Naturalism in Epistemology

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ABSTRACT. Naturalism is often presented as a methodological assumption, that the best way to find things out is by empirical means. But since no one doubts this, we understand it at once as signaling that there is nothing else to be found out in any case. That links the methodological dictum at once to Naturalism understood as the ontological view that everything there is, is physical or material or within the domain of the natural sciences. That side is not my topic; I will focus on traditional problems of epistemology. There is, as I see it, a definite parting of the ways, where Naturalism and Empiricism develop a completely different conception of the that subject. I will trace this development through the past half century or so, from the *1944 School of Naturalism*, via Willard van Orman Quine, to such nearer contemporaries as Stephen Leeds, Michael Devitt, and Penelope Maddy.

Contents

Naturalism is a coat of many colors. My interest here is in similarities and differences between current forms of Empiricism and Naturalism, concerning epistemology.

Issues in metaphysics will all be left aside. Naturalism is often enough presented as a methodological assumption, that the best way to find things out is by empirical means. But since no one doubts this for things that can be found out by empirical means, we understand it at once as signaling that there is nothing else to be found out in any case. That links the methodological dictum at once to Naturalism understood as the ontological view that everything there is, is physical or material or within the domain of the natural sciences. Those terms have their own obscurities, it is hard to say what is definitely excluded. But it is typically understood to classify, as needing to be 'reduced' or explained away, such items as consciousness, reference, goodness, beauty, purpose, functions, …. [1]

None of that will be within my topic here; I will focus on traditional problems of epistemology. For my exemplars I will take just a few writers of the past half century or so, from the *1944 School of Naturalism*, via Willard van Orman Quine, to such nearer contemporaries as Stephen Leeds, Michael Devitt, and Penelope Maddy.

Naturalism's and Empiricism's Common Ground

For Naturalism and Empiricism I see three commonalities. I will briefly announce these here, and then explain each at some length. Once we reach the end of the common ground there will be a parting of the ways.

First, in epistemology at least, Naturalism , like Empiricism, proclaims itself as a *stance* rather than a thesis or dogma. But it is not the same stance.

Second, contemporary Empiricism and Naturalism agree in their *anti-foundationalism* in epistemology. Both reject the idea that rational belief or knowledge must, or even can, rest on foundations that ensure truth (or even likelihood of truth) which can be seen a priori to provide such a guarantee.

Third, Naturalism and Empiricism place the admiration of empirical science, as a paradigm of rational inquiry, at center stage. This generally includes a rejection of certain forms of metaphysics; but not altogether the same forms. Nor is the centrality of science quite the same centrality.

The two stances are not the same stance. We need to ask how there can be a dialogue between two such stances, in some ways so alike and in some ways sharply opposed to each other.

Naturalism As Stance

Philosophical positions can be roughly divided into those identifiable with a dogma or

thesis about what there is, or what the world is like, and positions not so identifiable. As I have argued elsewhere (2002), whatever Empiricism can be today, it cannot be of the former sort. It is a philosophical *stance*. By a stance I mean a position which consists in attitudes, commitments, and a characteristic approach to philosophical problems, possibly including or presupposing some beliefs as well, but not defined by a thesis or doctrine.

Naturalism is also in important instances presented as a philosophical stance. This can be illustrated with examples from the mid 20th century to the present.

Stance (1) 1944 and the turn against theory

The Columbia University centered group of philosophers somewhat mockingly called "the 1944 School of Naturalism " published a manifesto, *Naturalism and the Human Spiri*t (Krikorian 1944; cf. Kim 2003: 86-87). The contributors you may recognize include John Dewey, Sidney Hook, Ernest Nagel, and John Herman Randall.

In their essays we see two themes emphasized. The first is an explicit rejection of doctrine, in favor of commitment, attitude, approach, and method. Thus John Herman Randall refers to Naturalism as "an attitude and temper," and says that it is "essentially a philosophic method and a program." Sidney Hook calls Naturalism "commitment to a procedure, not to a theory of metaphysics." The second is that the paradigm example inspiring their philosophical approach comes from the sciences: the same procedures that characterize scientific inquiry are to be the core of an naturalistic philosophical approach. As a review of this volume describes it:

> Starting from the acknowledged achievements of scientific inquiry so far, the "naturalists" intend to show that these same methods, or others essentially "continuous" with them, are adequate also to those aspects and dimensions of "the human spirit" which in the past have often been held on philosophical grounds to transcend the methods and aims of science." And Murphy quotes Hook as saying that in regard to religious beliefs the naturalist only asks "to be given an opportunity to examine the evidence and to evaluate it by the same general canons which have led to the great triumphs of science. (Murphy 1945: 405)

Sidney Hook exemplifies both themes in his definition of Naturalism as "the wholehearted acceptance of scientific *method* as the only reliable way of reaching truths about the world of nature, society, and man" (my italics).

Thus we can see, in this mid-century manifesto, both the insistence that Naturalism is -- to use my own term -- a *stance*, and the special place that the sciences occupy for this stance. In Hook's words, but equally evident elsewhere in this volume, science provides a paradigm for rational methodology, to be taken as guide for what philosophy can be.

The similarities in these two respects to the Logical Positivism and Logical Empiricism, which were at that time beginning to exert their special influence on American philosophy, spring to the eye. Yet there are clues to how the differences would persist and in some respects intensify, in the style and the topics of concern of these writers, both far from what characterized those empiricist movements,.

Stance (2) Penelope Maddy's Version

A half century later we find Penelope Maddy's sustained advocacy for a Naturalism tailored for the present, but continuing these pervasive themes of the 1944 School. Maddy's article "Naturalism: Friends and Foes" begins with the meta-philosophical view locating her Naturalism :

> 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 …. (Maddy 2001: 37)

thus characterizing it quite explicitly as a *philosophical stance*.

The agreement with the Empiricist stance I have advocated may not go much farther, but the similarities and parallels certainly do. She sees her position as the current instance of a tradition identifiable through this common approach, while feeling free to reject much of what the earlier advocates (*proto-naturalists*, as she calls them) took as essential.

Maddy goes on to display her Naturalism as a recurring revolt against transcendentalism, in a broad sense -- against "two-level" philosophical views, citing Kant, Carnap, and myself as main examples. There is a similarity again, in that Empiricism certainly stands out as a recurring revolt against realist metaphysics, against what Kant called "transcendental realism". The difference -- but again, an interestingly symmetric one -- is that in ontology, her Naturalism is best likened to transcendental realism (and she does sometimes speak of naturalistic metaphysics!), while she ranks Constructive Empiricism among the foes on the transcendentalist side.

These parallels could, despite the differences, inspire a certain natural sympathy. We are arraigned against forms of traditional metaphysics that purport to be a pursuit of truth by the same means as, but beyond, the empirical sciences. Yet we may already be detecting a tension in her position that, from my point of view, is irresolvable. Maddy dissolves or displaces a goodly array of traditional philosophical problems by concentrating our attention on what, in Moorean fashion, she can take as rock bottom common sense. This is the common sense of the educated layperson, in our culture today, who

> is a member of the scientific community [and] regards the methods of science as her own, as the best methods we have for finding out what the world is like (Maddy 2001: 39)

This description does not by itself select philosophers, obviously; it applies to the Naturalistic philosopher only as one instance among the other intellectually-non-alienated. Yet this passage began with the subject term "The naturalistic philosopher", and it continues with

> 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.

This is the Naturalistic *philosopher*, not simply a member of the scientific community.

The tension between the two identities, that of a member of the scientific community (as conceived by a Naturalist philosopher) and a Naturalistic philosopher proper, grows as we see Maddy turning to specific philosophical problems in her book *Second Philosophy: A Naturalistic Method* (2007).

I do not want to use the name "Second Philosopher" here. Use of this epithet in our discussion here would too easily invite a confusion of the character portrayed with the portrayer, and allows for too much slippage between the natural attitude and the philosophical orientation in which she reflects on that attitude. Taking a cue from her phrase "a Native of the contemporary scientific world view" (Maddy 2001: 48), let us call anyone who fits her description a *Naturalistic Native* – or where the context precludes ambiguity, just say *Native*.

It is the Naturalistic philosopher who describes and defines the Native. Paraphrasing Umberto Eco, we could say that the Naturalistic philosopher holds up this Native as the 'model reader' of the texts which the sciences offer the public. We all know how to play the part of the model reader, whether of those texts or of historical novels, science fiction tales, or ideological manifestos. So even if we are not in the same role, we can understand what it is to play such a role.

But is this Native a second *philosopher*, or just any native of the scientific worldview? This is where that tension becomes apparent when we follow her engagement with philosophical issues. Obviously Maddy does more here than describe a certain subclass of the world's population. She selects and identifies with this selection in a way quite different from her attitude toward the comparable elements of the artistic or religious or political or philosophical communities in which any of us may have grown up. Indeed, she portrays the Native as scientifically detached from all that, with its interest limited to

the anthropology, psychology, and sociology of those areas. The Native

> is equally at home in anthropology, astronomy, biology, botany, chemistry, linguistics, neuroscience, physics, physiology, psychology, sociology .... (Maddy 2007: 2)

a Renaissance woman, we'd almost say; but then the passage continues

> Her interest in other subjects, at least as far as we see her here, is limited to her pursuit of their anthropology, psychology, sociology, and so on. (ibid.)

The description that the philosopher gives of the Native depends crucially on the distinction between what is science -- what are its methods, mentioned saliently above, and what is the content of scientific theories (so far) that currently counts as common knowledge -- and what is not. Identifying that distinction is well known as the 'demarcation problem', and Maddy has no more taste for it than any of us does these days. She agrees with Arthur Fine, she writes, that "it is probably hopeless to search for necessary and sufficient conditions that separate science from the rest" but continues at once to face the obvious critique:

> isn't Naturalism the view that scientific methods are the only legitimate source of evidence …; doesn't it take a viable demarcation even to state the position? (Maddy 2001: 48)

In shunting this objection aside Maddy reveals what seems to me her main philosophical tactic whenever it comes to a truly worrisome question for her position. She *retreats* dialectically from the level of Naturalistic philosopher to that of the ordinary Naturalist Native:

> 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. (*ibid.*)

She is quite right here about the Native, the Naturalist's model reader of today's science text books. But the random, arbitrarily selected Native is no philosopher, and cannot be confronted with philosophical questions, unlike the naturalistic philosopher who has to answer her peers. At this point the term "naturalist" has entered her vocabulary in studied ambiguity between "Naturalistic Native" and "Naturalistic philosopher".

What precisely is the rhetorical tactic put in play here? The Native's intellectual position, in general not even rising to the level of conscious attention, consists in her identifying herself, or rather her maintaining her self-identification, as a member of this community, with its tradition and tacit agreements about what counts and what doesn't, who counts as a peer, and what counts as common knowledge. For the paradigm Naturalistic Native this is not the result of a deliberate choice, no alternatives being faced or considered or even brought to awareness, except perhaps those already classified as beyond the pale -- astrology, creationism, augury or haruspicy -- and hence not present as 'live'. But as soon as this character confronts traditional philosophical positions, hence acts as philosopher, all of that does rise to the level of consciousness, while remaining somehow outside the scope of any critical scrutiny. Rhetoric aside, we will need to examine those reactions to traditional philosophy at a later point.

Anti-Foundationalism: Epistemic Life Without Security

We come now to the second feature that Empiricism and Naturalism have in common: anti-foundationalism in epistemology. This commonality is not surprising: both are heirs to the realization of the failures of traditional epistemology.

In our time, this anti-foundationalism was (re-)announced in a veritable clarion call by Willard van Orman Quine, who liked to cite Otto Neurath's (1932) striking metaphor of mariners at sea.[2]

> "Neurath has likened science to a boat which, if we are to rebuild it, we must rebuild plank by plank while staying afloat in it" (Quine1960: 3)
>
> "The Humean condition is the human condition." (Quine 1969: 72)

These quotes address the two steps in foundationalist epistemology:

1) there is a secure basis for rational opinion, belief, and knowledge
2) there is secure ampliative generation of justified, reliable opinion, starting from that basis

The first step has seen such slogans as "nothing is probable unless something is certain" (C. I. Lewis), the second refers to Hume's critique and the traditional problem of Induction.

Quine targeted both in his critique, and announced the demise of both. I want to take a

few moments to spell out this demise, which happened not at the hands of Quine, nor due to the advent of naturalized epistemology. On the contrary, it is part of Naturalism's heritage from an earlier Empiricist avant-garde. Specifically, the thesis that rational opinion has or requires a foundation in certainty was subjected to a thorough and definitive critique by Hans Reichenbach in his *Experience and Prediction*, and in his debate with C. I. Lewis at the 1951 American Philosophical Association (Reichenbach 1938, 1952; Lewis 1952). But I shall concentrate here on the second, which is, even now, less un-controversially to be discarded.

Induction

Is there a Problem of Induction? Hume stated it, didn't he? And many philosophers after him tried to solve it, to justify Induction, some of them announcing real success? But in order for there to be a problem about justifying something, that thing has to exist.

Let's just say that *induction*, written with a little "i", is something we all do: it includes any move to an opinion that goes beyond what we know, or have as evidence. Obviously we do that all the time. That includes hasty generalizations and superstitious credulousness, as well as responsible, careful drawing of conclusions. But what there was supposed to be a problem about I will call *Induction* spelled with a capital "I": a putative method that is

- genuinely ampliative, but
- humanly learnable,
- objective, and
- rationally compelling, while
- leading reliably to true conclusions.

Induction was certainly one of those many philosophical creatures that baptized before it was born. In fact, if we insist on criteria of success, this one did not ever actually see the light of day.

What exactly did Hume call into question? The great scientific hero of the time, Isaac Newton, claimed to have established his System of the World by the Method of Induction, something that could not have been done by his rival Descartes' Method of Hypotheses. We have to suppose that Hume was addressing this famous claim by his famous compatriot. Now, Newton did not say what Induction was. As spelled with a little "i", Newton certainly engaged in induction; but the ways in which we go beyond our evidence in the opinions and beliefs we form range from the totally precipitate to the milque-toastly chary. There is indeed a question about how to find a good balance between the two, just as there is for investing, courtship, diplomacy, and all the other practical arts. But that is not what Hume set about doing, and he was not concerned with straightening out our little "i" inductive moves. The idea Hume calls into question is that there really is a *method* for doing that in such a way as to get things right, *a method*

in the proper sense of the word, a sort of general recipe that we finite beings can follow, and justifiably rely on.

Reichenbach's pragmatic turn, and its failure

To my mind the best chapter in this history of the Problem of Induction is Reichenbach's. First of all he diagnosed the British empiricists' debacle: they had accepted a criterion of adequacy imposed by their rationalist opponents, who asked for certainty (Reichenbach 1948). By such a criterion it would follow at the very least that, to be justified, an inductive method would have to be shown to lead to conclusions for which we can say with certainty that they are reliable, whenever applicable, and where the conditions of applicability are directly accessible to us. As Reichenbach pointed out, that is over-reaching: the world might be such as to confute any method at all for predictions. That we cannot rule out beforehand. So he proposed a weaker criterion that would, if satisfied, be the best we could have, *and* would be good enough. It was this:

> that the method should lead demonstrably to the truth, if any method could -- that is, under conditions in which any method at all that could succeed.

Not only that, he advanced the notorious straight rule, the very numerical induction rule that everyone had despised, as the basis of all good induction. And his justification was this: if in a series of instances there is a limiting relative frequency for a certain property, then the straight rule provides a series of estimates for prediction of what will come next that converge to that frequency. If there is no such limiting relative frequency then no method at all can concoct a reliable scheme for prediction.[3]

So wasn't this success? And wasn't it the very success that was wanted? Certainly not for this straight rule, baldly stated. If a weather forecaster always gave us his predictions for rain the next day, following that simple rule, he would after the first thousand days or so become extremely boring and uninformative. For the probability for rain he would announce for tomorrow would differ from today's by at most 1/1000, even if meanwhile the once blue heavens had become dark with threatening clouds, or winter had turned to spring.

We would want his predictions to be sensitive to conditions that single out *sub-sequences* in the long series of days: summer days, or days following ten days of rain, or Federal Holidays, for example. To make this challenge realistic, these conditions must be ones that we and he could check on before the announcement. It is possible to make that requirement quite precise: the challenge is allowed to apply to all and only the finitely or recursively definable subsequences of future days, with the definition drawing only on what is settled beforehand.

But *demonstrably*, no rule can meet that standard, not even if we add "if any rule could". For any scenario at all there is a rule that does well in that way, but there is demonstrably no rule that does well under all scenarios. It was Reichenbach's own student Hilary Putnam who first introduced the argument to prove this; later it was extended and made precise by statisticians.[4]

So precisely what Reichenbach wanted to have met as criterion, although seemingly so modest, when refined by what interests us in practice, was something demonstrably impossible.

Quine was right when he concluded that "The Humean condition is the human condition"(Quine 1969: 72).

Anti-foundationalism reigns!

The reaction, common to Naturalists and Empiricists, is not skepticism! It is a resolute decision to adopt a starting point, without demanding a guarantee of reliability, let alone certainty, as epistemic basis or epistemic policy for belief change.

Starting with G.E. Moore's "Proof of the External World", we have claimed the *right*, while remaining philosophers, to set skeptical doubts aside. In our epistemic life, there is no information-less prior, or rationally justified prior, we start from where we are … From our own current opinion, always with an eye to improvement … As Quine writes, "There is no point of cosmic exile", and more specifically:

> In studying and criticizing our cognitive procedures, we should use whatever powers and procedures we antecedently have and accept. There is no starting 'from scratch' ... (Quine 1960: 275).

We are mariners at sea; we rely, of necessity, on the very boat whose rotten planks we tear out and replace. We rely, of necessity, on what this very boat affords that we are trying constantly to change, repair, improve. There is no secure prior to which we can withdraw, we have to start whatever we do from where we are.

For a striking example of this reaction in a contemporary Naturalist, consider Stephen Leeds.[5] Let's begin with a passage which manifestly express his Naturalist starting point, his intellectual home-ground:

> our methodology … is in a sense parochial: there could be people whose standards of simplicity or explanatory adequacy differ from our own (20th century, Western). From this, it is sometimes taken to follow that our methodology, as one among many, has a small chance of selecting true theories. But if we

> are interested in the chance that *this* methodology, described in all its particularity – i.e., *with our actual standards* of simplicity and explanatory adequacy – will select true theories, then it is irrelevant whether it is one among many …. (Leeds 1994: 202; my italics)

I would submit that the 'careful Realist', whose position Leeds here articulates, can make a relevant, pertinent case only if he speaks in the first person, to express his commitment to his own established position, that is, to express a stance. If he does so speak, I do think that he is in a reasonable, in fact rationally unassailable position. He has the right to speak from that point of view, and can do so, as it seems to me any philosopher has the right to do, until and unless he is toppled from that pedestal by acknowledged insuperable difficulties.

The support that Leeds details in third-person terms actually hides, and not very deeply, that essential first-person commitment. He emphasizes that the sort of Realist he is "will deny that we choose our methodology at random. Rather, he will tell a story according to which the world in some sense has guided us to the right standards of explanation and simplicity"(Leeds 1994, p. 204) This is a point about the *coherence* of the position: the commitment to *this* methodology coheres with his beliefs about what the world is like, in respects affecting the possible success and failure of such a methodology:

> A careful Realist will present his story as a corollary to what he takes himself already to know about the world, and in particular, a consequence of the fact – remarkable, but solidly integral to the Realist's world picture – that some, although by no means all, of the most fundamental laws of Nature involve processes which appear also on the macroscopic level …. It is for this reason that, a Realist can argue, a creature who can find his way around the macroscopic world must inevitably be equipped with the 'right' standards of simplicity and explanation. (ibid.)

That is right *precisely*, *but only,* for the Realist who accepts his science as true. There is no general theory *independent of what this careful Realist holds as true about the world*, which will establish anything like that. It sounds objectively impersonal, but in fact continues the personal expression of his creed.

We can see the stance at work, quite clearly and quite effectively, in Leeds' writings about language, where he continues within the 'Received View' when needed, but once again expresses an specific epistemic position within which all that is to be understood. Leeds is arguing here for a deflationist rather than correspondence notion of truth, and the

way he locates that advocacy clearly reveals a committed starting point:

> For [my deflationist], the entire discussion of truth takes place against a background theory which our semantical discussions do not call into question: roughly the non-semantic part of a garden-variety Realist's theory about the world; it is against this background, in which the existence of people and ships and atoms is taken for granted, that we give our definitions of [...] relations between language and the world." (Leeds 1995: 10)

This expresses Leeds' commitment, it expresses what is, as he says, "taken for granted", the framework within which he lives and moves and has his being

This is not an objection but a clarification. A philosopher taking this position does not claim to have a basis shared with his opposition that would establish his position as correct, but rather the right to speak from his own conviction. He has taken his stand, and he has a right to do that. This is proper to a philosophical stance, provided of course it is acknowledged as stance.

The Parting of the Ways: Naturalism Takes Off

To reject the presupposition of epistemic security is precisely to accept Neurath's metaphor, that there is ultimately no safe harbor, that we are like mariners who are at sea already, and must maintain and repair and improve our ship while at sea. This is a common starting point for both Naturalist and Empiricist in epistemology. Yet, with that same rationale, they take different positions. How we relate to that realization, that as Quine put it 'The Humean condition is the human condition', is quite different. The starting point we have in common, but there are forks in the road from there on. How does this happen?

A diagnosis of the forking path

The Naturalist lives on Neurath's ship too, but with such sanguine and irenic confidence, that expressions of fear and trembling, or anguish for our epistemic responsibility, sound incongruous there.

It is a truism, Neurath's truism, that we must start from where we are. What we *can* take as our starting point is not up to us; but the attitude toward our starting point is. So how could this attitude be different?

The first part of the explanation must be that, despite the truism's seeming implication that we have no choice (where we are is where we find ourselves, we are thrown into a world not of our making) there is in fact some choice involved after all. For, actually, the truism notwithstanding, if we ask where we are just now, we are confronted by a plethora of answers among our neighbors and peers, as well as by uncertainties and

ambiguities in what we find ourselves tending to answer. I become aware of my mariner's fate: does where I am now, epistemically, include all that I learned in college? in high school? at my mother's knee? Actually, what was it that I learned, as opposed to 'learned'? The word "learn" is factive, while "accept unthinkingly and docilely when told" is not!

So there is selection involved in what we will *take to be* our actual starting point. But there is a much more important difference, which lies precisely *in the attitude taken toward the starting point.*

Whatever it is that we take for granted, pro tem, at the outset, we still have two options about how to take it. Firstly, we can take it as the basis we willingly affirm as the rock to build on, our personal foundation, so to say. That is certainly one way to follow suit with respect to science, or whatever else we accorded that status, taking it as the received knowledge, or securely founded opinion, about what is the case. This we can indeed do without falling into the trap of dogmatic foundationalism. But alternatively, we can take it as our natural first target for analysis and reflection, for critique and interpretation, as precisely what we need tot subject to critical reflection.

This is where we see the crux to this parting of the ways. As I see it, the Naturalist opts for the first option, while the Empiricist sees it as the philosopher's task to opt for the second. To make this plausible, let me try to articulate what I see as the Naturalist orientation, the one that I take to exhibit the first rather than second form, illustrated by passages from some among the inheritors of Neurath's and Moore's laudable and courageous step. I mean certain latter-day Naturalists who take the *currently accepted scientific view* as part of our commonsense knowledge: Willard van Orman Quine, Stephen Leeds, Michael Devitt, and Penelope Maddy.

The 'fundamental naturalistic impulse'

While Moore wasn't much concerned with the sciences, these latter day followers certainly are. Thus Quine characterizes his own position in a theory-adjusted echo of Moore by saying that he

> begins his reasoning within the inherited world theory as a going concern (Quine 1975: 72),

that is,

> from the point of view of our own science, which is the only point of view I can offer (1981: 181)

and as involving

> the recognition that it is within science itself … that reality is to be identified and

described. (1981: 21).

These passages are all cited by Penelope Maddy (2001: 42-43) to illustrate what she classifies as the "fundamental naturalistic impulse". In a further example Quine adds what sounds like a surprising touch of 19th century Idealism:

> Naturalism looks only to natural science, however fallible, for an account of what there is and what there is does. Science ventures its tentative answers in man-made concepts, perforce, couched in man-made language, but we can ask no better. . . . To ask what reality is *really* like, however, apart from human categories, is self-stultifying." (Quine1992: 9).

What is startling in all this? The 1944 School of Naturalism looked to science for a paradigm of *approach* and *method.* What Quine looks to science for must presumably include method, but highlights here instead a received *content* of belief, as the voluntarily accepted starting point for all inquiry.

This starting point is indeed embraced (if not evidently in these passages) with expressions of attitudes contrary to dogmatism, of the right to stake claim to one's own orientation while avowing insight into one's own fallibility. That is right, but since the emphasis characterizing the earlier Naturalism was not beliefs but approach, not theories but method, not content but commitment, we can only be startled to find that after all, what is taken as basis is a content for belief.

We can continue this series of expressions of the attitude which marks the Naturalist philosopher's starting point, noting that the realism invoked is realism in the traditional sense of a thesis about what there is:

> with respect to our *prima facie* commitment to [arguments supporting scientific realism] ….I know of no reason to think that any of this needs to be validated …. (Leeds 2007: 3-4)
>
> The defense of realism depends on distinguishing it from other doctrines and on choosing the right place to start the argument. And the defense of that choice depends on naturalism (Devitt 1999: 90)
>
> [the view that there] is only one way of knowing, the empirical way that is the basis of science (whatever that way may be) (Devitt 2002: 31)

These philosophers are implicitly, but I think quite obviously, claiming the right to carry on philosophy in just this way, without needing to offer any special justification. They claim the right to start in the place where they are, epistemically speaking. I have no quarrel with this claim as such: *they do have the right. Let a thousand stances bloom!*

What does this 'impulse' imply concerning what Naturalism is?

But now see how this thought is developed there. Suppose we begin our philosophical tasks always with common knowledge taken for granted, and take the accepted scientific theories of our days as common knowledge.

Nothing is known that is not understood; I imagine that too is a truism. That means then that science tells us the true, and *well understood* story about whatever it has brought into its own domain. It means on the one hand that anything less than belief in the contents of the current sciences is just skeptical doubts. On the other hand, it has this curious effect: it implies that questions of interpretation have no place at all with respect to science. They can have a place only with respect to whatever is seen to lie outside that home-turf. Indeed, we seem to have arrived at precisely the picture of the Naturalist Native that we saw earlier, as painted by Penelope Maddy.

Resistance to Interpretation

When we see the Naturalistic philosopher characterizing and endorsing a particular intellectual posture, this involves not just resting in one's inherited beliefs, the content of the currently accepted scientific theories, but resting within confidence in one's understanding of those theories, and of what it is that constitutes scientific practice and activity which produces those theories.

"Understand" is also factive, in a sense: a claim to understand is a claim to 'have it right' against implicitly or explicitly posed alternative interpretations. Or so we see it from the outside. For the native speaker, who lives in the language, the words are transparent. For the Naturalist Native, who lives in the language of science, the scientific terms and theoretical statements are transparent, no interpretation is needed or involved in their understanding. Once again, that is not how it looks to the philosopher outside this schema. From the outside it seems that an interpretation ***is*** involved: an implicit view of *what science* -- this so valuable, important, wide-spread cultural phenomenon -- *is*.

The interpretation involved on the part of the Naturalist is precisely the identification of that Native as the paradigm participant in the scientific enterprise. This brings with it a concordant view of the scientific enterprise, of its aim and structure. That Naturalist interpretation contrasts with competing answers to the parallel questions:

- what is an empirical scientist -- more broadly, participant in the scientific enterprise -- according to the Transcendentalist of today?

- what is an empirical scientist -- more broadly, participant in the scientific enterprise -- according to the Pragmatist of today?
- what is an empirical scientist -- more broadly, participant in the scientific enterprise -- according to the Empiricist of today?

The common core of these questions lies at the heart of the scientific realism debates of the 1980s, and of their current Nachlass.

Leeds: The scientific realism debates

Already in the passages I quoted above, we do not have to read deeply between the lines to see Leeds' philosophical conception of science. It suffices to recall the above "But if we are interested in the chance that *this* methodology ... will select true theories", or to note the confident assertion that

> a creature who can find his way around the macroscopic world must inevitably be equipped with the 'right' standards of simplicity and explanation. . (Leeds 1994: 204)

A very traditional view in philosophy of science: that it is such aims as simplicity and explanation that drive the engine of science. in order to produce true theories. For Leeds this pursuit of simplicity and explanation is a method for arriving at *belief*.

The one place where this view finds immediate application is the scientific realism debate, of which one strand is very saliently related to the failure of the idea of Induction. I mean of course such replacements for Induction as forms of e.g. Inference to the Best Explanation (IBE), which realists typically claim to be what motorizes scientific inquiry. If that is so, they can claim that therefore the philosopher has a right, or even more than a right, to use that as vehicle for metaphysical inquiry as well. The claim of a right to such forms of inference with broad application, *without any need to justify them*, is clearly expressed by Stephen Leeds. Here is the complete relevant passage:

> with respect to our *prima facie* commitment to IBE - our willingness to use it, not only in cases involving observables, but also in cases in which we don't yet know whether the inferred entities or properties, if they exist, are observable or not, and finally in cases where they are known to be unobservable – I know of no reason to think that any of this needs to be validated …. (Leeds 2007: 3-4)

This is a laudable and courageous step, and must be seen as such even by someone not taking it. It is a step that goes beyond full and unqualified belief in the truth of currently accepted scientific theories, for it involves adherence to patterns of inference also outside of the empirical sciences.

But the most crucial point here is that it is *a step*, a choice, a choice of epistemic attitude

not forced on us.[6]

The one thing that would not sit well with this dismissal of any challenge to justify an epistemic attitude would be *denial* in the sense of psychopathology: sustained oblivion of the choice involved. The good scientific Naturalist, I would say, must be one who is fully aware of the specter of a third-person view on his situation, in which that *choice* appears in a horizon of philosophical alternatives, and hence, is a choice for which responsibility is inescapable.

The Naturalistic Native, whom I will not identify with any naturalistic philosopher, does not meet this criterion, since interpretation of its home turf is just not on. Others can take such a view of its home turf ; these others are not Native there, if Maddy's picture of the two cultures is right.

Maddy: the Naturalistic Native, cont'd

Are Naturalistic philosophers, as opposed to the Naturalist Native, good scientific Naturalists, by that criterion?

As I see it, the fundamental Naturalistic impulse is not so much to take science for granted as to take for granted *a particular but unacknowledged philosophical view of what science is*. Precisely if that is the case, the stance is not taken consciously and explicitly, but seen as unavoidable or inherent in what it is to be scientific at all. When Leeds says, for example, that there is no reason to think that reliance on IBE needs to be validated, it seems quite clearly to be based on a conviction that IBE is at the heart of scientific practice. But this conviction is part of a view, an *interpretation*, of science offered by philosophers in answer to the question *of what science is*. The neglect or refusal to see it that way is part of a strong resistance to interpretation that is actually more characteristic of Naturalism than any of its more public stances.

If this requisite sense of choice and responsibility were added as a leavening to current Naturalism , what would that do? Let's look at this again in the light of the views currently so ably and inventively defended by Penelope Maddy.

The task of actually laying out the presupposed interpretation of science is not so much lacking as deliberately eschewed in Maddy's presentation of Naturalism. Everywhere she claims the right, as philosopher, to retreat to the position of what she has presented as the Native, when a challenge comes up. When it comes to an answer to the question "What is science?" she simply quotes one of a much quoted scientist's philosophical remarks:

> 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)

Any empiricist will say *Amen!* to this. But what does Feynman write, if read through

Maddy's eyes? In her eyes this means, for example, that the observed outcomes of Perrin's experiments establish that atoms are real, and that not to believe this is to believe that science did not meet its own criteria of success in this case. But she does not make her interpretation explicit. That would require discussion of what the internal criteria of success in the empirical sciences are, or how they are related to the principle that observation is the judge, or to the status of theories involved in the assimilation of those experimental results. *That she owes us no such discussion is part of her philosophical stance.* Nevertheless, not giving it does not preclude her from advancing at every point not simply what generally, currently accepted science says, but her interpretation of what it does.

The Naturalistic Native is, in my eyes, not simply someone who is scientific, but someone who has not faced interpretative questions about science. Dismissing such questions when they arise, the Native is credulous in a way that the practicing scientist most assuredly is not. What of the Naturalistic philosopher then? How is it possible for her, who is not just any sort of Native, to eschew exploration of interpretative alternatives, or even to recognize the need or relevance thereof?

Fundamentalism explained

The analogy that springs to the eye is to scriptural fundamentalism. It is not weird beliefs that characterize fundamentalists as such. There are fundamentalists in different religions Christian, Muslim, Jewish, Hindu, and in ideologies, Leninist, Maoist. They are not singled out as such by the beliefs, of which they don't have many in common, nor by weirdness, which is after all in the eye of the beholder.

Rather, the common feature is this: there is for them no question to be considered about what the 'Scriptures' mean, or what is the status of their content. The 'Scriptures' mean what they say, all of 'us' (in that cultural context) know ('know'?) what that is. Indeed, to the fundamentalist it is not just that the question of interpretation does not arise, the very idea is anathema, a heresy, a snare and a delusion.

We can generalize on this, to diagnose fundamentalism wherever it occurs. On the fundamentalist's home ground there is a received story, let's call it the *Text*, and a received context of discussion, whose parameters are not to be altered. Anathema are precisely two activities of interpretation:

- of our relation to the *Text,*
- of the *Text*'s relation to the world.

Can we see this resistance to interpretation, the insistent assumption of total transparency, at work in the philosophy of science? To detect it, we must find it in the silences rather than in the explicit pronouncements of the participants.

For example, as we saw, Maddy cites Feynman's assertion that observation is the judge

of whether something is so or not. But she sees no need to investigate the relations between *observation* and the final answer as to what is so or not. In just the same way the fundamentalist will insist that the judge of whether something is so or not is *sola scriptura*, without seeing the need to investigate the relation of the scriptural text (taken as a historical entity) to the conclusions drawn from it. *It means what it says*, and we, the cultural in-crowd, the only relevant community, all know what that is.

Similarly, the scriptural fundamentalist will understand the words in the Scriptures, regardless of where and when they were written, precisely as words uttered today in current discourse. When the book of Joshua says that the sun stood still, it means that the sun was moving and stopped moving. When the chemist Dumas writes, in 1836, "never in chemistry must we go beyond the realm of experiment", he means just what a logical positivist or operationalist in the 20th century would mean by that, hence expresses a positivist philosophical prejudice.

The Native's uncomprehending stare

The Naturalist Native displays a disturbingly uncomprehending stare at the history of philosophy. Thus Maddy writes:

a) about Kant

> the Native "simply comes away unpersuaded." (Maddy 2007: 4); "is deaf and blind to the Kantian transcendental project in the first place" (ibid., 101)

b) about Carnap

> "this sort of debate flies over [its] head …. What they are squabbling about escapes [it] in the first place …." (Maddy 2007: 80-81)

c) about Reichenbach: the Native remains

> "unimpressed by philosophical systems that place a second level of analysis above that of science" (Maddy 2001: 39)

d) about van Fraassen:

> "What we have here is yet another two-level view ..." (Maddy 2007: 308)

e) about Quine, who seems to have wavered in the Native's eyes: the Native

> "simply proceeds according to [its] own methods, unimpressed by proposed alternatives" (Maddy 2001: 55)

f) about Putnam:

> "none of these projects holds any appeal for [it], leaving [it] with no motivation to undertake these … inquiries." (Maddy 2007: 308)

g) about Richard Boyd's scientific realism: the Native

"feels no need for this extra stamp of the foot". (Maddy 2007: 310)

How does this sit with what is done in foundational studies of physics? I am thinking of the various interpretations of quantum mechanics as answers to the question "how could the world possibly be as this theory says it is?" Arthur Fine, expressing his position (NOA: the Natural Ontological Attitude) decries "the totally bizarre question of how the world could possibly be as the theory says".[7] Undoubtedly the Naturalist Native's reaction as well! But these interpretations of quantum mechanics were center stage in philosophy of physics during much of the second half of the twentieth century. Theories are formulated, their formulation is investigated in the context of the alternatives that are open: for example, quantum mechanics is understood better now that we have seen Bohmian mechanics and the GRW theory. We could see all three, and compare them, discuss agreements and possible disparities in the empirical predictions, try to imagine at least thought experiments in which their differences would become manifest … We could much more clearly, because of the displayed contrasts, address he question what the world could possibly be like if it were as quantum mechanics says it is. What could I call this except a 'level of analysis above that of science'?

The sliding back and forth between Naturalistic philosopher and Naturalistic native, which served Maddy well in some of her defenses, is a slide she needs to perform in both directions, as the need arises, and so becomes a vacillation that renders her version of Naturalism unstable.

Interpretation

The important difference I see between such a position as Leeds' or Maddy's Naturalism on the one hand and Empiricism on the other, is with respect to interpretation. Both accept that the epistemic agent can only start from where s/he is, epistemically: with the presumed knowledge and accumulated opinion and belief at the point in question. Neither would take the agent to have a completely uncritical attitude toward this starting point taken as a whole: Neurath's mariners are after all repairing their boat at sea.

What we do not see in the more simplistic picture, however, is precisely how or in what way we can reflect on our own starting point and look at it with a critical eye, without for a moment disavowing it as (as yet, and still) our own. When it comes to what is earmarked as currently accepted science, the Naturalist Native has the attitude that it is to be retained in full belief unless and until that earmark is removed by the relevant scientific community to which s/he defers, and whose pronouncements she takes as univocal.

One part of this is unexceptionable: we do not remove the earmark of accepted science on our own. It takes the authority of the scientific community to rule on what is and is not currently accepted science, notwithstanding any vagueness in this ruling or in the constitution of that community. But does the 'full belief' part prevent even comparisons

with alternatives, or assessment of science's own history, except as obviously mistaken, unless …. *unless what*? All we can have is Whig history[8], and Whig assessment of anything comparable to what we have, *unless* it is possible to have as part of one's current epistemic stance a 'bracketing' of what is currently accepted, even by oneself.

Such 'bracketing' is a prerequisite for any interpretative approach to one's subject matter. The sailors on Neurath's ship cannot step back from the ship, in the sense of stepping back onto some other platform, such as a dry-dock, from which to assess its problems. But this point misleads us if we forget that these sailors are persons, not the bare particulars of an impoverished descriptive epistemology. They are able to evaluate their ship, and to plan different strategies and tactics for its repair and improvement, precisely because they can 'bracket' some of the beliefs they started with, in which there was little doubt on any point of safety. If they could not, we would have to expect their discussions to take some such form as this:

A. Our safety does not require that we put on lifejackets before a storm comes up.

B. But what if a storm comes up very quickly, so as to leave no time to put on a life jacket after the storm strikes?

A. That will not happen, given that our safety does not require that we put on lifejackets before a storm comes up.

A's reply is a simple logical deduction from his current beliefs. His response is mechanical: facing a question, he offers the answer implied by what he believed prior to the questioning. But this inability to 'step back' and evaluate his prior in the light of that question, which is at the same time a challenge, reveals him as not intellectually endowed to sail on Neurath's ship.

So I offer the Naturalist the dilemma: (a) to be in a position to share a point of view from which the sciences can be discussed, bracketing our beliefs about to what extent they are true, or (b) to be in no position even to survive as a Naturalistic Native in times of trouble. With Naturalism we can agree that we are on Neurath's ship, can only ever go on from where we are, but we can add, and invite agreement, that an interpretative stance is open to us and indeed, is *needed* to understand our own situation properly.

Enter scientific realism

But now, consider what sorts of questions should -- and if not resisted, will -- arise in that 'bracketed' context. They will most certainly include questions as to how to understand this scientific activity, of the criteria of success apparently applied in intra-community assessment of the work in that area, and therefore of its aim. Two rival proposals will quickly emerge, for they have been on the table in scientists' and philosophers' discussions for many centuries now. I mean of course the contrary views of the aim: is it

truth or empirical adequacy? The aim of scientific activity is here meant to be identified in terms of what it has as its criterion of success in practice, and the question is whether that aim is to achieve theories that are *true throughout*, or theories (whether or not entirely true) that meet the *empirical* criteria of success. Either view is logically compatible with full and unqualified belief in the science in question. The difference is only that on the first view, such full and unqualified belief just amounts to the opinion that this science has achieved its proper aim, while on the empiricist view such belief is to some extent supererogatory and involves belief that the theory has virtues beyond those required by the aim pursued.

At this point we can certainly see some differences among contemporary Naturalists. They do all seem to enter bodily into the realist/anti-realist debate, but not in the same way. Is it possible for a Naturalist to couple full and unqualified belief in a science with an attitude in which that belief is 'bracketed', and to explore diverse rival interpretations of the science, of its content, its practices, its values and norms? Does Naturalism allow for this?

Penelope Maddy does not.[9] But in this respect neither Leeds nor Devitt seem to me to follow suit.

What is Scientific Realism for the Naturalist?

Maddy insists that the scientific realism debate can be disregarded as soon as one agrees to fully accept the currently accepted science. When Maddy's Naturalist Native turns to this debate, it must of course try to find the questions there that it can understand, just as it did when listening to Kant, Carnap, Reichenbach, and so on. Science is the Native's home-ground, and questions of interpretation with respect to its home-ground do not arise at all. So Maddy writes:

> what's at issue is whether or not the conclusions drawn by ordinary science methods should be regarded as definitive. So, for example, the scientific realist might argue that the best explanation for the success of the scientific enterprise is the assumption that the entities it describes really do exist, while the instrumentalist denies that this is so.
>
> [...]for present purposes, it's enough to note that this sort of debate flies over the head of the Second Philosopher. From her point of view, one either accepts her evidence or explains in her terms why it is inadequate, something neither the realist nor the instrumentalist purports to do. What they are squabbling about escapes her in the first place …. (Maddy 2007: 80-81)

The uncomprehending stare! As a result, the Naturalist Native sees just one possible intelligible motivation for the empiricist, and one possible factual question that could

admit of evidence one way or another:

(a) Motive: we should not believe more than what the evidence establishes

(b) Question: are unobservable entities, such as atoms, real?

The two are then connected, in this putative empiricist's mind, by the apparent conviction that our evidence could establish that a theory that postulates unobservable entities is empirically adequate, but not that it is true.

This misconstrues both the empiricist position and the historical debate over the atomic theory.

Just note to begin: a claim of empirical adequacy for a theory *also* goes far beyond what our evidence could establish! For it entails that there are no phenomena anywhere in the universe or its history that contradict the theory. In that respect, truth and empirical adequacy are on a par. So how could this possibly be what is at issue between empiricist and realist in the scientific realism debates?

It seems that for Maddy's Native, or her Second Philosopher for that matter, the puzzle is how to find an intelligible (to her!) question that could possibly be at issue, and the only thus intelligible questions are scientific. Isn't this strangely reminiscent of some other famous episodes of philosophical puzzling over philosophical issues? Recall Sir Alfred Ayer's version of the uncomprehending stare directed to moral philosophy in the darkest days of Logical Positivism:

> in so far as statements of value are significant, they are ordinary 'scientific' statements, and in so far as they are not scientific, they are not in the literal sense significant. (Ayer 1946: 102-3).

So when Maddy looks into the scientific realism debates, the only question she can find that makes sense to the naturalist Native is the factual question whether unobservable entities such as atoms or molecules exist. That is a question she takes to have been addressed, and answered, by scientists themselves. So what's the issue? Skepticism about whether scientists know their job! And yet, curiously, no party in the debate ever expressed such skeptical doubt ....

Looking into atomic theory

The development of the modern atomic theory in the 19th century was a scientifically much contested affair, but the experiments of Jean Perrin in 1905 ended the contest. The Perrin episode (as recounted in Perrin 1910), and its handling in philosophical debate, I have discussed at length elsewhere (van Fraassen 2009). Until recently, the story of Perrin appeared recurrently in the form of a bit of philosophical lore, as the story of the discovery that atoms and molecules are real. Characteristic of the way this lore was called

into service for Naturalism is the presumption that *precisely what the scientific advance was* is crystal clear, transparent, and not in any way subject to interpretation.

Once this way of telling the story is accepted, the only question a philosopher may still address is how we can understand Perrin's work as legitimating the conclusion drawn from it, that is, the reality of atoms and molecules. This question was addressed by Wesley Salmon, Clark Glymour, and Peter Achinstein, with its presuppositions intact. Maddy does not depart from this standard line when she repeatedly returns to the Perrin episode. But the presupposed bit of conventional wisdom is an interpretation of this historical scientific episode. The interpretation is readily supported by quotes from Perrin and others. Such 'text-proofing' (to use the Bible scholars' term) is a weak reed.

Now it seems to me that, *malgré eux*, the discussions by Salmon, Glymour, and Achinstein brought to light precisely the implications and puzzles that should lead us to an entirely different view of Perrin's achievement. If that is so, if there is a feasible interpretation at odds with the standard lore, then that shows that the standard lore was itself an interpretation as well. What passed for a literal reading of a transparent text was actually an un-avowed interpretation.

Wesley Salmon (1978: 698-699; 1984: 221; 1989: 124-126) begins by noting that Perrin's measurements, in conjunction with his hypothesis that the Brownian particles mimic the dance of the molecules, establish a value for Avogadro's number. In the context of relevant background theory, as Perrin also noted, this is one of a number of different forms of measurement to establish Avogadro's number, and these measurements have concordant results, yielding the same value for that number. This Salmon saw as the clue to a compelling scientific realist argument.

From his teacher Hans Reichenbach Salmon takes the concept of 'common cause' explanation, and its use in an inference 'back' from effects to causes: if very similar effects have been produced by very different procedures, mutually independent, then it can be concluded that these effects all result from a common cause. In his famous "Why Ask 'Why'?" he first introduces the concept as follows:

> If two or more events of certain types occur at different places, but occur at the same time more frequently than is to be expected if they occurred independently, then this apparent coincidence is to be explained in terms of a common causal antecedent. If, for example, all of the electric lights in a particular area go out simultaneously, we do not believe that they just happened by chance to bum out at the same time. We attribute the coincidence to a common cause such as a blown fuse .... (Salmon 1978: 691)

Experiments on various phenomena—including Brownian motion, alpha particle decay, X-ray diffraction, blackbody radiation, and electrochemical phenomena—all yield approximately the same value for Avogadro's number. That, Salmon claims, invites

precisely the pattern of common cause reasoning. Describing the above noted concordance in measurement results, Salmon presents his common cause argument for scientific realism:

> The fundamental fact to which I wish to call attention is that the value of Avogadro's number ascertained from the analysis of Brownian motion agrees, within the limits of experimental error, with the value obtained by electrolytic measurement. Without a common causal antecedent, such agreement would constitute a remarkable coincidence. (*ibid*. 698)

In fact, Salmon has here stretched the concept of common cause to the breaking point. For that concept, which is technically explained in terms of probabilities of occurrence, pertains to relations of correlated simultaneous physical events to the same preceding event in their common history. Those measurement procedures are indeed physical occurrences, but their agreement is not a case of statistical correlation, nor is it due to a common causal antecedent – on the contrary, care is taken that those procedures are physically and causally independent of each other.

But, *lesson number one*, Salmon has put his finger on a crucial node in scientific practice: the concordance of the outcomes of physical procedures that are classified, relative to theory, as measurements of the same quantity. That is an empirical criterion applied in the evaluation of theory: *concordance.*

Clark Glymour diagnosed the significance of Perrin's experiments differently, by presenting Perrin's reasoning as following his bootstrapping method:

> For example, some of Jean Perrin's tests of equations of the kinetic theory are exactly of the kind illustrated. Perrin had, for instance, to use one of the equations to be tested to determine a value for a constant (Avogadro's number) it contained. (Glymour 1975: 409, fn. 12).

As shown in further detail in Glymour's *Theory and Evidence* (1980: 226-263), the development of the atomic theory in the 19th century bears out the bootstrapping view of evidential support. According to this view of how experimental results intertwine with hypotheses in a theory to confirm those hypotheses, characterizes measurement as yielding its results relative to a theory, but empirically significant. The bootstrap method involves a crucial demand for concordance of the sort noted above, while placing theoretical dependence center-stage, and adding insurance against trivial or vacuous ways of satisfying that criterion. This theory-dependence of what counts as measurement is the second lesson.

Nevertheless, as Peter Achinstein pointed out, that Perrin's measurements can be understood in Glymour's way does not suffice to show how Perrin, or the scientific community, legitimately concluded that atoms are real.(Achinstein 2001: 252-3.) That is so, but Glymour's point has a different, if implicit, significance. It is characteristic of

the bootstrap method, as explication of how scientific theories earn their credentials, that greater support can accrue to logically stronger theories (cf. van Fraassen 1983). That is a puzzling point at first blush: if theory T2 is logically stronger than T1, that is, if T2 implies T1, then T2 *cannot* be more probable than T1, on any body of evidence. How can we solve this puzzle? Glymour's framework makes it very clear: the logically weaker theory may not offer the conceptual resources to design telling experiments that *could* offer support. And so we see in the development of the 19th century molecular-kinetic theory the amazing increase in confidence in that theory as more and more hypotheses are added to it, to increase its logical strength! For only thus do we arrive at the design of procedures that will count as measurements of the theoretical quantities involved -- will thus count relative to theory, that is! And Perrin took the last step in this sequence of additions when he postulated that the observable Brownian motion mimics the unobservable molecular motion.

Finally Achinstein himself displays a different sort of causal reasoning to provide his own diagnosis (2001: 254-258). His criticism of both Salmon's and Glymour's accounts is that they do not succeed in legitimizing the inference of the reality of molecules from Perrin's results. His own 'legitimation' thereof posits certain premises involved in Perrin's reasoning, re-construed as explicitly probabilist. Surprisingly, the premises he lists include a prior probability of at least ½ for the atomic hypothesis, and the conclusion is only that it is "greater". If this was what established the reality of molecules, that increment in probability was the proverbial straw to break skepticism's back! That does not seem plausible, so I would suggest the opposite conclusion: Achinstein is right that Perrin's achievement was ***not*** to raise the probability of the reality of molecules to the threshold-of-belief point, it was a different sort of achievement!

Putting these three conclusions together we arrive at this: the atomic theory introduced by Dalton early in the 19th century, and successively strengthened with additional hypotheses by Avogadro, Boltzmann, Maxwell, ..., and eventually Perrin, became strong enough to make *empirical grounding* for all its theoretical quantities possible. Empirical grounding is a requirement upon physical theory, within scientific practice, and it consists in this: that there should be procedures that would count as measurement for the theory, for the theoretical quantities, procedures which could in principle have outcomes contrary to the theory, but which do in fact have concordant outcomes to fix the values of those quantities. That is a mouthful; I have explored this demand elsewhere and will not elaborate further here (van Fraassen 2012 and forthcoming). This demand is closely related to Glymour's bootstrapping concept, and as Achinstein pointed out for that, compliance does not suffice to show that the theory is true. But it is the empirical criterion that must be met to satisfy the norms in force in scientific practice. There is no grist for the scientific realist's mill here; on the other hand, to grant this should offend no realist scruples either.

Conclusion

There are varieties of Naturalism, and not all Naturalists are to be shorn over the same comb. If we take Quine's "Epistemology Naturalized" as the seminal paper for post-mid-20th century Naturalism, however, Penelope Maddy's dismissal of the 'second level of analysis above that of science' should be taken as representative. Equally, it is as true heir of Quine that her 'second philosopher' is 'a Native of the contemporary scientific world view' who lives there as unselfconsciously as a fish in water or a bird in air.

This is in strong contrast with what I take to be at the heart of the Empiricist tradition, which involves as a main philosophical task to achieve an understanding of the sciences as providing us with empirical knowledge. That is a task of *interpretation*, in which the empirical, conventional, and postulational elements in a theory are distinguished, and the practice of science is understood as an empirically driven norm-governed activity. Quite the opposite of any attitude that deems interpretation, or analysis from a detached epistemological point of view, anathema.

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van Fraassen's Approach.

NOTES

[1] Thanks to Angela Mendelovici for clarifying this in an early seminar discussion of this material.

[2] Quine mentions Neurath's boat already his 1950. The image became a mainstay of later writings and appears as an epigram at the start of *Word and Object*.

[3] Just to elaborate on the first part: the sequence of predictions will be well calibrated if the announced probabilities match the actual frequencies. That is not the success that the straight rule can automatically claim. We must not confuse that with its obvious virtue: that since the straight rule's announced probability for the next item I always the actual frequency so far, that will match what has been found up to that point -- and these probabilities will converge to the relative frequency in the entire series even if it is infinite.

[4] Putnam was offering a critique of Carnap rather than of Reichenbach. For details on this critique and its continuation and completion by Putnam, Gaifman, Oakes, Dawid, and others see my 2000. Note that the argument applies to any computable forecast systems, as defined by Dawid et al., and applies mutatis mutandis to any significant subclass we might want to consider (such as those basing their calculations on 'acceptable' scientific theories plus data, etc.).

[5] I classify Stephen Leeds as a Naturalist if only because of his able defense of a 'Naturalistic Instrumentalism" in philosophy of language (Leeds 1978). It has generally been taken as obvious that scientific realism requires, for its intelligibility, adherence to a correspondence theory of truth, or at least to the idea that some notion of correspondence, subject of such a theory, is crucial to the discussion of what is and is not true in science. But two prominent realists, David Lewis (2001) and Stephen Leeds (1994, 1995, 2007), argued strongly against this impression, insisting that the realist is barking up the wrong tree when thinking that the position requires anything going much beyond the Tarskian equivalences also beloved of the deflationist. I agree, and add that the same goes for the constructive empiricist. It is fascinating to reflect on (a) how comfortably Leeds can develop his views on scientific realism, given that as a convinced naturalist he takes his departure from its characteristic epistemic position, and (b) how in turn this Naturalism drives him inevitably to recast scientific realism as a *stance*, or I should say, as a *stance*-part of the naturalist stance.

[6] Leeds at least is very aware of its being a choice of attitude: for example, he dismisses problems that have been pointed out with respect to IBE with the remark: "I pass over the Dutch book argument against having any non-Bayesian inductive policy. This would worry me if I were forced to bet with the devil; otherwise I cannot see that it proves anything at all." (Leeds 2007: 5; note 5)

[7] Fine wrote this in correspondence, and gave me permission to quote it; it is very close to what he wrote in his 2001: 118: "I favor a more open attitude toward science, one not committed to reconfiguring scientific practice to suit the needs of pre-set philosophical agendas – the attitude I

call NOA. One irony of van Fraassen's global approach is that it merges with more or less traditional metaphysical projects when it asks, as van Fraassen does, after the "content" of a theory or "how the world could possibly be the way this theory says it is".

[8] The term comes from Herbert Butterfield's influential book *The Whig Interpretation of History* (1931). Whig historiography evaluates the past as a progression toward the present, with errors, failures, and successes so classified by the historian's (community's) present values.

[9] Note that Maddy 2007: 308-311 contains a sustained argument to the effect that the sort of argument I gave here does not succeed in bringing the Naturalist to the table. I will leave the verdict to the reader.