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NATURALISM AND THE A PRIORI

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In *Coming to Our Senses* (1996), I argue for and apply a naturalistic semantic methodology to defend an anti-holistic truth-referential view of meaning. The naturalism in question is an epistemological doctrine that I take from Quine: there is only one way of knowing, the empirical way that is the basis of science (whatever that way may be). So I reject “a priori knowledge.” I do not give a detailed argument for my rejection but I do give two reasons (2.2): Briefly, first, with the recognition of the holistic nature of confirmation, we lack a strong motivation for thinking that mathematics and logic are immune from empirical revision; and, second, the idea of a priori knowledge is deeply obscure, as the history of failed attempts to explain it show.

In this paper I will defend this view of the a priori from the criticisms of two other naturalistically inclined philosophers with whom I am usually in a great deal of agreement, Georges Rey (in this volume, 1998b; also 1993) and Hartry Field (in this volume, 1998; also 1996). Rey disagrees with me explicitly and sharply. Field does so implicitly and less sharply.¹

I. REY'S RELIABLIST A PRIORI

Rey rightly insists “that whether or not there is a priori knowledge is an empirical issue” (1998b, p. 25); on my view, *every* issue is an empirical one. But he thinks that this issue is open and may well be settled *in favor* of a priori knowledge.² In response to my first reason against the a priori – the lack of motivation – Rey is rather scornful. In response to my second reason – the obscurity – he appeals to reliabilist theories of knowledge and claims to give at least a sketch of how we might have a priori knowledge of logic, mathematics, and analytic truths. He finds my comments on this sketch in *Coming* “a



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little bewildering” (n. 9). I shall discuss his responses in turn. But I start with some preliminary points.

(1) I am claiming that knowledge can be *justified* only by experience, that the *evidence* for it must be experiential. I doubt that there is any innate knowledge and so am inclined to think that experience must also be part of the *source* of a person’s knowledge. But that is another matter and not my concern in *Coming*. Rey sometimes writes as if he thinks some knowledge may be innate. Suppose that some is. My second reason against the a priori still applies. If what is innate is indeed knowledge, it must be justified. We have *some* idea how we *might* establish the justification for innate empirical knowledge: experiences of the worldly facts that are the subject of the beliefs might play a role via adaptation in the production of the innate beliefs. But we have no idea how we might establish the justification for innate a priori knowledge.

(2) Although Rey is not a naturalist in my sense, he claims to be one in some sense. I think that he is a bit confused about the sense in which he can be one, given his position on the a priori. Two sharply different doctrines are often called “naturalism,” one metaphysical and the other epistemological. Metaphysical naturalism is *physicalism*: the view, roughly, that all entities are physical entities and that the laws they obey are in some way dependent on physical laws. This is a reductive doctrine. It has *nothing* to say about ways of knowing except that they must be, like everything else, physicalistically acceptable: so it alone entails nothing one way or the other about a priori knowledge. Rey can endorse this doctrine and it is clear that he does. Quine and I do too, but we call it “physicalism” not “naturalism.” The other doctrine called “naturalism” is an epistemological one that is *not* reductive and *is* opposed to a priori knowledge. Quine is expressing this doctrine in rejecting first philosophy and insisting that reality must be examined scientifically.³ It is what he aims to capture in his vivid metaphor of the seamless web and I aim to capture in my claim that the only way of knowing is the empirical way. Epistemological naturalism applies to all knowledge. So it applies to knowledge of ways of knowing themselves, yielding what Quine calls “naturalized epistemology.” In rejecting my naturalism, Rey is rejecting this epistemological

naturalism, exemplified in naturalized epistemology and taken from Quine. So he is contradicting himself when he goes on to claim that the naturalism that he assumes “is Quine’s own” (1998b, p. 32). Furthermore, this claim is at odds with his own argument which attempts to show that epistemological naturalism may well be false: he is arguing that there may well *be* a priori knowledge and first philosophy! He does go on to quote approvingly Quine’s description of naturalized epistemology (pp. 32–33), but it is clear from his discussion that all he is thereby endorsing is the use of the empirical method to argue for the a priori. Now if one must argue this – and one mustn’t and shouldn’t – it is certainly better to do so empirically rather than a priori: better not to start in sin even if one ends up there. But using the empirical method from time to time does not make you a naturalist, else everyone would be one. What makes you a naturalist, in Quine’s epistemological sense, is a commitment to there being *no other* method, a commitment that Rey manifestly does not have.

In sum, Rey is a metaphysical naturalist or physicalist. He is neither an epistemological naturalist nor a naturalized epistemologist, but he does think that the a priori ought to be argued for empirically.

(3) Rey’s discussion of Quine’s attack on the analytic/synthetic and the a priori/empirical distinctions is subtle and highly illuminating. Yet in one respect it strikes me as somewhat obtuse: Rey trivializes Quine’s revisability thesis by characterizing it as follows: “any belief can be reasonably revised in the light of experience.”⁴ He finds this indistinguishable from “banal fallibilism” (1993, p. 72): “people could be wrong about anything; they can make errors in reasoning, rely on experts that mislead them, or just reason themselves into strange corners” (1998b, pp. 26–27). For example, Rey, in balancing his checkbook concludes, “ $16 + 17 + 18 + 23 + 100 = 174$ ” but abandons this arithmetic truth on being told by the bank that he is wrong. He reasons that surely the bank is better at addition that he is (1993, pp. 70–1). He points out that this fallibilism “*has nothing to do with the a priori*,” for surely no rationalist ever denied that you could make errors in your a priori reasoning (1998b, p. 27). True enough, but this fallibilism also has *nothing to do with Quine’s thesis*, for surely Quine and the empiricists were aware that

the rationalists accepted this fallibilism. Banal fallibilism *can't* be the right way to understand Quine and the dispute over the a priori.

An empiricist ought to accept a distinction between two ways that further experiential evidence can and should lead a person to change her mind about a statement *p*. (i) On the one hand, the evidence might bear for or against *p itself*. (ii) On the other hand, the evidence might throw light on the goodness of *her thinking about p*. She is a fallible calculator. New evidence may throw no direct light on *p* but may suggest that she has made a mistake in her thinking about the relation of the evidence to *p*. Quine's revisability thesis is surely concerned only with (i): "no statement is immune to revision" (Quine, 1953, p. 43) in that experiential evidence might directly bear against *it*. The thesis is simply concerned with the relation between evidence and statement not with the relation between evidence and the view that a particular person (or even a particular community) has thought well about the statement. In fact, the thesis is the epistemological naturalism that I have just described, captured by the metaphor of the seamless web. Understood in this way the thesis strikes at the heart of apriorism and is far from trivial.

Rey has responded to criticisms that may seem similar to this but his responses do not address adequately this particular point. First (in response to my 1993, p. 53n, among others) he discusses the following view: the Quinean "point is that someone could hold on or revise any statement *and still be rational*" (1993, p. 71). But this is not the present point. Rey is right that the apriorist should accept that it may be rational for someone to stop holding any belief in the face of evidence of type (ii); for example, Rey at the bank. The Quinean point is that the *content* of the belief can be objectively disconfirmed by recalcitrant experience. Second (in response to Field 1996, p. 4) he describes something like the distinction between (i) and (ii) and then construes it as a "distinction between matters of empirical evidence and matters of reason alone," a distinction he rightly thinks the Quinean is not entitled to (1998b, p. 28). But the distinction between (i) and (ii) should not be construed in this way. It is a distinction between two ways in which *experiential evidence* can lead to belief revision. (ii) is not a matter of revision by "reason alone" but a matter of revision because empirical evidence –

for example, the disagreement of the bank with Rey's addition – suggests a failure in reasoning.

This having been said, Rey's discussion raises an interesting possibility. It is common to confront the apriorist with historical examples of allegedly a priori knowledge abandoned in the face of experience; for example, the Euclidean view of space. In the light of the above distinction, it is always open to the apriorist to respond that this experience was relevant in way (ii). The evidence does not count for or against the statement in question, it simply shows that our process of nonempirical justification was defective in this case. Of course, it remains to be argued that this is a plausible response in a particular case. In my view, it is rather clearly not in the case of Euclidean geometry.

In any case, the argument against apriorism and for the seamless web that we should take from Quine does not rest primarily on these historical examples. It rests primarily on the two reasons I gave. The first of these involves confirmation holism, but not quite in the way Rey seems to think (1998b, part I; 1993, pp. 78–81). The argument starts by pointing out how scientific laws that are uncontroversially empirical are holistically confirmed. Evidence for this is not to be found only in the discussions of Duhem and Quine: most of the evidence comes from the movement in the philosophy of science inspired by Thomas Kuhn and Paul Feyerabend. It is then plausible to extend this holism to all beliefs, even those of logic and mathematics; there is no motivation for a seam in the web.

I turn now to Rey's response to this reason. Rey has a lot of rhetorical fun mocking the remarks that Quine and I make about the empirical way of knowing and about the application of this way to logic and mathematics. He thinks that I am under “the illusion” that these remarks amount to “a serious theory” (1998a, p. 146). I am not. I agree with Rey that “*no one yet has an adequate theory of our knowledge of much of anything*” (1998b, p. 29); as I say, “we do not have the rich details of the empirical way of knowing that we should like to have” (1996, p. 50). In any case, Rey's mockery is largely beside the point. Since we do not have a serious theory that covers even the easiest examples of empirical knowledge, the fact that we do not have one that covers the really difficult examples from logic and mathematics hardly reflects on the claim that these are empir-

ical knowledge too. We all agree that there *is* an empirical way of knowing. Beyond that, this part of the argument against the a priori needs only the claim that the empirical way is holistic. *We have no reason to believe that a serious theory would show that, whereas empirical scientific laws are confirmed in the holistic empirical way, the laws of logic and mathematics are not; that it would show there is a principled basis for drawing a line between what can be known this way and what cannot.*

I would be the first to concede that this part of the argument alone is far from conclusive, a long way from *proving* that the holism extends to logic and mathematics. That is why I put a lot of weight on the rest of the argument: my second reason, which is about the obscurity of a priori knowledge. In this part, I attempt to show that the alternative a priori explanation of our knowledge of logic and mathematics, indeed of anything, is very unpromising. If this is right, we have a nice abduction: the *best* explanation of that knowledge is that it is empirical.

The a priori way of knowing is typically characterized by what it is *not*: it is not empirical. But what we need if we are to take the a priori way seriously is some idea of what it *is*. We need a positive account, not just a negative one. Why? This question may seem particularly pressing since I have just agreed that we do not have a serious theory of the empirical way. However, there are two crucial differences in the epistemic status of the two ways. First, the existence of the empirical way is not in question: everyone believes in it and Rey is even urging us to use it to show that there is an a priori way. In contrast, the existence of the a priori way is very much in question. Second, even though we do not have a serious theory of the empirical way, we do have an intuitively clear and appealing general idea of this way, of “learning from experience.” In contrast, we do not have the beginnings of an idea of what the a priori way might be; we lack not just a serious theory but *any account at all*.

Rey claims to provide just such an account, appealing to the idea that knowledge is true belief arrived at by a reliable process. He is pained by my unenthusiastic response. This response is certainly very brief (1996, 51n; it draws on my 1993). I shall expand it here.

The objection to a priori knowledge is that we don't know *what it would be* for something to be known a priori. So a successful resurrection of a priori knowledge must describe a nonempirical way of knowing, a process for justifying a belief that does not give experience the role indicated above. The difficulty in meeting this demand is well-demonstrated by the failure of traditional attempts: on the one hand, these assumed that we have Cartesian access to meanings; on the other hand, they took knowledge of logic for granted. The trouble with Rey's ingenious proposal is that it does not meet the demand either. He has seriously underestimated what is required for the resurrection.

Rey proposes that we know logical truths a priori because they are produced by a sub-system of the brain that enjoys a reliability “*completely independently of whatever input (i.e. experience) an agent may receive*” (1993, p. 91); for example, Ellen realizes in her brain “a non-axiomatic system of natural deduction, relying entirely on the operation of standard rules like *modus ponens*, universal generalization, conditionalization. etc.” An example of its output is:

- (R) Nothing bites all and only those things that don't bite themselves.

This “would be *knowledge* because each of the rules she used were surely justified if anything is. As soundness proofs of first-order logic show, they are, indeed, *absolutely reliable* in this sense: *it is impossible for them to produce a falsehood as a theorem*” (1998b, p. 34). The system's insensitivity to sensory input is intended to show that this is not an account of the empirical way of knowing. My objection is that *it is not an account of a way of knowing at all*.

It has been clear, at least since Plato, that for a belief to count as knowledge, it must not only be true, it must be *justified*: there has to be something about the way it was produced or sustained that makes it *epistemically nonaccidental*. Thus, if I believe on no good basis that it will rain tomorrow, or that a certain number is a prime, and I should turn out “by accident” to be right, my belief does not count as knowledge. The problem with Rey's account is that it does not show how Ellen's belief in (R) is, in the appropriate way, epistemically nonaccidental.

Consider another belief of Ellen's: the clearly empirical belief, (E), that Tom is mortal. Suppose that Ellen knows that Tom is a man. Suppose further that (E) is the product of a sub-system that took Ellen's knowledge that Tom is a man as input and that always yields the output that x is mortal given the input that x is a man. Inspired by Rey, we then argue: "(E) is *knowledge* because the rule she used was surely justified if anything is. As a vast amount of empirical research has shown, this rule is *reliable* in this sense: *given a truth of the form 'x is a man' as input it will always produce a truth as output*. And the rule took a known truth as input." Clearly something is wrong with this argument. *More needs to be said* to show that the production of truths like (E) by "the mortality sub-system" is epistemically nonaccidental. To help see this, suppose that the sub-system in Ellen was itself produced by a "random" process that also produced other sub-systems which frequently yield false outputs; for example, given the same known input, yield the output that Tom is wise, round, brave, etc.; or, given other inputs such as that x is a bachelor, yield the output that x is rich. Among these many sub-systems, one *just happens* to reliably produce truths, the mortality sub-system. *We need to say more* to rule out that the sub-system is thus accidental. We might say something about how the sub-system was produced: that experiences of a mortal-man world played an essential role in that production. But it is not necessary to say this.⁵ It is necessary to show that the sub-system, however it was produced, is *causally sensitive in an appropriate way to the fact that we live in a mortal-man world*. The outputs of Ellen's sub-systems in the imagined situation are not knowledge because those sub-systems are impervious to the way the world is; impervious to whether it is a mortal-man, wise-man, round-man, brave-man, rich-bachelor world.

The problem with Rey's proposal can be put briefly: *he does not say more*. What he says about his logical sub-system is analogous to my Rey-inspired argument about the mortality sub-system. We have just seen that that argument fails. Rey's argument fails for the analogous reason. He needs to show that the production of truths like (R) is epistemically nonaccidental. Once again, we can bring out the problem by supposing that the logical sub-system in Ellen was produced by a "random" process that also produced other similar

sub-systems which frequently yield false outputs. Perhaps these sub-systems include one that realizes the gambler's fallacy; one that fails to take proper account of the base in probabilistic reasoning; one that commits the fallacy of asserting the consequent. Rey needs to say something to rule out that it is a mere accident that one of Ellen's sub-systems yields truths. And, of course, he cannot say that it yields truths because it is causally sensitive in an appropriate way to the fact that we live in a logical world. For then the knowledge of (R) would be empirical.

It does no good to insist that the logical sub-system is *reliable* in that it regularly produces truths, for that is true also of the mortality sub-system which, as we have just seen, may not be producing knowledge at all. Being reliable *in this respect that Rey emphasizes* is simply not enough. It does no good to appeal to proof theory to demonstrate how sure we theorists are of the reliability, in that respect, of the logical sub-system. For the reliability of the sub-system in that respect is not in question; and, in any case, we theorists are also sure of the reliability of the mortality sub-system in that respect. We need to know something more *about Ellen* to establish that she arrived at (R) by a procedure that is, *in a broader respect*, epistemically reliable.

It does no good to claim, as Rey does, that "the logical truths are those sentences that are true by virtue of the pattern of operators alone, independently of the assignments to the referential devices"⁶ and to insist that Ellen arrives at her belief in these truths because her sub-system is appropriately sensitive to these patterns (1998b, p. 35). For, it may be a mere accident that her sub-system is sensitive to these patterns rather than others that would lead her into falsity. Suppose, for example, that the sub-system was produced by a random process that produces other such sub-systems sensitive to a vast variety of different patterns. Ellen just happens to be one of the few people lucky enough to have scored a valid sub-system. Manifestly, we would need a story about how Ellen's beliefs are nonetheless justified.

In saying all this I am *not* insisting that Ellen can only know (R) if she knows that she knows – the KK principle – or if she has available to herself some justification of the rules of the logical sub-system (cf. Rey, 1998b, p. 36; 1993, p. 92), nor am I simply refusing to

accept a reliabilist approach to knowledge.⁷ I think that the reliabilist idea for empirical knowledge, briefly indicated above, may well be along the right lines: Ellen knows (E) if the mortality sub-system that produced it is causally sensitive in an appropriate way to the fact that we live in a mortal-man world. This can be the case without Ellen knowing that it is, let alone knowing that it must be the case for her to know (E). I am insisting that there *be* a justification for Ellen's believing (E), not that she *knows* the justification. Putting this reliabilist idea together with my naturalism, I think that it may well be the case that Ellen knows (R) because it was produced by a logical sub-system that is causally sensitive in an appropriate way to the fact that we live in a logical world. (We do not know the details, of course, but this does not count against (R) being empirical because we are hardly better off with (E).)

There is a difference between the logical and the mortality sub-systems which seems to play a role in Rey's thinking: the former unlike the latter yields *necessary* truths.

If empirical knowledge can be the result of a process that reliably . . . issues in true beliefs in relevant circumstances, why couldn't *a priori* knowledge be the result of a process that reliably issues in true beliefs in *all possible circumstances*? (1998b, p. 34; see also p. 28 and 1993, pp. 92, 95)

The answer is: because that *metaphysical* difference in the output is not *epistemically* relevant. In the case of the mortality sub-system, we have seen that we have to say more than that it is reliable in Rey's respect if the contingent (E) that its output is to count as knowledge. It would be strange indeed if we were relieved of the responsibility of saying more in the case of the logical sub-system by the fact that its output (R) is necessary; so the requirements on knowing a necessary truth would be *less* demanding! Indeed, a person might have sub-systems that yield necessary truths which are *obviously* empirical and so where the need to say more should be uncontroversial; for example, the truths that water is H₂O and that Hesperus is Phosphorus. The fact that a statement is necessarily true can no more show that the process of arriving at it is epistemically nonaccidental than can the fact that it is true. That fact does not undermine my argument.

Aside. How might the necessity of the output of the logical sub-system be accommodated by a reliabilist account of *empirical*

knowledge of the output? What could the appropriate causal sensitivity to a logical world amount to? We are tempted to say that Ellen's mortality sub-system is sensitive to a mortal-man world in that if the world were different she would not have had that system. If we then apply this approach to Ellen's logical sub-system, we seem to have a problem: the world *cannot* be different from the logical world; it is *necessarily* logical. So, it is common to think, the logic of subjunctive conditionals makes it *trivially* the case that if the world were nonlogical she would not have had that logical sub-system. I doubt this view of subjunctive conditionals.⁸ In any case, perhaps we were wrong to be tempted by this approach. It builds a certain sort of infallibility into knowledge: Ellen knows (E) only if she could not have the mortality sub-system unless she lived in a mortal-man world. This seems to overlook the message of Descartes' *First Meditation* and the underdetermination of theories by the evidence. Perhaps what we need to say is that Ellen would not have had the mortality sub-system unless her world *appeared* to be a mortal-man world, unless she *had experiences* appropriate to such a world. She might have had other experiences, perhaps caused by an Evil Demon, even though she does live in such a world. Similarly, perhaps we should say that she would not have had the logical sub-system unless she had experiences appropriate to living in a logical world. She might have had other experiences, perhaps caused by an Evil Demon, even though the world she lives in is necessarily logical.

The point that we need to say more than Rey does for (R) to be knowledge may be more obvious if we change the examples a little: replace talk of sub-systems with talk of general beliefs. Suppose that (E) was not produced by the mortality sub-system but is inferred from the general belief that all men are mortal. Clearly, (E) will count as knowledge only if the general belief does. So we need to say more to show that general belief does. Now suppose that (R) was not produced by a non-axiomatic system of natural deduction but is inferred from some general logical beliefs. Once again, the epistemic status of (R) depends on the status of the general beliefs. So we have to say more to show that they are knowledge. It is hard to see how the change from general beliefs to sub-systems of rules could remove the need to say more.

I wonder if the talk of reliability is confusing the issue. So, consider what Rey tells us about (R) that might constitute its *justification*. First, of course, it *is* true. Second, it is the product of a sub-system that regularly produces such truths. Obviously, we have no justification so far; think of the mortality sub-system. Third, the logical sub-system is insensitive to sensory input. But this insensitivity clearly cannot justify the system's output.

Note that it would do Rey no good to suppose that his logical sub-system is innate. What *nonempirical* story could possibly be told of its innate presence that would support the view that its output is knowledge? The evidence suggests that some "good" and some "bad" logical sub-systems may be innate. What could justify the "good" ones apart from some empirical story?

In sum, the comparison with the mortality sub-system shows that the respect in which, according to Rey, his logical sub-system is reliable is insufficient to establish that its output is knowledge. To characterize a way of knowing he needs to say more, showing that the system is, in a broader respect, epistemically reliable. There is no reason to suppose that if he were to say more he would characterize a way of knowing different from the empirical way.

II. FIELD'S A PRIORI LOGIC

The attraction of the empirical view of logic is nicely brought out by Hartry Field:

there is a 100% correlation between what is indeed true in logic and what the evidential systems that idealize our epistemic practice say is true in logic. How is this correlation to be explained? (1996, p. 13)

A satisfactory explanation of this correlation between the logical facts and our logical beliefs should

make sense of the idea that if the logical facts had been different then our logical beliefs would have been different too. (pp. 13–14)

And making sense of that idea is precisely what the empirical view of logic claims to do.

Still, Field thinks that this claim is wrong: "we can simply make no sense whatever of the question of what we would believe were the

logical facts different" (p. 17) because of logic's "inextricable role in ascertaining the dictates of evidential systems" (pp. 18–19). Field argues that logic, by which he has in mind classical logic, is a priori. He thus implicitly disagrees with my Quinean naturalism. However, his requirements on a priori knowledge are unusually moderate and his conclusion is very qualified, so the disagreement may not be great. I shall not discuss all the nuances of his argument but simply respond to what I take to be his argument against my Quinean view.

According to Field the view that logic is a priori amounts to the following two claims:

- (i) that it is reasonable to infer according to the rules of that logic without any empirical evidence for the legitimacy of those rules;
- (ii) that those rules are empirically indefeasible, in the sense that no possible combination of observations should count as evidence against their legitimacy. (1998, p. 1)

There are also two related claims, (i_w) and (ii_w) concerning "logical principles" (Quine's "logical truths") which are statements inferable from the rules of logic alone. I shall discuss (i) and (i_w) first and then (ii) and (ii_w).

Field thinks that "logic is *almost incontrovertibly* a priori in the weak sense of (i) and (i_w)."¹ His reason is clear: in order for anything to count as evidence for anything, we have to use logic; logic licenses the inferences from evidence to conclusion and so must come first; "if one is debarred from believing logical principles prior to empirical evidence for them, one is effectively debarred from *ever* believing them (p. 2; also 1996, pp. 5–7). This exemplifies the dominant idea of Field's discussion: logic is special and must be seen as a priori because we need logic to get evidence for or against anything.

Clearly Field has a point. A person must begin the pursuit of knowledge using some innate evidential system – a set of deductive and inductive rules – before she has any empirical evidence for its legitimacy. So, in that pragmatic respect it is indeed "reasonable" for her to use this system – call it "*S*" – and logic comes out weakly a priori according to Field's definition (i). However, in another respect, most of us would hold that it remains to be seen whether it is reasonable for her to do so. In this other respect, we

are concerned with the *justification* of S , with whether or not S is a *good* system, whatever the particular person may have reason to believe about this. This other respect demands a revision of Field's (i): for logic to be weakly a priori is for its rules to be legitimate and for that legitimacy not to depend on any empirical evidence; legitimacy must be established by some nonempirical way of knowing. On this revised definition, the apriority of logic is certainly not incontrovertible. I, for one, do controvert it.

Of course, Field is an epistemological nonfactualist and so denies that there is any such other respect, any issue of justification beyond what is pragmatically reasonable:

It makes no sense to ask whether logic *really* is justifiable a priori; the only issue is whether it is advantageous to employ an evidential system that licenses adhering to a logic whatever the empirical evidence may be. (1996, p. 19)

So Field feels entitled both to claim that logic is weakly a priori even though he has not described a nonempirical way of knowing it, and to see this claim as naturalistic (p. 19).

I think that his nonfactualism is too extreme. I do not dispute most of the considerations that lead him to it; for example, trade-offs to do with reliability and power; and the problem of saying just how successful an evidential system must be to be "*good enough*" (1998, pp. 6–9). So I accept that there is a certain amount of indeterminacy in judgments about whether one system is better than another, and about whether a system is justified (1997, pp. 77–78). But the indeterminacy does not seem to me to stretch as far as Field thinks. He allows that the choice of an evidential system is only "partly" nonfactual. I don't think he has shown that the nonfactual parts include the a priori issue. He claims: "If, as I have been arguing, questions of justification or evidence are not fully factual, then presumably questions of a priori justification aren't either" (1998, p. 11). I resist the presumption.

Consider S . I accept that there may be many possible evidential systems that are not determinately better or worse than S . Still, so far as I can see, nothing Field says shows that S might not be determinately a *good* system, a justified system,⁹ even if not determinately the *best* system. Suppose that it is good. Suppose, as Field does, that it contains classical logic: deductions within S always follow the rules of classical logic. Now suppose that every possible rival

evidential system *that is not determinately worse* than S also contains classical logic. If this supposition is correct then we have good reason to think that classical logic is determinately justified. So far as I can see, Field does not show that this supposition is not correct. The reasons Field gives for thinking that justification in general is partly nonfactual do not show that the justification of *logic* is nonfactual.

If logic is indeed determinately justified, then we need an explanation of *how* it is. Quineans will seek an empirical explanation. Since S is innate, the explanation must be found in evolutionary history: S has been a success in the past and as a result of that has been inherited by the person in question. Spelling out the details here is at least as difficult as it is anywhere else in epistemology. If Field is to persist in his view that logic is a priori, he must reject any such empirical explanation. But then he would, after all, owe us an account of how logic was otherwise justified, an account of a nonempirical way of knowing. I shall return to this point.

Turn now to Field's (ii) and (ii_w) and the issue of the empirical indefeasibility of logic. First, some preliminary remarks about the empirical revision of logic.

S is a system of inference rules including the rules of classical logic, for example, *modus ponens*. So, a person embodying S is disposed to infer according to the pattern:

If p then q ,

p ,

So, q .

And, as a result, she is disposed to believe instances of the schema, ‘If p then q and p , then q ’. But the person who embodies S need not have any *theory* of these matters. So she need not *believe* that *modus ponens* is a valid inference nor that ‘If p then q and p , then q ’ is a logical truth. Indeed, in my view, it is almost certain that she does not have such beliefs innately, and unlikely that she will ever come to have them, given our ordinary indifference to logical theory. Call the epistemological theory recommending S , and hence the rules of classical logic, “ T .” The Quinean empiricist view of logic is more explicitly concerned with T rather than S , with beliefs about logic rather than with logical practices.¹⁰ The claim is that these beliefs, although central in the web of belief, are nonetheless revisable in the

light of experience. Still, this claim has clear consequences for S , the system that “holds the web together.” Suppose that experience leads a person to abandon T in favor of T' , a theory that recommends an evidential system S' built around a nonclassical logic. Then clearly the person should abandon S in favor of S' . In this way the Quinean thinks that our logical practices are themselves open to rational revision in the light of experience.

This is not to say that it is *psychologically* possible for a person to make any of these revisions in the light of experience. Perhaps there are all sorts of psychological difficulties in rationally replacing T by T' , or, having done so, in replacing S by S' . However, we should not exaggerate these difficulties in light of the improvements in reasoning we observe in ourselves and others brought about by the study of logic and probability theory. In any case, the psychological possibility of making these replacements is not the point. The point is that experience could supply rational grounds for these replacements. (Nonrational replacements are obviously possible; for example, by violence or surgery.)

Still, there seems to be a problem. A person’s rational revision of her evidential system S is supposed to come from her abandoning T in the face of empirical evidence. But she must use S , in order to bring the evidence to bear on T . How then could this use show that T is false and hence that she ought not to use S ? Field is very impressed by this problem: “It is hard to see how the logic employed could itself be subject to empirical confirmation” (1998, p. 12). This is the dominant idea of his discussion. As a result, he thinks that we have no choice but to treat our logic as a priori.

The Quinean solution to this problem is captured by the famous image from Neurath of rebuilding a boat whilst staying afloat on it. We can rebuild any part of the boat but in so doing we must take a stand on the rest for the time being. So we cannot rebuild it all at once. Analogously, we can contemplate revising any part of our evidential system S but in so doing we must hold fast to the rest of the system for the time being. So we cannot contemplate revising it all at once. This applies to the logic that is built into S . Field rightly insists that we need some such logic to draw any conclusions from the empirical evidence. Still, we could contemplate revising T and the system S that it recommends. Suppose that system S' , recom-

mended by T' , holds fast to most of classical logic but weakens the distributive law. We could employ S' to test our total science against the empirical evidence. If total science comes out better on this test than on our present test employing S , then we have empirical grounds, of the usual Duhem-Quine holistic sort, for preferring S' .

To say that we could do this is not, of course, to say that we should do it. As I understand the situation, we do not have any promising S' to be empirically tested against S , despite the quandry of quantum theory. As Field says, “without some idea as to what a better system of evidence would be, it makes little sense to criticize the one we have.” And coming up with a better system is “no easy matter” (1996, p. 11). So the possibility of revision is a theoretical one not a practical one at this time. But that is all that Quinean epistemology requires.

Field is, of course, aware of this Quinean solution to the problem of using logic to test logic, but he strangely ignores it in his 1996 discussion. Even his 1998 treatment of it is rather brusque. He complains that the solution is “vague” and doubts that we can “spell out” the Quinean inductive method (p. 13).

This criticism is unconvincing. The vagueness cannot be denied: consider, for example, my talk of total science “coming out better” using S' than S . Quine is upfront about the vagueness, as Rey notes (1998b, p. 29). But it is hard to see why this vagueness constitutes an effective criticism of the Quinean view of logic. Perhaps it would if *realistic* alternative evidential systems that treated logic as a priori had been spelled out. Field often writes as if he thinks that there have been, mentioning hypothetico-deductive and Bayesian formalizations (e.g. 1998, p. 12). Doubtless these formalizations throw a good deal of light on our inductive methods, but I take it as generally agreed that they fall very far short of a realistic, albeit idealized, picture of those methods. Rey is surely right in claiming that “*no one yet has an adequate theory of our knowledge of much of anything*” (1998b, p. 29). At this point we do not have a good detailed theory of the evidential system we should use. This is sad, but no special problem for Quine.

Field has a further point to make against the Quinean theory (a point he put to me in conversation). The evidential system recommended by the Quinean theory is not really S , embodying classical

logic, but rather the system described a page back with the help of the image of Neurath's boat. This latter "higher level" system uses a "lower level" system like S whilst keeping open the possibility that S might be replaced on empirical grounds in the way described. Suppose that we accept this view of how S is empirically revisable. Still, how could the higher level Quinean system itself be empirically revisable? Employing that system according to which all justification is based on experience, how could experience show that the system is wrong and that some justification is not so based? Applying the dominant idea of his discussion once again, Field thinks that the Quinean system could not be thus revisable and so it must be regarded as a priori. For the Quinean, it is a priori that there is no a priori!¹¹ This is a neat and perplexing point. Still, I think there is a reason for thinking that his dominant idea, however appealing, is "fishy."

Start by considering epistemological theories rather than evidential systems. Whether or not these theories are *empirically* revisable, they are indubitably revisable *somewhat*. Thus, an epistemologist may start out committed to a priori knowledge because she can think of no other way to explain knowledge of logic and mathematics. Then she reads Quine, becomes impressed with confirmation holism, struck by the obscurity of a priori knowledge, and adopts the Quinean theory. This seems to be a rational procedure in the descriptive sense, even if not in the normative sense: it seems to be governed by her underlying evidential system, whatever that may be. Later she may have second thoughts. She may, like Rey, be influenced by reliabilism and change back to a belief in the a priori. Again this seems like a descriptively rational procedure.

Now consider the implications of this for evidential systems. Our epistemologist's initial evidential system either takes justification to be based only on experience or it does not; either it is Quinean or it is apriorist. If it is Quinean then she should change it once she comes to believe that logic is a priori; the same rational procedure that leads her to change her epistemological theory should lead her to change her evidential system. Of course, if her initial evidential system is apriorist, then she should not change it at this point. But she should change it when she later converts to Quinean epistemology. And, either way, she should change it again when she has second thoughts

and returns to apriorism. The moral of this is that an underlying evidential system, whatever it may be, can govern a procedure which, by its own lights, supplies a rational basis for its own revision. So there is something wrong with Field's dominant idea: an evidential system *can* undermine itself. Field uses the idea all the time to argue against the *empirical* revision of an evidential system. Yet, if the idea is good, it counts against *any rational revision of the system at all*.

One might respond to this by claiming that a person cannot rationally revise her evidential system. So the changes that our epistemologist went through are not governed by her evidential system but are the result of malfunctions in the system, of "noise": the changes are no more rational than those brought about by surgery or violence. But this line seems very implausible. And it is not a line that is likely to appeal to Field. Although he opposes the empirical revision of logic he is happy to contemplate its "conceptual" revision (1998, pp. 3, 4, 12).

So, Field's neat point against Quinean epistemology seems to be in trouble: if evidential systems are to be rationally revisable at all, they must be able to undermine themselves. Of course, this alone does not show that they are *empirically* revisable. But, first, it leaves untouched the Quinean argument that they *are* empirically revisable. And, second, it prompts an argument against their being otherwise revisable. For, *how* are they otherwise revisable? If there is this nonempirical way of knowing, we need an explanation of it. Yet there is no explanation in sight. That is *my* dominant idea.

Earlier, I argued that Field had not shown that the justification of logic is a nonfactual issue and so he owes an explanation of how logic is nonempirically justified. His talk of revising logic on "conceptual" grounds reinforces the debt. For the Quinean, conceptual grounds are *parts of* the empirical grounds, the parts distant from experience and central to the web. Field must reject this because his conceptual grounds are *contrasted with* empirical ones. So he needs to tell us about these nonempirical grounds.

I have argued that Field's dominant idea is "fishy" but I have not located the source of the smell. And this is something that should be done, because the dominant idea is appealing. Alas, I do not know how to do it. I comfort myself with the thought that we know so little about our evidential system.

NOTES

¹ I criticized Rey (1993) in my 1993, a criticism modified slightly in my 1996 (p. 51n). Ancestors of Rey's present response to my views and my present response to his response were delivered at a conference on "Naturalistic Semantics and its Methodology" at the University of Maribor in June 1996 and at the D. C. Williams Conference on "Naturalism, Analyticity and the A Priori" at the University of Maryland in April 1997; see Rey (1998a) and Devitt (1998), sec. 4. An ancestor of Field (1998) was also delivered at the D. C. Williams conference.

² And he agrees (1998b, p. 41) with Joseph Levine view (1993, 1998) that my "molecular localism," according to which a few of the inferential properties of a word may constitute its meaning, commits me to a priori knowledge. The crux of my response to Levine (1996, sec. 1.11 and 1998, secs 2 and 8) is that localism is a purely semantic doctrine with no commitment to anything epistemological. In particular, localism need not, and should not, subscribe to the view that competence with a word alone yields knowledge about its meaning, a Cartesian view that has played such a prominent role in traditional doctrines of analyticity and in semantics generally.

³ See, for example, Quine (1981, pp. 21, 67, 72). The distinction between his naturalism and his physicalism is implicit in a passage on p. 85. (In response to p. 72, Rey draws a red herring "about whether empirical science as a whole can be justified by some means external to it" (1998b, p. 41). Quine is pointing out that if epistemological naturalism is true then there is no such justification. Rey responds by rejecting the reverse conditional: that if epistemological naturalism is not true – if there is a priori knowledge – then there is such a justification. This is indeed "a quite different issue" but it is not one Quine has raised.)

⁴ My thoughts on this matter were sharpened in extensive correspondence with Rey prior to his 1993.

⁵ In my 1993 I wrongly insisted that it was necessary (p. 55).

⁶ Rey's view here, and in the accompanying discussion, seems to be that the logico-syntactic form of a logical truth is *wholly* responsible for its truth. This is surprising: Does he not think that *the world* has something to do with its truth? I certainly do (1996, sec. 1.6; 1998, sec. 1).

⁷ Indeed, for all I know, my view may be consistent with the particular reliabilist theories that Rey cites as the basis for his view (1993, 91n).

⁸ As does Field (1996, p. 17).

⁹ That there is no determinate matter of fact whether *some* systems are good enough to be justified does not show that there is no determinate matter of whether *any* system is good enough to be justified; cf. baldness and other cases of vagueness.

¹⁰ See, for example, 1952, pp. xiii–xiv.

¹¹ Much to the delight of Rey (1998a).

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