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Naturalism: Friends and Foes — Source link

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Naturalism: Friends and Foes

These days, it seems there are at least as many strains of naturalism as there are self-professed naturalistic philosophers. My personal favorite has its primary roots in Quine, though it branches off from Quinean orthodoxy at some fundamental points.[1] Unfortunately, when it comes to spelling out the precise contours of this preferred version, there is an immediate difficulty: naturalism, as I understand it, is not a doctrine, but an approach; not a set of answers, but a way of addressing questions. As such, it can hardly be described in a list of theses; it can only be seen in action.[2] And this is a long-term undertaking.[3]

What I propose to do here is to triangulate on the position in two ways that I hope will be illuminating. For the first perspective, I trace three conspicuous earlier flowerings of this naturalistic impulse; though I won't agree with every opinion of these proto-naturalists, a look at their practices provides us with models of the fundamental naturalistic bent in familiar

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[1] For examples, see the treatments of scientific methodology and the status of mathematics in [1997] and [200?].

[2] I hope this will come clearer toward the end of §I.

philosophical settings. For the second perspective, I take up a range of well known objections to 'naturalism' -- including its purporting interconnections with the theory of truth, a recurring theme in many discussions, pro and con -- and indicate how the naturalist I envision would react. In the end, I hope at least to have clarified the outlines of the position I recommend. If it also comes off as reasonable, so much the better.

I. Roots

The first story I want to tell begins with Kant, not an easy philosopher to discuss briefly.[4] To make things simple, let me suggest, without further discussion, that one attractive way of reading Kant's notorious combination of empirical realism with transcendental idealism is to distinguish two levels of inquiry: empirical and transcendental. In empirical inquiry, we use ordinary scientific methods to investigate an objective world of spatiotemporal objects interconnected by causal relations. So, for example, we might infer the existence of an unobservable because it is related to what we do observe by causal laws. In transcendental inquiry, on the other hand, we recognize that this 'objective' world is in fact partly constituted by our discursive cognitive structures (the pure categories) and our human forms of sensible intuition (space and time); we realize that, viewed

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[3] [1997] and [200?] are earlier installments in this effort.

[4] For a slightly more complete discussion of this approach to Kant, see [200?].

transcendentally, certain elements of the world -- its spatiotemporality, its causal structure -- are not real, but ideal.

To call this ideality 'transcendental' is to distinguish spatiotemporality and causality from mere accidents of human cognition that might be studied at the empirical level; rather, they are necessities for any discursive intellect with our forms of intuition, and the forms of intuition are necessities of human cognition. It follows that we can know a priori that the world of our experience will be spatiotemporal and causally structured, and indeed, that spacetime and causation will satisfy certain a priori principles also gleaned by this transcendental analysis. So the spatiotemporal, causally conditioned world is real, viewed empirically, but ideal, viewed transcendentally, and this transcendental ideality is what makes a priori knowledge possible.

While it is clear that transcendental inquiry must differ markedly from empirical inquiry if results of these sorts are to achieved, it is not so clear what tools or methods or principles *are* involved, or what justifies them. As commentators have noted, many of the transcendental claims of the *Critique* seem not to qualify as knowledge claims at all by the explicit standards of that work. On top of this comes the further, well known embarrassment that modern science has falsified Kant's supposedly a priori Euclidean geometry and undermined the supposedly inescapable notion of causality.

The task of the many neo-Kantians has been to find a satisfying reaction to these challenges. In the 1920s, those distinctive neo-Kantians who would soon become logical empiricists or logical positivists focused particularly on how Kant could be reconciled with Einstein. Two of these were Reichenbach and Carnap, the one in Berlin, the other in Vienna. Let's begin with the Berliner.

Reichenbach's noble neo-Kantian effort revolved around an attempt to preserve something of the Kantian notion of a priori by dividing it into two notions. The idea was to separate 'certain truth' and 'prior to (partly constitutive of) knowledge', with the thought of preserving only the later. In this way, a priori principles (that is, constitutive principles), like those that produce Euclidean geometry, could be revised on empirical grounds.[5] In reply, Schlick argued that any properly Kantian philosophy must identify these two notions:

> Now I see the essence of the critical viewpoint in the claim that these constitutive principles are *synthetic a priori judgements*, in which the concept of the *a priori* has the property of apodeicticity (of universal, necessary and inevitable validity) inseparably attached to it. (Schlick [1921], p. 323)

In the end, Reichenbach came to agree that claims subject to empirical confirmation or disconfirmation could hardly be considered a priori:

> The evolution of science in the last century may be regarded as a continuous process of disintegration of the Kantian synthetic a priori. … the synthetic principles of knowledge which Kant had regarded as *a priori* were

[5] See Reichenbach [1920].

> recognized as *a posteriori*, as verifiable through experience only and as valid in the restricted sense of empirical hypotheses. (Reichenbach [1936], p. 145; Reichenbach [1949], p. 307)

Thus began Reichenbach's move from neo-Kantianism to logical empiricism.[6]

For our purposes, what's most important in all this is the attitude towards philosophizing that Reichenbach developed as he charted his course away from Kant's transcendental method. Consider once again the Kantian scheme: there are the methods of science, at the empirical level, and the methods of transcendental analysis, at the transcendental level; the transcendental method produces additional insights, one might even say corrections, to the empirical theorizing of science; ordinary scientific methods are fine for scientific purposes, but for deeper understanding, we must turn to the transcendental. But Reichenbach comes to oppose those who believe

> that philosophical views are constructed by other means than the methods of the scientist … (Reichenbach [1949], p. 289)

Instead, he holds that

> [M]odern science … has refused to recognize the authority of the philosopher who claims to know the truth from intuition, from insight into a world of ideas or into the nature of reason or the principles of being, or from whatever super-empirical source. There is no separate entrance to truth for philosophers. The path of the philosopher is indicated by that of the scientist … (ibid., p. 310)

[6] For an historical discussion, see Coffa [1991], chapter 10.

Of Kant's two levels, Reichenbach admits the cogency only of the empirical, the scientific. Philosophy is part of science, conducted by scientific means.

This reaction of Reichenbach's to the Kantian two-level system embodies what I consider the fundamental naturalistic impulse: a resolute skepticism in the face of any 'higher level' of inquiry that purports to stand above the level of ordinary science. The naturalistic philosopher is a member of the scientific community; she regards the methods of science as her own, as the best methods we have for finding out what the world is like; until some new method is clearly proposed and defended, she is unimpressed by philosophical systems that place a second level of analysis above that of science. Reichenbach frankly adopts just such a stance in the face of Kantian transcendentalism. In light of scientific progress, he abandons the goal of a Kantian a priori knowledge; he sets out instead, armed only with ordinary scientific methods, to study science itself. In place of the old 'constitutive' quasi-a-priori, he now attempts to separate the definitional or conventional elements from the empirical elements in our scientific theorizing.[7] Whatever we may think of the actual results of his analyses, we must recognize that a distinctive approach has been staked out.

To isolate the second episode of proto-naturalist sentiment, let's return to the neo-Kantian Carnap, back in

[7] See Reichenbach [1928]. For overview, see Reichenbach [1936], p. 146.

Vienna.[8] Like Reichenbach, Carnap hoped to preserve the Kantian idea that certain elements of our knowledge are 'constitutive', and again like Reichenbach, he sought these elements in the conventional or definitional. But here the similarity ends. Even in his neo-Kantian phase, Reichenbach favored careful analysis of actual scientific theorizing, but Carnap, inspired in this case by Russell, turned instead to logic.[9] Early on, this orientation produced an attempt to construct ordinary physical objects of everyday experience, by logical means, out of a sensory 'given';[10] later, it produced a focus on language and syntax. To see how this difference between Reichenbach and Carnap plays out, let's turn to Carnap's fully positivistic self, the Carnap of linguistic frameworks and the principle of tolerance.[11]

The general features of Carnap's thinking are familiar. A linguistic framework consists of a set of names, variables, predicates, connectives, quantifiers, etc., a set of formation rules for forming sentences from these, a set of primitive assumptions and deductive and evidential rules. So, for example, there is a linguistic framework for a 'thing language' with classical logic; there is a linguistic framework for arithmetic

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[8] The following discussion of Carnap, Quine, the a priori, and naturalism draws on portions of my [200?].

[9] See Carnap [1928].

[10] Actually, out of the relation that holds between a current experience and a past experience when I recognize them as similar.

with intuitionistic logic; there is a linguistic framework for general relativity with complex geometric and mathematical machinery; and so on. Carnap's idea is that we are free to choose any of these linguistic frameworks that suit our purposes:

> *In logic there are no morals*. Everyone is at liberty to build up his own logic, i.e. his own form of language, as he wishes. (Carnap [1934], p. 52)

Once we have selected our preferred linguistic framework and are working within it, some judgments will be part of our adopted language, or follow from parts of our adopted language by our adopted deductive rules. Even if the evidential rules of that language require empirical input for the assertion of many of our sentences,[12] there will some others, like the evidential rules themselves, that are assertable on the basis of the linguistic framework alone. From the point of view of a speaker of the adopted language, these judgments are a priori.

Clearly, Carnap has done Reichenbach one better in the attempt to preserve something from Kant: he has preserved a variety of a priori knowledge. In some linguistic frameworks, like the one for general relativity, even geometric principles will enjoy a priori status. And Carnap achieves this, as Kant achieved it, by distinguishing two levels of inquiry: internal questions asked within a linguistic framework, and prior

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[11] See Carnap [1934] and [1950].

[12] E.g., the 'thing language' presumably includes evidential rules that specify certain experiences as evidence for certain physical object claims. The evidential rule would be a priori in that framework, but the claim that the physical object exists would not follow from the framework alone.

pragmatic questions about which framework to adopt in the first place. At the level of these pragmatic decisions, we see that the choice of framework is purely linguistic or conventional, but once the decision is made and one framework adopted, at the level of those working inside the framework, the framework's assumptions and evidential rules and what follows from them using the framework's deductive rules -- all these are absolute, unrevisable, a priori.

Of course, not all of Kant's valued outcomes are preserved. On Carnap's account, the higher-level, pragmatic decision on which framework to adopt is a pre-scientific, conventional decision on what language to use for science; on Kant's account, what's uncovered at the higher, transcendental level are necessary, absolute truths about the structure of the world as experienced by any discursive knower with human forms of intuition. In other words, while Kant's a priori truths are unrevisable certainties of human knowledge, Carnap's are a priori only in the sense that revising them would constitute a revolutionary change in language, not a garden-variety change in belief.[13]

To view this difference from another angle, notice that Carnap distinguishes sharply between these conventional linguistic decisions and the philosopher's answers to what he calls external questions:

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[13] See Carnap [1963], p. 921.

> From these questions [questions internal to the linguistic framework of the thing language, decided by the evidential rules of that framework] we must distinguish the external question of the reality of the thing world itself. In contrast to the former questions, this question is raised neither by the man in the street nor by scientists, but only by philosophers. Realists give an affirmative answer, subjective idealists a negative one, and the controversy goes on for centuries … because it is framed in a wrong way. (Carnap [1950], p. 243)

The problem, as Carnap sees it, is that the philosopher tries to raise the question of reality outside the scientific framework whose evidential rules would give the question sense. The only legitimate question that can be raised outside the framework is that of which framework to adopt, and this question is decided on purely pragmatic grounds:

> The thing language in the customary form works indeed with a high degree of efficiency for most purposes of everyday life. … However, it would be wrong to describe this situation by saying, 'The fact of the efficiency of the thing language is confirming evidence for the reality of the thing world'; we should rather say instead: 'This fact makes it advisable to accept the thing language'. (ibid., p. 244)

Here the difference is stark: Kant's transcendental analysis is designed to answer the illegitimate external question; his answer is transcendental idealism.

More important for our purposes, however, are the differences between Carnap and Reichenbach. Though both seek to identify some portions of our scientific theorizing as linguistic or definitional or conventional, the structure of this inquiry is very different in the two cases. Reichenbach, as we've seen, undertakes to perform this analysis within science, making full use of scientific methods and theories.[14] Carnap, by contrast, traces the linguistic/conventional elements to a pre-scientific, pragmatic decision to opt for a particular framework for scientific inquiry. Because this deliberation takes place prior to the adoption of the scientific framework, it cannot be carried out as Reichenbach recommends, using scientific methods and the results of its empirical investigations. On the other hand, Carnap's two-level approach does deliver a priori knowledge at the internal level, which Reichenbach's cannot: if our Reichenbachian scientific inquiry into science determines that element x is present in our theory by convention, we can hardly be said to know that the world is x, and ipso facto, cannot be said to know it a priori. So Carnap's two-level approach has advantages and disadvantages when compared with Reichenbach's proto-naturalism: following Kant more closely, Carnap preserves a variety of a priori knowledge; at the same time, Carnap's approach short-circuits Reichenbach's detailed intra-scientific study of the conventional elements in science.

Moreover, Carnap's kinship with Kant leaves his position open to worries parallel to those about Kant's transcendental perspective. At Carnap's higher level, we don't ask or answer external philosophical questions as Kant would have us, but we do

[14] Reichenbach himself contrasts the work of his group with Carnap's Vienna Circle, emphasizing the intra-scientific approach of the Berlin group: 'In line with their more concrete working program, which demanded analysis of specific problems in science, [the members of the Berlin group] avoided all theoretic maxims like those set up by the Viennese school and embarked upon detailed work in logistics, physics, biology and psychology.' (Reichenbach [1936], p. 144)

make pragmatic, conventional choices between linguistic frameworks, and here, as in the Kantian case, we must face the question of which modes of evidence are applicable: are we then operating within yet another conventionally-chosen linguistic framework, a framework where the principle of tolerance reigns, rather than another, more absolutist framework? If so, why have we chosen the tolerant framework; if not, what is the ground of these non-conventional evidential rules? These questions vex Carnapians much as the corresponding questions vex Kantians.

Still, the most devastating challenge to Kant's two-level scheme was the discovery that some of his synthetic a priori judgments were actually a posteriori (and false). In Carnap's case, the analogous objection comes in one strand of Quine's wide-ranging response to Carnap. In brief, Quine argues that the evidential rules governing decisions at the higher, pragmatic/conventional level of Carnap's model are precisely the same as the rules governing the adoption of ordinary scientific hypotheses at the lower, empirical/theoretical level of that model. For example, where Carnap would distinguish between the methods used to settle an internal scientific question about the combining volumes of various chemicals and those used to settle the external, purely linguistic, question of whether or not to adopt the framework of atomic theory, Quine insists that this is a distinction without a difference.[15] Notice the close analogy

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[15] See Quine [1948], pp. 16-19, and [1951], pp. 45-46. For a more complete presentation of the argument in the text, see my [200?].

between this objection -- 'there's really no difference between your higher and lower levels' -- and the older objections to Kant's transcendentalism -- 'your cherished synthetic a priori judgments are really just a posteriori'.

Here Quine's reaction is analogous to Reichenbach's; he rejects the two-level model in favor of his own naturalism:

> the recognition that it is within science itself, and not in some prior philosophy, that reality is to be identified and described. (Quine [1981], p. 21)

Metaphysical questions -- are there atoms? are there numbers? -- epistemological questions -- how do we humans come to know the things we do? -- all these are to be treated as broadly scientific questions, to be answered using the methods of science and its results. What's ruled out is 'first philosophy', any 'supra-scientific tribunal' (Quine [1975], p. 72) that would justify or criticize science on extra-scientific grounds. The Quinean naturalist 'begins his reasoning within the inherited world theory as a going concern' (op. cit.) and operates 'from the point of view of our own science, which is the only point of view I can offer' (Quine [1981a], p. 181). Here again we meet the fundamental naturalistic impulse.

The third and final episode I'd like to sketch dates to the 1980s, when van Fraassen introduced his 'constructive empiricism': though we have good reason to believe in what we observe, we should refrain from belief in the unobservable posits of our theories. This is not to say that we should give up our theories entirely; rather we should regard them as 'empirically adequate' -- that is, as producing truths about observables -- while remaining agnostic about their theoretical claims. What, then, are we to say to the practicing scientist who believes in atoms? A first try might be to suggest that she is misstating her actual position -- that she actually believes only that atomic theory is empirically adequate -- but this is seems untrue to the history of the situation. Before 1905, there *was* an important debate over the reality of atoms, one side of which held that they were only useful fictions, a claim I think we can safely view as a crude version of empirical adequacy.[16] But the calculations of Einstein in 1905 and the meticulous experiments of Perrin on Brownian motion around 1910 proved decisive.[17] Are we to understand van Fraassen as holding that the scientific community was in error when it judged the work of Einstein and Perrin to be conclusive evidence for the actual existence of atoms?

To answer this question, van Fraassen separates it into two. For the practicing scientist, he says,

> the distinction between *electron* and *flying horse* is as clear as between *racehorse* and *flying horse*; the first

[16] E.g., see the chemist Ostwald in 1904: 'the atomic hypothesis has proved to be an exceedingly useful aid to instruction and investigation … One must not, however, be led astray by this agreement between picture and reality and combine the two'. For references and fuller discussion, see my [1997], §II.6.i.

[17] E.g., see Ostwald in 1908: 'the agreement of Brownian movement with the demands of the kinetic hypothesis…which have been proved through a series of researches and at last most completely by J. Perrin, entitle even the cautious scientist to speak of an experimental proof for the atomistic constitution of space-filled matter'. (See op. cit.)

> corresponds to something in the actual world, and the other does not. (van Fraassen [1980], p. 82)

For the scientist immersed in her science, van Fraassen imagines that this distinction might even be a methodologically beneficial one:

> We might even suggest a loyalty oath for scientists, if realism is so efficacious. (ibid., p. 93)

But he insists that

> the interpretation of science, and the correct view of its methodology, are two separate topics. (op. cit.)

As far as methodology goes, the actual practice of science, it is perfectly reasonable for our scientist to take the Einstein/Perrin evidence as establishing the real existence of atoms. But for the proper 'interpretation' of atomic theory, we must adopt a point of view other than that of the practicing scientist; we must use a method different from that of science: 'stepping back for a moment', we adopt an 'epistemic attitude' towards the theory (ibid., p. 82). Only then, answering the question as epistemologists, do we determine that the Einstein/Perrin evidence is not enough, and indeed, that no evidence can be enough to establish the existence of entities that cannot be perceived by unaided human senses. Here we have yet another two-level theory: at the ordinary scientific level, we have good evidence that atoms are real; at the interpretive, epistemic level, we do not.

This time, one voice of dissent comes from Fine. Why should we decide, at the epistemic level, to believe in what we can observe unaided rather than in what we can detect (as Perrin detected atoms)? After all, the method of detection can be put to any number of scientific tests:

> Faced with such substantial reasons for believing that we are detecting atoms, what, except purely a priori and arbitrary conventions, could possibly dictate the empiricist conclusion that, nevertheless, we are unwarranted actually to engage in *belief* about atoms? (Fine [1986a], p. 146)

Fine sees no grounds for this higher-level decision:

> an attitude of belief has as warrant precisely that which science itself grants, nothing more but certainly nothing less … when [the empiricist] sidesteps science and moves into his own courtroom, there to pronounce his judgments of where to believe and where to withhold, he [commits] the sin of epistemology. (ibid., p. 147)

Fine's own position, which he calls the 'Natural Ontological Attitude' or NOA, includes the fundamental naturalistic impulse:

> All that NOA insists is that one's ontological attitude towards … everything … that might be collected in the scientific zoo (whether observable or not), be governed by the very same standards of evidence and inference that are employed by science itself. (ibid., p. 150)

There is only one level at which to evaluate the evidence for the existence of atoms, and that is the ordinary scientific level, where even van Fraassen admits that we are justified in believing in them.

Now we shouldn't imagine that only transcendental idealists (like Kant) or conventionalists (like Carnap) or constructive empiricists (like van Fraassen) are tempted by two-level accounts; even realists occasionally succumb. To see how this might happen, consider again the case of the scientist who believes in atoms on the basis of the Einstein/Perrin evidence. Suppose this scientist is confronted by a constructive empiricist who claims that this evidence is good enough for scientific purposes, but not good enough to establish the actual existence of atoms. The proper naturalistic response would be to ask what other purposes the skeptic has in mind, what other modes of evidence he's applying; until these matters are explained, the scientist is surely within her epistemic rights to continue to adhere to normal scientific standards and to assert the reality of atoms. But given human nature, a scientist confronted with this stubborn agnosticism about atoms, with this condescension towards her cherished evidential standards as merely 'good enough for science' -- such a scientist is all too likely to rise to the occasion by trying to defeat the van Fraassenite on his own terms, by insisting that atoms *really* exist.

The fatal flaw in this reaction is that by agreeing (implicitly) to 'step back' with van Fraassen into his 'epistemic attitude', the scientist has forfeited all her actual evidence for the existence of atoms: that evidence has already been declared 'good enough for science' but not 'good enough for epistemology'. Having ascended to the higher level, where her ordinary scientific evidence is no longer relevant, she is left without resources; this is what leads to the foot-stomping *really* of the Realist.[18] Let me distinguish between a lower-case 'realism' about atoms in the ordinary scientific sense, supported by ordinary scientific evidence, and an upper-case 'Realism' about atoms which asserts, at the higher, 'epistemic' level, on

[18] See Fine [1986a], p. 129.

who knows what grounds, that atoms *really* exist. Our scientist had perfectly good evidence for her realism about atoms, but in response to van Fraassen's challenge, she sets herself up to defend Realism, an epistemological rather than a scientific view. By the naturalist's lights, this is a fool's errand.

The case of Boyd, van Fraassen's most tenacious philosophical opponent, is somewhat more subtle. Boyd undertakes to show that

> a realistic account of scientific theories is a component in the only scientifically plausible explanation for the instrumental reliability of scientific methodology. (Boyd [1983], p. 207)

Leaving aside the detail of this argument, it is clear that Boyd intends it to take place entirely within science, using ordinary scientific methods:

> The epistemology of empirical science is an empirical science. (Boyd [1990], p. 227)

This certainly has the sound of a purely naturalistic undertaking. But consider again our scientific believer in atoms, the one convinced by the Einstein/Perrin evidence. While van Fraassen challenges this evidence at his higher level of epistemological inquiry, the naturalist remains at the lower level, the ordinary scientific level, and regards it as conclusive, just as the scientist does. Notice that on this contrast, Boyd sides with van Fraassen: he, too, sees the ordinary scientific evidence as standing in need of supplementation, presumably in response to the higher-level considerations raised by van Fraassen. So, though the supplementation Boyd goes on to offer is purely scientific, the perceived need for it is not. In this sense, Boyd, too, has bought into van Fraassen's higher level of evaluation.

Notice also that buying into van Fraassen's perspective tends to push Boyd away from the details of the local debate over atoms and towards global debates over such questions as whether or not the theoretical terms of mature scientific theories typically refer. The naturalist is wary of such blanket assertions, given the complexity of actual science: the particularity of arguments for the existence of individual theoretical entities, like atoms or quarks; the subtle gradations in levels of belief in the various parts of science; the widespread use of idealizations and mathematizations; and so on.[19] At least at the outset, it seems unlikely that a single attitude towards 'the posits of mature science' will be correct across the board.

On this point, Reichenbach agrees.[20] Speaking of the Berlin group, he endorses its

> concrete working-program, which demanded analysis of specific problems in science … (Reichenbach [1936], p. 144)

He writes with approval that

> They concentrated on minute work; and hoped to advance the work of the whole step by step. (ibid., p. 150)

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[19] This is a central theme of my [1997], especially §II.6.

[20] Also Fine, see below.

Reichenbach proposes that scientific philosophy proceed by examining particular theories in particular sciences, e.g., 'in logistics, physics, biology and psychology' (ibid., p. 144); he himself concentrated his energies on space, time and geometry in the theory of relativity. While it is possible that this piecemeal approach will lead to a uniform theory of all parts of science, this is neither presupposed nor required as a measure of success. Carnap's fondness for all-inclusive systems was another central point of disagreement between his Viennese positivists and Reichenbach's Berlin empiricists.[21]

These, then, are the three historical episodes that I hope illuminate the fundamental naturalistic impulse. Much as I applaud the reactions of Reichenbach, Quine and Fine, each in opposition to a particular two-level view, I must allow that I cannot agree with all they have to say in their pursuit of their proto-naturalistic projects. In the case of Reichenbach, my own expertise is inadequate for a full accounting, but Friedman has argued persuasively against Reichenbach's later theory of confirmation and in favor of a more naturalistic approach;[22] here, it seems, Reichenbach forsakes the internal, the scientific, in favor of the a priori. In Quine's case, I think the lure of global accounts -- of confirmation (holism), of ontology (to be is to be the value of a bound variable) -- has overshadowed the detailed analysis of actual scientific theory and practice that's

[21] See footnote 14 and Reichenbach [1936], pp. 149-150.

[22] See Friedman [1979].

incumbent upon the true naturalist. I've written at length on this and my other departures from Quinean orthodoxy elsewhere, so I won't go into detail here.[23]

My understanding of where and how Fine's NOA differs from the naturalistic stance I'll be espousing is compromised by my uncertainty over precisely what NOA involves. Many passages, like those cited a moment ago, sound naturalistic in spirit:

> we cannot actually do more, with regard to existence claims, than follow scientific practice. (Fine [1986a], p. 132)

> Trust that science is open to providing all the resources and nourishment that we who study science need. (Fine [1996], p. 176)

And Fine also embraces the secondary naturalistic theme traced in Reichenbach above: a preference for local rather than global analyses in our scientific study of science. In fact, he sometimes goes further, declaring outright that there are no 'general, substantive' (Fine [1996], p. 176) theories of confirmation, explanation, cause, etc., indeed any of 'the concepts used in science' (Fine [1986a], p. 149), but in careful moments, he admits that the question remains open:

> A question that NOA must face is whether going local … means automatically restricting the range of judgments and principles away from the fully general or universal. I think the answer is no. All that NOA urges is that we not impose a universalist framework from the outside as a precondition for trying to investigate or understand a practice. … It remains to be seen how much universality is actually required for understanding. … Induction again; let us look and see. (Fine [1996], pp. 179–180)

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[23] See my [1997], especially II.2, II.6, III.3, and III.4. There I also disagree with Quine's treatment of mathematics.

Fine and the NOAer make an exception to this open-minded policy in the case of the concept truth -- insisting outright that there is no theory of truth[24] -- but I'll leave that issue for later.

Still, despite this agreement (again leaving truth aside), there are hints that Fine's posture is not quite that of the naturalist. He writes, for example, that NOA means

> to situate humanistic concerns about the sciences within the context of ongoing scientific concerns, to reach out with our questions and interests to scientist's questions and interests -- and to pursue inquiry as a common endeavor. (Fine [1996], p. 174)

This suggests that we humanists, which presumably includes us philosophers, begin somewhere else, somewhere outside science, and need to be encouraged to embrace the results and methods of science. In contrast, my naturalist is simply born native to late twentieth-century common sense and the scientific attitude that extends it. The only decision to be made is whether or not to go beyond these means of finding out how the world is, whether or not to add extra-scientific standards of justification to our repertoire. The naturalist, holding to her own standards, will see no reason to do this.

Perhaps these issues come clearest in Fine's rejection of 'essentialism':

> NOA is, therefore, basically at odds with the temperament that looks for definite boundaries demarcating science from pseudo-science, or that is inclined to award the title

[24] See Fine [1986a], pp. 149-150.

> 'scientific' like a blue ribbon on a prize goat. (Fine [1986a], p. 149)[25]

This passage raises the key questions of demarcation criteria and pseudo-science. On the first, I agree with Fine that it is probably hopeless to search for necessary and sufficient conditions that separate science from the rest. Instead, our naturalist might begin from simple idea that

> Science [is] a method of finding things out. This method is based on the principle that observation is the judge of whether something is so or not. (Feynman [1998], p. 15)

This simple idea brings others in its wake: the importance of falsification in ruling out hypotheses, of precision and thoroughness, of objectivity, of specificity, of theory formation and the rejection of authority, of universality, and so on (ibid., pp. 15-28). As science develops successfully along various paths, so do higher level norms, like the rejection of action-at-a-distance, or the emergence of mechanism, or its overthrow by field theories. But in none of this do we find necessary-and-sufficient conditions. Rather, the moral seems to be that we do best to keep an open mind on the progress of scientific methodology.

Now this conclusion might seem troublesome for the naturalistic approach: after all, isn't naturalism the view that scientific methods are the only legitimate source of evidence, that we should eschew the extra-scientific; doesn't it take a viable demarcation criterion even to state the position?!

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[25] These ideas, Fine says, 'bring NOA in line with certain postmodern and feminist writings' (Fine [1996], p. 174).

Perhaps some of my proto-naturalistic precursors would agree to this, but I hope to take a somewhat different line. My naturalist's methodology isn't 'trust only science!'; her methodology just *is* a certain range of methods, which happen to be those we commonly regard as scientific. When asked why she believes in atoms, she says, 'because of the experiments of Perrin' and such-like, not 'because science says there are atoms and I believe the methods of science'. So my naturalist applies no necessary and sufficient conditions; as a native of the contemporary scientific world view, she simply proceeds by the methods that strike her as justified.

Still, though the naturalist can proceed naturalistically without appeal to any demarcation criterion, a new question arises when I attempt to describe her behavior in general terms, when I end up saying things like: the naturalist has internalized the standards and methods of contemporary science. My reading is that in these contexts, terms like 'scientific methods' are informal terms of ordinary language, used in familiar, rough-and-ready fashion, without the backing of necessary and sufficient conditions.[26] I contend that what carries the weight here is not these general terms, but the individual behaviors: e.g., the faith in 'ordinary evidence' like the Einstein-Perrin case for atoms. That's why my efforts to outline this version of naturalism consist largely (and

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[26] I hope my general remarks in other parts of this paper will be understood in the spirit described here.

fundamentally) of a list of naturalistic reactions in specific cases to particular challenges. I count on our shared ability to extrapolate from these, with no guarantee that all cases will be beyond controversy.

It's worth noting that in the historical episodes we've just been surveying, the naturalist's opponents have often themselves presupposed a general characterization of science when they grant that such-and-such is acceptable on ordinary scientific grounds (as an empirical matter (Kant), as an internal question (Carnap), for scientific purposes (van Fraassen)). They then introduce an explicitly extra-scientific perspective, from which the view is supposed to be starkly different. Now again, when I describe her, I say that my naturalist, born into the contemporary scientific approach, balks at extra-scientific demands. But what actually happens is not that she insists 'you're proposing methods that go beyond the legitimate range of science', but that she is puzzled: she asks for a better description of the new evidential standards being proposed; she asks to be told why they are needed and how they are justified. Unless some explanation is given that ties into her own methods, the ones her opponents describe as 'ordinary scientific methods', she is unlikely to be persuaded that her original grounds are inadequate. Again, none of this requires her to launch any blanket condemnation of 'extra-scientific methods'.

So far, then, I agree with Fine that we should avoid the losing battle of specifying demarcation criteria, but I don't think this is enough to keep the naturalist from condemning so-called 'pseudo-scientific' practices like astrology. The kind of thing the naturalist might say is once again nicely illustrated by Feynman, our sample naturalist:

> Astrologists say that there are days when it's better to go to the dentist than other days. There are days when it's better to fly in an airplane, for you, if you are born on such a day and such and such an hour. And it's all calculated by very careful rules in terms of the position of the stars. If it were true it would be very interesting. Insurance people would be very interested to change the insurance rates on people if they follow the astrological rules, because they have a better chance when they are in the airplane. Tests to determine whether people who go on the day that they are not supposed to go are worse off or not have never been made by the astrologers…
>
> Maybe it's still true, yes. On the other hand, there's an awful lot of information that indicates that it isn't true. We have a lot of knowledge about how things work, what people are, what the world is, what those stars are, what the planets are that you are looking at, what makes them go around more or less … so what are you going to do? Disbelieve it. There's no evidence at all for it. … The only way you can believe it is to have a general lack of knowledge about the stars and the world and what the rest of the things look like. If such a phenomenon existed it would be most remarkable, in the face of all the other phenomena that exist, and unless someone can demonstrate it to you with a real experiment, a real test, took people who believe and people who didn't believe and made a test, and so on, then there's no point in listening to them.
>
> Tests of this kind, incidentally, have been made in the early days of science. It's rather interesting. I found out that in the early days, like in the time when they were discovering oxygen and so on, people made such experimental attempts to find out, for example, whether missionaries -- it sounds silly; it only sounds silly because you're afraid to test it -- whether good people like missionaries who pray and so on were less likely to be in a shipwreck than others. And so when missionaries were going to far countries, they checked in the shipwrecks whether the

> missionaries were less likely to drown than other people. And it turned out that there was no difference. (Feynman [1998], pp. 92–3)

This straightforward sort of thinking requires no general characterization of science to be persuasive. If the NOAer is reluctant to withhold some sort of blue ribbon in such cases, it would seem that he isn't 'born to the contemporary scientific world view', that he hasn't 'internalized its methods', and hence, that he is no naturalist, by my lights.[27]

Let me summarize, then, my description of the naturalist's behavior, using rough-and-ready general terms that she herself need not: the naturalist begins her inquiry from a perspective inside our scientific practice, which is, in turn, an extension of common sense. She approaches philosophical questions as broadly scientific questions, insofar as this is possible. When faced with a challenge framed in terms of extra-scientific requirements, she is open-minded but puzzled. Until the motivations and standards for this other style of inquiry are spelled out and justified, she rests with her own evidential principles, with a healthy skepticism toward first philosophy. From this perspective, she pursues a scientific study of science, understood as an undertaking of human beings -- as described by her theories of psychology, physiology, linguistics, etc. -- who

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[27] I would also disagree with Fine's assessment of the status of the belief that scientific methods are responsive to more than purely social pressures. Fine counts this as an extra-scientific 'add-on' to NOA (Fine [1996], p. 185); I would count it as internal to the scientific theory of science. The process of weeding out methods that are largely responsive to factors like social pressure is part of the process of scientific correction to scientific method.

inquire into the structure of the world -- as described by her theories of physics, chemistry, biology, botany, astronomy, etc. In the process, she aims to understand how and why particular principles and practices either help or hinder her efforts to determine how the world is, and she attempts to fine-tune her overall methodology in light of this understanding. As simple as that.

II. Putnam against naturalism

Having first approached naturalism by describing some of its philosophical roots, I now turn to the objections of Putnam, a prominent contemporary opponent. The irony here is that Putnam was once himself a proto-naturalist; e.g., in response to Duhem's fictionalism, he wrote:

> it is silly to agree that a reason for believing that *p* warrants accepting *p* in all scientific circumstances, and then to add 'but even so it is not *good enough*'. Such a judgment could only be made if one accepted a trans-scientific method as superior to the scientific method; but this philosopher, at least, has no interest in doing *that*. (Putnam [1971], p. 356)

Ten years later, the author of 'Why there isn't a ready-made world' and 'Why reason can't be naturalized' attacks both 'contemporary attempts to "naturalize" metaphysics' and 'attempts to naturalize the fundamental notions of the theory of knowledge' (Putnam [1982b], p. 229). This is the Putnam I propose to discuss here.

Unfortunately, despite the simplicity of these declared goals, the target of Putnam's challenge in these two papers is not always clear.[28] At various points in the first paper, he uses the terms 'metaphysical realism', 'materialism', 'scientism', and even 'metaphysical materialism'. Here the materialist is said to view physics as the best source of metaphysical or ontological information, that is, information about how the world is. Putnam continues

> … we don't *need* intellectual intuition to do *his* sort of metaphysics: his metaphysics, he says, is as open ended, as infinitely revisable and fallible, as science itself. In fact, it *is* science itself! … The appeal of materialism lies precisely in this, in its claim to be *natural* metaphysics, metaphysics within the bounds of science. (Putnam [1982a], p. 210)

This has a proto-naturalistic ring, and indeed, it seems to me not entirely unfair to tag naturalism with the pejorative 'scientism'. This last is a view that Putnam considers not only false, but pernicious:

> metaphysical materialism has replaced positivism and pragmatism as the dominant contemporary form of scientism. Since scientism is, in my opinion, one of the most dangerous contemporary intellectual tendencies, a critique of its most influential contemporary form is a duty for a philosopher who views his enterprise as more than a purely technical discipline. (Putnam [1982a], p. 211)

For simplicity, I won't attempt to sort out the precise target or targets of Putnam's critique; instead, I propose to consider his arguments as if they were addressed to the form of naturalism I'm advocating. This may well have no bearing on their cogency against the view or views Putnam himself has in mind, but I hope it may suit my goal of clarification.

To begin with, I suspect that the root of Putnam's unhappiness is his conviction that his opponents have failed to learn the lesson of Kant:

> The approach to which I have devoted this paper is an approach which claims that there *is* a 'transcendental' reality in Kant's sense, one absolutely independent of our minds … *but* (and this is what makes it 'natural' metaphysics) we need no *intellektuelle Anschauung* … the 'scientific method' will do … 'Metaphysics within the bounds of science alone' might be its slogan. (Putnam [1982a], p. 226)

Earlier, he identifies 'metaphysical realism' with Kant's 'transcendental realism' (ibid., p. 206), the view Kant rejects in favor of 'transcendental idealism'. Now whatever other positions Putnam might have in mind, I hope the previous section has made it clear that this is *not* what I mean to advocate under the label 'naturalism', nor, I would argue, is it what Reichenbach or Quine or Fine advocates. The most fundamental naturalistic impulse, as I understand it, consists in a stubborn resistance to 'transcendental' levels of analysis of any sort; in the Kantian idiom, the naturalist begins and ends in at the empirical level. However strong the human urge towards the transcendental (Putnam [1982a], pp. 210, 226), it is not the naturalist who succumbs.

That much is easy: whatever the naturalist's sins, she has not transgressed against Kant's rejection of transcendental realism, because she hasn't risen to Kant's transcendental level in the first place. But there may be more to the Kantian lesson

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[28] Putnam himself regards naturalized metaphysics as a 'unified movement' and naturalized epistemology as expressed in many

that Putnam accuses us of having missed, perhaps in some version of what he calls Kant's 'corollary':

> The corollary Kant drew from all this is that even experiences are in part constructions of the mind … the idea that all experience involves mental construction, and the idea that the dependence of physical object concepts and experience concepts goes *both* ways, continue to be of great importance in contemporary philosophy … (Putnam [1982a], pp. 209–210)

Now the idea that human cognizers perform some processing on raw sensory stimulations is a commonplace of contemporary psychology; there is a concerted scientific effort to determine how this is done, to describe the mechanisms involved. Putnam sees more than this in the Kantian corollary; he sees some form of idealism. Before we can offer any naturalistic response, we need to know what sort of idealism is in question.

As we've seen, the trick to understanding any Kantian utterance is to be alert to its level: we shouldn't, for example, try to determine whether or not Kant is an idealist, tout court, for he is an idealist at the transcendental level and a realist at the empirical level. Now Putnam himself so well understands the difficulties of the transcendental level that he is moved to suggest that

> one's attitude to it must, perhaps, be the concern of religion rather than of rational philosophy. (Putnam [1982a], p. 226)

So it seems unlikely that Putnam intends his Kantian corollary to be understood transcendentally.

'incompatible and mutually divergent ways' ([1982b], p. 230).

If, on the other hand, the Kantian corollary is to be interpreted empirically -- contrary to Kant's own empirical realism -- and if we are to avoid reducing it to the commonplace of empirical psychology -- that human cognition adds some processing to raw sensory inputs -- then Putnam must tell us more. And he does: it is 'silly' to think that

> we can have knowledge of objects that goes beyond experience. (ibid., p. 210)

For the 'one idea … definitely sunk by Kant …' is the view that

> We can think and talk about things as they are, independently of our minds. (ibid., p. 205)

Of course, Kant didn't sink this view at the empirical level, he embraced it, but here our concern is with Putnam.

If Putnam's point here is not the commonplace of empirical psychology, then it must be that we cannot hope to know what the world is like independently of our perceptual and conceptual processors or independently of our scientific theories. As a thesis about psychology or science, this seems either false or unproblematic. When psychology tells us that we are prone to certain sorts of perceptual and cognitive mistakes, it is telling us that the world is not as our basic processors tend to see it. Likewise, progress in the physical sciences sometimes takes the form of the discovery that the way the world most naturally appears to us is not the way it actually is: as Einstein showed that our perception of the world as Euclidean was actually a parochial take on a larger non-Euclidean universe, or as quantum mechanics suggests that our everyday ideas of causation are not applicable in the small. In all these cases, careful application of the scientific method allows us to 'see around' our most basic forms of perception and conceptualization, to better understand the world as it is independently of our cognitive structures. And it is clearly possible for us to 'see around' any particular scientific theory; this is how science progresses, by replacing one theory with another. So the complaint can only be that we can't know what the world is like without using scientific methods -- something the naturalist is quite ready to grant![29]

In sum, then, it seems that Putnam's Kantian corollary must either be a variety of transcendental idealism that functions at a level rejected by Putnam and beyond the range of the naturalist, or a sort of empirical idealism that's rejected by both Kant and the naturalist and ought to be rejected by Putnam as well. Whatever Kantian lessons Putnam's other opponents may have failed to learn, I don't see that this underlying inspiration for his displeasure with them should carry any weight against the naturalist. So far, my naturalist adheres to an ordinary string of trivialities of science and the common sense it extends: the world is as it is (largely)[30] independently of our modes of perception and conceptualization; by careful

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[29] Of course this is not to say that we are getting what Putnam dramatically characterizes as 'a coherent theory of the noumena … arrived at by the "scientific method"' (Putnam [1982a], p. 226). What we come to know is the ordinary empirical world, not its transcendental counterpart.

application of scientific methods, we can gradually overcome our prejudices and better understand how the world is.

This talk of 'the way the world is' brings us to the doorstep of one of the more specific areas of Putnam's critique, the idea that his opponent is committed to the existence of

> the one true theory, the true and complete description of the furniture of the world. (Putnam [1982a], p. 210)

He elaborates:

> this belief in one true theory requires a *ready-made* world … : the world itself has to have a 'built-in' structure. (ibid., p. 211)

Part of Putnam's resistance to this view is intertwined with his views about truth, which I postpone to the next section, but before delving into that question, we should compare Putnam's notion of 'the one true theory' with our naturalistic commonplaces.

In some sense, the naturalist does think the world has a 'built-in' structure, supposing this to mean that the world is as it is (largely) independently of our cognition. Saying that (most of) the world's structure is 'built-in', in this sense, only means that it isn't imposed by our perception, cognition or thought; this is the part of the world's structure that we're trying to capture in our scientific efforts to screen off our various prejudices and reveal the world as it is. This much I would count as commonplace, but Putnam characterizes his opponent

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[30] Of course, our modes of perception and conceptualization are themselves part of the world, so not everything about the world is independent of them.

as embracing something more: the assumption that there is one and only one theory that reveals the world as it is. I don't see how the belief that the world has a built-in structure forces one to the conclusion that only one theory can describe that structure. Putnam's case against the 'one true theory' involves purportedly 'equivalent descriptions', but let's not worry about the persuasiveness of his examples; let's simply ask why the bare admission that there might not be 'one true theory' should be troublesome for the naturalist.

There's a hint of one possible worry in another of Putnam's writings:

> Any sentence that changes truth-value upon passing from one correct theory to another correct theory … will express only a *theory-relative* property of THE WORLD. And the more such sentences there are, the more properties of THE WORLD will turn out to be theory-relative. (Putnam [1976], p. 132)

Saying that the world's properties are 'theory-relative' makes it sound as if our theories impose their properties, perhaps even as if the world has no structure of its own and can be imposed upon in any old way we happen to choose. Whether or not Putnam himself intends any of these views, I think the naturalist can be seen to reject them, again with a series of commonplaces.

To see this, consider a crude analogy: suppose the world consists of a deck of cards; then one true theory describes the universe as made up of 52 card-like objects, another describes it as made up of 4 suit-like clump-objects, yet another as consisting of one complex whole. It seems reasonable to say that all these theories are correct, that each of them describes aspects of the way this world is, that each of them ascribes to the world properties that are 'built-in'. Analogously, our naturalist holds that the world our science studies has a built-in structure, that our methods are designed to help us get at this structure, but she needn't insist that there is only one correct way to do this, and she needn't deny that which built-in properties we tend to pick up on is at least partly a function of our cognitive structures and our interests. And to say that there might be several correct ways of describing the world is not to say that *every* way of describing the world is equally good. The history of science is littered with ways of describing the world that didn't work.

But there's another issue lurking in the background of the 'one true theory' discussion, an issue that goes to the heart of our understanding of naturalized metaphysics. In Quine's original version of the view, our ontological commitments were to be assessed by figuring out which things our best scientific theory says 'there are'; we were to imagine an all-inclusive theory T, of 'science' in the broadest sense, and to search through its existential assertions.[31] If there are in fact two equally good theories of the world, two theories that assert the existence of different things, then it seems Quinean naturalized metaphysics is in trouble. To take a simple example, if we have two complete scientific theories of the world, T and T', where T involves points, line segments and lines, and T' involves line

[31] See the classics, Quine [1948] and [1951].

segments, lines and convergent sequences of line segments,[32] the Quinean naturalist seems unable to determine whether or not there are points. Perhaps even worse, this very approach to metaphysics seems to attribute serious ontological import to an issue of theory formulation that strikes most scientists as entirely without significance.[33]

Now it seems to me (as indicated above) that the Quinean picture of scientific theorizing at work here is too simple to do the job he assigns to it: e.g., the existence of atoms was asserted in atomic theory -- part of our best theory -- before the Einstein/Perrin evidence that convinced the scientific community that atoms are more than useful fictions; the existence of continuous substances is asserted in fluid dynamics, though no one believes there are such things; some mathematical aspects of our theories (like the continuity of spacetime) are considered open questions despite the fact that we have no better way to represent the world. The naturalist's scientific study of science will happen upon these and related observations early on, and the moral of the story seems obvious: reading the ontological conclusions off the face of our scientific theorizing is a complex and subtle undertaking, far more complex and subtle than Quine's proto-naturalist would imagine.

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[32] See Putnam [1976], pp. 130-131. The idea, obviously, is that the convergent sequences of line segments of the second theory take the place of the points of the first theory.

[33] See Putnam [1982a], p. 227.

Clearly, our scientific study of science will need to address the problem of when and why two theories with superficially different ontologies are in fact two ways of describing the same underlying reality; the problem of differentiating the many varieties of idealizations and some mathematizations from literal claims, and revealing how they work; the problem of understanding how our more complex mathematical machinery is functioning in our most basic theories; and many more. But these important and legitimate inquiries into the structure and function of scientific theorizing in no way undermine the core of metaphysics naturalized, the idea that science is the best way we know of finding out how the world is.[34] We must face the fact that this 'finding out' is a difficult task, not something that can simply be read off the logical form of our theories, but none of this gives our naturalist reason to suppose that this approach is somehow doomed or that there is any better way to proceed.

Turning now to Putnam's epistemological critique of naturalism, let me first take brief note of a common criticism of epistemology naturalized, namely, that in foreswearing the project of answering the Cartesian skeptic, the naturalist also gives up any normative aspirations. Putnam repeats this as a criticism of Quine in particular, while admitting that many

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[34] As a reminder of the observations at the end of §I, notice that we put the point this way in describing the naturalist's practice; she simply proceeds according to her own methods, unimpressed by proposed alternatives -- e.g., philosophical intuition -- until their merits can be established by her standards.

naturalized epistemologists do undertake normative analyses (Putnam [1982b], pp. 244–245). I'm not sure this is fair to Quine,[35] but in any case, I hope it is clear that my naturalist's scientific study of science includes the effort to evaluate and improve the methodology of science from within, an explicitly normative undertaking. So let's set this issue aside.

A more central theme of Putnam's epistemological critique paints his opponents as prone to versions of relativism or imperialism, both of which he considers self-refuting. I should grant that the opponents Putnam sometimes has in mind here -- the likes of Richard Rorty -- inhabit a different intellectual province from the naturalistically-minded, but I think, nevertheless, that an examination of these issues, as they impinge upon the naturalist, might be illuminating. So let's first ask just how the naturalist might come to be accused of relativism or imperialism.

Suppose that our naturalist has begun her scientific study of science: she calls on her physiological and psychological theories of human perception and conceptualization, her linguistic theories of the workings of human language, and her physical, chemical, astronomical, biological, botanical, and geological theories of the world in which these humans live; she uses these, and any other of her scientific findings that seem relevant, to attempt to explain how these humans, by these means,

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[35] See, for example, Quine [1981a], p. 181.

come to know about this world. Now suppose that along the way, she also takes note of other human linguistic practices, practices different from hers. Some of these, say some forms of chanting or story-telling, don't seem to play the characteristic role of bodies of assertions, but others, like astrology and theology, apparently do. Our naturalist also notices that the evidential standards and norms of these assertion-like practices are not the same as the ones she uses in her own investigations.[36] How should the naturalist treat these cases?

We might imagine a brand of quasi-naturalist[37] who reacts by saying: "Clearly their norms are different from mine. I think mine are justified, as I attempt to show in the course of my scientific study of science. Still, I acknowledge that this justification relies on my norms; I can't expect them to be any more impressed by a justification of my norms in terms of my norms than I'm impressed by a justification of their norms in terms of their norms. Given the symmetry of the situation, I must conclude that their practice is as good as mine." Putnam objects that when this quasi-naturalist says something like 'their assertions are justified by their norms', she's using her own norms of assertion, and he argues that this makes it

[36] In [1997], I argue that the naturalist will discover that mathematics is also a seemingly-assertive discourse with norms differing from those of science, but that the naturalist has reason to treat mathematics as a special case (see pp. 203-205). I leave mathematics aside here.

[37] This may be Fine's NOAer, but I don't pretend to be sure. The discussion at the end of the previous section even suggests that the NOAer's investigation of science may be undertaken from a perspective

impossible for her claim of symmetry to convey what it ought to convey.[38]

While this relativistic position has perhaps some claim to be called 'naturalism', it is not the version of naturalism I'm attempting to describe and intending to advocate. In some cases, my naturalist might conclude that the seemingly-assertive practice is actually pursued for other reasons: perhaps in hopes of producing a certain spiritual state in the case of theological discourse or perhaps as a tool in a sort of psychoanalytic process in the case of astrological discourse. But suppose the naturalist's scientific analysis, drawing on anthropology, sociology, psychology, etc., determines that one or another of these practices *is* aimed, as the naturalist's scientific practice is aimed, at telling us how the world is; suppose, for example, that the astrologer asserts that human behavior can be predicted from the position of the stars or the theologian asserts that certain phenomena are supernatural miracles. In those cases, my naturalist holds that the norms of these practices are outright incorrect, that they are not effective procedures for supporting the stated claims (recall Feynman's rejection of astrology in the long passage quoted in §I). The others might protest that she reaches these conclusions using her own evidential standards, but

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other than that of science, but this is not part of the quasi-naturalistic view under consideration here.

[38] That is, when she says, 'from their point of view, my assertions are justified by my norms', this claim is justified by her norms, not by theirs. See Putnam [1982b], pp. 237-238.

this she happily grants. They are her standards, the best standards she knows. Of course, she admits that they are subject to criticism and modification, but only on legitimate scientific grounds, and neither the theologian nor the astrologer has presented any such critique.

But perhaps cases like astrology and theology seem too easy. The sociologists of science draw attention to episodes from the history of science when theories or even 'conceptual schemes' different from ours have held sway, arguing that these alternatives were equally successful at justifying themselves on their own terms and that their eventual demise was not rationally justified. Now the naturalist, with her stubbornly piecemeal approach, will consider such examples case-by-case, with an eye to explicating the details of each, but perhaps one general observation might be offered: the naturalist's scientific study of such episodes will aim to assess the relative merits of the discarded, alternative scheme; in many such cases, existing studies give us reason to suppose that the decisions of the scientific community were considerably less arbitrary than the sociologists would have us believe;[39] still, it is would be foolish for the naturalist to ignore the possibility, indeed the likelihood, that evidentially-irrelevant, irrational factors *have* played an unsavory role in the development of science.

[39] See, for example, Kitcher's skeptical treatment (in his [1993]) of cases studies of Kuhn, Doppelt, Shapin and Schaffer.

Quine makes a similar point, against the background metaphor of Neurath's boat:

> The ship may owe its structure partly to blundering predecessors who missed scuttling it only by fools' luck.

Ferreting out these improperly-supported passages is a first step towards the naturalist's goal of improving science from within. Still, as Quine goes on to caution:

> … we are not in a position to jettison any part of it, except as we have substitute devices ready to hand that will serve the same essential purposes. (Quine [1960], p. 124)

Once the weak planks are found, the next job is find more stable replacements. All this is part of naturalism; none of it constitutes relativism.

There remains the logical, as opposed to historical, objection that there might be a methodology completely different from ours that would generate a science completely different from ours, but would nevertheless be as good as our scientific methodology at uncovering the way the world is. I think there is no denying this bare possibility. As Quine puts it:

> Might another culture, another species, take a radically different line of scientific development, guided by norms that differ sharply from ours but that are justified by their scientific findings as ours are by ours? And might these people predict as successfully and thrive as well as we? Yes, I think that we must admit this as a possibility in principle; that we must admit it even from the point of view of our own science, which is the only point of view I can offer. I should be surprised to see this possibility realized, but I cannot picture a disproof. (Quine [1981a], p. 181)

But this bare possibility is methodologically empty.

At this point, it appears that our naturalist is far more susceptible to a charge of imperialism than to a charge of relativism, so it is worth asking why Putnam thinks imperialism is self-refuting. As it happens, the argument turns on Putnam's understanding of what a naturalist like mine, an imperialistic naturalist, would have to say about truth. Thus we are returned to the question set aside in connection with Fine at the end of §1: the question of truth.

III. Naturalism and truth

What's striking is that the notion of truth enjoys a special status in all these discussions. Putnam thinks that both his materialistic opponent (in [1982a]) and his imperialistic opponent (in [1982b]) are committed by the very structure of their positions to particular views about truth. And though Fine's general approach is summed up in the imperative 'Induction again; let us look and see' (Fine [1996], p. 180), he also thinks that his NOAer is committed at the outset to a particular position on truth. Here the contrast with the naturalism I've been describing is stark: my naturalist isn't committed to any particular position on truth simply on account of her naturalism; she is committed to a scientific approach to the question, but this alone doesn't prejudge or predict how that inquiry will turn out.[40] Let me glance at what I take to be the current state of

[40] This goes for other topics as well, e.g., the status of logic.

naturalistic, that is, scientific inquiry into the notion of truth, then return to the arguments of Putnam and Fine.

In fact, I think we've already made one relevant observation in connection with van Fraassen's 'empirical adequacy'. Recall that in a case like that of the post-Einstein/Perrin atomic theorist, it seems incorrect to interpret the claim 'there are atoms' to mean that the assertion of the existence of atoms is empirically adequate: it was considered empirically adequate *before* Einstein and Perrin; afterwards it graduated to another status. I think similar observations of the practice of science will rule out the range of verificationist-style notions of truth. Ordinary scientific practice distinguishes between the claim that 'our meters read so-and-so' and the existence of particles, between 'we have experiences such-and-such' and the existence of medium-sized physical objects, between 'it's useful to act as if there are atoms' and 'there are atoms'. The only hope for such positions is to remove the discussion to a higher level, where the ordinary scientific evidence for existence is judged inadequate, but the naturalist will stubbornly resist any such ascension.

Setting verificationism aside, there remains an ongoing scientific debate about the nature of truth. In the early 70s, Field claimed that Tarski's theory of truth does not do the full job of showing that 'truth' is a scientifically-acceptable notion; Field's thought is that Tarski's account needs supplementation by a robust account of reference (see Field [1972]). In the course of this argument, Field admits that

> this sort of argument … is only as powerful as our arguments for the utility of semantic terms; and it is clear that the question of the utility of the term 'true' … needs much closer investigation. (Field [1972], p. 374)

In a subsequent paper, Leeds ([1978]) undertakes this closer investigation, concluding that the role 'truth' actually plays in science can be filled by something much more modest than what Field has in mind, namely, by a disquotational or deflationary theory of truth, derived from Quine. Thus the question is raised: does science require a robust correspondence theory of truth or can all its explanatory purposes be served by a deflationary theory? The debate continues to this day.[41]

Under these circumstances, what is the proper theory of truth for the naturalist? Given the naturalist's scientific approach, it seems clear that the question remains open. If it should turn out that the purposes of science require a robust correspondence theory, so be it; if not, the naturalist rests content with a deflationary theory. Perhaps it will turn out that both these options are misguided in some fundamental way. The only specifically naturalistic commitment in all this is to follow scientific inquiry wherever it might lead.

With this mundane observation as background, let's return to Putnam's case against the imperialist. Addressed to cultural imperialism, Putnam's argument begins like this:

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[41] See, for example, Field [1986], Horwich [1990], Gupta [1993], Field [1994], Leeds [1995].

> He [the imperialist] can say, 'Well then, truth -- the only notion of truth I understand -- is defined by the norms of *my* culture.' ('After all', he can add, 'which norms should I rely on? The norms of *somebody else's* culture?') (Putnam [1982b], p. 238)

Thus, the imperialist's notion of truth 'cannot go beyond right assertibility' (ibid., p. 239). The trouble, according to Putnam, is that our culture does not include a norm of the form:

> A statement is true … only if it is assertable according to the norms of modern European and American culture. (ibid., p. 239)

So, Putnam concludes:

> if this statement is true, it follows that it is not true … Hence it is not true QED. (op. cit.)

Thus imperialism is self-refuting in 'modern European and American culture', though it might not be if

> as a matter of contingent fact, our culture were a totalitarian culture which erected its own cultural imperialism into a required dogma, a culturally normative belief. (op. cit.)

Our job is to consider how this style of argument might apply to our naturalistic imperialist. We begin, again, with the notion of truth. To determine whether or not a statement is true, the naturalist applies the norms and standards of her science. From here, the Putnamanian line of thought concludes that she is committed to an account of truth in terms of 'right assertibility' rather than 'correspondence'. But why should this be so? When the naturalist is asked to settle a question of truth, she will indeed appeal to her scientific norms and standards, but she needn't view this as a *definition* of truth;

furthermore, we've seen that such a verification-based theory is not likely to emerge from her scientific study of the notion. Indeed, defining truth as 'right assertibility' would convert one important challenge for her scientific study of science -- the task of showing that her norms and standards are dependable methods for determining how the world is -- into an analytic certainty. Any theory of truth that trivializes this difficult undertaking should certainly be rejected.

So, I think my naturalist is clearly not committed to the Right Assertibility theory that Putnam attributes to the imperialist. But Putnam also has a truth-based argument against his other main opponent, the materialist. Indeed, in his [1982a], Putnam goes so far as to *define* his opponent's position to include a correspondence theory of truth:

> What the metaphysical realist holds is that we can think and talk about things as they are, independently of our minds, and that we can do this by virtue of a 'correspondence' relation between the terms in our language and some sorts of mind-independent entities. (Putnam [1982a], p. 205)

We've seen that the naturalist does hold that we can think and talk about mind-independent things; we've also seen that whether or not this involves a robust correspondence theory of truth is still open to debate. This debate will be resolved in terms of the actual role of truth and reference in the explanations of science, an idea that was once clear to Putnam:

> the *success* of [human language use] may well depend on the existence of a suitable correspondence between the words of a language and things, and between the sentences of a language and states of affairs. The notions of truth and

> reference may be of great importance in explaining the relation of language to the world … (Putnam [1978], p. 100)

If this explanatory role, or some other, is served by a correspondence theory in ways it can't be served by a deflationary theory, we obviously have strong scientific grounds to try to develop a viable correspondence theory. But a correspondence theory is not mandated by naturalism tout court.

That point made, we should consider Putnam's reasons for holding that adherence to the correspondence theory serves to undermine his opponent's position; if what Putnam puts forth is a properly scientific objection, then the naturalist should take note and factor this into the ongoing debate. Alas, Putnam returns instead to the vicinity of his Kantian corollary:

> The problem that the believer in metaphysical realism (or 'transcendental realism' as Kant called it) has always faced involves the notion of 'correspondence'. … How can we pick out any *one* correspondence between our words (or thoughts) and the supposed mind-independent things *if we have no direct access to the mind-independent things*? (German philosophy almost always began with a particular answer to this question -- the answer 'we can't' -- after Kant.) (Putnam [1982a], pp. 206-207)

What Putnam disapproves here is not a scientific correspondence theory that attempts to describe a connection between the words humans use -- as understood by linguistics, psychology, etc. -- and things -- as understood by physics, chemistry, biology, etc. Rather, what he has in mind is a transcendental Correspondence Theory -- capital 'C', capital 'T' -- formulated without the help of ordinary scientific theorizing, connecting our words with transcendental things in themselves.[42] Obviously this is not the sort of correspondence theory -- small letters -- that interests the naturalist in the first place, so Putnam's critique is irrelevant. In sum, then, I think that the naturalist isn't, and shouldn't be, committed to either of the truth theories Putnam proposes -- the Right Assertibility theory or the transcendental Correspondence Theory -- and that the jury is still out on what theory she should embrace.

Let me close this discussion of truth with a few words about Fine and the NOAer. While it is sometimes difficult to reconcile this position with other passages in Fine,[43] he clearly takes the NOAer to reject both correspondence and verificationist theories of truth:

> Thus NOA is inclined to reject *all* interpretations, theories, construals, pictures, etc., of truth, just as it rejects the special correspondence theory of realism and the acceptance pictures of the truthmongering anti-realisms. (Fine [1986a], p. 149)

As this passage suggests, Fine's NOAer also rejects deflationary theories; though Fine admits elsewhere to some passing fondness for them, he does not succumb:

> Although I am sympathetic to the deflationary approach to truth defended by Horwich [1990], I still prefer a plain no-theory attitude. (Fine [1996] p. 184)

[42] Putnam himself distinguishes between 'a "correspondence" between words and sets of things … as part of an *explanatory model* of speakers' collective behavior … [as] a scientific picture of the relation of speakers to their environment' and the Correspondence Theory involved in 'metaphysical realism' (Putman [1976], pp. 123-4).

[43] See Musgrave [1989] for discussion.

So the question for us is: why does Fine think the NOAer should eschew all theories of truth?

A partial answer comes in this argument against the correspondence theory:

> The correspondence relation would map true statements … to states of affairs … But if we want to compare a statement with its corresponding state of affairs, how do we proceed? How do we get at a state of affairs when that is to be understood … as a feature of the *World*? … The difficulty is that whatever we observe … or causally interact with … is certainly not independent of us. … whatever information we retrieve from such interaction is, prima facie, information about interacted-with things. (Fine [1986b], p. 151)

We have here a rerun of Putnam's argument that the correspondence theorist needs but cannot have 'direct access to the mind-independent things' (Putnam [1982a], p. 207), a consequence of his Kantian corollary.[44] In other words, what Fine, like Putnam, is rejecting is a transcendental Correspondence Theory of the sort our naturalist would never so much as consider. Surely we can agree that this is not the sort of theory the NOAer should embrace, but this fact leaves untouched the question of the scientific correctness of the ordinary (small letter) correspondence theory.

A more complete answer to our question begins from this passage:

> If pressed to answer the question of what, then, does it *mean* to say that something is true (or to what does the truth of so-and-so commit one), NOA will reply by pointing out the logical relations engendered by the specific claim and by focusing, then, on the concrete historical circumstances that ground that particular judgment of

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[44] Compare Musgrave [1989], pp. 53-58, discussing Fine: 'Kant is, of course, the philosopher who started the rot here' (p. 56).

> truth. For, after all, there *is* nothing more to say. (Fine [1986a], p. 134)

So far, this is little more than a reiteration of the claim that the NOAer has no theory of truth, but in a footnote to the final sentence, Fine goes a bit further:

> Not doubt I am optimistic, for one can always think of more to say. In particular, one could try to fashion a general, descriptive framework for codifying and classifying such answers. Perhaps there would be something to be learned from such a descriptive, semantical framework. (op. cit.)

This sounds like the sort of scientific study of the role of truth in scientific explanations that the naturalist proposes to undertake. Fine continues:

> But what I am afraid of is that this enterprise, once launched, would lead to a proliferation of frameworks not so carefully descriptive. These would take on a life of their own, each pretending to ways (better than its rivals) to settle disputes over truth claims, or their import. What we need, however, is less bad philosophy, not more. So here, I believe, silence is indeed golden. (op. cit.)

In other words, Fine is not holding that a scientific study of truth is impossible, or that it cannot lead to a useful semantic account of language, but that it is also so likely to lead to bad philosophy that it should not be undertaken in the first place. In response to this concern, the naturalist simply trusts to the safeguards of science.

IV. Conclusion

I have tried to illuminate the contours of my post-Quinean version of naturalism first by tracing early occurrences of what I take to be the fundamental naturalistic impulse in Reichenbach, Quine and Fine, and by indicating where my naturalist would disagree with the further elaborations of these proto-naturalists. I then outlined a range of contemporary objections to vaguely naturalistic projects of various sorts and showed how they fail to touch the naturalism I'm recommending. Finally, I sketched Putnam's and Fine's thoughts on the theory of truth and attempted to turn away the suggestion that a naturalist, simply by virtue of her naturalism, is committed to one position or another on this issue. In the end, I hope at least that the position has been clarified. I leave to the reader any further musings on its viability.[45]

Penelope Maddy
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[45] My thanks to my colleagues Jeffrey Barrett and Kyle Stanford for pressing me on these questions (and to an anonymous referee for further critique). I regret that my answers haven't satisfied either of them!

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