

## UNDERDETERMINATION AND REALISM

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### 1. Introduction

The underdetermination of theories by evidence often leads to skepticism about the theories and hence to antirealism about the worlds described by the theories. At its most extreme, this skepticism is about "Commonsense Realism," about our knowledge of the observable world of stones, trees, cats, and the like. This skepticism must spread to "Scientific Realism," to our knowledge of the unobservable world of atoms, viruses, photons, and the like. But underdetermination sometimes leads to a less extreme view aimed only at Scientific Realism: against a background acceptance of realism about the observable world, realism about unobservables is rejected.

The underdetermination theses that concern us claim that a theory (belief) has rivals that stand in some sort of equivalence relation to it with respect to certain evidence. The theses vary with the equivalence relation and with the evidence. In particular, sometimes the relation is deductive and sometimes ampliative; and sometimes the evidence is the actual given evidence and sometimes it is some sort of possible evidence.

There are two dimensions to the realisms that are challenged by underdetermination. "The existence dimension" of Commonsense Realism is a commitment to the existence of most observables such as stones, trees, and cats and to these entities having, for the most part, the properties attributed to them by science and commonsense. The existence dimension of Scientific Realism is a similar commitment about such unobservables as atoms, viruses, and photons. Typically, idealists, the traditional opponent of realists, have not denied this dimension; or, at least, have not straightforwardly denied it. What they have typically denied in response to the skeptical challenge is "the independence dimension." According to some idealists, the entities identified by the first dimension are made up of mental items, "ideas" or "sense data," and so are not external to the mind. In recent times, under the influence of Kant, another sort of idealist has been much more common. According to these idealists, the entities are not, in a certain respect, "objective": they depend for their existence

and nature on the cognitive activities and capacities of our minds. Realists reject all such mind dependencies. Relations between minds and those entities are limited to familiar causal interactions long noted by the folk: we throw stones, plant trees, kick cats, and so on. We could say a lot more to make these doctrines precise and I have done so elsewhere.<sup>1</sup> But these definitions will suffice for our purposes.

In part I, I will consider whether underdetermination has any consequence for Commonsense Realism. If it does, this must spread to Scientific Realism. In part II, I will consider whether underdetermination has any consequence for Scientific Realism alone.

### I. Underdetermination and Commonsense Realism

#### 2. Extreme Skepticism

In the *First Meditation* Descartes famously doubted the evidence of his senses. We can see this as an argument about underdetermination. Descartes believes that he is sitting by the fire. But perhaps he is suffering from an illusion, perhaps he is dreaming, perhaps he is being stimulated by an evil demon. There seem to be a range of alternative hypotheses to Descartes' belief, each equally compatible with the evidence available to him: his belief is underdetermined by the sensory evidence.

This is the first step in the argument of the extreme skeptic. We do not need to imagine evil demons and the like to find support for this underdetermination: it is supported by our psychological and neurophysiological theories of perception. Consider this example: I look in front of me and come to believe that there is a cat there. Our scientific explanation of this is roughly: the cat reflects light waves which provide stimulus to my retina causing sensory neurons to fire leading to my belief. But that scientific account also tells us that the cat is not necessary for the belief, for a multitude of reasons. The cat is not necessary for the light waves that strike the retina: the waves might have other causes. The light waves are not necessary for the stimulus: there are other ways of providing that stimulus. Similarly the stimulus is not necessary for the neuron firings and those firings are not necessary for the belief. So the belief has a range of rivals that are compatible with the sensory evidence.

Let us call beliefs like Descartes' and mine—beliefs about the external environment caused by perception—"observational beliefs." We seem to have established the following basic underdetermination thesis:

**DG1:** Any observational belief has rivals that are equally compatible with the actual given sensory evidence for that belief.

The 'G' in this name indicates that the thesis concerns *the actual given* evidence. In part II we will be concerned with underdetermination theses that con-

cern *possible* evidence, theses named with a 'P'. I shall explain the 'D' in the name in a moment.

DG1 needs some clarification. What exactly is "the sensory evidence"? We might take it to be the perceptual experiences themselves, but then it is not clear what it is for an observational belief to be "compatible with" the evidence. If we suppose that perceptual experiences produced beliefs about ideas or sense data then we could take beliefs about about them to be the sensory evidence; for example, the belief that the sense datum I am now perceiving is cat-like. But the supposition is controversial at best. We can be more ontologically cautious, taking the sensory evidence to be beliefs, prompted by perceptual experience, about how things appear (with no commitment to mental entities); for example, the belief that it appears to me that there is a cat in front of me. Then we can take DG1 to be claiming that this belief is logically consistent not only with the belief that there is a cat in front of me but with many other beliefs. That is surely true.

We move to the second step in the extreme skeptic's argument. Given DG1, how could an observational belief be justified? What basis is there for eliminating rivals that are equally compatible with the evidence? The skeptic's position is that there is no basis. She infers the troubling epistemological thesis:

**AG1:** Any observational belief has rivals that are equally supported by the actual given sensory evidence for that belief.<sup>2</sup>

DG1 is concerned with the *deductive* relation of *logical compatibility* between the sensory evidence and observational beliefs; hence the 'D' in its name. AG1 is a much stronger underdetermination thesis because it is concerned with the *ampliative* relation of *epistemic support* between the sensory evidence and observational beliefs; hence the 'A' in its name.<sup>3</sup> If AG1 is correct, even ampliative inferences like induction and abduction (inference to the best explanation) do not provide a basis for preferring any observational belief over many alternative hypotheses. Indeed, the skeptic is dubious of all such inferences. Our observational beliefs about stones, trees, cats and the like are all unjustified. Commonsense Realism must be abandoned.

Suppose that we found some way to reject AG1 and justify our observational beliefs. We still have a way to go to escape extreme skepticism. We want to move from observational beliefs to singular beliefs about unobserved objects and to general beliefs that cover such objects; to take a boring but familiar example, we want to move from the evidence of many ravens all observed to be black to the belief that Oscar, an unobserved raven, is black, indeed to the "theory" that all ravens are black. The skeptical tradition once again presents us with an underdetermination problem:

**DG2a:** Any theory has rivals equally compatible with the actual given observational evidence for that theory.

This seems to be indubitably so. Thus our evidence of many sightings of ravens is compatible not only with the theory that all ravens are black but also with the theory that all ravens are black except the ones on island X which has never been investigated. Another related underdetermination thesis also causes trouble:

**DG2b:** Any theory has rivals that entail the same actual given observational evidence.

Thus the theories that all ravens are black and that they are all black except on X both entail our evidence of raven sightings.

These deductive underdetermination theses are the first step in the extreme skeptic's argument. She then, once again, infers an ampliative thesis. Given these deductive theses, how could a theory be justified? She arrives at another troubling epistemological thesis:

**AG2:** Any theory has rivals that are equally supported by the actual given observational evidence for that theory.

According to this ampliative underdetermination thesis, a theory enjoys no epistemic support over some of its rivals.<sup>4</sup> Our general beliefs about stones, trees, cats and the like are unjustified. Again Realism must be abandoned.

We note that the pattern of the skeptic's argument is:

Deductive underdetermination → ampliative underdetermination → antirealism.

Now, it might well be objected that my discussion does not do skepticism justice for it takes skepticism to be *committed* to AG1 and AG2. The cautious skeptic would avoid this commitment, like almost all others, simply putting the onus on the nonskeptic to undermine AG1 and AG2. I think this is a good objection but, in the end (section 8), we shall see that it makes no difference whether the skeptic is committed or not. Meanwhile, for convenience, I shall take the skeptic to be committed.

### 3. "First Philosophy" Responses

We could accept the inferences to AG1 and AG2 and give up on our knowledge of the world: we do not know what there is and what it is like. This abandons the existence dimension of Realism and is very unappealing. The traditional responses of "First Philosophy," were different.

One response started by seeking a more basic area of knowledge than (what I have called) our observational beliefs, an area that was not open to skeptical doubt and that could serve as a foundation for all or most claims to knowledge.

This foundation was found in the sensory evidence for our observational beliefs: we were thought to have indubitable knowledge of our own ideas (sense data); this knowledge was not underdetermined. Even if we go along with this highly dubious claim, we still have to get from this foundation to knowledge of the world of stones, trees, cats, and the like, thus rejecting AG1 and AG2. In attempting to solve this problem, foundationalists nearly always gave up the view that the world is external to the mind, thus abandoning the independence dimension of Realism. It was thought that only by constituting the world somehow out of ideas could we hope to save our knowledge of it. Realism leaves a "gap" between our ideas and the world that makes knowledge of the world impossible. Idealism closes the gap by bringing the world into the mind.

Another traditional response, currently much more popular, is also idealist. It seeks to reject AG1 and AG2 by taking the world to be partly constituted by the mind's imposition of concepts, theories, or languages. We can know about that world because we partly create it. Realism's independence dimension is abandoned once again.<sup>5</sup>

So the price of saving our knowledge in the face of underdetermination and skepticism was typically an idealist metaphysics of one sort or another. Even if we were prepared to pay the price of such a bizarre metaphysics, these responses would not be very convincing. Although First Philosophy aims to take skepticism seriously and hence meet the very demanding skeptical standards for rational belief, it often seems to fall short of those standards: it assumes what no self-respecting skeptic should allow (for example, indubitable knowledge of ideas). So even with an idealist metaphysics we still seem not to have the knowledge we want. I have argued for this, and against idealism, elsewhere (1991, 1997a, 1999, 2001a).

We are faced with a choice between skepticism and idealism. Surely something has gone seriously wrong. It is time to think again. I shall first make a Moorean response to the skeptical challenge, then a naturalistic one.

#### 4. A Moorean Response

The disaster has come from the epistemological theses AG1 and AG2 which make Commonsense Realism untenable: we are supposed to doubt our commonsense beliefs in an external world. But why should we accept these skeptical theses? How much confidence should we have in a view that undermines Realism? Realism is a compelling doctrine almost universally held outside intellectual circles. From an early age we come to believe that such objects as stones, cats, and trees exist. Furthermore, we believe that these objects exist even when we are not perceiving them, and that they do not depend for their existence on our opinions nor on anything mental. This Realism about ordinary objects is confirmed day by day in our experience. It is central to our whole way of viewing the world, the very core of common sense. A Moorean point is appropriate: Realism is much more firmly based than the epistemological the-

ses AG1 and AG2 that are thought to undermine it. We have started the argument in the wrong place: rather than using AG1 and AG2 as evidence against Realism, we should use Realism as evidence against AG1 and AG2. We should, as I like to say, "put metaphysics first."

Descartes puts us in an armchair and asks us to start by clearing our minds of all knowledge and doing some epistemology. The Moorean puts us in an armchair and asks us to start by assessing the evidence for Realism. In so doing we must resolutely decline to *theorize about* standards of good and bad evidence, for that epistemological path was what led to the disaster: we simply apply our ordinary evidential standards, just as we presumably did in childhood when we became Realists in the first place. Once we have done that, we turn to the epistemological theses AG1 and AG2. In assessing them we have little to go on but the skeptical argument that puts these theses at odds with Realism. We ask: Is it more likely that the theses are mistaken than that Realism is? Is it more likely that there is a flaw in the skeptical argument than that, contrary to what we have always supposed, we lack knowledge of the external world? The Moorean answers these questions with a resounding "Yes."

#### 5. A Naturalistic Response

What else do we have to go on in assessing AG1 and AG2? DG1 does not entail AG1, nor do DG2a and DG2b entail AG2. Where can we look for more evidence? Not, First Philosophy assumes, to empirical science, for science itself is doubted by the extreme Cartesian skeptic. So the evidence must be of some nonempirical sort. Thus the various idealist positions that rejected the theses were thought to be, like mathematics and logic, known a priori. The a priori approach is the very essence of First Philosophy and its response to underdetermination. Reflecting from the comfort of armchairs First Philosophers decide what knowledge *must* be like, and from this infer what the world *must* be like. If the world were the way the Realist says it is, we could not know about it. Yet, it is typically thought, we surely do know about it. So the Realist cannot be right.

The Moorean response alone casts doubt on any such arguments. Given the strength of Realism, it is simply not plausible that we could know something a priori that undermined it, whether that something is the skeptic's AG1 and AG2 or the idealist's response to them. But the Moorean response is not, of course, sufficient. We need to do better and we can. Manifestly we could not have a priori knowledge damaging to Realism if we could not have a priori knowledge at all. According to Quinean naturalism, we could not: *there is no a priori knowledge*. There is only one way of knowing, the empirical way that is the basis of science (whatever that way may be); in Quine's vivid metaphor, the web of belief is seamless.<sup>6</sup> So we could not know AG1 and AG2 a priori because we could not know anything a priori.

Drawing on earlier works (1996, 1997b, 1998), I shall now present two arguments for this naturalism: first, we no longer have a strong motivation for thinking that there is any a priori knowledge; second, the idea of such knowledge is deeply obscure.

#### 6. *Lack of Motivation for the A Priori*

It is overwhelmingly plausible that *some* knowledge is empirical, justified by experience. The radical, yet attractive, naturalistic thesis is that *all* knowledge is; there is only one way of knowing. This thesis faced an embarrassing problem that dogged empiricism: some truths—most notably those of mathematics and logic—did not seem open to empirical confirmation or disconfirmation, but rather to be known by armchair reflection.<sup>7</sup> It did not seem *possible* that such truths could be revised in the way that ‘All swans are white’ was by the sighting of black swans in Australia. Quine, following in the footsteps of Duhem, argued that we must break free of the naive picture of justification suggested by the swan example and view justification in a much more holistic way: beliefs, even whole theories, do not face the tribunal of experience alone, but in company of auxiliary theories, background assumptions, and the like. Much evidence for this “Duhem-Quine thesis” has been produced by the movement in philosophy of science inspired by Thomas Kuhn and Paul Feyerabend. In light of this, we have no reason to believe that whereas scientific laws, which are uncontroversially empirical, are confirmed in the holistic empirical way, the laws of logic and mathematics are not; no reason to believe that there is a principled basis for drawing a line between what can be known this way and what cannot; no reason to believe that there is a seam in the web of belief.

Quine is fond of an image taken from Otto Neurath. He likens our web of belief to a boat that we continually rebuild whilst staying afloat on it. We can rebuild any part of the boat but in so doing we must take a stand on the rest of the boat for the time being. So we cannot rebuild it all at once. Similarly, we can revise any part of our knowledge but in so doing we must accept the rest for the time being. So we cannot revise it all at once. And just as we should start rebuilding the boat by standing on the firmest parts, so also should we start rebuilding our web. So we normally take the propositions of logic and mathematics for granted in rebuilding our web. Still each of these propositions is in principle revisable in the face of experience: taking a stand on other such propositions, and much else besides, we might contemplate dropping the proposition.

In suggesting that mathematics is in this holistic way empirical, I do not mean to suggest that the epistemological problem of mathematics is even close to solution. How *could* it be since the *ontological* problem of mathematics—what mathematics is *about*—remains so intractable? The point is rather that we no longer have any reason to think that, if we solved the ontological problem, the epistemological problem would not be open to an empirical solution. The

motivation to seek an a priori way of knowing is removed. This is so even though we do not have the rich details of the empirical way of knowing that we would like to have. For, what we do have is an intuitively clear and appealing general idea. It starts from the metaphysical assumption that it is the worldly fact that  $p$  that makes the belief that  $p$  true. The empiricist idea then is that experiences of the sort produced by that fact are essentially involved in the justification of the belief.

In a critique of my naturalism, Georges Rey has a lot of rhetorical fun mocking the remarks that Quine and I make about the empirical way of knowing and about the application of this way to logic and mathematics. He thinks that I am under “the illusion” that these remarks amount to “a serious theory” (1997: 146). I am not. I agree with Rey that “no one yet has an adequate theory of our knowledge of much of anything” (1998: 29). In any case, Rey’s mockery is largely beside the point. Since we do not have a serious theory that covers even the easiest examples of empirical knowledge, the fact that we do not have one that covers the really difficult examples from logic and mathematics hardly reflects on the claim that these are empirical knowledge too. We all agree that there is an empirical way of knowing. Beyond that, the present argument against the a priori needs only the claim that the empirical way is holistic. *We have no reason to believe that a serious theory would show that, whereas empirical scientific laws are confirmed in the holistic empirical way, the laws of logic and mathematics are not.*

#### 7. *The Obscurity of the A Priori*

The argument that a priori knowledge is unmotivated casts doubt on it but does not alone establish that there is none. We need also the second argument about the obscurity of a priori knowledge. This argument attempts to show that the a priori alternative to an empirical explanation of our knowledge of logic and mathematics, indeed of anything, is very unpromising. If this is right, we have a nice abduction: the *best* explanation of that knowledge is that it is empirical.

We are presented with a range of examples of what is claimed to be a priori knowledge. But what are we to make of this claim? What is the nature of a priori knowledge? We have the characterization: it is knowledge “not derived from experience” and so *not* justified in the above empirical way. Doubtless we can expand this negative characterization in a satisfactory way.<sup>8</sup> But what we need if we are to take the a priori way seriously is a *positive* characterization, not just a negative one. We need to describe a process for justifying a belief that does not give experience the role indicated above and that we have some reason for thinking is actual. We need some idea of what a priori knowledge is not just what it *isn't*.

Why? This question may seem particularly pressing since I have just agreed that we do not have a serious theory of the empirical way. However, there are

two crucial differences in the epistemic status of the two ways. First, the existence of the empirical way is not in question: everyone believes in it.<sup>9</sup> In contrast, the existence of the a priori way is very much in question. Second, even though we do not have a serious theory of the empirical way, we do have an intuitively clear and appealing general idea of this way, of “learning from experience” (briefly described above). In contrast, we do not have the beginnings of an idea of what the a priori way might be; we lack not just a serious theory but *any idea at all*.

The difficulty in giving a positive characterization of a priori knowledge is well-demonstrated by the failure of traditional attempts based on analyticity. Let the example of allegedly a priori knowledge be our belief that all bachelors are unmarried. Before considering the process of justifying this belief, we need some metaphysical information: What fact makes the belief true? According to the tradition, the fact was one about the relation between the “concepts” making up the belief: the concept (bachelor) “includes” the concept (unmarried), thus making the belief analytic. This seemed promising for an account of a priori knowledge because it was thought that simply in virtue of *having* a concept, a person was in possession of a “tacit theory” *about* the concept; in virtue of having (bachelor), a person tacitly knows that (bachelor) includes (unmarried). So a person’s conceptual competence gave her privileged “Cartesian” access to facts about the relations between concepts. The required nonempirical process of justification was thought to be one that exploited this access, a reflective process of inspecting the concepts to yield knowledge of the relations between them which in turn yielded such knowledge as that all bachelors are unmarried. This alleged process is that of “conceptual analysis.”

I think that this story is better told speaking of meanings (or contents) rather than concepts. According to the “Representational Theory of the Mind” (“RTM”) the belief that all bachelors are unmarried involves a mental representation—a token in the head—that means **ALL BACHELORS ARE UNMARRIED**.<sup>10</sup> One part of this representation, ‘bachelor’, means **BACHELOR** and another part, ‘unmarried’, means **UNMARRIED**. The story is that simply in virtue of being competent with the various representations—for example, being competent with ‘bachelor’—a person has access by conceptual analysis to the fact:

1. ‘Bachelor’ means **ADULT UNMARRIED MALE** and so means the same as ‘adult unmarried male’.

Hence she can reason along the following lines:

2. ‘All bachelors are unmarried’ means the same as ‘All adult unmarried males are unmarried’.
3. ‘All adult unmarried males are unmarried’ is true.
4. So, ‘All bachelors are unmarried’ is true.

5. So, all bachelors are unmarried.

In some such way conceptual analysis was thought to yield a priori knowledge of the world.

But, even if we grant the Cartesian access to meanings that yields 1, this account fails as an account of our allegedly a priori knowledge of 5. For our knowledge of 5 depends also on our knowledge of 3. Where did the justification of 3 come from? It does no good to say, rightly, that ‘All adult unmarried males are unmarried’ is a logical truth (of the form ‘All *FGH* are *G*’). For, what justifies logical truths? Logical truths were, of course, one of the main things that we were supposed to know a priori. Yet no satisfactory nonempirical account was ever given of how we could justify logical truths (nor, as a matter of interest, of how we could justify logical inferences like those involved in the above argument). Without such an account we have not described a nonempirical way of knowing.

In any case, we should not grant the Cartesian view that competence gives privileged access to meanings, despite its great popularity. There is a much more modest view of competence according to which it is an *ability or skill* that need not involve any tacit theory, any semantic propositional knowledge; it is knowledge-how not knowledge-that (1996). Why then should we believe the inmodest Cartesian view, particularly since it is almost entirely unargued?

The meaning of a person’s token is presumably constituted by relational properties of some sort: “internal” ones involving inferential relations among tokens and “external” ones involving certain direct causal relations to the world. Take one of those relations. Why suppose that, simply in virtue of the fact that her token has that relation, reflection must lead her to *believe that* it does? Even if reflection does, why suppose that, simply in virtue of the fact that the relation partly constitutes the meaning of her token, reflection must lead her to *believe that* it does? Most important of all, even if reflection did lead to these beliefs, why suppose that, simply in virtue of her competence, this process of belief formation *justifies* the beliefs, or gives them any special epistemic authority, and thus turns them into *knowledge*? Suppositions of this sort seem to be gratuitous. We need a plausible explanation of these allegedly nonempirical processes of belief formation and justification and some reasons for believing in them.

In his “Rationalist Manifesto” Laurence Bonjour also rejects traditional attempts to explain apriority in terms of analyticity, finding them “entirely bankrupt” (1992: 69). For him, “a priori justification occurs when the mind directly or intuitively discerns or grasps or apprehends a necessary fact about the nature or structure of reality” (p. 56). He accepts that “the task of giving a really perspicuous account of such justification has hardly been begun” (p. 88). Quite so. And it seems to me that his later book (1998), despite its many subtleties, does not throw any significant light on where in his quasi-perceptual process of apprehending a necessary fact the justification is to be found.<sup>11</sup>

At this point, it remains a mystery what it would be for something to be known a priori. Any attempt to remove this mystery must find a path between the Scylla of describing something that is not a priori knowledge because its justification is empirical and the Charybdis of describing something that is not knowledge at all because it has no justification.<sup>12</sup> The evidence suggests that there is no such path.

In sum, the case against a priori knowledge is that history has shown that the notion is deeply obscure and Quine has shown that we don't need it.

### 8. Naturalism and the Underdetermination Arguments

We have been considering the underdetermination arguments against Commonsense Realism that involve the theses AG1 and AG2. The Moorean point is that it is simply not plausible that we could know these theses a priori given that they undermine a doctrine as well-established as Realism. The naturalistic point is that we could not know them a priori because there is no a priori. It is time now to consider these theses from a naturalistic perspective.

Naturalism is an over-arching epistemological doctrine claiming that the only way of knowing *anything* is the empirical way of science: for each area of knowledge  $x$ , naturalized  $x$ . When the area is physics, this yields naturalized physics, when the area is biology, it yields naturalized biology, and when the area is epistemology, it yields naturalized epistemology. Everyone believes in a naturalized physics. Everyone but a few benighted creationists in places like Kansas believe in a naturalized biology. But those in the tradition of First Philosophy do not believe in a naturalized epistemology. The radical consequence of naturalism is that philosophy, including epistemology, becomes continuous with science.

From this naturalistic perspective, the troubling epistemological theses, AG1 and AG2, have no special status. They have to be assessed empirically, contrary to the assumptions of First Philosophy, because there is no other way to assess them. As empirical theses, they do not compare in evidential support with our view of stones, trees, cats and the like. Experience has taught us a great deal about such objects but rather little about how we know about them. So epistemology is just the wrong place to start the argument: it is one of the weakest threads in the web of belief (cf. Neurath's boat). Instead, we should start with an empirical metaphysics and use that as the basis for our naturalized epistemology, as the basis for our empirical study of what we can know and how we can know it.<sup>13</sup> Instead of the traditional pattern of argument, exemplified by the underdetermination arguments against Realism,

a priori epistemology → a priori metaphysics,

we should follow the pattern,

empirical metaphysics → empirical epistemology.

The underdetermination arguments not only use the wrong methodology, they proceed in the wrong direction.

Proceeding empirically in the right direction, we start with metaphysics. Realism is then irresistible. Indeed, it faces no rival we should take seriously. We then turn to naturalized epistemology. This is a very difficult matter. Still, with Realism established, we already know that AG1 and AG2 are false. We can go a bit further. It is clear from scientific practice that we are entitled, despite DG1, to dismiss implausible theses like the evil demon one; and that we are usually entitled, despite the equivalence that a theory has with some rivals according to DG2a and DG2b, to prefer that theory to its rivals; we are entitled to believe that all ravens are black, for example. The epistemic standards implicit in scientific practice clearly give us these entitlements. We would like to know, of course, exactly what those standards are but it has proved notoriously difficult to say. Nevertheless, it is indubitable that, whatever the standards are, they give us these entitlements.

Finally, we must consider the objection raised in section 2. For convenience, we have taken the skeptic to be committed to AG1 and AG2. The objection is that this does not do the skeptic justice. The cautious skeptic would be dubious of these epistemological theses as of all other substantive theses. After all, she is a skeptic. Since she is not committed to the theses she is not committed to knowing them a priori. So what then is the nature of her challenge to Realism? She puts the onus on the Realist to justify his rejection of AG1 and AG2. She does not boldly assert the badness of ampliative inferences, thus embracing skeptical standards of justification. She simply points to these epistemic standards, which yield AG1 and AG2, and asks for a justification for ruling the standards out in favor of Realist alternatives that reject AG1 and AG2.

From our naturalistic perspective the challenge of this cautious skeptic is no more difficult to meet than that of the incautious one. For, what I have presented is just what the cautious skeptic wants: a case against AG1 and AG2 and against any epistemic standards that would sustain them. On the one hand, the strength of the case for Realism counts against them. On the other hand, the practices of science counts against them. And these practices are the only place to look in assessing epistemic standards because there is no a priori knowledge. These practices support the use of ampliative inferences for preferring a theory to many, if not all, of its rivals. Sometimes, of course, a theory will face a rival that cannot be ruled out in this way but it is not the case that *all* theories *always* face such rivals. Commonsense Realism is not threatened by the underdetermination that remains.

In conclusion, naturalism appeals to scientific practice to dismiss the skeptic's underdetermination theses AG1 and AG2. Any alternative way of dismissing extreme skepticism must rest on a priori knowledge. I have argued that there is no such knowledge. Even if there were, alternatives that rest on it have tended to involve bizarre metaphysics and to be otherwise unsatisfactory.

If the sort of underdetermination by the actual given evidence discussed in this part had threatened Commonsense Realism it would, of course, also have threatened Scientific Realism. But there is alleged to be a stronger form of underdetermination, underdetermination by the possible evidence, that is thought to challenge Scientific Realism alone. This challenge is associated with the empiricism. I now turn to it.

## II. Underdetermination and Scientific Realism

### 9. *The Argument from Empirical Equivalence*

I begin by emphasizing that this new empiricist challenge to Scientific Realism arises only on the assumption that the old extreme skeptical one, discussed in part I, can be dealt with. For, unless there is an acceptable answer to that challenge to Commonsense Realism, hence challenge to Scientific Realism, the cause of Scientific Realism is already lost. If there is no basis for rejecting AG1 and AG2 and somehow or other allaying Cartesian doubts about the very clearest cases of knowledge of the observable world, then of course there can be no basis for believing in the unobservable world. We have seen that rejecting AG1 and AG2 requires the acceptance of some methods of ampliative inference, methods strong enough to rule out the likes of the evil-demon hypothesis and sustain hypotheses, including generalizations, that cover unobserved observables. I have argued that the basis of rejection is to be found in scientific practice. Others may reject this naturalistic view, claiming that the basis must be found a priori. But such disagreements are beside the present point. This point is that the new challenge arises only once we have, on whatever basis, accepted some ampliative methods of inference and put extreme skepticism behind us. Armed with those methods, whatever they may be, and confident enough about the observable world, empiricists think that there is a further problem believing what science says about unobservables.<sup>14</sup> So the challenge of the underdetermination argument to be considered in this part is not to refight the part I battle with extreme skepticism; it is to respond to this special skepticism about unobservables. We shall see that this point has not been kept firmly enough in mind.

The underdetermination argument we are to consider—really several arguments—is one of the most influential arguments against Scientific Realism.<sup>15</sup> The argument starts from the thesis that any theory that posits unobservables has “empirically equivalent” rivals. What does this amount to exactly? The basic idea is:

**DP.** Any theory positing unobservables has rivals that entail the same possible observational evidence.

This is a deductive underdetermination thesis like DG2b but differing in that it is concerned not only with the set of actual given observations entailed by a theory but with any possible observations entailed by it; hence the name ‘DP’.

On the basis of DP, the empiricist infers the very strong ampliative underdetermination thesis:

**AP:** Any theory positing unobservables has rivals that are equally supported by all possible observational evidence for that theory.

Not only does no actual evidence support a theory over some of its rivals, no possible evidence does so. So what the theory says about the unobservable world can make no evidential difference. Surely, then, commitment to what the theory says is a piece of misguided metaphysics. Even with extreme skepticism behind us, Scientific Realism must be abandoned.<sup>16</sup>

There is disagreement over what counts as “possible evidence.” In my initial assessment of AP I will take a liberal and, it seems to me, intuitive view of what counts. To mark this I will call the thesis “AP(I).” Quine and Bas van Fraassen have a more restricted view of possible evidence yielding a thesis that I will call “AP(r)” and consider in section 13.

Since the possible evidence must include the actual given evidence, AP(I) would entail AG2 were it not for AP(I)’s limitation to theories positing unobservables; so our part I rejection of AG2 would suffice to reject AP(I). It is only because of the limitation to theories positing unobservables that, even with extreme skepticism behind us, we must still be concerned about AP(I). We need a powerful argument to show why the limitation makes this difference. The argument from empirical equivalence is supposed to fulfil this need. The argument from empirical equivalence is another illustration of the pattern:

Deductive underdetermination  $\rightarrow$  ampliative underdetermination  $\rightarrow$  antirealism.

In the next section, I shall argue that this argument, as it stands, is a dismal failure. I shall go on to consider various reinterpretations that are intended to be in the spirit of the argument.

### 10. *Algorithms for Empirical Equivalence*

A good reason for believing DP is that there is an empiricist algorithm for constructing an equivalent rival to any theory  $T$ . Consider  $T_0$ , the theory that the observational consequences of  $T$  are true.  $T_0$  is obviously empirically equivalent to  $T$ . Still, it may not count as a rival because it is consistent with  $T$ . That is easily fixed:  $T^*$  is the theory that  $T_0$  is true but  $T$  is not.  $T^*$  is an empirically equivalent rival to  $T$ . So DP is established.

It is tempting to respond that  $T^*$  is produced by trickery and is not a *genuine* rival to  $T$  (Laudan and Lepin 1991; Hofer and Rosenberg 1994). But this response seems question-begging and unconvincing, as Andre Kukla argues (1998: 66–81). A better response is that, in counting theories generated by the empiricist algorithm as rivals, DP is far too weak to sustain AP(1). For, with extreme skepticism behind us, we *are* justified in choosing  $T$  over  $T^*$ .

In considering this choice, the first half of  $T^*$ ,  $T_o$ , is key. In van Fraassen's terminology,  $T_o$  is the claim that  $T$  is "empirically adequate." He has some famous remarks comparing this claim with the bolder claim that  $T$  is true: "the empirical adequacy of an empirical theory must always be more credible than its truth" (1985: 247); "it is not an epistemological principle that one may as well hang for a sheep as for a lamb" (p. 254). The extra boldness of  $T$  comes, of course, from its Realist commitment to certain truths about unobservables. Because van Fraassen thinks that  $T$  takes no further empirical risk than  $T_o$ , he claims that this extra boldness "is but empty strutting and posturing," a "display of courage not under fire"; (p. 255). We should prefer the weaker  $T_o$ .

Now if van Fraassen were right about this, no evidence could justify a move from  $T_o$  to the bolder  $T$ . So it could not justify a preference for  $T$  over its rival  $T^*$  ( $= T_o$  & not- $T$ ). AP(1) would be established.

Here is a reason for thinking that van Fraassen is not right. If it were really the case that we were only ever justified in adopting the weakest theory compatible with the possible evidence for  $T$ , we would have to surrender to extreme skepticism. For,  $T_o$  is far from being the weakest such theory. For example, consider  $T_e$ , the theory that  $T$  is "experientially adequate." Where  $T_o$  claims that the observable world *is* as if  $T$ ,  $T_e$  claims only that the observable world *appears to be* as if  $T$ .  $T_e$  is much weaker than  $T_o$ ; it does not require that there be an observable world at all; perhaps an evil demon is at work. Those, like van Fraassen, who believe theories of the observable world are displaying courage not under fire all the time.<sup>17</sup>

This argument exemplifies one side of an important, and quite general, Realist strategy to defend unobservables against discrimination, to defend "unobservable rights."<sup>18</sup> The Realist starts by reminding the anti-Realist that the debate is not over extreme skepticism: it is agreed that we have knowledge of observables (sec 9). The Realist then examines the anti-Realist's justification for this knowledge. The above argument exemplifies the side of the strategy that attempts to show that if the case for skepticism about unobservables produced by the anti-Realist were good it would undermine her justification for knowledge of observables. So it cannot be good.

This is the negative side of the Realist strategy. There is also a positive side: attempting to show that the epistemology involved in justifying our knowledge of observables also supports knowledge of unobservables. We can apply this here too. Any methods of ampliative inference that support the move from  $T_o$  to  $T$ , and free us from extreme skepticism must justify the dismissal of the evil-demon hypothesis and a whole lot of others, and must support many hy-

potheses covering unobserved observables ('All ravens are black' and the like). Whether or not these methods alone support the further move to  $T$ , hence support Scientific Realism, they will surely justify the dismissal of  $T$ 's rival  $T^*$ , produced by the empiricist algorithm. And they will justify the dismissal of another empirically equivalent rival produced by Kukla's algorithm according to which the world changes when unobserved (1993). It would be nice to know, of course, what these methods are. But it is a strategic error for the Scientific Realist to attempt to say what they are in responding to the anti-Realist. For, the anti-Realist believes in observables and *whatever* ampliative inferences support that belief will justify the dismissal of the likes of  $T^*$ .

The anti-Realist might, of course, simply insist that inferences that work for observables do not work for unobservables. Certainly there is no logical inconsistency in this insistence.<sup>19</sup> Nevertheless, the insistence is epistemically arbitrary and unprincipled. The Realist need say no more.<sup>20</sup>

We conclude that DP cannot sustain AP(1):  $T$  is indeed justified over empirically equivalent rivals like  $T^*$ . If an argument from empirical equivalence is to work, it needs to start from a stronger equivalence thesis, one that does not count any theory as a rival to  $T$  that can be dismissed by whatever ampliative inferences support our knowledge of the observable world and avoid extreme skepticism. Let us say that the rivals that can be thus dismissed are not "genuine."  $T^*$  and the output of Kukla's algorithm are surely not genuine. Precisely how far we can go in thus dismissing rivals remains to be seen, of course, pending the details of how to avoid extreme skepticism. And, given the Realist strategy, the details that matters are the ones given by the anti-Realist.

In sum, once we keep firmly in mind that our task is not to reflight the battle with extreme skepticism, the argument from DP obviously fails.

### 11. Reinterpreting the Thesis of Empirical Equivalence

DP is too weak to do the empiricist job. We need a reinterpretation of the thesis of empirical equivalence that restricts it to genuine rivals. The natural first stab simply adds 'genuine' to DP yielding:

EE1: Any theory positing unobservables has genuine rivals that entail the same possible observational evidence.

(The talk of entailment makes this seem to be a deductive underdetermination thesis but the restriction to *genuine* rivals makes it partly ampliative. So it does not fit happily into my naming convention. I have not tried to force it.)

Whether or not EE1 is true, it is easy to see that it is inadequate to support AP(1). This inadequacy arises from the fact that any theory  $T$  is likely to entail few observations on its own and yet the conjunction of  $T$  with auxiliary hypotheses, theories of instruments, background assumptions, and so on—briefly, its conjunction with "auxiliaries"—is likely to entail many observations.  $T$

does not face the tribunal of experience alone (Duhem-Quine). By failing to take account of these joint consequences, EE1 leaves many ways in which evidence could favor  $T$  over its rivals, contrary to AP(1). To sustain AP(1) and challenge Scientific Realism, we need a still stronger reinterpretation of empirical equivalence.

Consider Laudan and Leplin's influential critique of the underdetermination argument (1991). They propose the thesis, "The Instability of Auxiliary Assumptions" according to which "auxiliary information providing premises for the derivation of observational consequences from theory is unstable in two respects: it is defeasible and it is augmentable" (p. 57).<sup>21</sup> As the accepted auxiliaries that can be conjoined with  $T$  change, so do its consequences. So, any determination of  $T$ 's empirical consequence class "must be relativized to a particular state of science," the state that supplies the auxiliary hypotheses. Thus "any finding of empirical equivalence is both contextual and defeasible" (p. 58). To determine the consequences of  $T$  we need more than logic, we need to know which auxiliaries are acceptable, an "inescapably epistemic" matter (p. 59).

To avoid the consequences of this argument, Kukla (1993) proposed a reinterpretation along the following lines: for two theories to be empirically equivalent at time  $t$  is for them to entail the same observations when conjoined with  $At$ , the auxiliaries that are accepted at  $t$ . This yields:

**EE2:** Any theory positing unobservables has genuine rivals which are such that when it and any of the rivals are conjoined with  $At$  they entail the same possible observational evidence.

Set aside for a moment whether or not EE2 is any threat at all to Scientific Realism. It is clearly too weak to sustain the threat posed by AP(1). Let  $T$  be a theory and  $T'$  an empirically equivalent rival according to this interpretation. So  $T \& At$  and  $T' \& At$  entail the same observations. This sort of equivalence is relative to  $At$ , to the auxiliaries accepted at a certain time. It amounts to the claim that  $T$  and  $T'$  cannot be discriminated observationally if conjoined only with those auxiliaries. But this does not show that  $T$  and  $T'$  could not be distinguished when conjoined with any acceptable auxiliaries at any time. And that is what is needed, at least, to sustain the claim that  $T$  and  $T'$  cannot be discriminated by any possible evidence, as AP(1) requires. AP(1) demands a much stronger reinterpretation of the empirical equivalence: for two theories to be empirically equivalent is for them to entail the same observations when conjoined with any possible acceptable auxiliaries (cf. section 13 on the demands of AP(r)). This yields:

**EE3:** Any theory positing unobservables has genuine rivals which are such that when it and any of the rivals are conjoined with any possible acceptable auxiliaries they entail the same possible observational evidence.

If  $T$  and  $T'$  were thus related they would be empirically equivalent not just relative to certain auxiliaries but *tout court*, absolutely equivalent. Only then would they be observationally indiscriminable. So if a thesis of empirical equivalence is to support AP(1), it must be interpreted as EE3.

Laudan and Leplin's critique leads them to two somewhat different conclusions about the doctrine of empirical equivalence. They claim, (i), that the doctrine "loses all significance for epistemology" (1991: 57); and, (ii), that we have no reason to believe the doctrine (e.g., p. 55). (i) is an exaggeration. The truth underlying it is that EE2 cannot sustain AP(1). Still, we shall see (sec. 12) that were EE3, or even EE2, true, they would be epistemologically significant. I take Laudan and Leplin's main claim to be (ii). And what they have in mind is that we have no reason to believe EE3.<sup>22</sup> This is dead right. If  $T$  and  $T'$  cannot be discriminated observationally relative to, say, currently accepted auxiliaries, they may well be so relative to some future accepted auxiliaries. Some currently accepted auxiliaries may cease to be accepted and some new auxiliaries are likely to become accepted. This point becomes particularly persuasive, in my view (1991: 119), when we note our capacity to invent new instruments and experiments to test theories. With a new instrument and experiment comes new auxiliaries, including a theory of the instrument and assumptions about the experimental situation. Given that we can thus create evidence, the set of observational consequences of any theory seems totally open. Of course, there is no guarantee of successful discrimination by these means: a theory may really face a genuine empirically equivalent rival. Still, we are unlikely to have sufficient reason for believing this of any particular theory.<sup>23</sup> More importantly, we have no reason at all for believing it of all theories, as EE3 requires. We will seldom, if ever, have a basis for concluding that two genuine rivals are empirically equivalent in the absolute sense required by EE3.

Behind this argument lies the following Realist picture.  $T$  and  $T'$  describe different causal structures alleged to underlie the phenomena. We can manipulate the actual underlying structure to get observable effects. We have no reason to believe that we could not organize these manipulations so that, if the structure were as  $T$  says, the effects would be of one sort, whereas if the structure were as  $T'$  says, the effects would be of a different sort.

The argument against EE3 does not depend on any assumption about the breadth of  $T$ . So EE3 cannot be saved by taking it to apply to "total sciences" (Boyd 1984: 50). Should such a broad conjunction of theories seem to face an equivalent rival at a certain time, we are unlikely to have sufficient reason for believing that experimental developments will not enable us to discriminate the conjunction from its rival by supplying new auxiliaries. There is no known limit to our capacity to generate acceptable auxiliaries.

AP(1) would be a disaster for Scientific Realism but it needs a powerful argument. With extreme skepticism behind us, DP does not come close to providing the argument. Nor does EE1 because it entirely overlooks the role of auxiliaries in providing evidence. Nor does EE2 because it does not take ap-

*appropriate account of the role of auxiliaries.* EE3 cannot provide the needed argument because we have no reason to believe it. I conclude that there is no powerful argument for AP(1). Still, there remain some loose ends to clear up.

### 12. Possible Consequences for Scientific Realism

Suppose, nonetheless, that EE3 were true. Would it then establish AP(1) and undermine Scientific Realism? It might well do so.<sup>24</sup> If EE3 were true, Realists would have to appeal to “nonempirical virtues” to choose between empirically equivalent theories. Empirical virtue is a matter of entailing (in conjunction with accepted auxiliaries) observational truths and not entailing observational falsehoods. The nonempirical virtues are explanatory power, simplicity, and the like. I think that the Realist is entitled to appeal to explanatory virtues, at least. But if it really were the case that all theories faced genuine rivals equally compatible with all possible evidence, the appeal to these virtues would seem epistemologically dubious.<sup>25</sup> For, in those circumstances, there could be no way to judge the empirical success of these virtues, no way to show, for example, that theories that provide the best explanation tend to be observationally confirmed. So the defense of Scientific Realism might well depend on there being no good reason for believing EE3.

What about EE2? It will not sustain AP(1), but perhaps it is otherwise threatening to Scientific Realism. So, first, we need to consider whether it is true; then, whether, if it were, it would undermine Realism.

There are surely some theories that face a genuine rival that is empirically equivalent relative to the accepted auxiliaries at a certain time. But do *all* theories face such rivals at that time, let alone at *all* times? EE2 guarantees that all theories do at all times. But the ampliative methods, whatever they may be, that support our knowledge of the observable world and avoid extreme skepticism will count many rivals as not genuine, so many as to make this guarantee seem baseless. How could we know a priori that a theory must always face such a genuine rival?

Suppose, nonetheless, that EE2 were true. So, if a theory *T* and its rivals are restricted to the accepted auxiliaries at a certain time, *T* could not be justified over some rivals on the basis only of the observations that the theories and auxiliaries entail and the ampliative methods that save us from extreme skepticism. So, without recourse to some further ampliative methods, *T* would be underdetermined by the evidence that the restriction allows into play. Of course, once new acceptable auxiliaries were discovered and the restriction changed, the further methods might well not be needed to justify *T* over those old rivals. So this underdetermination would not be as serious as AP(1), but it would be serious enough: at any time, we would not know what to be Realist about. But then perhaps the Realist *would be* entitled to the further ampliative methods that would remove this underdetermination. Given that the case for EE3 has not been made, I think that the Realist might be so entitled.<sup>26</sup>

We have no reason to believe EE2 or EE3 and so they cannot undermine Scientific Realism. However, if EE3 were true, it might well do so, and if EE2 were true, it could. Once we have set aside extreme skepticism then, contrary to received opinion, the nonempirical virtues are not central to defending Scientific Realism from the argument from empirical equivalence; the rejection of the equivalence thesis is.

### 13. Restricting Possible Evidence

In assessing AP and its consequences I have taken what seems to be an appropriately liberal view of what counts as “possible observational evidence,” taking note of our capacity to create evidence by inventing new instruments and conducting new experiments; hence the name ‘AP(1)’. Quine has a more restricted view reflecting, no doubt, his distaste for modality. He takes the possible evidence to be what would have been observed had there been an observer at each point of *actual* space-time (1970b: 179). This is also van Fraassen’s view of the phenomena he wishes to save: all *actual* observable things and events, past, present and future, whether or not anyone in fact observes them (1980: 12, 60, 64). On this restricted view, acts of observation are the only nonactual aspects of possible evidence. On the liberal view, in contrast, the possible evidence includes many things that we do not do, but could have done, other than merely observing. If we had had more time, energy, and money perhaps we could have invented the right instruments and conducted the right experiments to discriminate between *T* and *T'*. There may be many differences between them which we never detected because we *passively observed* points of actual space-time where we could have *actively intervened* (Hacking 1983) to change what happened.

Let us make explicit what AP becomes on the restricted view of possible evidence:

**AP(r):** Any theory positing unobservables has rivals that are equally supported by all the actual truths about observables.

Quine may believe AP(r)<sup>27</sup> but, as Laudan points out (1996: 41–2), he offers no evidence for it. Yet it needs a powerful argument for much the same reason that AP(1) did (sec. 9). Since the actual truths about observables must include the observed truths, AP(r), like AP(1), would entail AG2 *were it not for its limitation to theories positing unobservables*. So our part I rejection of AG2 would suffice to reject AP(r). We need a powerful argument to show why, with extreme skepticism behind us, this limitation makes a difference. Set that aside for a moment.

What would the consequences be for Scientific Realism if AP(r) were true? In a previous work I found AP(r), in effect, “too weak for the task of undermining Realism” (1991: 121). Certainly, AP(r), unlike AP(1), would not show

responses of First Philosophy to these theses rest on a priori knowledge. I argue that there is no such knowledge. Even if there were, these traditional responses tend to involve bizarre metaphysics and to be otherwise unsatisfactory. Instead, I urge a naturalistic response that appeals to scientific practice.

In part II we considered AP, the very strong thesis that scientific theories positing unobservables are underdetermined by *all possible* evidence. If AP were true it would threaten Scientific Realism even though Commonsense Realism was secure. There are two interpretations of AP, one with a liberal view of possible evidence, AP(l), and one with a restricted view, AP(r). On each interpretation, AP needs a powerful argument that it does not have. In particular empirical equivalence theses do not provide the argument either because we have no reason to believe them or because they would not support AP(l) and AP(r) if they were true, or both. The underdetermination argument against Scientific Realism turns out to be rather weak. To see this we need to keep firmly in mind that the argument only arises once extreme skepticism is behind us.<sup>28</sup>

#### Notes

1. 1991, 1997a, 1999. My definitions are unfashionable in not being, or even appearing to be, semantic. I argue that it is very important to disentangle the metaphysical doctrine of realism from any semantic doctrine.
2. Quine thinks this likely even given all the sensory evidence that there will ever be, "man's surface irritations even unto eternity" (1960: 23).
3. Larry Laudan emphasizes the importance of the distinction between the deductive and the ampliative in his helpful discussion of underdetermination theses (1996: ch. 2).
4. Laudan calls this "the nonuniqueness thesis". A stronger thesis, "the egalitarian thesis" claims that a theory enjoys no support over *all* of its rivals. He is sadly persuasive in attributing the latter thesis to Quine, Thomas Kuhn, and Mary Hesse (1996: 33–43).
5. For example, Kuhn 1970 (on which see Hoyningen-Huene 1993); Feyerabend 1975, 1981; Goodman 1978; Putnam 1981; Latour and Woolgar 1986.
6. See particularly Quine 1952: xi–xvii; 1961: 42–6. Quine uses 'naturalism' to stand for this epistemological doctrine. Others use it to stand for the reductive metaphysical doctrine like physicalism.
7. We are concerned whether there is a nonempirical way of *justifying* knowledge. We are not concerned whether there is a nonempirical *source* of knowledge, whether there is *innate* knowledge. Innate knowledge is sometimes called 'a priori' but that is not my usage. Naturalism is consistent with there being some innate knowledge (although I strongly doubt that there is). Naturalism simply insists that any such knowledge must be justified empirically: presumably, experiences of the worldly facts that are the subject of the innate knowledge must play a role via adaptation in the production of the knowledge.
8. See, for example, Kitcher 1980.
9. Rey (1998) even urges us to use it to show that there is an a priori way!
10. What exactly is *believing* according to RTM? The literature often seems to suggest that *believing* is a functional relation between agents and mental tokens. But there

that we *could not* find evidence that would discriminate between a theory and its genuine rivals, just that we ran out of time before we did. So AP(r) would not show that commitment to a theory's unobservables was a piece of misguided metaphysics. But I overlooked that AP(r) would still be a problem for Realism because it would have the consequence that we would *never as a matter of fact* know what to be Realist about; we would *never as a matter of fact* be justified in preferring the unobservables of our chosen theory over those of its rivals. So the Realist must resist AP(r) as well as AP(l).

Where might we find an argument for AP(r)? Once again we must look to an empirical equivalence thesis. What thesis? We found no reason to believe EE3 but, in any case, its talk of "any possible acceptable auxiliary" would not recommend itself to the restricted view of possible evidence. On that view, any empirical equivalence thesis that is to support AP(r) must surely restrict the relevant auxiliaries to actual ones. And it will not be sufficient to restrict them to actual ones that are accepted "at any point of space-time" because some that are accepted at one point are later rejected; think, for example, of auxiliaries about the number of planets. It seems that we must restrict auxiliaries to the ones that are still standing "at the end of human inquiry"! This yields:

**EE4:** Any theory positing unobservables has genuine rivals which are such that when it and any of the rivals are conjoined with auxiliaries that are accepted at the end of human inquiry they entail the same possible observational evidence.

But why should we believe EE4? In discussing EE2 we found no reason to believe that all theories face genuine equivalent rivals relative to the auxiliaries at any time (sec. 12). So we have no reason to believe that all theories do relative to the auxiliaries at the end of human inquiry. We have no reason to believe even that *some* theories do. EE4 is baseless.

Suppose that EE4 were true? Would it sustain AP(r)? If not, would it be otherwise damaging to Scientific Realism? It is not clear that the answer to either question is "Yes." In thinking about these questions we should look, once again, for ampliative methods beyond those that save us from extreme skepticism.

In sum, AP(r), with its restricted view of the possible evidence, would threaten Scientific Realism, albeit not as badly as AP(l) with its liberal view. But AP(r), like AP(l), needs a powerful argument that it does not have. In particular it can get no support from the equivalence thesis EE4 because the thesis is baseless. And it is not clear that EE4 would support AP(r) or anti-Realism even if it were true.

#### 14. Conclusion

In part I we considered the underdetermination theses AG1 and AG2 of extreme skepticism. If true these would count against nearly all our knowledge and hence undermine Commonsense and Scientific Realism. The traditional

are problems with this. I am inclined to think that we should not take it as a relation at all. We should take 'belief' to have a dual function, part relative term and part quantifier (2001b).

11. Hilary Kornblith (2000) raises another concern about Bonjour's notion of the a priori: Can it do any epistemological work?
12. My objection (1997b, 1998) to Rey's attempt to give a reliabilist characterization of the a priori (1997, 1998) is, in effect, that it falls victim to Charybdis.
13. Van Fraassen, in his critique of what he calls (independently), "Moorean Scientific Realism" (2000: 261–71), seems to misunderstand the naturalist's view of the relation of epistemology to science. It goes without saying that *epistemology* implies the methods of science. But van Fraassen seems to take the naturalist view to be that *basic science, or special sciences like biology, medicine, and psychology*, imply the methods of science, a view that van Fraassen rejects. But this is not the naturalist view. The naturalist view is that *epistemology is itself a special science*. As such it is no more simply implied by another science than is any other special science: it has the same sort of relative autonomy, and yet dependence on basic science, as other special sciences. Naturalized epistemology, like any special science, applies the usual methods of science, whatever they may be, mostly taking established science for granted, to investigate its special realm. In the case of epistemology that realm is those very methods of science. The aim is to discover empirically how we humans learn, and should learn, about the world (1991: 75–9). We have no reason to suppose that the methods that have yielded knowledge elsewhere cannot yield knowledge in epistemology.
14. This further problem should also concern somebody who takes an idealist path to rejecting AG1 and AG2. This idealist has, of course already abandoned the independence dimension of Scientific Realism but she should be as interested as the Realist in defending the existence dimension. She should be as interested in preserving the idea that science is more or less right about the unobservables even though, for her, these unobservables are not mind-independent. The existence dimension of Scientific Realism is challenged by this new underdetermination argument.
15. In my view (2002), another influential argument, the "pessimistic meta-induction," is more powerful although ultimately ineffective.
16. The argument has no one clear source. But see Duhem 1906, Quine 1960, 1961 ("Two Dogmas"), and 1975; van Fraassen 1980, Putnam 1983 ("Equivalence").
17. I develop this argument more thoroughly in my 1991: 150–3.
18. For examples of this strategy, see Boyd 1984, Churchland 1985, Gutting 1985, Musgrave 1985, Clendinnen 1989, Devitt 1991 (pp. 147–53), and Psillos 1999 (pp. 186–91). Van Fraassen 1985 responds to Churchland, Gutting, and Musgrave.
19. Kukla emphasizes this (1998: 25–6, 84).
20. However, I think that an examination of the epistemic significance of observation helps to bring out the arbitrariness (1991: 143–7).
21. See also Ellis 1985 and Devitt 1991: 117–21.
22. Note that this is not the claim that EE3 is "demonstratively false"; cf. Kukla 1998: 58.
23. For some theories where we may have sufficient reason, and for some past ones where we wrongly thought we had, see Psillos (1999: 166–8) and the works he cites.
24. Laudan and Leplin (1991: 63–8) think it would not, arguing that *T* can be indirectly supported over its rival by evidence that confirms another theory that entails *T* but not its rival; and that some consequences of *T* and its rival might support only *T*.

But, as Kukla points out (1998: 84–90), this argument begs the question: if EE3 really were true, this evidential support would seem to disappear.

25. I emphasize that since it has not been established that *all* theories do face such rivals, it might well be appropriate to appeal to explanatory virtues, or indeed to the evidential support mentioned by Laudan and Leplin (note 24), to prefer *some* theory that does face such a rival.
26. In a reply to Kukla 1993, Leplin and Laudan (1993: 10), in effect, doubt EE2 but in any case emphasize that EE3 is what matters to the underdetermination argument. Kukla disagrees, claiming, in effect, that EE2, when applied to total sciences, "brings in its train all the epistemological problems that were ever ascribed to the doctrine of EE" (1998: 64). According to my discussion EE2 would bring some epistemological problems if it were true, but they are not as extreme as those that would be brought by EE3 if it were true.
27. Quine starts, in effect, by saying that AP(r) is *conceivable* (1960: 22), which it surely is, and then moves on to the earlier-mentioned claim (note 2) that the somewhat different AG1 is *likely* even when the given evidence is "man's surface irritations even unto eternity" (1960: 23).
28. Comments on my 2002 by Andre Kukla, Jarrett Leplin, and David Papineau have helped me in writing this paper.

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