_2\cdot R$. This expansion is *fallacious* in quantum logic; thus the above derivation also fails. Someone who believes classical logic must conclude from the failure of the classical law that one photon can somehow go through two slits (which would invalidate the above deduction, which relied at many points on the incompatibility of A_1 and A_2), or believe that the electron somehow 'prefers' one slit to the other (but only when no detector is placed in the slit to detect this mysterious preference), or believe that in some strange way the electron going through slit 1 'knows' that slit 2 is open and behaves differently than it would if slit 2 were closed; while someone who believes quantum logic would see no reason to predict $P(A_1 \vee A_2, R) = \frac{1}{2}P(A_1, R)+\frac{1}{2}P(A_2, R)$ in the first place.

For another example, imagine a population P consisting of hydrogen atoms, all at the same energy level $\mathbf{e}$. Let D be the relative distance between the proton and the electron, and let E be the magnitude 'energy'. Then we are assuming that E has the same value, namely $\mathbf{e}$, in the case of every atom belonging to P, whereas D may have different values $d_1, d_2, \ldots$ in the case of different atoms $A_1, A_2, \ldots$

The atom is, of course, a system consisting of two parts – the electron and the proton – and the proton exerts a central force on the electron. As an analogy, one may think of the proton as the earth and of the electron as a satellite in orbit around the earth. The satellite has a potential energy that depends upon its height above the earth, and that can be recovered as usable energy if the satellite is made to fall. It is clear from the analogy that this potential energy P associated with the electron (the satellite), can become large if the distance D is allowed to become sufficiently great. However, P cannot be greater than E (the total energy). So if E is known, as in the present case, we can compute a number d

such that D cannot exceed d, because if it did, P would exceed **e** (and hence P would be greater than E, which is absurd). Let us imagine a sphere with radius d and whose center is the proton. Then if all that we know about the particular hydrogen atom is that its energy E has the value **e**, we can still say that wherever the electron may be, it cannot be outside the sphere. The boundary of the sphere is a 'potential barrier' that the electron is unable to pass.

All this is correct in classical physics. In quantum physics we get

Every atom in the population has the energy level **e** (3)

and we may *also* get

10% of the atoms in the population P have values of D which *exceed d.* (4)

The 'resolution' of this paradox favored by many people is as follows. They claim that there is a mysterious 'disturbance by the measurement', and that (3) and (4) refer to values *after measurement*. Thus, in this view, (3) and (4) should be replaced by

If an energy measurement is made on any atom in P, then the value **e** is obtained, (3′)

and

If a D-measurement is made on any atom in P, then in 10% of the cases a value greater than d will be obtained. (4′)

These statements are consistent in view of

An E-measurement and a D-measurement cannot be performed at the same time (i.e. the experimental arrangements are mutually incompatible). (5)

Moreover, we do not have to accept (3′) and (4′) simply on the authority of quantum mechanics. These statements can easily be checked, not, indeed, by performing both a D-measurement and an E-measurement on each atom (that is impossible, in view of (5)), but by performing a D-measurement on every atom in one large, fair sample selected from P to check (3′) and an E-measurement on every atom in a different large, fair sample from P to check (4′). So (3′) and (4′) are both known to be true.

In view of (3′), it is natural to say of the atoms in P that they all 'have the energy level **e**'. But what (4′) indicates is that, paradoxically, some of the electrons will be found on the wrong side of the potential barrier. They have, so to speak, 'passed through' the potential barrier. In fact,

quantum mechanics predicts that the distance D will assume arbitrarily large values even for a fixed energy **e**.

The trouble with the above 'resolution' of this paradox is twofold. In the first place, if distance measurement, or energy measurement (or both) disturb the very magnitude that they seek to measure, then there should be some *theory* of this disturbance. Such a theory is notoriously lacking, and it has been erected into an article of faith in the state of Denmark that there can be no such theory. Secondly, if a procedure distorts the very thing it seeks to measure, it is peculiar it should be accepted as a good measurement, and fantastic that a relatively simple theory should predict the *disturbed* values when it can say nothing about the *undisturbed* values.

The resolution provided by quantum logic is quite straightforward. The statement

(Such-and-such electrons in P [some specific 10%] have a D-value in excess of d). The energy level of every electron in P is **e**

is logically false in *both* classical and quantum logic.

Let $S_1, S_2, \ldots, S_R$ be all the statements of the form 'such-and-such electrons in P [some specific 10%] have a D-value in excess of d', i.e. let R be the number of subsets of P of size 10%, and let there be one S_i for each such subset. Then what was just said can be rephrased thus: for each *fixed i*, $i = 1, 2, \ldots, R$ the statement below is logically false:

$$S_i \cdot E = \mathbf{e}. \qquad (6)$$

Also, in *both* classical and quantum logic, the following statement is likewise false:

$$S_1 \cdot (E = \mathbf{e}) \vee S_2 \cdot (E = \mathbf{e}) \vee \cdots \vee S_R \cdot (E = \mathbf{e}) \qquad (7)$$

(since the o-space, any number of times, only spans the o-space).

However,

$$(E = \mathbf{e}) \cdot (S_1 \vee S_2 \vee \cdots \vee S_R) \qquad (8)$$

is *not* logically false in quantum logic! In fact, the subspace of $(E = \mathbf{e})$ is included in the subspace of $(S_1 \vee S_2 \vee \cdots \vee S_R)$, so that (8) is *equivalent* to just

$$E = \mathbf{e} \qquad (9)$$

which has a *consequence*

$$S_1 \vee S_2 \vee \cdots \vee S_R. \qquad (10)$$

In words: the statement (9) (or (3)) is not incompatible with but *implies* the statement (10) (or (4)).

These examples should make the principle clear. The only laws of classical logic that are given up in quantum logic are distributive laws, e.g. $p\cdot(q \vee r) \equiv p\cdot q \vee q\cdot r$; and every single anomaly vanishes once we give these up.

V. The quantum mechanical view of the world

We must now ask: what is the nature of the world if the proposed interpretation of quantum mechanics is the correct one? The answer is both radical and simple. *Logic is as empirical as geometry.* It makes as much sense to speak of 'physical logic' as of 'physical geometry'. We live in a world with a non-classical logic. Certain statements – just the ones we encounter in daily life – *do* obey classical logic, but this is so because the corresponding subspaces of $H(S)$ form a very special lattice under the inclusion relation: a so-called 'Boolean lattice'. 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'.

The world consists of particles, in this view,† and the laws of physics are 'deterministic' in a modified sense. In the classical physics there was (idealizing) *one* proposition P which was true of S and such that every physical proposition about S was implied by P. This P was simply the *complete description of the state of* S. The laws of classical physics say that if such a P is the state of S at t_0, then the state after the lapse of time t will be a certain $f(P)$. This is classical determinism.

In quantum mechanics, let us say that any P whose corresponding S_p is one-dimensional is a *state-description*. Let $S_1, S_2, \ldots, S_R$ be all the possible positions of a one-particle system S, and let $T_1, T_2, \ldots, T_R$ be all the possible momenta. Then

$$S_1 \vee S_2 \vee \cdots \vee S_R \qquad (1)$$

is a valid statement in quantum logic, and so is:

$$T_1 \vee T_2 \vee \cdots \vee T_R \qquad (2)$$

In words:

Some S_i is a true state-description (1′)

and Some T_j is a true state-description. (2′)

† This is so only because we are quantizing a particle theory. If we quantize a field theory, we will say 'the world consists of fields', etc.

However, as we have already noted, the conjunction $S_i \cdot T_j$ is *inconsistent* for all i, j. Thus the notion 'state' must be used with more-than-customary caution if quantum logic is accepted. A system has *no complete description* in quantum mechanics; such a thing is a *logical* impossibility, since it would have to imply one of the S_i, in view of (1), and also have to imply one of the T_j in view of (2). A system has a position-state *and* it has a momentum-state (which is not to say 'it has position r_i and it has momentum r_j', for any r_i, r_j, but to say [(it has position $r_i \vee \cdots \vee$ it has position r_R). (It has momentum $r_1 \vee \cdots \vee$ it has momentum r_R)], as already explained); and a system has many other 'states' besides (one for each 'non-degenerate' magnitude). These are 'states' in the sense of *logically strongest consistent statements*, but not in the sense of 'the statement which implies every true physical proposition about S'.

Once we understand this we understand the notion of 'determinism' appropriate to quantum mechanics; if the state at t_0 is, say, S_z, then quantum mechanics says that after time t has elapsed the state will be $U(S_z)$, where U is a certain 'unitary transformation'; and, similarly, quantum mechanics says that if the state at t_0 is T_j, then the state after t will be $U(T_j)$. So the state at t_0 determines the state after any period of time t, just as in classical physics. *But*, it may happen that I know the state after time t has elapsed to be $U(T_j) = T_z$, say, and I measure not momentum but position. In this case I cannot *predict* (except with probability) what the result will be, because the statement T_j does not imply any value of position. 'Indeterminacy' comes in not because the laws are indeterministic, but because the states themselves, although logically strongest factual statements, do not contain the answers to all physically meaningful questions. This illustrates how the conflict between 'determinacy and indeterminacy' is resolved in quantum mechanics from the standpoint of quantum logic.

Finally, it remains to say how probabilities enter in quantum mechanics under this interpretation. (This has been pointed out by David Finkelstein, whose account I follow.) Suppose I have a system S, and I wish to determine the probability of some magnitude M having a value in a certain interval, given some information T about S. I imagine a system P consisting of a large number N of non-interacting 'copies' of S, all in the same 'state' T. This new system P has a Hilbert space $H(P)$ of its own, i.e. $H(P)$ is a vector space representing all possible physical propositions about P. Let R_p be the statement that $R\%$ of the systems S in P have an M-value in the interval I am interested in. It turns out that, as N is allowed to approach infinity, the subspace corresponding to R_p either contains the subspace corresponding to 'all the "copies" are in state T' 'in the limit' or is orthogonal to it 'in the limit'. In other words, given that all of the systems in P are in state T and that they do not interact, it *follows with almost certainty* that $R\%$ of them have $M(S)$ in the interval or with *almost certainty* that some other percent have $M(S)$ in the interval, if the number of such systems in P is very large. But, if we can say 'if we had sufficiently many identical copies of S, $R\%$ of them would have the property we are interested in', then, on any reasonable theory of probability, we can say 'the probability that S has the property is R'. In short, *probability* (on this view) *enters in quantum mechanics just as it entered in classical physics, via considering large populations.* Whatever problems may remain in the analysis of probability, they have nothing special to do with quantum mechanics.

Lastly, we must say something about 'disturbance by measurement' in this interpretation. If I have a system in 'state' S_z (i.e. 'the position is r_z'), and I make a *momentum* measurement, I must 'disturb' S_z. This is so because whatever result T_j I get is going to be *incompatible* with S_z. Thus, when I get T_j, I will have to say that S_z is *no longer true*; but this is no paradox, since the *momentum* measurement disturbed the *position* even according to *classical* physics. Thus the only 'disturbance' on this interpretation is the classical disturbance; we do not have to adopt the strange view that *position* measurement 'disturbs' (or 'brings into being', etc.) *position*, or that *momentum* measurement disturbs (or 'brings into being', etc.) *momentum*, or anything of that kind.

The 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_z is true, then I know that for *each* T_j the conjunction $S_z \cdot T_j$ is false. It is natural to conclude ('smuggling in' classical logic) that $S_z \cdot (T_1 \vee T_2 \vee \ldots \vee T_R)$ 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 the falsity of $S_z \cdot T_1 \vee S_z \cdot T_2 \vee \ldots \vee S_z \cdot T_R$ to the falsity of $S_z \cdot (T_1 \vee T_2 \vee \ldots \vee T_R)$. This latter statement is *true* (assuming S_z); 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.

At this point let us return to the question of 'determinism' for the last time. Suppose I know a 'logically strongest factual statement' about S at t_0, and I deduce a similar statement about S after time t has elapsed

– say, S_3. Then I measure *momentum*. Why can I not predict the outcome? We already said: 'because S_3 does not *imply* T_j for any j'. But a stronger statement is true: S_3 is incompatible with T_j, for all j! But it does *not* follow that S_3 is incompatible with $(T_1 \vee T_2 \vee \ldots \vee T_R)$.

Thus it is still true, even assuming S_3, that 'the particle *has* a momentum'; and if I measure I shall find it. However, S_3 cannot tell me what I shall find, because whatever I find will be incompatible with S_3 (which will no longer be true, when I find T_j). *Quantum mechanics is more deterministic than indeterministic in that all inability to predict is due to ignorance.*

Let $U_1, U_2, \ldots, U_R$ be statements about S at t_0 such that 'U_i at t_0' is equivalent to 'T_i after time t has elapsed', for $i = 1, 2, \ldots, R$. Then it can be shown that

$$U_1 \vee U_2 \vee \ldots \vee U_R$$

is logically true – i.e. *there is a statement which is true of S at t_0 from which it follows what the momentum (or whatever) will be after the lapse of time t.* In this sense, my inability to say what momentum S has now is due to 'ignorance' – ignorance of what U_i was true at t_0. However, the situation is not due to *mere* ignorance; for I *could* not know which U_i was true at t_0, given that I knew something that implied that S_3 would be true now, *without knowing a logical contradiction.*

In sum:

(1) For any such question as 'what is the value of $M(S)$ now', where M is a physical magnitude, *there exists* a statement U_i which was true of S at t_0 such that *had I known* U_i was true at t_0, I could have predicted the value of $M(S)$ now; but

(2) It is *logically impossible* to possess a statement U_i which was true of S at t_0 from which one could have predicted the value of *every* magnitude M now.

You can predict any *one* magnitude, if you make an appropriate measurement, but you cannot predict them *all*.

VI. The 'change of meaning' issue

While many philosophers are willing to admit that we could adopt a different logic, frequently this is qualified by the assertion that of course to do so would merely amount to a 'change of language'. How seriously do we have to take this?

The philosophical doctrine most frequently associated with this 'change of language' move is conventionalism. In, say, Carnap's version this amounts to something like this:

(1) There are alleged to be 'rules of language' which *stipulate* that certain sentences are to be true, among them the axioms of logic and mathematics.

(2) Changing our logic and mathematics, if we ever do it, will be just an instance of the general phenomenon of *change of conventions*.

I have criticized this view at length elsewhere, and I shall not repeat the criticism here. Suffice it to say that if there *were* such conventions I do not see how they could be *justified*. To stipulate that certain sentences shall be immune from revision is *irrational* if that stipulation may lead one into serious difficulties, such as having to postulate either mysterious disturbances by the measurement (or to say that the measurement brings what it measures into existence) or 'hidden variables'. Moreover, if our aim is a true description of the world, then we should not adopt arbitrary linguistic stipulations concerning the form of our language unless there is an argument to show that these cannot interfere with that aim. If the rules of classical logic *were* really arbitrary linguistic stipulations (which I do not for a moment believe), then I have no idea how we are supposed to know that these stipulations are compatible with the aims of inquiry. And to say that they are nonarbitrary stipulations, that we are only free to adopt conventions whose *consequences* are consistent (in a non-syntactical sense of 'consequence') is to *presuppose* the notion of 'consequence', and hence of logic. In practice, as Quine has so well put it, the radical thesis that logic is true by language alone quickly becomes replaced by the harmless truism that 'logical truth is truth by virtue of language plus *logic*'. Those who begin by 'explaining' the truth of the principles of logic and mathematics in terms of some such notion as 'rule of language' end by smuggling in a quite old fashioned and unexplained notion of *a prioricity*.

Even if we reject the idea that a language literally has rules stipulating that the axioms of logic are immune from revision, the 'change of meaning' issue may come up in several ways. Perhaps the most sophisticated way in which the issue might be raised is this: it might be suggested that we identify the logical connectives by the logical principles they satisfy. To mean 'or' e.g. a connective must satisfy such principles as: 'p implies p or q' and 'q implies p or q', simply because these formulate the properties that we count as 'the meaning' of 'or'.

Even if this be true, little of interest to the philosophy of logic follows, however. From the fact that 'a language which does not have a word V which obeys such-and-such patterns of inference does not contain the concept *or* (or whatever) in its customary meaning' it does not follow either that a language which is adequate for the purpose of formulating true and significant statements about physical reality *must* contain a

word V which obeys such-and-such patterns of inference, or that it *should* contain a word V which obeys such-and-such patterns of inference. Indeed, it does not even follow that an optimal scientific language *can* contain such a word V; it may be that having such a connective (and 'closing' under it, i.e. stipulating that for all sentences S_1, S_2 of the language there is to be a sentence $S_1 \vee S_2$) commits one to either changing the laws of physics one accepts (e.g. quantum mechanics), or accepting 'anomalies' of the kind we have discussed. If one does not believe (1) that the laws of quantum mechanics are false; nor (2) that there are 'hidden variables'; nor (3) that the mysterious 'cut between the observer and the observed system' exists; one perfectly possible option is this: to *deny* that there are *any* precise and meaningful operations on propositions which have the properties classically attributed to 'and' and 'or'. In other words, instead of arguing: 'classical logic *must* be right; so something is wrong with these features of quantum mechanics' (i.e. with complementarity and superposition of states), one may perfectly well decide 'quantum mechanics may not be right in all details; but complementarity and superposition of states are probably right. If these are right, and classical logic is also right, then either there are hidden variables, or there is a mysterious cut between the observer and the system, or something of that kind. But I think it is *more likely that classical logic is wrong* than that there are either hidden variables, or "cuts between the observer and the system", etc.' Notice that this completely *bypasses* the issue of whether adopting quantum logic is 'changing the meaning' of 'and', 'or', etc. If it is, so much the worse for 'the meaning'.

From the classical point of view, all this is nonsense, of course, since no empirical proposition could literally be *more likely* than that classical logic is right. But from the classical point of view, no empirical proposition could be *more likely* than that straight lines could not behave as depicted in Figure 1. What the classical point of view overlooks is that the *a prioricity* of logic and geometry vanishes as soon as *alternative logics* and *alternative geometries* begin to have serious physical application.

But *is* the adoption of quantum logic a 'change of meaning'? The following principles:

$$p \text{ implies } p \vee q. \tag{1}$$

$$q \text{ implies } p \vee q. \tag{2}$$

$$\text{if } p \text{ implies } r \text{ and } q \text{ implies } r, \text{ then } p \vee q \text{ implies } r. \tag{3}$$

all *hold* in quantum logic, and these seem to be the basic properties of 'or'. Similarly

$$p, q \text{ together imply } p \cdot q. \tag{4}$$

(Moreover, $p \cdot q$ *is the unique proposition that is implied by every proposition that implies both* p *and* q.)

$$p \cdot q \text{ implies } p. \quad (5)$$
$$p \cdot q \text{ implies } q. \quad (6)$$

all *hold* in quantum logic. And for *negation* we have

$$p \text{ and } -p \text{ never both hold. } (p \cdot -p \textit{ is a contradiction}) \quad (7)$$
$$(p \vee -p) \text{ holds.} \quad (8)$$
$$- -p \text{ is equivalent to } p. \quad (9)$$

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

$$p \cdot (q \vee r) \text{ is equivalent to } p \cdot q \vee p \cdot r \text{ (which fails in quantum logic).} \quad (10)$$

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.

My own point of view, to state it summarily, is that we simply do not possess a notion of 'change of meaning' refined enough to handle this issue. Moreover, even if we were to develop one, that would be of interest only to philosophy of *linguistics* and not the philosophy of *logic*.

The important fact to keep in mind, however one may choose to phrase it, is that the whole 'change of meaning' issue is raised by philosophers only to *minimize* a conceptual revolution. But only a demonstration that a certain *kind* of change of meaning is involved, namely, *arbitrary linguistic change*, would successfully demolish the philosophical significance I am claiming for these conceptual revolutions. And *this* kind of 'change of meaning' is certainly *not* what is involved.

VII. The analogy between logic and geometry

It should now be clear that I regard the analogy between the epistemological situation in logic and the epistemological situation in geometry as a perfect one. In the remainder of this essay, I shall try to deal with two points of apparent disanalogy: (1) that geometrical notions such as 'straight line' have a kind of operational meaning that logical operations lack; and (2) that physical geometry is about something 'real', viz. physical space, while there is nothing 'real' in that sense for logic to be 'about'. But it is useful at this point to summarize how far the analogy has already been seen to extend.

We saw at the beginning of this essay that one *could* keep Euclidean geometry, but at a very high cost. If we choose paths which obey Euclid's axioms for 'straight line' in some arbitrary way (and there are infinitely many ways in which this could be done, at least in space *topologically* equivalent to Euclidean space), and we retain the law that $F = ma$, then we must explain the fact that bodies do not follow our 'straight lines', even in the absence of differential forces† by *postulating mysterious forces*. For, if we believe that such forces do not really exist, and that $F = ma$, then we have no choice but to reject Euclidean geometry. Now then, Reichenbach contended that the choice of any metric for physical space is a matter of 'definition'. If this is so, and, if '$F = ma$' is true even when we change the metric (as Reichenbach assumed), then *what forces exist is also a matter of definition*, at least in part. I submit that:

On the *customary* conception of 'force' it is *false* (and *not* a matter of a conventional option) that a body not being acted upon by differential forces is being pushed about by mysterious 'universal forces'; and that the whole significance of the revolution in geometry may be summarized in the following two propositions:

(*A*) *There is nothing in reality answering to the notion of a 'universal force'*.

(*B*) *There is something in reality approximately answering to the traditional notion of a 'straight line', namely a geodesic. If this is not a 'straight line', then nothing is.*

(Reichenbach's view is that (A) represents a *definition*; it is precisely this that I am denying, except in the trivial sense in which it is a 'definition' that 'force' refers to force and not to something else.)

Now then, the situation in quantum mechanics may be expressed thus: we *could* keep classical logic, but at a very high price. Just as we have to postulate mysterious 'universal forces' if we are to keep Euclidean geometry 'come what may', so we have to postulate equally mysterious and really very similar agencies – e.g. in their indetectability, their violation of all natural causal rules, their *ad hoc* character – if we are to reconcile quantum mechanics with classical logic *via* either the 'quantum potentials' of the hidden variable theorists, or the metaphysics of Bohr. Once again, anyone who really regards the choice of a *logic* as a 'matter of convention', will have to say that whether 'hidden variables exist', or

† By a 'differential' force what is meant is one that has a source, that affects different bodies differently (depending on their physical and chemical composition), etc. The 'forces' that one has to postulate to account for the behavior of rigid rods if one uses an unnatural metric for a space are called 'universal forces' by Reichenbach (who introduced the terminology '*differential/universal*'); these have *no* assignable source, affect *all* bodies the same way, etc.

whether, perhaps, a mysterious 'disturbance by the measurement exists'. or a fundamental difference between macro- and micro-cosm exists, etc., is likewise a matter of convention. And, once again, our standpoint can be summarized in two propositions:

(A′) There is nothing really answering to the Copenhagen idea that two kinds of description (classical and quantum mechanical) must always be used for the description of physical reality (one kind for the 'observer' and the other for the 'system'), nor to the idea that measurement changes what is measured in an indescribable way (or even brings it into existence), nor to the 'quantum potential', 'pilot waves', etc. of the hidden variable theorists. These no more exist than Reichenbach's 'universal forces'.

(B′) There are operations approximately answering to the classical logical operations, viz. the $\vee$, $\cdot$, and $-$ of quantum logic. If these are not the operations of disjunction, conjunction, and negation, then no operations are.

VIII. The 'operational meaning' of the logical connectives

It is well known that operationalism has failed, at least if considered as a program of strict epistemological reduction. No nonobservational term of any importance in science is strictly definable in terms of 'observation vocabulary', 'measuring operations', or what not. In spite of this, the idea of an 'operational definition' retains a certain usefulness in science. What is usually meant by this – by scientists, not by philosophers – is simply a description of an idealized way of determining whether or not something is true, or what the value of a magnitude is, etc. For example, the 'operational meaning' of relativity is frequently expounded by imagining that we had errorless clocks at every space–time point.

Now the physicist who expounds the 'operational meaning' of a theory in terms of 'clocks at every space–time point' knows perfectly well that (1) all clocks have, in practice, some error; (2) no criterion can be formulated in purely observational language for determining the amount of error exactly and in all conceivable cases; (3) anyway, clocks have a certain minimum size, so one could not really have a clock at each point. Nevertheless, this kind of *Gedankenexperiment* is useful, because the situation it envisages can at least be approximated (i.e. one can have pretty accurate clocks which are pretty small and pretty close together), and because seeing exactly what happens in such situations according to the theory (which is made easier by the idealization – that is the whole reason for 'idealizing') gives one a better grasp of the theory. Provided one does not slip over from the view that 'operational definitions' are a

useful heuristic device for getting a better grasp of a theory, to the view that they really tell one what theoretical terms *mean* (and that theoretical statements are then mere 'shorthand' for statements about measuring operations), no harm results. It has to be emphasized, however, that the idealized clocks, etc., involved in typical 'operational analyses' are themselves highly theoretical entities – indeed, their very designations 'clock', 'rigid rod', etc., presuppose the very theoretical notions they are used to clarify – so that operational analyses would be *circular* (in addition to other defects) if they really were intended as definitions in the strict sense of 'definition'.

What the 'operational meaning', in the loose sense just discussed, of the geometrical terms is, is well known. A 'straight line', for example, is the path of a light ray; or of a stretched thread. It is also the 'shortest distance' between any two of its points, as measured again by, say, a tape measure, or by laying off rigid rods, etc. The idealizations here are obvious. Light, for example, is really a wave phenomenon. A 'light ray', strictly speaking, is simply a normal to a wave front. And the notion of a 'normal' (i.e. a perpendicular) presupposes both the notions of straight line and angle.

If we avoid the wave nature of light by speaking of the 'path' of a single photon, we run into difficulties with complementarity: the photon can never be assigned a particular position r_i and a particular momentum r_j at the some time (in quantum logic the conjunction: the position of E is r_i and the momentum of E is r_j is even inconsistent), so the notion 'path of a photon' is operationally meaningless. And the idealized character of 'operational definitions' of 'straight line' in terms of stretched threads, rigid rods, etc., should be completely evident to anyone.

In spite of this, as we remarked before, such 'operational definitions' have a certain heuristic value. It enables us to grasp the idea of a non-Euclidean world somewhat better if we are able to picture exactly how light rays, stretched threads, etc., are going to behave in that world. But do the *logical connectives* have analogous 'operational definitions', even in this loose sense?

The answer 'yes' has been advanced in a provocative paper by David Finkelstein (Finkelstein, 1964). In order to explain the sense in which Finkelstein ascribes operational meaning to the logical vocabulary, it is necessary to first summarize a few well-known facts about logic. (These hold in both classical and quantum logic.)

First of all, propositions form what is called a *partial ordering* with respect to the relation of *implication*; that is, the relation of implication has the properties of being reflexive (p implies p), and transitive (if p

implies q and q implies r, then p implies r). Moreover, if we agree to count equivalent propositions as the same, then implication has the property: p implies q and q implies p both hold only when p and q are the same proposition.

The proposition $p \vee q$ is what is called an *upper bound* on both p and q: for it is implied by p (think of the implicandum as 'above' the implicans) and implied by q. Moreover, it is the *least upper bound*; for every proposition which is 'above' both p and q in the partial ordering of propositions by the implication relation is above their disjunction $p \vee q$. Similarly, the conjunction $p \cdot q$ is the *greatest lower bound* of p and q in the partial ordering.

In mathematics, a partial ordering in which there are for any two elements x, y a least upper bound $x \vee y$ and a greatest lower bound $x \cdot y$ is called a lattice; what we have just said is that propositions form a *lattice* with respect to implication.

The tautological proposition $p \vee -p$ is the greatest element in the whole lattice (we denote it by '1') and the inconsistent proposition is the least element, since every proposition implies a tautology and is implied by an inconsistency.

The proposition $-p$ has the property that its greatest lower bound with p (i.e. its conjunction with p) is 0 and its least upper bound with p is 1. A lattice in which there is for every x a complement $-x$ with these properties is called a *complemented lattice*. Thus propositions form a complemented lattice. (So do *sets*, under inclusion, and many other things.)

A lattice in which the laws:

$$x \cdot (y \vee z) = (x \cdot y) \vee (x \cdot z) \quad \text{and} \quad x \vee (y \cdot z) = (x \vee y) \cdot (x \vee z)$$

hold (where '$\cdot$' denotes the greatest lower bound and '$\vee$' denotes the least upper bound) is called a *distributive* lattice. The whole difference between classical and quantum logic lies in this: that propositions do not form a distributive lattice according to quantum logic, whereas according to classical logic they do.

In this paper we explained quantum logic in terms of the vector space representing the states of a system: however, one could equally well take the lattice of propositions as basic, for the subspaces of the vector space are in one-one correspondence to the propositions, and all the inclusion relations are the same.

Since conjunction and disjunction are simply greatest lower bound and least upper bound under implication, and negation is likewise characterized in terms of the implication lattice, it suffices to give an operational meaning to 'implication'.

This is accomplished by Finkelstein with the aid of the notion of a *test*. Let us pretend that to every physical property P there corresponds a test T such that something has P just in case it 'passes' T (i.e. it *would* pass T, if T were performed). This is no more than the idealization which is always made in all operational analysis. Then we define the following natural 'inclusion' relation among tests:

$$T_1 \subset T_2 \text{ just in case everything that 'passes' } T_1 \text{ 'passes' } T_2. \quad (1)$$

This inclusion relation may be operationally tested in the following way: take a large population of things which are supposed to all pass T_1 (say, on the basis of theory). Take out a large fair sample S_1, and apply test T_1 to every member of the sample. If they all pass, then our hypothesis that P consists of things which pass T_1 is operationally confirmed. Otherwise, we have to look for a different P. Now (assuming they all pass), we take a *different* fair sample S_2 from P and apply T_2. If all the elements of S_2 pass T_2, the hypothesis that 'all things that pass T_1 also pass T_2' has been confirmed. (Note that we do *not* test $T_1 \subset T_2$ by applying T_2 to the things to which T_1 was applied; this would be bad practice because those things may no longer have P *after* T_1 has been performed – i.e. they might not still pass T_1 – if T_1 were repeated.)

Now then, if quantum mechanics is true, then it turns out that there is an idealized test $T_1 \vee T_2$ which is passed by everything which passes T_1 and by everything which passes T_2 and which is such that the things that pass this test pass *every* test T such that $T_1 \subset T$ and $T_2 \subset T$. This test $T_1 \vee T_2$ is the *least upper bound* on T_1 and T_2 in the lattice of tests. Similarly, there is a greatest lower bound $T_1 \cdot T_2$, with the properties $T_1 \cdot T_2 \subset T_1$, $T_1 \cdot T_2 \subset T_2$, and $T \subset T_1 \cdot T_2$ for all T such that $T \subset T_1$ and $T \subset T_2$, and a test $-T$ which is a 'complement to T' in the sense that $T \cdot -T = 0$ (the impossible test) and $T \vee -T = 1$ (the vacuous test).

Quantum mechanically these tests may be described as follows. Let p be a proposition corresponding to a subspace S_p and q be a proposition corresponding to a subspace S_q of the vector space $H(S)$. Let S_t be the subspace spanned by S_p and S_q – i.e. the smallest subspace containing both S_p and S_q. Then there is always a physical magnitude m, and a set Σ of values of m, such that S_t is exactly the subspace corresponding to the proposition that $m(S) \in \Sigma$. Let T be the test which consists in measuring m, and 'passing' if a value in Σ is obtained. Then it follows from quantum mechanics that everything that passes the test corresponding (in the same sense) to the subspace S_p passes T, i.e. $T_p \subset T$, and similarly $T_q \subset T$. Moreover, as Finkelstein shows, any possible test T' such that $T_p \subset T'$ and $T_q \subset T'$ is such that $T \subset T'$; so T is indeed a 'least upper bound' on T_p and T_q.

Now then, suppose the proposition '$P \vee Q$' has any operational meaning at all (i.e. that there is any test T at all which is passed by all and only the things which have either property P or property Q).

Since the things that have property P all pass the corresponding test T_p, and everything that has P certainly has $P \vee Q$, it must be that $T_p \subset T$. Similarly, it must be that $T_q \subset T$. On the other hand, let T' be any test such that $T_p \subset T'$ and $T_q \subset T'$. Since everything that has P passes T_p, by hypothesis, it follows that the things with property P are a subset of the things which pass T', and similarly the things with property Q are a subset of the things which pass T'. So the things with $P \vee Q$ are a subset of the things which pass T', and since the things with $P \vee Q$ are assumed to be just the things which pass T, it follows that $T \subset T'$. Thus 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.

But, by Finkelstein's result, 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. Similarly, if conjunction is to correspond to any test at all, it must be the test determined by the intersection of the spaces S_p and S_q, and negation must correspond to the orthocomplement of S_p. Thus we are led directly to *quantum logic* and not to classical logic!

In sum: if we seek to preserve the (approximate) 'operational meaning' that the logical connectives *always* had, then we have to *change* our logic; if we insist on the old logic, then *no* operational meaning at all can be found for the logical connectives that will work in all cases. (Of course, for *macroscopic* propositions the lattice is distributive; so we may keep the classical logic *and* the classical tests for these cases. But as soon as one tries to extend this to an operational meaning for *microscopic* propositions in any consistent way, we will be in trouble in view of Finkelstein's result.)

Two points may now be made with regard to the philosophical significance of Finkelstein's work.

First, the operational analysis of the logical connectives has the same heuristic value that the operational analysis of the geometrical notions does. If we interpret '$P \vee Q$' (as applied to a system S) as meaning 'S passes the test T which is the least upper bound on T_p and T_q' – this is equivalent to 'S passes *every* test which is passed by everything that passes T_p and passed by everything that passes T_q' – and similarly interpret '$P \cdot Q$' as 'S passes the test T which is such that $T \subset T_p$ and $T \subset T_q$ and T is passed by everything that passes any test T' such that $T' \subset T_p$ and $T' \subset T_q$', and '$-P$' as 'S passes the test T which is such that nothing that passes T passes T_p and everything passes any test

which is passed by everything that passes T_p and everything that passes T'', then we can figure out exactly what to expect in a quantum logical world. Reichenbach once replied to the neo-Kantian claim that Euclidean geometry is the 'only geometry that can be visualized' roughly as follows: 'If to imagine a non-Euclidean world is to imagine a world upon which a Euclidean description *cannot* be forced, even by introducing "universal forces", then of course no such world can be imagined: for *any* geometry can be imposed on a world, if we are willing to adopt enough *ad hoc* hypotheses. But if to imagine a non-Euclidean world is to imagine a world which conforms to the standard operational significance of *non*-Euclidean geometry, then of course such a world can be imagined.' (Poincaré had earlier made a similar point: to imagine a non-Euclidean world, imagine the experiences you would have if you lived in such a world. – Assuming, of course, the standard 'operational definitions'.)

In exactly the same way, one can say: if to imagine a world which does not obey classical logic is to imagine a world upon which a description presupposing classical logic cannot be *forced*, even by introducing 'hidden variables' or Copenhagen double-think, then of course this cannot be done. But one can imagine a world which conforms to the *operational significance* of quantum logic. Or, adapting Poincaré: 'to imagine a quantum logical world, imagine the experiences you would have in such a world'. (*You live in one.*) Assuming, of course, the 'operational definitions' of the logical connectives, as we just analyzed them.