OXFORD STUDIES IN EPISTEMOLOGY

Volume 1

Edited by

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CLARENDON PRESS · OXFORD

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3. Recent Debates about the A Priori

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1. BACKGROUND

At least from the time of the ancient Greeks, most philosophers have held that some of our knowledge is independent of experience, or ''a priori''. Indeed, a major tenet of the rationalist tradition in philosophy was that a great deal of our knowledge had this character: even Kant, a critic of some of the overblown claims of rationalism, thought that the structure of space could be known a priori, as could many of the fundamental principles of physics; and Hegel is reputed to have claimed to have deduced on a priori grounds that the number of planets is exactly five.

There was however a strong alternative tradition, empiricism, which was skeptical of our ability to know such things completely independent of experience. For the most part this tradition did not deny the existence of a priori knowledge altogether, since mathematics and logic and a few other things seemed knowable a priori; but it did try to drastically limit the scope of a priori knowledge, to what Hume called ''relations of ideas'' (as opposed to ''matters of fact'') and what came later to be called ''analytic'' (as opposed to ''synthetic'') truths. A priori knowledge of analytic truths was thought unpuzzling, because it seemed to admit a deflationary explanation: if mathematical claims just stated ''relations among our ideas'' rather than ''matters of fact'', our ability to know them independent of experience seemed unsurprising. So, up until the mid-twentieth century, a major tenet of the empiricism was that there can be no a priori knowledge of synthetic (non-analytic) truths.

But in a series of influential articles, W. V. Quine (1936, 1951*a*, 1951*b*) cast a skeptical eye on the manner in which the empiricists of his day were trying to explain a priori knowledge of logic and mathematics. His critiques led some (including Quine himself) to a full-blown empiricism in which there is no a priori knowledge at all, not even in logic and mathematics. Others (Bonjour 1998) were led in the opposite direction, to a fuller-blooded rationalism: since logic and mathematics seem obviously a priori, and the empiricist attempts to explain this away seem dubious, we must conclude that the mind just does have the power to ascertain ''matters of fact'' independent of experience, perhaps by a faculty of rational intuition. Still others (Boghossian 2000; Peacocke 2000) have tried to base a priori knowledge on meaning in a subtler way than the empiricists did, so as to evade Quine's critique while avoiding the need for a full-blooded rationalism; and another strategy for accepting a priori knowledge while avoiding full-blooded rationalism will be mentioned below.

This is the cluster of issues to bear in mind in what follows.

2. A PRIORITY: WEAK AND STRONG, DOGMATIC AND UNDOGMATIC

We need to be a bit more precise about what 'a priori knowledge' means. Presumably someone knows that p only if p is true, she believes it, and she is *entitled* to believe it; the issue of a priority concerns the kind of entitlement that is in question. Roughly speaking, a priori entitlement is entitlement that is independent of experience.

But what is it for one's entitlement to be independent of experience? There are at least three issues here.

(i) Having the belief that p requires that we have the concepts involved in p. Experience is involved in the acquisition of concepts; if there is to be any realistic chance of there being a priori knowledge, experience involved merely in the acquisition of the concepts must ''not count''. Just how to allow it not to count is not entirely clear, especially given that learning concepts often involves the acquisition of information.

(ii) Proofs in logic and mathematics would seem to confer a priori entitlement if anything does. But long proofs need to be carefully checked, which normally involves looking at the written proof, and perhaps asking others to look for errors. Experience is clearly involved here, but this sort of experiential involvement must also ''not count''.

These first two issues will not be my concern here. They do point to the need of clarification, and it is certainly a respectable position that adequate clarification is impossible to come by and that the concept of a priority is so hopelessly obscure that it ought to be simply abandoned. But let us make the working hypothesis that it is possible to clarify the concept in a way that doesn't either rule out a priority trivially or make it uninterestingly weak.

More important to the subsequent discussion is

(iii) 'Entitled independent of experience' is ambiguous. On a weak construal, to say that a person is entitled to believe that p, independent of experience, means only that she is entitled and *none of the experiences she's had* figure in making her so. On a strong construal, it also requires (roughly) that the fact that she *hasn't* had certain experiences plays no role in making her entitled: it requires that no experience she might come to have could defeat the entitlement.

The stronger notion is the more philosophically important: the philosophical interest of the claim that Euclidean geometry or classical logic is a priori would be much reduced if that claim were taken to be compatible with the claim that experience might undermine them. (When we come to consider the possibility of "default entitlement" in §§ 3 and 7, we'll see that the philosophical interest of weak a priority is indeed quite minimal.)

Why the parenthetical qualifier 'roughly' in stating the strong construal? Because of a problem analogous to that under (ii). Suppose we have carefully worked through a correct mathematical proof and thoroughly understand it. Still, as Kitcher 1983 (ch. 1) has observed, it is possible that we might acquire (misleading) evidence that we were suffering delusions every time we went through the proof, and/or that every respected mathematician regarded our "proof" as demonstrably fallacious. Few if any a priorists would deny that such experiences are possible and that they could undermine our entitlement, so we ought to rule them as "not counting" as regards strong a priority (or else stick to weak a priority). A rough stab at explaining why they shouldn't count—doubtless inadequate—is to put the empirical unrevisability requirement as follows: there is no possible empirical evidence against p which is "direct" as opposed to going via evidence of the reliability or unreliability of those who believe or disbelieve p. Whatever the merits of this, the problem of explaining why the experiences shouldn't count seems no worse than the analogous problems for weak a priority that arose under (i) and (ii).

Note that for p to be strongly a priori, it is not required that p be rationally unrevisable: if thought alone, unaided by evidence, could rationally undermine the belief in p, that has no bearing on the a priority of our entitlement to believe that p. (It needn't even undermine a priori *knowledge* that p, since the undermining thought could be rational but erroneous.) What is required for a priority is only that p be unrevisable *on empirical grounds*. But even this could mislead, for it is slightly ambiguous: it means, of course, that it is *possible* to rationally revise the belief on empirical grounds, but the question is whether we understand the possibility as ''genuine'' or ''merely epistemic''. The merely epistemic conception of possibility is the one we use when we say ''It is possible, though unlikely, that standard set theory might be inconsistent''. What we're saying is: this is something we can't totally rule out. By contrast, the ''genuine possibility'' that set theory is inconsistent would entail that set theory actually is inconsistent: for if set theory is consistent (i.e. it is not possible to derive an inconsistency in it), then it is necessarily consistent. Most of us believe set theory to be consistent, hence believe there to be no ''genuine possibility'' of its being inconsistent; but we are undogmatic in this belief, in that we believe that there are conceivable developments (e.g. the derivation of a contradiction within it) that would lead us to alter our opinions.

On the epistemic interpretation, strong a priority would involve the claim that there is no epistemic possibility whatever of revising our mathematics or logic on empirical grounds. Adhering to such an incredibly strong claim would seem like pure dogmatism: we may not be able to see how our mathematics or our logic could be revised on empirical grounds, but the track record of philosophers' pronouncements on epistemological matters is hardly good enough to inspire complete confidence that we might not be overlooking something. I take strong a priority in this dogmatic version to be indefensible: there's no way to *completely rule out* revising logic as the best way of accommodating, say, quantum mechanical evidence, even if we can't conceive the details of how such a revision would go.

But on the ''genuine possibility'' interpretation, no such dogmatism is required: the claim is merely that good standards of evidence wouldn't allow for empirical evidence against mathematics or logic, in any genuinely possible circumstances. Someone could believe this claim strongly, while retaining the fallibilist attitude that for this belief, as for all others, there is an epistemic possibility of someday rationally revising it.

3. PHYSICAL GEOMETRY

Why have philosophers often been skeptical of claims to a priority? In the case of claims about the number of planets, or even about the structure of physical space, it just seems obvious to many people that these aren't the sort of things that we could possibly be entitled to believe without evidence. But that is less an argument than an assertion of the doctrine to be argued.

Something closer to an argument can be given in the case of strong a priority (even of the undogmatic kind). Consider Euclidean geometry (viewed as a theory of physical space, which I believe is how it was almost universally viewed until at least the mid-nineteenth century). Despite the fact that none of its axioms is based on empirical evidence in any obvious way, still the system as a whole does have consequences that might be questioned on empirical grounds. An obvious example is that the surface area of a sphere is 4π times the square of the radius: it seems possible to imagine finding an object, verifying by repeated careful measurements that every point on its surface is indeed the same distance from a certain point (the center), and verifying by repeated careful measurements that the surface area was very different from 4π times the square of the radius (different by an amount far greater than could plausibly be attributed to systematic experimental error). Of course, we could explain this away by far-out hypotheses (systematic but undetectable expansion of our measuring rods, deceptive demons who made us misread the instruments, and so forth), but the possibility of saving a claim from empirical refutation by such far-out hypothesis is not normally thought to make that claim non-empirical, so why should it here?

Does this argument also go against the weak a priority of geometry? It would to someone who thought that you couldn't be entitled to believe a claim without having evidence for each of its empirical consequences, but that is generally implausible, and seems implausible even in the case at hand since most of us would think that Euclid was eminently entitled to his geometric beliefs without having made careful measurements of space.

Indeed, it isn't clear that we should doubt the weak a priority of geometry. It is not unreasonable to think that evolution might have endowed us with a tendency to believe Euclidean claims, barring evidence to the contrary, even in absence of arguments for them; and it isn't clear why, if this is true, such beliefs shouldn't count as ones we're entitled to when there is no evidence against them. (And why they shouldn't count as known, if in addition they are true.) If so, this would seem to be a case of weak a priority. Perhaps claims to *weak* a priority shouldn't be regarded as such a big deal.

The suggestion here—which will play an important role in § 7—is that some of our beliefs count as entitled *by default*: we need no positive reason for them, experiential or otherwise, to count as entitled to believe them. If these beliefs are empirically undeterminable they won't be a priori in the interesting strong sense, but they are trivial cases of the weak a priori.

''But where does this default entitlement come from?'' It needn't ''come from'' anywhere: entitlement isn't a fluid whose creation needs explanation. Probably the best view is that we simply have an attitude of regarding some beliefs as entitled under some circumstances, others not; and we regard some of them as entitled in absence of evidence for or against, even though there might someday be evidence that disconfirms them. And to put it crudely, there are no ''facts about entitlement'', there is nothing beyond these attitudes; we can evaluate attitudes as good or bad, but such evaluation is not a ''factual'' enterprise.

4. LOGIC, MATHEMATICS, AND METHODOLOGY

Though physical geometry seems not to be a domain of strongly a priori knowledge, there are other candidates that fare better. Perhaps the best candidates are logic and pure mathematics. The reason for thinking of these as strongly a priori is evident: they don't seem to be based on empirical evidence, and it is hard to see how empirical evidence could undermine them. What possible empirical evidence could undermine the logical belief that *if snails exist then snails exist,* in the way that evidence of spatial measurements could and did undermine Euclidean geometry as a theory of physical space? I'll return to this in a moment.

Another case worth mentioning is empirical methodology itself: there are reasons for thinking that empirical methodology is strongly a priori, in the sense that its rules are rationally employable independent of evidence and can't be undermined by evidence. The impossibility of undermining evidence may be less evident in the case of empirical methodology than in the case of logic and mathematics. Presumably our empirical methodology includes a bias for simplicity. We recognize that in so far as we can account for all past and present observations by our present body of theory T, we could account for it equally well by an alternative theory T* according to which T holds until 1 January 2004, after which Aristotelian physics, Lamarckian biology, etc., take over. Why do we rule out T*, and base our predictions instead on the approximate truth of T? We certainly have no evidence favoring T over T* (since they yield exactly the same probabilities for everything in the present and past), so presumably it's that T is a vastly simpler way of accommodating our evidence than is T*. But now it might seem that our methodology of choosing the simpler is empirically revisable (either by revising the principle ''choose the simpler'' or by revising the simplicity judgements that give this slogan its content). Suppose we had evidence that in each past year on New Year's Day, the laws of nature drastically changed; that would seem like good inductive evidence that they'd change on New Year's Day in 2004 too. Doesn't this show that our empirical methodology (our system of simplicity judgements and the methodological import we give them) is itself empirically revisable?

No, it doesn't show this at all. What it shows is only that we regard theories T** according to which the laws of nature change every year as more plausible than corresponding theories T*** according to which the laws change every year until 2004, but don't change then. It seems that we have two pre-existing biases: one for T over T*, which licenses a belief that the laws won't change in 2004 given evidence that they haven't changed in the past; the other for T** over T***, which would license a belief that the laws will change in 2004 were we to be given evidence that they have changed each year in the past. So the fact that the laws of nature haven't changed drastically in the past is indeed inductive evidence that they won't change drastically in 2004; but this fact is based on a fixed bias (for T over T* and for T** over T***) which there is no obvious way to undermine by empirical evidence.

In the case of mathematics, it is hard to come up with even a prima-facie case for the evidence-based revision of an accepted theory (say, the theory of real numbers). We could, to be sure, imagine discovering that the structure of physical space was not accurately describable (even locally) as a product space of the real numbers; perhaps physical space is discrete, or countable, or whatever. But this would surely not be best thought of as showing that the theory of real numbers is wrong, but only that that theory is inapplicable to physical space.

The case of logic is more interesting, for there have been proposals to revise logic in order to solve certain anomalies in quantum mechanics, and the proposals have at least some prima-facie attractiveness even though no clear sketch has been given of just how the development of quantum mechanics in such a logic would go. As noted before, it seems dogmatic to insist in advance that there is no epistemic possibility that a case for such a revision could ever be made compelling; on the other hand, we certainly do not now understand even what it would be like to use such revised logics as our sole logic, let alone understand just how the case for switching from classical logic to the revised logic would be rationally argued, so these proposals do not yet constitute an argument that there is a genuine possibility of rationally revising logic on the basis of quantum mechanical considerations.

But suppose that such a proposal could be worked out in detail, and could lead to a rational revision in logic. Would that revision be on empirical grounds? It's hard to say: perhaps it would be a case showing logic to be revisable on the basis of quantum mechanical evidence, but perhaps it would be a case where quantum mechanical considerations pointed up the need for a conceptual revision that could have been made independent of evidence. (Consider someone who is led to a logic that allows for negative existence claims involving names by the empirical discovery that there is no Santa Claus.) It seems idle to speculate whether, were it possible to work out the details of the case, the revision would be empirical or conceptual: that's rather like the question of what features an inconsistency in set theory is likely to have should one be discovered. In each case there's no way to answer the question, absent a clearer idea of what the alleged possibility might be like.

If it does make sense to suppose that logic might be rationally revised on empirical grounds, that might give reason to think that mathematics could too: after all, proof in mathematics goes via logic! To my mind this is the only serious possibility for revising mathematics empirically. But even here, it is not obvious that we would have a case for an empirical revision of mathematics, for not all revisions of logic would be relevant to mathematics. Consider proposals to revise logic on non-empirical grounds, for example, the proposal to abandon the law of excluded middle (*B or not B*) as a general principle so as to deal with the Liar paradox. Such proposals allow keeping all instances of excluded middle that don't involve 'true' (or other predicates that give rise to analogous pathologies), and in particular excluded middle can be assumed for all mathematical sentences (though it may be demoted to the status of a *non-logical* axiom schema). No revision of mathematics need ensue. The same point would seem to arise for a revision of logic on empirical grounds, if that is possible: if experience tells us that the distributive law doesn't apply generally, still it may (not as a matter of logic but for other reasons) apply to many special objects (e.g. those that can't undergo quantum superpositions), and mathematical objects seem like very good candidates for being among those to which the distributive law would still apply.

5. THE BENACERRAF PROBLEM FOR MATHEMATICS

Even in cases, like mathematics, where strong a priori knowledge (of an undogmatic sort) seems highly plausible, there are puzzles about it. The most famous one was articulated by Benacerraf (1973). (He raised it not as a problem specifically about *a priori* knowledge of mathematics, but about *any* sort of knowledge of mathematics; but those who take knowledge of mathematics to be empirical, e.g. Hart (1996), often claim that by doing so they have a way around the argument.)

I will not consider Benacerraf's own formulation—it relies on a causal theory of knowledge that simply seems inapplicable to a priori knowledge—but rather, will try to capture its general spirit. The key point, I think, is that our belief in a theory should be undermined if the theory requires that it would be a huge coincidence if what we believed about its subject matter were correct. But mathematical theories, taken at face value, postulate mathematical objects that are mind-independent and bear no causal or spatiotemporal relations to us, or any other kinds of relations to us that would explain why our beliefs about them tend to be correct; it seems hard to give any account of our beliefs about these mathematical objects that doesn't make the correctness of the beliefs a huge coincidence.

Of course, no one would propose that we reject mathematics on the basis of such arguments; Benacerraf's point was simply to raise a puzzle about why not. There are various answers to this that seem satisfactory. Some of these (e.g. Field 1989; Yablo 2000) involve fictionalism about mathematics: on these it is simply not the function of mathematical theories to be true, so the puzzle just doesn't arise. (So we have no knowledge at all of mathematics, a priori or otherwise.) Others

(Balaguer 1995; Putnam 1980; perhaps Carnap 1950) solve the problem by articulating views on which though mathematical objects are mind-independent, any view we had had of them would have been correct. (In Balaguer's case that's because the mathematical universe is so plenitudinous that, whatever view we had had of it, there is some part of the mathematical universe of which it would have been true; and we are talking about whichever part makes our theory true.[1]) Unlike fictionalist views, these views allow for a priori knowledge in mathematics, and unlike more standard Platonist views, they seem to give an intelligible explanation of it.

Those who argue that Benacerraf's problem doesn't arise for the empiricist seem in considerably worse shape: although they say that empirical evidence bears on mathematical claims, they have not offered (and could not easily offer) even a clear sketch of how the experiences that allegedly might overturn our mathematics are reliable symptoms of the facts about mathematical objects. The problem isn't the indirectness of the evidence, or the fact that its being evidence depends on theoretical assumptions: evidence for black holes shares these characteristics, but raises no Benacerraf problem because there's a straightforward causal story that explains the correlation between the facts about black holes and the evidence for them. In the mathematical case such a story is lacking, which seems embarrassing to an empiricist view.

6. A BENACERRAF-LIKE PROBLEM FOR LOGIC?

Many philosophers think that to whatever extent there is a Benacerraf problem for mathematics, there is also one for logic: the fact that mathematics deals with special objects and logic doesn't is, in their view, an irrelevant difference. At first blush this seems reasonable: the worry would seem to be that there is no obvious explanation of how our logical beliefs can depend on the logical facts, and this should engender skepticism that they do depend on the logical facts. It would seem that only a huge coincidence could have made our logical beliefs accurately reflect the logical facts.

[1] In Putnam's case, it is because there is no constraint on the extension of our mathematical predicates other than that it be such as to make our mathematical beliefs true; so that they are bound to be true as long as there are infinitely many mathematical objects. Carnap's view is open to more than one interpretation.

This isn't really an optimal formulation of the problem about logic. After all, logic seems primarily concerned not with ''logical beliefs'' but with *inferences*.[2] Inferences connect claims (not primarily about logic) to other claims (not primarily about logic); they involve *conditional commitments*, which are distinct from beliefs. So ''logical beliefs'' don't enter the picture in any very direct way.[3] It would be better, then, to put the Benacerraf problem in terms of the lack of an explanation of how our logical *inferences* depend on the logical facts. And here we should presumably take the logical facts to involve meta-properties of the inference: for example, the fact that the inference is (necessarily) truth-preserving. To say that the inference from *A* to *A or B* is (necessarily) truth-preserving just means that (necessarily) if *A* is true then so is *A or B*; on a minimal notion of truth, that's just equivalent to the claim that (necessarily) *if A then either A or B*.

So the way to put a Benacerraf problem for logic is something like the following:

(i) it seems in principle impossible to explain such things as how our acceptance of the inference from *A* to *A or B* depends on the logical fact that necessarily *if A then either A or B*;[4]

(ii) without such an explanation, to believe in a correlation between our accepting the inferences we do and the logical facts requires belief in a massive coincidence;

(iii) the need to believe in such a massive coincidence undermines the belief in the correlation, which in turn should undermine our acceptance of the inference.

[2] Harman (1973) questions this, on the basis of the fact that when an argument leads us from antecedently believed premises to an antecedently disbelieved conclusion we may reject a premise rather than accept the conclusion. The view of inference in the next sentence is designed to accommodate his point.

[3] They may enter indirectly, in one of two ways. First, in classical logic and most of the popular alternatives to it, some claims are assertible without premises: e.g. in classical logic any claim of form *A or not A*, and in most logics any claim of form *If A, then either A or B*. If we employ a logic of this sort, these will be ''logical beliefs''. Second, we can take ''logical beliefs'' to mean meta-claims about the inferences involved: e.g. the claim that the inference is valid, or necessarily truth-preserving, or necessarily preserving of some other semantic status. But in either case, the logical beliefs seem to have a status secondary to the inferential behavior.

[4] There are logics in which one can accept the inference from $X_1, \ldots, X_n$ to Y without accepting the claim that *if* X_1 *and . . . and* X_n *then* Y. In those logics, one does not accept the claim that the inferences in the logic are truth-preserving on a minimal notion of truth, and so if a Benacerraf problem can be raised at all it must be raised in a different way.

At first blush this may seem as compelling as the corresponding problem about mathematical objects is (on the naive Platonist picture for which the Benacerraf problem is genuinely a problem).

At second blush, the logical case seems very different from the mathematical case. For in the logical case, isn't it clear that evolution provides the answer? Isn't it clear that the correct logical beliefs are selected for (i.e. creatures whose logical beliefs didn't reflect the logical facts would die out)? In the mathematical case, on the other hand, it is hard to see how such selection could work: given that mathematical objects have no causal, spatio-temporal, etc. relations to us, what mechanisms could select for correctness of beliefs in that case?

At third blush, though, the evolutionary explanation is not obviously satisfactory in the logical case either. For in the mathematical case, it isn't in principle problematic to see how *a particular* mathematical theory *T* might have been selected for: perhaps belief in *T* leads to a subtle odor which our predators found repugnant. What is problematic is to figure out the connection between what is selected for and the actual mathematical facts. Doesn't this affect the situation for logic too? We could easily tell some sort of story (at least as plausible as the one about repugnant odors!) on which there were selection pressures for the acceptance of classical logic. But what we need is a story on which there is a selection pressure for acceptance of *the correct logic, whichever one that happens to be*. And it isn't so obvious that we can do that, so the Benacerraf problem for logic seems to remain.

At fourth blush (Field 1998), one might question the distinction between

(i) selection pressure for acceptance of a given logic, which is in fact correct,

and

(ii) selection pressure for acceptance of the correct logic, whichever one that happens to be.

In the mathematical case, such a distinction seems quite clear: we can see that in the odor story the mathematical facts themselves played no role in our survival (it isn't as if they had a relevant role in producing the odor), so in this case there is no doubt that the selection pressure was for the acceptance of a particular theory rather than for whichever one is true. But part of what makes this clear is that we can assume, with at least some degree of clarity, a world without mathematical objects, or a world in which the particular theory *T* of them that we happen to believe in isn't true; and with ordinary logic we can then argue that the belief in *T* would still produce odors, so that the theory selected for would be a false one. But how are we to argue what would be selected for in a world with an alternative logic? We would apparently need to conduct the argument in the alternative logic in question, and we have so little idea how to do this that the counterfactual begins to look nonsensical. This casts serious doubt on the intelligibility of the distinction between (i) and (ii).

If that and similar distinctions really are unintelligible, that may itself provide an answer to the Benacerraf problem for logic, though not an evolutionary one (Field 1998). The Benacerraf problem in mathematics or logic seems to arise from the thought that we would have had exactly the same mathematical or logical beliefs, even if the mathematical or logical facts were different; because of this, it can only be a coincidence if our mathematical or logical beliefs are right, and this undermines those beliefs. In the mathematical case there is a reasonably clear content (at least prima facie) to the thought that we would have had exactly the same mathematical beliefs even if the mathematical facts were different; that's what gives the Benacerraf problem its initial bite in the mathematical case. But in the logical case, we have no idea how to determine what we would have believed had the logical facts been different: reasoning about what our beliefs would be in alternative circumstances requires logic, and if we contemplate a radically altered logic we have no idea how to conduct the reasoning. This seems to undermine the intelligibility of the counterfactual (about what we would have believed given different logical facts); in which case we have undermined, not just the evolutionary solution to the Benacerraf problem for logic, but the problem itself.

7. Justification, Disagreement, and Meaning

Many of our beliefs and inferential rules in mathematics, logic, and methodology can be argued for from more basic beliefs and rules, without any circularity. But this is not so for the most basic beliefs and rules: we must be, in a sense, entitled to them by default. At the end of § 3 it was suggested that we don't have to regard our being default-entitled to them as a mysterious metaphysical phenomenon: it's basically just that we *regard it as* legitimate to have these beliefs and employ these rules, even in the absence of argument for them, and that we have no other commitments that entail that we should not so regard them. (Of course, there are things we can say about *why* we regard it as legitimate to have these beliefs and employ these rules, and why anyone who didn't would be worse off; but the things we can say would be disputed by anyone who didn't have those beliefs and employ those rules, so the justification is circular. The circularity is broken by our attitudes—by what we *regard as* legitimate. See Field 2000 for more details.)

Many philosophers think more needs to be said to explain default-entitlement: they think that the only way we can be entitled to anything is for there to be some ''source'' for the entitlement, and since basic features of our logic and empirical methodology and perhaps mathematics can't have their source in a non-circular argument for them, they must have some other kind of source. One possibility (Boghossian 2000; Peacocke 2000) is that the meanings of our concepts serve as the desired source of entitlement.

At least in the case of logic and of empirical methodology, a prima-facie reason for thinking a source of entitlement needed is the possibility of alternative views that are in genuine conflict. The possibility of genuine conflict is clear in the case of empirical methodology: our broadly inductive methodology conflicts with counterinductive methodologies, and with skeptical methodologies that don't license the belief in anything not yet observed, and with innumerable methodologies that while broadly inductive also differ in the extent of the conclusions licensed about certain matters. There seems to be an issue as to whose empirical methodology is *more reasonable*. We presumably think ours the more reasonable, but they think the same of theirs; if ours *really is* more reasonable, doesn't there need to be a source of this reasonableness? Doesn't there need to be some kind of non-question-begging justification, even if in a sense of justification in which justifications needn't be arguments?

In the case of mathematics there may be no such genuine conflict between alternative theories (at least when the alternative theories are not based on different logical views): it's natural to think that different mathematical theories, if both consistent, are simply about different subjects. (That's why the pluralist views of Balaguer and Putnam, cited earlier, are as plausible as they are.) Because of this, the need for justification (other than justification of the consistency of the theory) doesn't seem as pressing in the mathematical case. Or maybe, instead of lessening the need for justification, it means that the justification for consistent mathematical theories comes relatively cheap: by the purely logical knowledge that the theory is consistent. One way to develop this idea is to say that the axioms implicitly define the mathematical terms, and that consistent implicit definition in mathematics guarantees truth, so that only justification of the consistency of the theory is required.

But logic seems more like inductive methodology than like mathematics in this regard. In the first place, an implicit definition approach seems to face a serious limitation in the case of logic: it is only *consistent* implicit definition that could with any plausibility be held to guarantee truth, so we need an antecedent notion of consistency not generated by implicit definition; and what justifies a belief about consistency? (Admittedly, the notion of consistency required here may be one on which proponents of different logics may agree, so if this were the only point to be made it might seem that the implicit definition strategy could at least serve as a justification of the parts of logic about which controversies are likely.)

A more fundamental point is that those who advocate the use of alternative logics (and advocate them as more than just algebras for dealing with special subjects, but as systems for general reasoning) seem to be in genuine disagreement with us. There seems to be an issue as to which view is right (or at the very least, as to which is better); one that can't be removed by simply saying ''They're using their concepts, we're using ours''.

There are cases where this isn't so: for instance, someone might agree with us that there's no way for an argument from *A and not A* to *B* not to be truth-preserving, but call the argument invalid nonetheless *simply because it fails to respect some relevance condition that she imposes on consequence*. Here the disagreement seems to be a purely verbal one about the meaning of 'consequence'. But I take such cases not to be the interesting ones. What would be interesting is if someone rejected the rule because she thought it wasn't truth-preserving: that's the view of ''dialetheists'' (Priest 1998), who think that some claims of form *A and not A* are true. Dialetheists do seem to be in *genuine* (not merely verbal) disagreement with advocates of classical logic. So do ''fuzzy logicians'', who refuse to accept ''Either Harry is bald or he isn't'' in cases where Harry seems a borderline case. (''Fuzzy logic'' can be regarded as a weakening of classical logic; it yields full classical logic when the law of excluded middle, *B or not B*, is added.[5])

Admittedly, locating what it is that proponents of different logics disagree about is tricky. For instance, the advocate of classical logic and the ''fuzzy logician'' both agree that full classical logic, including excluded middle, is valid with respect to the standard two-valued semantics; they also agree that fuzzy logic is valid with respect to the Lukasiewicz continuum-valued semantics and that classical logic isn't. Where then do they disagree? Do they disagree as to whether instances of excluded middle are true? Not really: on a minimal notion of truth, a fuzzy logician won't deny the truth of any instance of excluded middle, he'll just refrain from asserting some. Do they disagree as to whether instances of excluded middle are *necessarily* true? If that's all we can say, it's hard to see why the distinction isn't verbal: the fuzzy logician might just be employing a more restrictive notion of necessity. I think in the end the only way to make sense of the distinction is in terms of the laws they take to govern rational belief: for example, the fuzzy logician is willing to tolerate having a low degree of belief in instances of excluded middle, the classical logician isn't.

But again: if we are entitled to take a stand one way or the other on this (either following the fuzzy logician in tolerating a low degree of belief in excluded middle, or following the classical logician in not tolerating it and using the assumption of excluded middle in reasoning), mustn't there be a source for this entitlement? But what can it be?

It is sometimes claimed that meaning provides such entitlement. There are two conceptions of meaning one might invoke: truth-theoretic and inferential role. An advocate of the law of excluded middle might ''justify'' this using a truth-theoretic conception of meaning as follows:

> If *B* is true then *B or not B* is certainly true. And if *B* is not true then *not B* is true, so again *B or not B* is true. So either way, *B or not B* is true.

But this is grossly circular: the 'So either way' disguises a use of excluded middle at the meta-level, that is, it assumes that *B* is either

[5] Of course, if we take ''logic'' to include attributions of logical truth and their denials, then fuzzy logic is no weakening of classical logic: it conflicts with classical logic in claiming that instances of excluded middle are not logical truths.

true or not true. The fact is that the advocate of classical logic and of fuzzy logic can agree on the same compositional rules about truth; whether these laws make all instances of excluded middle come out true depend on whether one assumes excluded middle. A similar point holds for inferential rules, like modus ponens or the inference from *A and not A* to *B*: the truth rules guarantee that the inference is truth-preserving *if you assume a logic that employs the rule*, but don't guarantee this otherwise. (Note that the circularity for inferential rules seems no less noxious than that for beliefs, contrary to some proponents of ''rule-circular justifications''.)

The way that an inferential semantics would provide a justification or source of entitlement is different: here the claim would be (i) that the acceptance of certain logical beliefs or inferences is central to the meanings of the connectives, and (ii) that this somehow guarantees the legitimacy of those beliefs or inferences.

If this is to help with our example, the law of excluded middle must be one of those that are central to the meanings of 'or' and 'not'. Moreover, for (i) to support (ii) it must be interpreted to require that any alteration of the beliefs and inferences that are central to the meaning of the connective engenders a change in the meaning of the connective. On this interpretation, (i) is somewhat questionable: certainly a classical logician would have no better translation of a fuzzy logician's 'not' or 'or' into his own idiolect than the homophonic translations 'not' and 'or'. At any rate, if this is a change of meaning, it is not what Putnam (1969) called a ''mere change of meaning'', a mere relabeling: rather, the fuzzy logician would have to be seen as regarding the use of connectives with the classical meanings as *illegitimate*, and substituting new connectives that her opponent takes to be illegitimate in their place.

In any case, the key issue is (ii): why should the fact, if it is one, that certain beliefs or inferences are integral to the meaning of a concept show that those principles are correct? Why should the fact, if it is one, that abandoning those beliefs or inferences would require a change of meaning show that we shouldn't abandon those beliefs or inferences? Maybe the meaning we've attached to these terms is a bad one that is irremediably bound up with error, and truth can only be achieved by abandoning those meanings in favor of different ones (that resemble them in key respects but avoid the irremediable error).

There is reason to think that this must be a possibility: in earlier days if not now, the principles of the naive theory of truth were probably central to the meaning of the term 'true' and the principles of classical logic central to the meaning of the connectives; but we know now that we can't consistently maintain *both* naive truth theory and classical logic, so at least some of the meanings we attached to our terms *must* have been bound up with error.

I doubt, then, that the appeal to the meaning of logical terms really serves the justificatory purpose to which some have tried to put it. It's worth remarking that any argument for thinking that we need a source of entitlement for our basic logical principles would seem to be a special case of a more general argument that we need a source of entitlement for all of our basic methodological principles, for instance our inductive rules. There, too, alternative rules are possible: not just counterinductive rules, but alternative rules with a broadly inductive character yet significantly different in details. (They might, for instance, allow for more rapid inductions to the next instance, and slower inductions to generalizations.) Here too it seems impossible to straightforwardly argue for one inductive rule over the other; and here the idea that one rule can be validated over another by being integral to the meaning of some of our concepts seems even less promising.

What, then, is involved in justifying a logic, or an inductive policy? To repeat two points:

(A) Our entitlement to use a logic or inductive policy can't depend on our having an argument for it; we are entitled ''by default''.

(B) Nor need our entitlement depend on there being some kind of justification other than argument (''source of entitlement''). The entitlement doesn't ''flow out of'' anything; in saying that we're default-entitled to our logic and methodology, I'm merely expressing an attitude of approval toward the use of the logic or methodology even by those who have no arguments on their behalf.

But to add a new point:

(C) There is still room for justification: questions of justification can arise when considerations are advanced *against* our logic or methodology. For instance, it is certainly possible to argue (whether persuasively or not, I won't here consider) that classical logic runs into trouble in dealing with certain domains, such as vagueness or the semantic paradoxes; and defending classical logic against such arguments is one form of justification. Positing

> our default entitlement to, say, the rules of classical logic by no means makes classical logic sacrosanct, it merely allows classical reasoning to be legitimate until a world view sufficient for reasonable debates about the principles of classical reasoning has been built.

There are puzzles about how debates about logic and methodology are ultimately to be conducted, puzzles that are beyond the scope of the present article. I think that the debates involve quite holistic considerations: the consequences of changing logical opinions, for example, about excluded middle, can be far-reaching, and we need to look at quite diverse consequences of the change and decide whether the benefits of the change to our overall world-view would outweigh the costs. As Quine (1951*a*) pointed out in response to Carnap, the fact that such debates are pragmatic does not preclude them from being *factual*: all high-level factual debates are pragmatic in this sense. Indeed, in the case of logic it is hard to see how such debates could be regarded as anything but factual. But the fact that debates about logic and methodology are holistic and pragmatic does not show that such debates are in any way *empirical*; and as argued in § 4, it is very hard to imagine how empirical evidence could be deemed relevant to such debates.[6]

REFERENCES AND FURTHER READING

Balaguer, M. (1995) 'A Platonist Epistemology', *Synthese*, 103: 303–25.

Benacerraf, P. (1973) 'Mathematical Truth', in Benacerraf and Putnam (1983: 403–20).

—— and H. Putnam (1983) *Philosophy of Mathematics: Selected Readings*, 2nd edn. (Cambridge: Cambridge University Press).

Boghossian, P. (2000) 'Knowledge of Logic', in Boghossian and Peacocke (2000: 229–54).

—— and C. Peacocke (2000) *New Essays on the A Priori* (Oxford: Oxford University Press).

BonJour, L. (1998) *In Defense of Pure Reason* (Cambridge: Cambridge University Press).

Carnap, R. (1950) 'Empiricism, Semantics and Ontology', in Benacerraf and Putnam (1983: 241–57).

[6] I thank Paul Boghossian, Paul Horwich, Chris Peacocke, and Stephen Schiffer for useful discussions.

Field, H. (1989) *Realism, Mathematics and Modality* (Oxford: Blackwell).
—— (1998) 'Epistemological Nonfactualism and the A Prioricity of Logic', *Philosophical Studies*, 92: 1–24.
—— (2000) 'A Priority as an Evaluative Notion', in Boghossian and Peacocke (2000: 117–49).
Harman, G. (1973) *Thought* (Princeton: Princeton University Press).
Hart, W. D. (1996) 'Introduction' to Hart (ed.), *The Philosophy of Mathematics* (Oxford: Oxford University Press).
Kitcher, P. (1983) *The Nature of Mathematical Knowledge* (Oxford: Oxford University Press).
Peacocke, C. (2000) 'Explaining the A Priori: The Program of Moderate Rationalism', in Boghossian and Peacocke (2000: 255–85).
Priest, G. (1998) 'What is So Bad about Contradictions?', *Journal of Philosophy*, 95: 410–26.
Putnam, H. (1969) 'Is Logic Empirical?', in R. Cohen and M. Wartofsky (eds.), *Boston Studies in the Philosophy of Science*, 5: 199–215.
—— (1980) 'Models and Reality', in Benacerraf and Putnam (1983: 421–44).
Quine, W. V. (1936) 'Truth by Convention', in Benacerraf and Putnam (1983: 329–54).
—— (1951*a*) 'Carnap on Logical Truth', in Benacerraf and Putnam (1983: 355–76).
—— (1951*b*) 'Two Dogmas of Empiricism', *Philosophical Review*, 60: 20–43.
Yablo, S. (2000) 'Apriority and Existence', in Boghossian and Peacocke (2000: 197–228).