Could Logic be Empirical? The Putnam-Kripke Debate

Abstract

Not long after Hilary Putnam published "Is Logic Empirical," Saul Kripke presented critique of Putnam's argument in a lecture at the University of Pittsburgh. Kripke criticized both the substance of Putnam's version of quantum logic and the idea that one could "adopt" a logic for empirical reasons. This paper reviews the debate between Putnam and Kripke. It suggests the possibility of a "middle way" between Putnam and Kripke: a way in which logic could be broadly *a priori* but in which empirical considerations could still bear on our views about the logical structure of the world. In particular, considerations drawn from quantum mechanics might provide an example.

Is Logic Empirical? The Putnam-Kripke Debate

Some years ago, Hilary Putnam published a paper called "Is Logic Empirical?" (Putnam 1968) in which he argued that quantum mechanics provides an empirical case for revising our views about logic. (The paper was republished in his collected works as "The Logic of Quantum Mechanics". Page references will be to the reprinted version.) In 1974, Saul Kripke presented a talk at the University of Pittsburgh called "The Question of Logic," offering a detailed rebuttal of Putnam's case. As of this writing, almost 40 years later, Kripke's paper still hasn't appeared in print and apart from my 1978 dissertation and a paper I published 28 years later (Stairs 2006), very little has been written on the disagreement between Putnam and Kripke. This is unfortunate; the issues are well worth investigating. In my 2006 paper, I adopted the device of writing about Paul Kriske and Prof. Tupman out of deference to the fact that there is no published version of Kripke's talk. Here I'll simply write directly about Putnam and Kripke. If I get Kripke wrong, I hope he'll let us know.

As for the plan of the paper, we begin by reviewing Putnam's arguments; after that we move to Kripke's rebuttal. This will lead to a larger discussion of what logic and the empirical might have to do with one another.

1 Putnam on Quantum Logic

We think of logical truths as a special case of necessary truths, but Putnam reminds us that we now reject certain claims about geometry that once seemed necessary. We would once have said that if two lines are straight and a constant distance apart over some portion of their span, they can't converge elsewhere. For anyone not familiar with non-Euclidean geometry, Putnam claims that this seems as intuitively clear as saying that there are no married bachelors, or that nothing can be scarlet all over and bright green all over at the same time. In the case of the lines, however, we've come to believe not just that the claim *might* be false but that in some instances it *is* false.

We might say that what Putnam describes applies to *geodesics*, but "geodesic" doesn't mean "straight line." However, Putnam insists that this won't do. On our intuitive conception, shortest paths are straightest and conversely. The notion of a geodesic preserves this, and lines that depart from geodesics will not seem straighter. One way to put it: if we say that geodesics behaving as Putnam describes aren't straight lines, we'll have to say that there can be points with no straight line between them. Putnam thinks we miss the significance of relativity if we represent its geometrical claims as mere changes of meaning. He writes:

> The important point is that ['straight line'] does not 'change meaning' in the trivial way 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. (p. 177)

Putnam argues that we've made a similar discovery about logic itself. We pair statements about quantum quantities with subspaces of Hilbert space and we can extend this map from simple statements to compound ones by associating "or" with subspace span ($p \vee q$), "and" with subspace intersection ($p \wedge q$), and "not" with orthocomplement ($p^{\perp}$). If we take the mapping seriously, however, we have a conflict with classical logic. Suppose the quantity A has two possible values a_1 and a_2, associated with rays α_1 and α_2. Suppose, likewise, that B has two values b_1 and b_2 associated with rays β_1 and β_2. Now consider the expressions

$(A=a_1 \text{ or } A=a_2)$, $(B=b_1 \text{ or } B=b_2)$

and associate them with the subspaces

$(\alpha_1 \vee \alpha_2), (\beta_1 \vee \beta_2)$

Quantum mechanics, read as Putnam reads it, gives us cases where both disjunctions are true. That means the conjunction

$(A=a_1 \text{ or } A=a_2)$ and $(B=b_1 \text{ or } B=b_2)$

is also true. However each of the following pick out the null subspace of Hilbert space

$(\alpha_1 \wedge \beta_1), (\alpha_1 \wedge \beta_2), (\alpha_2 \wedge \beta_1), (\alpha_2 \wedge \beta_2)$

and so the corresponding conjunctions are false. Hence

$(A_1=a_1 \wedge B_1=b_1) \vee (A_1=a_1 \wedge B_1=b_1) \vee (A_1=a_1 \wedge B_1=b_1) \vee (A_1=a_1 \wedge B_1=b_1)$

is false but this discrepancy between the distributed and undistributed formulas is impossible classically. Putnam writes:

> *Conclusion:* the mapping is nonsense – *or, we must change our logic*. (p. 179)

On the other hand, if we do “adopt the heroic course of changing our logic,” there’s a straightforward way to proceed:

> …*just read the logic off from the Hilbert space H*(*S*). (p. 179)

The advantage, says Putnam, is that

> *all* so-called anomalies in quantum mechanics come down to the *non-standardness of the logic*. (p. 179)

and the anomalies go away if we change our logic.

Putnam offers several illustrations. "Complementarity," understood as the failure of quantum mechanics to specify joint values for noncommuting quantities comes down to logical incompatibility in quantum logic; the complementary quantities don't share eigenspaces. He also argues that quantum logic accounts for the two-slit experiment. To derive the incorrect classical probabilities, we have to distribute a proposition R about where the photon hits the screen over a disjunction of propositions A_1 and A_2 about which hole the photon passes through. If we treat '$R \wedge (A_1 \vee A_2)$' as equivalent to '$(R \wedge A_1) \vee (R \wedge A_2)$', then we end up with the wrong probabilities.

Putnam also claims that if we analyze barrier penetration quantum logically, we avoid explaining the effect by appeal to a supposedly mysterious 'disturbance by the measurement.' (p. 182) In fact, the account he gives (on p. 183) can't be right for any finite population of atoms (exercise for the reader; look especially at statement (8) and Putnam's comment on it) but let that pass. In classical physics, the state provides a complete description relative to the terms of the theory, of the system. In quantum theory, there are "states" or "state descriptions," but Putnam writes that

> A system has no complete description in quantum mechanics; such a thing is a logical impossibility (p. 185)

Quantum states are "logically strongest consistent statements" but they aren't states "in the sense of statements which imply every true proposition about *S*" (p. 185) This might suggest that quantum states are like statistical states in classical mechanics, and that their failure to provide a complete list of all the truths is a reflection of our epistemic situation. However, this isn't Putnam's view. Rather, he tells us that a quantum system has, e.g., a position by virtue of the truth of a disjunction of position statements and it also has a momentum by virtue of the truth of disjunction of momentum statements.[1] Here is Putnam articulating what we will call the *value-definiteness* thesis:

> (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.
>
> We can predict any *one* magnitude, if we make an appropriate measurement, but we can't predict them *all*.

The advantage of giving up classical logic, according to Putnam, is this:

> These examples makes the principle clear. The only laws of classical logic that are given up in quantum logic are distributive laws… and every single anomaly vanishes once we give these up. (p. 184)

Putnam's argument for adopting quantum logic is that if we do, the interpretive puzzles of the theory dissolve. If we insist on classical logic, we have to say such supposedly objectionable things as that measurements create the values of the quantities measured or that there is a "cut between the observer and the observed" or that there are undetectable hidden variables. But Putnam says

> …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.

[1] Putnam knows that strictly, there are no position and momentum eigenstates; the oversimplification is merely for illustration.

This completes the analogy with geometry. We could preserve Euclidean geometry, but only by paying the high intellectual price of admitting gratuitous universal forces. Likewise for classical logic: we can preserve it only by paying an unacceptable price in the coin of untoward claims about quantum systems.

2 Kripke on Putnam

Kripke's critique of Putnam has two parts. One deals with the particulars of Putnam's argument. There Kripke's case is strong. However, granting that Kripke is right about Putnam's particular quantum logical proposal wouldn't show that logic isn't empirical, nor would it show that quantum mechanics doesn't give us a reason to change our views about logic. In the second part of his critique, Kripke argues that the very idea of changing logic for empirical reasons is confused.

In what follows, I quote at length from my partial transcript of Kripke's talk. The indirect debate between Kripke and Putnam was an important episode, and the reader will get a better sense of it if s/he reads Kripke's own words. Nonetheless, there is a matter of propriety here, but Kripke's own words do a better job than my paraphrase would of spelling out his view and therefore, it seems fairest him to use those words.

2.1 Quantum Logic and Simple Arithmetic

The first part of Kripke's argument is intended to show that if we follow Putnam, we have to agree to the untoward conclusion that 2x2 ≥ 5. According to Putnam, if M has possible values m_1, m_2… m_n then there is a true statement ascribing one of these value to M. The statement

$$M = m_1 \vee M = m_2 \vee \ldots \vee M = m_n$$

is true, Putnam would say, and the summary of the value-definiteness thesis above makes clear what he means: one of the disjuncts really is true, and if we knew which, we could predict the outcome of an *M*-measurement. However, the logically strongest statement about the system may not tell us which disjunct is true.

Is it really clear that Putnam meant this? Here's a passage that would be hard to make sense of otherwise. S_z is a position state and T_1, T_2, etc. are momentum states. (Substitute eigenstates of different spin components if you prefer.) We suppose S_z to be known

> 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 $S_z \bullet T_j$ is false. It is natural to conclude (smuggling in classical logic) that $S_z \bullet (T_1 \vee T_2 \vee \ldots \vee T_R)$ 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 – so the measurement must have 'brought it into being'. However, the error was in passing from the falsity of $S_z \bullet T_1 \vee S_z \bullet T_2 \vee \ldots \vee S_z \bullet T_R$ to the falsity of $S_z \bullet (T_1 \vee T_2 \vee \ldots \vee T_R)$. This latter statement is *true* (assuming S_z) and so it is *true* that 'the particle has a momentum'... and the momentum measurement merely *find*s this momentum (while disturbing the *position*); it does not create it, or disturb it in any way. It is as simple as that. (p. 186)

"Simple" or not, Kripke draws out an untoward consequence. Suppose we're given two quantities, *A* and *B*, each with two possible values 1 and 2. Thus the set {1,2} is the set of possible values of *A* and also of *B*. Putnam will say that

1) $A = 1 \vee A = 2$
2) $B = 1 \vee B = 2$

are both true. However, he will also say that each of the following are false:

3) $(A = 1 \wedge B = 1)$

4) $(A = 1 \wedge B = 2)$

5) $(A = 2 \wedge B = 1)$

6) $(A = 2 \wedge B = 2)$

But Kripke argues:

> The usual mathematical definition of multiplication is this: suppose we have two sets with two elements. Then the cardinality of their product is the cardinality of the Cartesian product of the two sets… where x comes from the first set and y comes from the second set, so where x comes from {a,b} and y comes from {c,d} where {a,b} and {c,d} are our two two-element sets. We want to consider how many ordered pairs there are. So the classical arithmetician says "There are four, namely <a,c>, <a.d>, <b,c>, <b,d>… But we can all see the fallacy in any conclusion that these are the only pairs.

The "fallacy" is that if x comes from {a,b}, then we have the disjunction

$$x=a \vee x=b$$

and similarly we have

$$y=c \vee y=d$$

Now suppose that the set {a,b} is the set of possible values of the quantity A above (i.e., {a,b} = {1,2}) and {c,d} is the set of values of the quantity B (i.e., {c,d} = {1,2} as well.) We'll let Kripke pick up the story:

> Now I claim that there is a fifth pair <*A*,*B*> where these are the two quantities mentioned by Putnam. Remember that Putnam does not think these are funny pseudo-numbers. The idea is that *A* was already one of the two numbers 1 and 2 [and] *B* was already one of the numbers 1 and 2. So *A* is certainly in the first set [i.e., {a,b} = {1,2} – AS] because *A* is equal either to 1 or to 2. *B* is certainly in the second set [i.e., {c,d} = {1,2} – AS] because *B* is either equal to 1 or to 2, though we may not have measured which. So the pair <*A*,*B*> is in our Cartesian product. But certainly we cannot say that <*A*,*B*> equals <1,1> if we adopt the usual criterion of identity of ordered pairs because that would mean that *A* = 1 and *B* equals 1, and that contradicts [the falsity of (3)]. Also, <*A*,*B*> does not equal <1,2> because it is false that *A* equals 1 and *B* equals 2. And <*A*,*B*> does not equal <2,1> [and] <*A*,*B*> does not equal <2,2>… So there is a fifth and hitherto overlooked, I might say, ordered pair in the Cartesian product of these two finite sets.

Kripke's point, of course, is that this is absurd, but that it's where he end up if we follow Putnam.

There may be various ways Putnam could respond, but Kripke insists that one obvious rejoinder won't do: it won't do to accuse Kripke of begging the question. Kripke insists: he'd only be begging the question if he had assumed a premise that Putnam rejects. However, the distributive law isn't a premise in his argument. Kripke simply reasons from premises that Putnam accepts to the conclusion that if none of the pairs <1,1>, <1,2>, <2,1>, <2,2> gives the joint values for A and B, then joint values require that there be another pair in the Cartesian product. Since Putnam would claim that none of the four ordered pairs gives the joint values, and would also claim that both quantities really do have values, the untoward (and absurd) conclusion follows. As Kripke puts it in connection with a closely-related example

> …if you say that I am begging the question then you yourself, I think, are begging the question, because only if my reasoning was invalid did I need any extra premise which I have begged against Putnam.

2.2 The impossibility of "adopting" a logic

Kripke is right, I believe: there's no convincing quantum-logical defense of the value-definiteness thesis. (See Stairs 1983 for more discussion) and in what follows, we will assume that value-definiteness doesn't hold. Kripke's larger point is that there is a problem at the core of Putnam's view. Putnam, he thinks, believes that we could somehow decide to "adopt" a logic; Kripke insists that this is incoherent. We misunderstand logic if we think there are "logics" among which we could somehow choose. There is *reasoning*. Specific formal systems may or may not adequately capture aspects of correct reasoning. But there is no neutral place outside logic from which to decide what "logic" to adopt.

Whether Putnam really holds the view of logic that Kripke attributes to him isn't clear. That said, it's a useful foil for making Kripke's own view of logic clearer. Therefore, while we won't ignore the question of how well Kripke's criticisms fit Putnam, the exegetical question won't be our main concern.

Putnam remarked on our intuitive sense of contradiction when faced with his geometrical example. Kripke reads Putnam this way:

> Just as in the case of non-Euclidean geometry we throw intuition to the wind and adopt an axiomatic system as supposedly describing the real physical world… so on every other domain we cannot rely on intuition. Once one has a rival system of axioms, the mere fact that an old system struck us as the only intuitively acceptable one should be given little weight. Once alternative geometries are under consideration, we abandon any mere intuitive preference for Euclidean geometry, and once alternative logics are under

> consideration, we abandon any mere intuitive preference for a particular system of logic.

Kripke thinks there is a deep confusion here. Formal systems are not *logic*. Formal systems may or may not faithfully reflect correct principles of reasoning, but we have no alternative to using "intuition," by which Kripke means *reasoning*, to assess the formal systems. If changing our formal system is supposed to entail changing the way we *reason*, then we have no place to stand outside of reasoning from which to do this. "Logics" *qua* formal systems aren't *logic*. As Kripke sees it, Putnam's fundamental error lies in missing this point. Once we grasp this, the idea that we could change our logic in response to empirical considerations makes no sense.

Even if we grant that there's no place to stand outside reasoning, there's a more general phenomenon here. What William Alston called *doxastic practices* (see his 1991, ch. 4, for instance) typically have the sort of self-supposing quality that Kripke's point relies on. We can reconsider how to evaluate beliefs based on sensory input; when we do, we'll need to rely on at least some such beliefs and hence on the practice of forming beliefs based on sense evidence. We can consider what memory can and can't teach us; we can't avoid relying on at least some memories when we do. Equally important, these practices aren't insulated from one another. In considering what weight to give memory, for example, we'll make use of claims that we've accepted on the basis of the implicit and explicit rules/practices we use for assessing other kinds of empirical claims. We can also reason about how to reason, as Kripke would be the first to insist. Putnam may seem to be saying that we can evaluate logic without relying on logic broadly conceived (i.e., on logic *qua* reasoning) but it's not clear that he means this or needs to say it. In order to rebut a measured version of Putnam's view, Kripke would have to show that reasoning is the one doxastic practice to which the deliverances of other doxastic practices are irrelevant. Putnam's larger point would be made if sometimes what we discover empirically can *properly* enter into our deliberations about how to reason.

Be that as it may, Putnam's main argument seems to be that if we give up the distributive law, we'll be blocked from drawing untoward conclusions. Thus, we won't be able to argue that the probabilities in the two-slit experiment must fit a crude application of the law of total probability, and we won't need to say that measurement creates the values that it records. However, this is too quick. We might be able to avoid any number of unwelcome conclusions if we simply refused to reason in certain ways; that hardly makes a case for merely opportunistic "revisions" of logic. And while Putnam might judge that a failure of the distributive law is "more likely" than hidden variables or "cuts between observer and observed," Kripke can reply that without something more than a mere and tendentious cost-benefit analysis, we haven't been presented with an intelligible alternative. The distributive law *seems* to be a correct way to reason. Putnam hasn't shown us any deep problems with the idea that there are Bohmian-style hidden variables; he merely tells us that he finds them unlikely. He objects to the idea that measurement might bring the values it yields into being. However, his main objection seems to be that this is a strange notion of "measurement." This threatens to turn the argument into a mere quibble. The idea that the interactions we *call* measurements bring new states of affairs into being might be a reason to pick or invent a different word, but it doesn't count against the possibility that things really work this way.

We leave the vexed issue of measurement (or "measurement") aside and turn to a different part of Kripke's reply: his case that the very idea of "adopting a logic" makes no sense. Kripke takes his cue from Lewis Carroll's "What the Tortoise Said to Achilles" and from Quine's "Truth by Convention". He says:

> The basic problem is this: if logical truths are mere hypotheses… and one can adopt them as one will, how, unless one has a logic in advance, can one possibly deduce anything from them?

Kripke develops the example of universal instantiation at greatest length. Imagine someone who doesn't see that from a universal claim, each instance follows. Imagine further that our poor reasoner is willing to accept Kripke's authority that all ravens are black and is also willing to accept Kripke's authority in more general logical matters. There's a raven, J, out of our subject's sight, but he doesn't see that believing this and accepting that all ravens are black commits him to accepting that J is black. Kripke tells the tale charmingly:

> So I say to him "Oh. You don't see that. Well let me tell you: from every universal statement, each instance follows." He will say "Oh. Yes. I believe you." So now I say to him, "Ah. So 'All ravens are black' is a universal statement and 'This raven is black' is an instance." "Yes. Yes" He agrees. So I say to him "All universal statements imply their instances. This particular statement that all ravens are black implies this particular instance." "Well, hmm, I'm not entirely sure," he will say. "I don't really see that I've got to accept *that*!"

The problem is clear. As Kripke puts it

> If he was not able to make the simple inference "All ravens are black, therefore J is black" where J is a particular raven, then giving him some super-premise like "Every universal statement implies each instance" won't help him either

It won't help because he would already have to be in command of the principle to apply it; the idea that he could *adopt* it is incoherent. Kripke makes similar points about non-contradiction, adjunction and Lewis Carroll's modus ponens example. We can embody these principles in formal systems, but there's no sense to the idea that someone, so to speak, standing outside these principles could adopt them.

These are all cases where we couldn't adopt a particular principle unless we already grasped it intuitively. Perhaps that doesn't apply to all logical principles, and in any case Putnam's example had to do with giving up rather than adopting a principle. However, Kripke thinks this would miss the point. Here's what he says:

> …I don't really mean that we adopt as basic just those things to which we can figure out that this argument applies, What I mean is this: you can't undermine intuitive reasoning in the case of logic and try to get everything on a much more rigorous basis. One has just to think not in terms of some formal set of postulates but intuitively. That is, one has to *reason*. One can't just adopt a formal system independently of any reasoning about it because if one tried to do so one wouldn't understand the directions for setting up the system itself. And so any comparison of logic to geometry which says that in the case of logic as in the supposed case of geometry, intuition can be thrown to the dogs – that is, any reasoning outside the system of postulates can be thrown to the dogs – must be wrong. One can only reason as we always did, independently of any special set of rules called "logic," in setting up a formal system or in doing anything else. And if proof by cases was part of our intuitive apparatus then there is no analogy to geometry which says that this should not be respected.

Kripke is surely right: logic isn't just a matter of formal systems. We can also agree that questions about how to reason have a special status among the various kinds of questions we can ask. We can agree further that that for at least some logical principles there's no sense to be made of the idea that we might "adopt" them, and we can even concede that nothing could count as adopting a logic wholesale. Whether this scuttles the idea that empirical considerations could bear on logic is less clear, however.

2.3 Rejecting subalternation as a case of change in logic

To make progress, we need to look at what Kripke concedes about changes in logic and how he accounts for them. The most useful place to begin is with what he says about the principle of subalternation for universal categoricals – in particular, that "All P are Q" implies "Some P are Q."[2] Logicians once accepted this principle, and yet we no longer do. What Kripke says is surely right: if we accept subalternation, we overlook the case where 'P' is empty. 'All deserters will be shot on sight' may be true, and that may be exactly the reason why there are no deserters. But if there are no deserters, it would be very odd to say that some deserters are shot. Intuitive reflection makes clear that something has to give. If we overlook empty terms, we'll be tempted to think subalternation is valid. We correct ourselves by mere reflection – by ordinary reasoning. However, we can ask if this is always so. When we discover that we've overlooked a case and accepted an incorrect logical principle as a result, is this always a matter of ordinary reasoning, or do empirical considerations sometimes come into play? ###

2.4 Future contingents, bivalence and the empirical

Consider a debate that Kripke doesn't mention but that has a long history: whether propositions about the future provide reasons to give up bivalence. Two sorts of views suggest that the answer might be yes. One is that some propositions about the future (e.g. "There will be a sea battle tomorrow" or "This atom will decay an hour from now") are contingent in a more-than-merely-logical sense. Another is the view that only the present exists, usually called "presentism" and the "growing block" view, according to which the present and the past but not the future are real. The difference between presentism proper and the growing block won't matter for our purposes, we'll use "presentism" for both.

---

[2] Kripke talks briefly about cases where we see that an argument we once accepted is invalid. Here we change our beliefs about logic, but we do so simply and straightforwardly by *reasoning*. He also offers a cursory discussion of intuitionism. Here he claims that the intuitionists introduced new connectives, defined in terms of provability, and so the intuitionist's apparent rejection, e.g., of excluded middle isn't really in competition with the classical principle. Whether that's the best reading of intuitionists such as Brouwer I will leave to others to decide.

Neither future contingent propositions nor presentism alone make the case against bivalence. Suppose some propositions about the future are contingent. If the so-called "block universe view" is correct, all events, past present and future, are ontologically on a par. If so, the facts about the events making up block entire settle the truth or falsity of future contingent propositions. This is so even if determinism is false. An event's being undetermined is a matter of its relationship to other events and to the laws of nature; whether we live in a block universe and whether the laws are deterministic are independent questions. On the other hand, suppose that presentism is correct. Then even though future states of affairs don't exist, deterministic laws plus the facts about the present could suffice to settle the truth or falsity of propositions about the future.

What, then, if presentism is true and determinism false? Perhaps bivalence about future contingent propositions can still be defended, but what if not? One response is to abandon excluded middle – to claim that when 'P' is indeterminate, 'P ∨ not-P' is likewise indeterminate (a view usually associated with Łukasiewicz.) However, there's a plausible objection: if 'P' is indeterminate, then it's not *true* that P, hence 'not-P' is true. If so, then even if 'P' is indeterminate, 'P ∨ not-P' will be true by virtue of its second disjunct. [3]

Another familiar account of future contingents appeals to branching time and supervaluation (See Thomason 1970). On this approach, a statement about the future is true at the present moment just in case it holds on each branch or history passing through this moment, and false if it is false on each such branch. Contingent statements about the future will therefore be neither true nor false. However, this permits true disjunctions with no true disjuncts. Suppose $\{P_1, P_2, \ldots P_n\}$ is a set of future contingent propositions that are mutually exclusive, *not* logically exhaustive,

---

[3] Scope matters here; using '*F*' as a future-tense operator, the claim is that when '*F*(X) ' is indeterminate, 'not-*F*(X)' is true, even though '*F*(not-X)' is indeterminate. See Bourne 2006, pp. 82 ff. for useful discussion.

but such that on each branch passing through the present, one of them is true. An artificial example: suppose a coin will be tossed, that the outcome isn't determined, but that on each branch the outcome is either Heads or Tails. Then

The coin will come up heads or the coin will come up tails

is true at the present moment even though neither disjunct is.

Supervaluation preserves excluded middle and non-contradiction. Whether it preserves all classical logical truths might be more of an accounting issue than a substantive one. Even with excluded middle intact, the possibility of a true disjunction with no true disjuncts isn't part of logical business as usual. The novelty seems at first to sit comfortably with Kripke's view. Our belief that true disjunctions require true disjuncts came from overlooking a (complex) possibility: the combination of presentism and future contingents. However, further thought may seem to favor Putnam. The case for true disjunctions without true disjuncts depends on assumptions about the world: that the block universe view and determinism are both false. The overlooked possibility is a substantive one, and reasoning alone won't tell us if it holds. This suggests that matters of logic depend on the way things are, as Putnam's view would maintain.

The status of determinism is a contingent, empirical matter. However, as we noted above, even if determinism is false, this wouldn't be enough to undermine bivalence. The crucial additional assumption is presentism, and it might be argued that this is not an empirical matter; certainly the debate has often proceeded as though it's not. However, there are able defenders of the coherence of presentism *and* of the block universe. If both views are indeed coherent, empirical considerations plausibly bear on which is correct. Indeed, Putnam himself famously invoked special relativity to argue against presentism (albeit not under that name) in "Time and Physical Geometry." (Putnam 1967) His argument that past, present and future are equally "real" don't rest on general philosophical considerations; it depends on the structure of Minkowski space-time. It may be, then, that whether presentism is true depends on the facts about space-time. If so, it suggests that assessing the need for the logical revisions at issue in the debate over future contingents depends on contingent, empirical facts about the world.[4] The broad issue is whether claims about reality could have consequences for logic. With future contingent propositions, we have a dilemma: if bivalence holds for such propositions, it's because either determinism or presentism or both fail. If bivalence fails, it's because determinism fails and presentism holds. In either case, the claim is empirical. The question of determinism is *certainly* empirical and the question of presentism is at least arguably so. Thus, whether bivalence holds is an empirical matter, and that, it seems, is enough to make Putnam's larger point.

The arguments above are skelatal and open to challenge, but suppose we grant them. There's a plausible Kripkean reply. Whether bivalence holds might be an empirical matter, but if so the correct conclusion is that bivalence is not a principle of *logic*. Furthermore, the conclusion that bivalence isn't a correct principle of logic is not an empirical one. We come to it by reflecting on the possibilities, and we discover that there is a genuine possibility we had overlooked: the possibility that there are no facts to ground the truth or falsity of certain propositions about the future. That this is *possible* remains so even if the possibility isn't realized.

2.5 Détente?

What's just been said concedes something important to Kripke, but suggests a possibility for *détente*. Logic writ large (let's write bold-face '**logic**' for that) would remain a matter of reasoning, broadly understood. The logic of the actual world could still be a contingent matter. The analogy with geometry helps here. Suppose

[4] Of course, not everyone agrees that Putnam's arguments are sound. See, for example, (Stein 1968) and Bourne (2006). To repeat, the point here is *not* to take sides in this debate.

(unlikely, but science sometimes takes strange turns) we became convinced that the world is Euclidean after all. We would still know that the scenario Putnam describes is possible in a broad sense. It would just be that it's never actualized. The question of what the detailed geometry of a world *could* be would remain, broadly speaking, *a priori*; the question of what it is in fact would be empirical. That the world *could be* pseudo-Riemannian is not empirical knowledge. That it is or isn't pseudo-Riemannian is an empirical claim. Likewise, that bivalence *could* fail is arguably not empirical knowledge. That it *does* (or does not) fail in a particular way is arguably empirical. And though we won't try to give a general account of what counts as a question of logic, questions about the status of bivalence plausibly count.

This raises two questions. The first is whether there's a case of this sort to be made by appeal to quantum mechanics. We'll take that up in the following section. The second question will be raised but no more than raised: in light of what quantum mechanics teaches us, is it quite so clear that **logic** really is something we can know by a priori?

3. Quantum logic reconsidered

Putnam's quantum logical proposal offered a formal structure and some interpretative principles and rationales. The structure is the lattice of subspaces of Hilbert space, but the beginnings of the disagreement with Kripke come from the interpretive overlay. Let 'Sz = +1/2' say that the electron has spin +1/2 in direction z, and similarly for 'Sx = +1/2' and 'Sx = –1/2.' Putnam, as we know, would say that when

$$Sz = +1/2 \wedge (Sx = +1/2 \vee Sx = -1/2)$$

is true, one of the disjuncts in parentheses really is true, but that it's logically impossible for us to know which. However, there's another approach: treat

'(Sx = +1/2 ∨ Sx = –1/2)' as a disjunctive fact – as a case of a disjunction that's true in spite of not having a true disjunct. We've already seen reasons of one sort for taking the idea of disjunctive facts seriously. Quantum mechanics gives reasons of a different sort.

What follows is intended merely as a sketch, and if the reader finds it hand-waving, that's because the author is waving his hands. The goal isn't to defend a view in detail (indeed, I am by no means certain that the view is correct) but simply to make its outlines clear enough to consider.

The paradigmatically curious quantum example is the case of two quantities – call them P and Q – that share no eigenstates. This is the heart of what Bohr called complementarity and it has two characteristic features. First, there's no arrangement that measures P and Q at the same time. Second, if we're certain what outcome a P-measurement would yield, we are *not* certain what outcome a Q-measurement would yield; all values of Q have at least some positive probability. The goal in this section is to see how we might move from here to something more clearly relevant to logic, and to do it in a way that doesn't stray far from what a typical physicist would find plausible. Note that we aren't following Putnam's approach. Putnam argued that if we adopt a strong set of logical claims, we solve the interpretive problems of quantum mechanics. There's no such goal here. We're trying to see what quantum mechanics might teach us about logic if we start from things that many physicists already believe.

The first point is simple: quantum mechanical quantities can have values. A system can have an energy or a spin in a particular direction. Few physicists would disagree.[5] The second point goes beyond ordinary common sense but not beyond

[5] Though *few* would disagree, this isn't the same as saying none would. Quantum Bayesians such as Carleton Caves, Christopher Fuchs and Rüdiger Schack are exceptions. See, for example, their (2007). For some relevant discussion see Stairs (2011)

the common sense of most physicists. Stick with our complementary quantities P and Q. When P has a value, Q does not. Thus: if there's a true statement

$$P = p_i$$

then there is *no* true statement

$$Q = Q_j$$

No doubt most physicists believed this before no-hidden-variable proofs became widely known, but those proofs provide another reason. If we accept a handful of plausible constraints, then it's impossible for all quantum quantities to have values at the same time. Those constraints aren't beyond challenge, but our purpose isn't to make an iron-clad case. It's to make it *plausible* that quantum mechanics has consequences for logic.

The third point starts with a piece of physics common sense and then moves a bit beyond. It's that there are purely disjunctive truths about quantum mechanical systems. To see why this is plausible, start with a special case of our first point: *degenerate* quantities can have values. For example: energy is often degenerate; the subspace that goes with

$$E = e$$

for some values e of the energy may *not* be one-dimensional. In spite of this, there's nothing strange about saying that the system really can have energy e – that 'E = e' can be true. With that in mind, consider a simple but instructive example: a spin-one system whose z-spin is 0. The state $|z_0>$ is a superposition of $|x_+>$ and $|x_->$. On any orthodox account, the statement

$$Sx = 0$$

is definitely *false*; $|z_0>$ and $|x_0>$ are orthogonal. It's also of a piece with our second point to say that the system doesn't have a definite x-spin. Neither 'Sx = +1' nor 'Sx = –1' is true. But consider the degenerate quantity $(Sx)^2$ – the square of the spin in the x direction. Again, on any orthodox account, *this* quantity has a value: +1. Few physicists would be shocked to be told that '$(Sx)^2$ = +1' is true when 'Sz = 0' is true. But if the square of the spin is +1, it would be gratuitously peculiar to say that 'Sx = +1' and 'Sx = –1' are both *false*. Instead, we can say that for $(Sx)^2$ to take the value +1 and for

$$Sx = +1 \vee Sx = -1$$

to be true are one and the same fact: 'Sx = +1 ∨ Sx = –1' is true even though neither disjunct is. In short, bivalence fails, though for different reasons than in the case of future contingents, and we have a true disjunction with indeterminate disjuncts.[6] 'Sx = +1' and 'Sx = –1' stand in a different relationship to 'Sz = 0' than 'Sx = 0' does. 'Sz = 0' excludes 'Sx = 0' in an old-fashioned classical way: the two are contraries. The relationship between 'Sz = 0' on the one hand and 'Sx = +1' and 'Sx = –1,' is not found in classical physics. For the states that go with these statements, the term is superposition, but there's no standard word for the relationship between the statements themselves. For present purposes, I propose *l-complementarity*.

In the language of Hilbert space, propositions are l-complementary when their associated projectors don't commute. But while that picks out the sorts of cases we're interested in, it doesn't make a connection with logic. It's also too restrictive: in principle l-complementarity is more general than Hilbert space non-commutativity. Kochen and Specker's (1967) partial Boolean algebra approach is a better way to characterize l-complementarity formally. When *X* and *Y* are l-

[6] Note that even if someone insisted that each disjunct is false, we'd still have a true disjunction with no false disjunct. Why anyone would insist on any such thing, however, is unclear to say the least.

complementary they do not belong to a common Boolean subalgebra of the partial Boolean algebra.[7] However, this leaves the logical point unclear. The proposal on offer is that l-complementarity goes with a particular kind of failure of bivalence: if propositions *X* and *Y* are l-complementarity, then there are possible states of affairs in which *X* is true but *Y* is neither true nor false.

With this in mind, consider distribution. In particular, consider

$$Sz = 0 \wedge (Sx = +1 \vee Sx = -1)$$

The proposal is that both conjuncts are true, but neither disjunct of the disjunction is true. That's why we can't distribute. The expression

$$(Sz = 0 \wedge Sx = +1) \vee (Sz = 0 \wedge Sx = -1)$$

either fails to pick out an element of the algebra of propositions (on the partial Boolean algebra approach) or picks out a statement that can't be true (on a lattice approach.) The distributive law fails, but not in a way that threatens looming arithmetical catastrophe; Kripke's "missing pair" is nowhere in the neighborhood.

This isn't what Putnam would say. He would say that the x-spin has a definite value, either +1 or −1 but that it's logically impossible to state this value along with the z-spin value. However, once we recognize the possibility of disjunctive facts, it's clear that Putnam's picture goes beyond saying that '$Sx = +1 \vee Sx = -1$' is true. We can assert the disjunction without accepting the value-definiteness thesis.

The proposal under consideration includes these points:

[7] A partial Boolean algebra is a family of Boolean algebras that share a common 0 and 1. $X \vee Y$ and $X \wedge Y$ are only defined when *X* and *Y* belong to a common member of the family.

1) Quantum mechanical quantities sometimes have values, though not all quantities have values at once.
2) Bivalence fails; some statements about quantum systems are neither true nor false;
3) Disjunctions can be true even though none of their disjuncts are.
4) Unrestricted distribution of “and” over “or fails.

Perhaps 1) – 4) fit quantum systems; perhaps not. What I hope to have made plausible is that they aren’t shocking. A full discussion would call for much more detail (see Stairs 2006 for some additional thoughts) but we turn to a different question: how well does the proposal meet Kripke’s worries?

First, there’s no question of “standing outside logic” and choosing a logic. This is a case of revision in light of finding an overlooked possibility: the possibility of l-complementary propositions. On the one hand, f l-complementarity is a genuine possibility, it’s one that we came to by way of quantum mechanics, and quantum mechanics was an empirical discovery. However, grasping the implications for logic comes from reasoning about the theory and the conclusions about logic, and it would survive a change of physics. Recall the case of geometry. We can (dimly) imagine discovering that the best theory of space-time is Euclidean after all. However even if non-Euclidean geometry didn’t fit *this* world, non-Euclidean space-time would be a genuine, albeit unrealized possibility. Reasoning won’t tell us the actual geometrical structure of the world, but empirical discoveries won’t tell us what the geometrical possibilities are. Similarly, for all we can say for sure, we’ll find that the correct account of quantum phenomena is some version of Bohmian mechanics. If we do, physics would give us no reason to believe that the world exhibits l-complementarity, nor disjunctive facts, nor failures of distributivity. However, this wouldn’t undermine the *possibility* of l-complementarity, nor the *possibility* of disjunctive facts, nor the *possibility* of a world where distributivity fails. The analogy with geometry is still apt: the *possible* structures, logical or geometrical, go beyond the actual. Empirical findings may prompt us to have thoughts we wouldn't have had otherwise, but the discovery that something is a non-actual possibility is not an empirical discovery. However, the structure that the world actually instantiates – logical or geometrical – is something we can only discover empirically.

4 Coda: some loose ends and some thoughts on logic and the limits of thought

A question that often comes up in discussions of quantum logic is whether it's meant to apply universally, so to speak – whether quantum logic is 'the true logic,' to use the phrase in Bacciagaluppi's "Is Logic Empirical?" (2009) The point of view of this paper is that this is an unfortunate question. In the sense of "logic" that matters for our discussion, there's not some special "logic" called "quantum logic." On this Kripke is right.[8] The proposal, rather, is that if quantum mechanics is true, the world embodies a logical relationship that hadn't been noticed before: the one we've called l-complementarity. If so, not all propositions are bivalent and distributivity fails in certain special circumstances. Even if l-complementarity is a genuine possibility, however, it doesn't apply to every set of propositions. Compare: suppose failures of bivalence are possible because it's possible that determinism and the block universe picture both fail. That admission wouldn't call for treating all propositions as neither true nor false, nor for saying that bivalence fails in every domain.[9] The point, rather, is that something we might have taken to hold in all cases – as a matter of **logic** – holds only in some.

What's been said also doesn't take issue with the idea that our knowledge of **logic** is a matter of reasoning. That's not because this is beyond dispute. It's because a central aim of the paper was to see where things stand if we concede to Kripke that what we've labeled **logic** is a matter of reasoning. We have argued that even if

[8] This doesn't mean that there are no formal systems meant to do certain jobs connected with quantum mechanics not that no one should call such constructions "quantum logics." Those kinds of constructions simply aren't what's at issue here.
[9] In particular, to take one important example, it gives no reason at all to think that mathematical propositions are non-bivalent.

Kripke is right and **logic** is not empirical, there's still a place for empirical considerations in thinking about logic. The empirical is not about what the logical possibilities are, but about which ones are realized.

That leaves a perplexing possibility that we'll raise but not unravel. The quantum logical story sketched here sees what we've called l-complementarity as a feature of the world. The world, so this story goes, has logical structure just as surely as it has geometrical structure; a bit too cutely, logic is empirical even if **logic** isn't. However, if this is correct it has an interesting implication: we might not be capable of grasping all of what **logic** encompasses. This, in turn *could* have the consequence that we are incapable of grasping the full logical structure of the actual world.

Go back to the case of geometry. Suppose space-time indeed has the structure of a pseudo-Riemannian manifold. In order to figure this out, we needed the capacity to grasp the relevant concepts. That wasn't inevitable; after all, there are individual people who lack that capacity. Even if we had all been unable to think the right thoughts, the world would still be pseudo-Riemannian. The same goes if the l-complementarity-based account of quantum mechanics gets the character of the world right. We are, collectively, lucky enough to be able to grasp the relevant structures and concepts; collective truth though this may be, it doesn't apply to everyone and need not have applied to anyone.

However, it *might* be that the actual geometrical structure *or* the actual logical structure of the world isn't what we think it is. And it *might* be that whatever that structure is lies beyond our cognitive reach. Logic would come unpinned from reasoning in a different way than the one Kripke argued against.

One might dismiss this as a silly kind of skepticism. That would be fair if the suggestion were that we might deeply and radically ignorant about logic. However, that's not the thought. On the contrary (though we haven't discussed this) a full explication of l-complementarity assumes that propositions are sometimes related exactly as classical logic says they are. (A partial Boolean algebra, after all, is a family of Boolean algebras. Similar remarks apply to orthomodular lattices.)

The point, rather, is this. What quantum mechanics may well represent is a case in which we stumbled on a surprising exception to logical business as usual. However, a full account of l-complementarity calls for positing relationships among properties that we don't grasp easily. Studying, for example, partial Boolean algebras, as abstract mathematical structures is, of course, not the issue. The difficulty is in grasping what it means for states of affairs in the world to mirror that structure. One might fairly say that the persistent difficulty in understanding quantum mechanics has been understanding what it means for *the world* to have the structure that the mathematics seems to attribute to it. In light of this, the possibility that there might be yet more esoteric exceptions to business as usual doesn't seem quite so silly. A proper modesty suggests that there's no guarantee that we'll find them even if they exist. And a healthy suspicion about our limitations suggests there's no guarantee we would be able to recognize them even if they're there. **Logic** in its fullness just might be beyond our grasp.

References

Alston, W. (1991) *Perceiving God* Ithaca New York: Cornell University Press.

Bacciagaluppi, G. (2009) 'Is Logic Empirical?', in D. Gabbay, D. Lehmann and K. Engesser (eds), Handbook of Quantum Logic (Amsterdam: Elsevier, 2009), pp. 49-78.

Bourne, C. (2006) *A Future for Presentism*, Oxford: Clarendon Press

Caves, C. M., Fuchs, C. A., & Schack, R. (2007). Subjective probability and quantum certainty. *Studies in History and Philosophy of Modern Physics*, 38, 255.

Kochen, S. and Specker, E. (1967): "The Problem of Hidden Variables in Quantum Mechanics", *Journal of Mathematics and Mechanics* 17: 59-87.

Putnam, H. (1968) "Is logic empirical?" *Boston Studies in the Philosophy of Science*, vol. 5, eds. Robert S. Cohen and Marx W. Wartofsky. (Dordrecht: D. Reidel, 1968), pp. 216-241. Reprinted as "The Logic of Quantum Mechanics" in *Mathematics, Matter and Method* (1975), pp. 174-197.

Putnam, H. (1967), "Time and physical geometry," *Journal of Philosophy*, 64, (1967) pp. 240–247. Reprinted in *Mathematics, Matter and Method* (1975), pp. 198-205.

Stairs, A. (1983) "Quantum Logic, Realism and Value-Definiteness," Allen Stairs, *Philosophy of Science*, December, 50 no. 4 pp. 578-602.

Stairs, A. (2006) "Kriske, Tupman and Quantum Logic: the quantum logician's conundrum," in W. Demopoulos and I. Pitowsky (eds.) *Physical Theory and its Interpretation*, Springer.

Stairs, A. (2011) “A Loose and Separate Certainty: Caves, Fuchs and Schack on quantum probability one. Studies in History and Philosophy of Modern Physics 42 (2011) 158–166

Stein, H. (1968) "On Einstein-Minkowski Space-Time," *The Journal of Philosophy* ***65***: 5-23.

Thomason, Richmond H. (1970) “Indeterminist time and truth value gaps”, *Theoria* 36: 264–281, 1970