

Contact with the Nomic: A Challenge for Deniers of Humean Supervenience about Laws of Nature Part II: The Epistemological Argument for Humean Supervenience

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In Part I, we presented and motivated a new formulation of Humean Supervenience about Laws of Nature (HS). Here in Part II, we present an epistemological argument in defense of HS, thus formulated. Our contention is that one can combine a modest realism about laws of nature with a proper recognition of the importance of empirical testability in the epistemology of science only if one accepts HS.

In Part I, we formulated Humean Supervenience about laws (henceforth HS) as the thesis that the facts about what is a law and what is not cannot vary between two worlds unless they disagree on the Humean base. We defined the Humean base at a possible world as the set of non-nomic, non-counterfactual facts at that world that are detectable by a nomically reliable, spatiotemporally finite measurement or observation procedure. We stipulated that two worlds “agree on the Humean base” just in case every fact in the Humean base of one of these worlds also obtains in the other (whether or not it belongs to the Humean base of the other world). Most of Part I was devoted to arguing that this formulation of HS captures what is at stake in the debate over HS without undertaking extraneous additional commitments. In Part II, we will present an epistemological argument for HS thus formulated.

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1. Introduction: Underdetermination Arguments, Realism, and Laws

A common strategy for arguing against realism about a subject matter X is to use an *underdetermination argument*. You first allege that the facts about X are underdetermined by everything that could serve as evidence about X—perhaps equivalently, that any theory about X has empirically equivalent rivals. Then you pose a dilemma: Either the facts about X are constituted at least in part by acts or decisions of ours, or else they are independent of our acts and decisions. In the first case, some kind of constructivism or conventionalism is true, which is one way that realism can fail. In the second case, the facts about X are epistemically inaccessible to us. Skepticism follows, which is another way that realism can fail. Either way, realism fails.²

Here we are not interested in arguing against realism about anything. In fact, we share with our anti-Humean opponents (including Armstrong (1983), Dretske (1977), Tooley (1977), Carroll (1994), Lange (2000), Maudlin (unpublished), and others) a commitment to realism about laws of nature. This shared realism says that there are laws of nature, that something's being a law of nature is an objective feature of the world not constituted by any convention or decision on our part, and that it is possible to be epistemically justified on empirical grounds in believing that something is a law of nature. We think that to deny HS is to imperil this kind of realism. For if HS is false, then the laws are underdetermined by all possible empirical evidence, and this underdetermination is incompatible with realism about laws. (Some philosophers, e.g. van Fraassen (1989) and Giere (1999), reject the very idea that science discovers laws. Our argument is not addressed to them. We will presuppose that science does aim to discover laws (among lots of other things), and that this is a sensible and attainable goal, and try to show that anyone who agrees with this presupposition has good reason to embrace HS.)

Strictly speaking, we are not giving an underdetermination argument against realism; rather, we are arguing that if HS were false, then there would be a sound underdetermination argument against realism, and inferring by *modus tollens* that HS is true. Nonetheless, we are relying on an underdetermination argument to establish a conditional: If HS is false, then so is realism about laws. So it is a potential worry for us that anti-realist arguments that appeal to empirical equivalence and underdetermination have lately been subjected to much criticism. In particular, the following complaints have been raised against such arguments:

Complaint 1: Empirically equivalent alternatives to a given theory are not as easily produced as is often supposed.

² See Laudan and Leplin (1991) and Earman (1983) for general discussions of underdetermination arguments.

Complaint 2: When empirically equivalent alternatives can be found, it is often not at all clear that they are genuinely incompatible rival theories that deserve to be taken seriously as scientific theories. In some cases, they are mere notational variants on the original theory. In some cases, they are “cheap instrumental rip-offs,” i.e. theories that cannot even be formulated without piggybacking on the theory to which they are supposed to provide alternatives. (Example: “The world contains some unobservable structure or other that conspires to cause the observable structures all to be exactly as if the general theory of relativity were true.” This is perhaps an alternative to general relativity, but it is not a rival *theory*; no *theory* has been specified here.) In some cases, they are philosophers’ skeptical-nightmare scenarios, involving Cartesian demons, brains in vats and the like, which are not serious contenders as scientific theories.

Complaint 3: Arguments for underdetermination by all possible evidence tend to place unreasonable restrictions on what can count as possible evidence.

Complaint 4: Even in the face of underdetermination by all possible empirical evidence, there are still ways of justifying acceptance of one theory rather another, for example by appeal to pragmatic considerations.

Complaint 5: If underdetermination arguments worked, then they would prove too much. For example, the facts about material objects are underdetermined by our sense data, and the facts about the not-yet-observed portions of the universe are underdetermined by the already-observed portions; hence, if the basic logic underlying anti-realist underdetermination arguments were good, then it would establish phenomenalism and inductive skepticism. But those are false, so there is something suspect about the logic of underdetermination arguments. At any rate, they cannot be relied on to establish any kind of *selective* anti-realism; to the extent that they work at all, they undermine just about all of our putative empirical knowledge.

(All five of these complaints can be found, for example, in Laudan and Leplin 1991.) We think that all five complaints make good points, and that each of the five can be used to undermine some anti-realist underdetermination argument that has actually been given. Hence, we do not think that the underdetermination strategy works to make a good case for general scientific anti-realism. But we think that the underdetermination of laws by evidence that would arise if HS were false is a special case. It really does pose a threat (or rather, it would pose a threat if HS were false), and none of the five complaints mentioned above will serve to block this threat.

In section 2, we will lay out our argument for HS. In sections 3 and 4, we will defend its two key premises, and in section 5, we will consider two

important objections. Along the way, we will briefly note ways in which our argument gets around the five complaints presented above; in section 6, we will do this more systematically. In section 7, we will situate our argument for HS within a broader philosophical context.

2. The Epistemological Argument for HS

First, some stage setting. *Realism about laws* (i.e. *nomic realism*) may be defined as the conjunction of *semantic realism about laws* and *epistemic realism about laws*, defined as follows:

Semantic realism about laws: There is an objective, mind-independent matter of fact about which propositions are laws of nature and which are not.

Epistemic realism about laws: It is possible for us to be justified on empirical grounds in believing propositions of the form *that P is a law*.

We might have defined epistemic realism in terms of the possibility of *knowing* that P is a law. But this weaker formulation serves as well. Some philosophers might want to relativize lawhood to a context (e.g., contexts where the classical limit of some physical theory is approximately true) or to a particular field of science (e.g., perhaps there are laws of economics that are not laws of physics, and vice versa; see Lange (2000)). Both kinds of realism defined above can accommodate this; just replace “law” by “law in context C” or “law in field F” throughout. Our argument is not affected by such a substitution.

Let T be any scientific theory that posits one or more laws. Let T be formulated thus:

T: L is a law of nature, and X.

Here X just stands for whatever content T has over and above the lawhood of L; it might be a conjunction of many other law-statements, and it might contain some conjuncts about the nomologically contingent details of the world; on the other hand, it might be empty. We can now define a rival theory T*:

T*: L is true but it is not a law, and X.

Note that by defining our rival theory T* in this way, we evade complaints 1 and 2: Once T is formulated, it is easy to state T*; T* is fully spelled out, without referring to T or otherwise saying that “observable things are just as if T were true”; T and T* are genuine rivals, since they are logically incom-

patible with one another; T* does not advert to Cartesian demons or other skepticism-inducing devices.

It might be thought that T* is a poor example of scientific theory, since it posits a “cosmic coincidence,” namely the accidental truth of L. Whatever the merits of the idea that scientific theories should never posit “cosmic coincidences,” this complaint is wide of the mark. T* need not imply that L is a “cosmic coincidence” in any interesting sense. For T* is consistent with there being a covering-law explanation of L. For example, it is consistent with the further hypothesis that there are laws of nature that entail that if L is true at one time, then it is true at all times. In that case, it would be odd to call L a “coincidence.” In fact, these further laws might not be a “further hypothesis” at all, for they might be contained in X, and thus already part of the content of T*.

Consider a concrete example: Of the four Maxwell equations, the two curl equations entail that if the two divergence equations are true at one time, then they are true at all times. So we can consider two distinct physical theories: M, which says that all four Maxwell equations are laws of nature, and M*, which says that only the two curl equations are laws of nature, and the two divergence equations are contingently true at some particular time, from which it follows that they are true at all times, though only contingently so. If HS is false, then it would seem to follow that for any world *w* where M is true, there is a world that agrees with *w* on the Humean base but in which M* is true.³ M and M* are logically incompatible, and each one can be formulated without reference to the other. So each is a well-defined theory in its own right, and the two are genuine rivals. This shows that our schematic example of T and T* has physically interesting concrete instances.

Our epistemological argument for HS has two premises:

Premise 1: If HS is false, then no empirical evidence can favor T or T* over the other.

Premise 2: If semantic realism about laws is true, and no empirical evidence can favor T or T* over the other, then we cannot be epistemically justified on empirical grounds in believing that T is true.

Some comments on these premises are called for before we move on.

First, it might not be clear what role semantic realism about laws is playing in Premise 2. If semantic realism is not true, then it could be that we are free to impute or project lawhood onto any true regularity we choose (as Rescher (1970) argues). In that case, the only empirical grounds we need in order to justify believing T would be empirical evidence that the non-nomic

³ But this claim needs to be qualified; see below, subsection 4.2.

content of T is true, and this need not favor T over T*. Hence the need for the first clause of Premise 2.

Second, in Premise 2, and in the definition of epistemic realism, we speak of being “epistemically justified on empirical grounds.” It would be difficult and perhaps impossible to say exactly what this means. What we have in mind can be made clear enough for present purposes, though: One is epistemically justified on empirical grounds in believing that P just in case empirical evidence-statements, together with the norms of the empirical sciences, can be used to offer a positive justification for believing that P. As vague as this is, it does rule certain things out, and it does leave room for justified beliefs that are not justified on empirical grounds, e.g. mathematical beliefs, ethical beliefs, and perhaps religious and metaphysical beliefs. It might be objected that “the norms of the empirical sciences,” as well as what can count as an “empirical evidence statement,” vary from historical period to historical period, from branch of science to branch of science, from culture to culture. We don’t need to deny any of this. Our argument contains two occurrences of “justified on empirical grounds”: one in the definition of “epistemological realism,” and one in Premise 2. So long as this phrase is understood in the same way in each occurrence, our argument is valid. So the argument, and thus HS, can remain in place throughout changes in what counts as empirical justification.

It follows from our two premises that if HS is false and semantic realism is true, then we cannot be epistemically justified on empirical grounds in believing that T is true. But T is an arbitrarily selected law-positing theory, and L is an arbitrary law posited by T. So by universal generalization: If HS is false and semantic realism about laws is true, then we cannot be justified in believing any law-positing theory—that is, we cannot be justified in believing of any proposition P that it is a law of nature. Therefore, if HS is false, then so is the conjunction of semantic and epistemic realism about laws. If you want to maintain realism about laws, you must embrace HS.

That concludes our argument. It is worth pausing to note that the reasoning it employs does not refute realism about laws if HS is true. For if HS is true, then if the disjunction of T and T* is true, then the question of which one is true is settled by facts that are in the Humean base and thus in principle empirically detectable. So empirical evidence can favor one of T and T* over the other, and there is no threat to realism about laws.

Our argument is deductively valid, so everything depends on whether its two premises are true. We will defend these premises in the next two sections, taking them in reverse order.

3. Defending Premise 2

Assuming that semantic realism about laws is true, Premise 2 says that if no empirical evidence can favor T or T* over the other, then we cannot be epistemically justified on empirical grounds in believing that T is true. Why should anyone believe this? There is one obvious argument for it that we do not accept: "In order to be justified in believing that T is true, one must have some evidence that rules out *every* possible alternative to T, or at least favors T over that alternative. Otherwise, for all one knows, the alternative could be true and T could be false. T* is such an alternative. So if one has no evidence that rules out T*, then one cannot be justified in believing that T is true." Call this the *No-Alternative-Left-Standing Argument*. The No-Alternative-Left-Standing Argument proves too much: It effectively rules out all justified non-demonstrative inference. Its opening premise sets the bar for justification too high, since it does not allow that one is justified in believing anything which could be false given all of one's evidence. So this is not the reason why we think Premise 2 should be accepted. (This point will be important for our reply to complaint 5; see below, section 5.)

It is noteworthy that if the No-Alternative-Left-Standing Argument were one's reason for believing Premise 2, then our formulation of Premise 2 would be unnecessarily weak. It could be strengthened, by weakening its antecedent from "No empirical evidence can favor either T or T* over the other," to "No empirical evidence can favor T over T*." For us, the real reason why Premise 2 should be accepted depends on the antecedent not being weakened in this way; it is just as important that there can be no evidence that favors T* over T as that there can be no evidence that favors T over T*. T and T* are thus treated symmetrically by the antecedent. The crucial point is that, if no empirical evidence can favor either of these theories over the other, then any evidence that tends to favor either one of them really only favors, at most, their disjunction, "T or T*," which is of course equivalent to "L is true, and X." T and T* both add something to this claim, and that addition does not get supported by any possible empirical evidence. This is because evidence that supported the something-extra would be empirical evidence that favored T over T* or vice versa, and *ex hypothesi* there can be no such empirical evidence.

This argument allows that non-demonstrative inferences can be justified, even though they take an epistemic risk by going beyond the available evidence. What it does not allow is a kind of sham epistemic risk, the risk of failure that can never be exposed. Once one accepts the disjunction of T and T* (i.e., the conjunction of L and X), the extra content that T adds to this disjunction can never be discovered to be false, because no empirical evidence can ever favor the other bit of extra content that T* adds over the extra content that T adds. Our claim is not that such epistemic risks are never justified

(for example, we do not deny that one can ever be justified in adopting religious beliefs that cannot be empirically disconfirmed), but only that when they are, their justification is not empirical, and the norms of science cannot be called upon to ratify the risky inference.

Our argument here is very similar to van Fraassen's argument against "pointless epistemic risks." In reply to Musgrave's (1985, p. 199) claim that it is unreasonable to restrict oneself to believing that a well-tested theory is empirically adequate (rather than true), since "the principle that one might as well hang for a sheep as for a lamb is a pretty sensible one," van Fraassen writes:

If I believe the theory to be true and not just empirically adequate, my risk of being shown wrong is exactly the risk that the weaker, entailed belief will conflict with actual experience. Meanwhile, by avowing the stronger belief, I place myself in the position of being able to answer more questions, of having a richer, fuller picture of the world, a wealth of opinion so to say, that I can dole out to those who wonder. But, since the extra opinion is not additionally vulnerable, the risk is—in human terms—illusory, and *therefore so is the wealth*. It is but empty strutting and posturing, this display of courage not under fire and avowal of additional resources that cannot feel the pinch of misfortune any earlier. What can I do except express disdain for this appearance of greater courage in embracing additional beliefs which will *ex hypothesi* never brave a more severe test? (van Fraassen 1985, 255)

Our argument is being used in a different context and for a different end than van Fraassen's. He imposes stricter constraints on what counts as empirical evidence than we do (see section 4, below), so he sees pointless epistemic risks—inferences that go beyond all possible empirical evidence—in many more places than we do. Also, our argument is intended to support a supervenience claim, whereas his is intended only to support the view that the aims of science as such do not require more than agnosticism with respect to what does not supervene on the observable. Unlike van Fraassen, we think that claims about what the laws are play a crucial role in at least some scientific theories. So we think that justifying claims about laws are among the aims of science. Like van Fraassen, we think the aims and norms of science justify taking epistemic risks, but not pointless ones that can never conflict with experience and (therefore) can never lead to new predictions. Hence, we hold that whatever laws of nature are, they must be such that we can infer to conclusions of the form "P is a law" without taking pointless epistemic risks. That is why we believe Premise 2.

4. Defending Premise 1

4.1. What Can Be Empirical Evidence?

Since Premise 1 quantifies over all possible evidence, we are going to have to impose some restriction on what can count as empirical evidence. We will

use “evidence” primarily to refer to *evidence statements*, by which we mean the propositions or facts that are believed or reported non-inferentially in response to observation, and can then serve as inputs to inferences. (By “facts,” we just mean *true propositions*.) There are other legitimate uses of “evidence,” but we will focus on this one, since our interest is in evidence *qua* premises for inferences. We aim to place a restriction on what can count as empirical evidence that is as liberal as can be while not obviously letting in too much. Toward this end, we assume that an evidence statement must be a proposition (or fact) that can be ascertained to be true by means of a spatio-temporally finite, nomologically reliable measurement or observation procedure. A proposition asserted on the basis of an unreliable procedure is not the report of an observation, but at best the report of a pseudo-observation. A procedure that is not spatiotemporally finite cannot actually be used by a finite observer, such as ourselves or anything else that could count as an empirical cognizer. Hence, these two requirements are a *sine qua non* for evidence statements. They imply that any proposition that could be the content of an observation report at world *w* must, if true, belong to the Humean base at *w*. In short, it is necessary that all veridical empirical evidence statements belong to the Humean base.

Some philosophers would impose further restrictions on what can count as empirical evidence. For example, van Fraassen (1985) allows only reports of observations made by normal, unaided human perception. We impose no further restrictions, however. In the spirit of Maxwell (1962), Sellars (1956), and Feyerabend (1962), we maintain that the line between the observable and the unobservable is both vague and fluid. Observations made with the help of artificial instruments and informed by sophisticated theory are not in principle any more epistemologically suspect than observations made with the help of humanity’s natural observational equipment and informed by common sense. We will not pause to argue the point here, though. For anyone who disagrees with us on this point, our argument will establish an even stronger supervenience claim than the one we endorse. Our quarrel with them is thus quite different from our quarrel with the deniers of HS, which is the one we are pursuing here.

4.2. Empirical Evidence Cannot Distinguish *T* from *T**

Premise 1 says that if HS is false, then it is impossible for any body of empirical evidence to support either *T* or *T** to a higher degree than it supports the other. Of course, it is controversial matter how degrees of evidential support should be assessed. Here we will not defend any particular account of degrees of evidential support. Instead, we will try to show that on all of the current, promising strategies for understanding evidential support, Premise 1 is true. This leaves open the possibility that the true account of evidential

support is not covered in our survey. If this is so, then it is also possible that the true account provides a way for evidence to differentially support T and T*. But at the end of the subsection 4.3, we will provide a reason for thinking that this second possibility is very doubtful.

To begin with, note that any body of evidence statements whatever is logically consistent with T if and only if it is logically consistent with T*. For T and T* differ only in what they say about whether L is a law of nature. And if L is a law of nature, all that follows *logically* about the non-nomic states of affairs that can be reported by observation reports is that they are consistent with the truth of L.⁴

It does not follow automatically that every set of observation statements that is metaphysically compossible with T is also metaphysically compossible with T*. For, there could be metaphysically necessary truths relating the laws of nature to the non-nomic facts which are not *logically* necessary (i.e. formally necessary). If HS is true, then there are such necessary truths, and there are some sets of possible observation statements that are compossible with T but not compossible with T*. In particular, if HS is true, then the *complete* set of facts that could be reported by observation statements (i.e., Humean-base facts—see subsection 4.1, above) in a world where T is true are not all true in any world where T* is true.

However, if HS is false, then there are no such non-logical, metaphysically necessary truths that relate the laws and the non-nomic facts. So if HS is false, then any set of possible observation statements that are metaphysically compossible with T are also metaphysically compossible with T*, and vice versa. Thus, if either T or T* is true, then no set of actual Humean-base facts can settle the question of which one is true. For every possible world where T is true, there is a world that agrees with it on the Humean base, but in which T* is true.

The claims of the last paragraph are neither logically true nor self-evident. It is logically possible that *some but not all* possible worlds agree on the Humean base with some other world that has different laws, but that the actual world is not one of them. If that is so, then HS fails, but the Humean base of the actual world suffices to determine the actual laws. So it need not be impossible to use empirical evidence to determine, for example, that T rather than T* is true.

⁴ Here, we are using “follows logically” in a narrow sense, in which it does not include inferences that are underwritten by analytic truths, if such there be. So we allow for the possibility that there is some analysis of laws in terms of non-nomic facts, and we classify any such analysis as logically contingent. We do assume, however, that a logical consequence of the lawhood of L is the truth of L. This can be thought of as a stipulation about how we here use the term “logical,” so long as it is granted that it is necessary that all laws are true. Some philosophers do not grant this, e.g. Cartwright (1983) and Lange (2000).

But it is extremely implausible that HS fails in this way. Once we have granted that HS is false, we are committed to the view that laws of nature are an extra metaphysical ingredient of the world, over and above the Humean base. What constraints could the Humean base then place on the laws? It is plausible enough that the laws could not include propositions that are logically inconsistent with the Humean base, but beyond this, it is difficult to see why there should be any additional constraints. Given a specification of the Humean base H, and a possible set of laws L, if H and L are logically consistent then H represents one way the world could (*logically* could) evolve if it were governed by L. If the laws really are metaphysically independent of the totality of facts in the Humean base, then what is there to stop H and L both being realized in some possible world?

Motivated by this consideration, most of those who argue against HS endorse the claim that the actual world has Humean *Döppelgänger*s, worlds that agree with it on the Humean base but have different laws. Armstrong (1983, pp. 71-72), for example, argues that: given a specification of all particular, non-nomic facts in the actual world, any true universal regularity (relating instances of two universals) entailed by that specification could (metaphysically) either be a law or fail to be a law. Lange puts the point nicely:

That two possible worlds could have exactly the same non-nomic facts, but differ in which non-nomic facts state laws, should not really be surprising, considering that the laws in a given world are tied up not just with what in fact comes to pass there, but also with what would have happened there had certain circumstances unrealized in that world instead come to pass there. (Lange 2000, 51)

This expresses the common view among realists about laws who deny HS.

So we are safe in supposing that if HS is false, then T and T* are metaphysically consistent with exactly the same sets of evidence statements. That is, T and T* are *empirically equivalent* in a very strong sense: every possible world where one of these theories is true has a Humean *Döppelgänger* where the other is true. When two theories are empirically equivalent in this strong sense, let us say that they are *Humean equivalent*. Their Humean equivalence suggests that the two theories are evidentially underdetermined, i.e. that Premise 1 is true. But of course, this does not follow immediately. For one thing, empirical evidence together with some well-supported background beliefs might be consistent with T but not with T*, even if T and T* by themselves cannot be distinguished empirically. This is true, but then the underdetermination argument can be run again, letting the new T be the old T conjoined with the relevant background theory, and letting the new T* be the old T* conjoined with another background theory. The same reply can be given again, but eventually we are going to run out of well-confirmed background theories, and at that point the underdetermination argument will win.

The bottom line is that if T and T^* are Humean equivalent, then no matter how many veridical empirical evidence statements we accumulate, our evidence leaves open the possibility that we are in a world where T is true iff it leaves open the possibility that we are in a world where T^* is true.

This is not the end of the matter. There are other ways in which a body of evidence can favor one theory over another, besides being consistent with one and inconsistent with the other. Here we will review the prominent other ways that have been examined in the literature on evidence and confirmation, and show that if HS is false then none of them makes it possible for empirical evidence to differentiate between T and T^* .

4.3. *Ways of Assessing Degrees of Evidential Support*

(i) *Bayesian confirmation.*

It might be thought possible that after some evidence E has been taken into account, T may be better confirmed, in a Bayesian sense, than T^* : $\Pr(T|E) > \Pr(T^*|E)$. If this is so, then either the prior probability of T must be higher than that of T^* , or the likelihood of the evidence E given T is higher than that given T^* , or both. But if there is to be a more-than-hollow victory for T here, it must come from an inequality in the likelihoods. For suppose that the difference in the posterior probabilities is due solely to a difference in the priors. Then either there is some objective justification for assigning a higher prior to T —and so there must already be some other way of showing that T can be better supported than T^* even though they are empirically equivalent—or else the priors reflect only the subjective degrees of belief that we started out with. In the former case, the Bayesian method of blocking the inference from empirical equivalence to evidential underdetermination is not sufficient on its own; the real work is done by some other trick, which is used to justify assigning T a greater prior than T^* . In the latter case, we cannot really say anything more than that we find T more plausible than T^* . It will be our own subjective degrees of belief that do all the work in showing that T is better confirmed (in the Bayesian sense) than its rivals, rather than the empirical evidence.

So if this way of showing that evidence favors T over T^* works, it has to be that the likelihoods are different: $\Pr(E|T) > \Pr(E|T^*)$. These likelihoods must reflect objective probabilities of some sort; otherwise, once again, all the work in showing that T is favored is done by our subjective degrees of belief rather than considerations of empirical evidence. But T and T^* agree on everything except the question of what is a law and what is not. So they agree on relative frequencies, other statistical features of the Humean base, and even on single-case objective chances (which might not supervene on the Humean base). The needed relevant difference in the objective probabilities is

not there to be found. We conclude that on a Bayesian approach to evidential support, no evidence can favor either of T and T* over the other.

(ii) *Passing tests.*

It might be thought that with the discovery of E, T or T* might pass a more severe test than the other, and that for this reason, it will be better-supported by E. Suppose we construe “more severe test” in Mayo’s way. On Mayo’s view, a theory passes a *severe test* with evidence E just in case that theory fits E well, and has a low error probability. The error probability for T (for T*) is the probability that E would fit T (T*) at least as well as it does, given that T (T*) is false (Mayo (1996, pp. 180-181)). If a theory can be false in more than one way, then Mayo’s notion of severity requires that for the error probability to be low, it must be low on *each* of the ways in which the theory could be false (Mayo (1996, p. 195)). But the ways in which T could be false include T*, and the ways in which T* could be false include T. T and T* both give the same probability to E (see above). Hence, on one way in which T could fail—namely T*—the evidence would fit T just as well as it actually does, which is to say that the test is not severe at all. (And similarly, with T and T* reversed.) Hence, if one construes evidence as severe tests in Mayo’s sense, then there can be no evidence that supports T and T* differently.

Alternatively, we might construe “more severe test” in something like the way Glymour (1977a) does: T is better tested than T* by the same piece of evidence E if knowledge of E makes possible the computation of some quantity that T refers to, but this is not so for T*. But in the case at hand, neither T nor T* refers to quantities that the other does not, and neither provides any way of computing the value of any quantity from empirical data that the other does not. (Unless one construes the “It is a law that ...” operator as expressing a quantity, whose value is 1 for laws and 0 for non-laws; but even if such a “quantity” is allowed, in the case at hand we have no “computation” of it.) So severe tests in Glymour’s sense cannot be used to distinguish T from T*, either.

(iii) *Reliable procedures.*

One way to view evidential support does not focus directly on relations between theory and evidence, but rather on the method that is used to decide, in the light of evidence, what theory to accept, or the method used to modify beliefs in the light of new evidence.⁵ On this view, a belief is justified only to the extent that it was generated by the use of a procedure for updating beliefs that is reliable in some salient sense. So perhaps there is some reli-

⁵ See, e.g., Goldman (1979), Nozick (1981), and Kelly (1996).

able belief-updating procedure that can justify preferring T or T* to the other in the light of empirical evidence.

But it is implausible that any belief-revision process is reliable in the requisite sense. Say that a set T of pairwise-incompatible theories are *strongly empirically equivalent* with respect to the set W of possible worlds iff: for any pair of theories T_1, T_2 in T, for any W_1 in W such that T_1 is true in W_1 , there is a W_2 in W such that W_1 and W_2 have identical Humean bases and T_2 is true in W_2 , and similarly with T_1 and T_2 interchanged. It is overwhelmingly plausible that if HS is false, then the theories {T, T*} are strongly empirically equivalent with respect to the set of metaphysically possible worlds W_E that corresponds to our epistemic situation prior to commencing scientific investigation. (See above, subsection 4.2.) It is very easy to show that no belief-revision procedure can be reliable for choosing between theories that are strongly empirically equivalent. For any such procedure would have to give the same results in worlds where the stream of incoming evidence statements is the same, and this would be true of any two worlds with identical Humean bases. Since these two worlds differ on which of T and T* is true, any belief-revision procedure is destined to lead to the wrong conclusion in one of them. So no belief-revision procedure can be reliable with respect to the question of whether T or T* is true.

Reliability is always reliability across some particular range of possibilities. In some cases, an epistemic procedure can be usefully evaluated by considering how reliable it is across *nomologically* possible situations. It might be objected that the preceding argument demands too much, by demanding a stronger form of reliability than nomological reliability. But here, we are concerned with the reliability of a procedure for arriving at a hypothesis *about what the laws are*, so it would be inappropriate to take the relevant range of possible situations to be just the nomologically possible situations. To see why, note that if we took “reliable” here to mean “reliable across nomologically possible situations,” then if we had a procedure that *just happened* to get us the right answer about what the laws are given the available evidence (e.g., looking up the laws of nature in a certain textbook written by a guesser who happens to be lucky in the actual world), then this procedure would count as nomologically reliable under our actual circumstances. For, in every possible situation where we have the same evidence and the laws are exactly what they actually are, this procedure will give us the right answer. But this kind of reliability is trivial; all it amounts to is that if we restrict our attention to those possible circumstances in which X is the same as it is in the actual world, then a procedure that gets it right about X in the actual world will be reliable across all these possible circumstances. If we are concerned with beliefs about what the laws are, then in order to get an epistemologically interesting sense of reliability, we have to consider possible situations

in which the laws vary, while other relevant features of our circumstances remain the same. Any procedure we use that gives us the answer "T" is going to give us the wrong answer in all of the possible situations where T* is true, but our evidence is all the same. Assuming that HS is false, there are such possible situations. So, no procedure will be reliable, in any sense that is epistemologically relevant.

(iv) Explanatory Virtues and Other Epistemic Virtues

We have argued that the logical and probabilistic relations among the available observations and the theories T and T* cannot make for any difference in the evidential support for T and T*. We have also argued that considerations of the reliability of inferential procedures cannot be used to distinguish between the evidential support for T and T*. The remaining logical possibility is that either T or T* has some virtue V, not shared (to the same extent) by the other theory, that can make a theory better-supported by the available evidence than another theory even if that theory is Humean-equivalent to it. The most obvious candidate for V is the virtue of providing a good explanation of the known observation statements. The view that this virtue does make for evidential support has many defenders; Dretske (1977), for example, claims that the relation of evidential support can be identified with the converse of explanation, and many authors have argued that we are epistemically justified in adopting a new theory to the extent that it provides a good explanation of what we already take ourselves to know (Harman 1965; Lycan 1988). Other candidates for the virtue V include conservativeness (Quine 1951) and simplicity (Hempel 1966). Explanatory virtue, conservativeness and simplicity are all rather vague notions, and one would like a more precise specification of the virtue V. But the question of how to make V more precise will not affect the argument to follow, so we will neglect it. For definiteness, we will assume that V is the virtue of providing a good explanation of the known observation statements, but nothing in our argument will hinge on this.

The proposal, then, is that whenever two theories or hypotheses are Humean-equivalent, and therefore equivalent with respect to their logical and probabilistic relations to all actual and possible observation statements, the available evidence favors the one that has a greater measure of virtue V. Moreover, the proposal continues, this is a basic feature of the relation of evidential support, in the sense that it need not be justified by deriving it from more fundamental principles of evidential support, whether these are articulated in terms of logical relations, probabilities, or reliability.

If such a proposal is acceptable, then it blocks our Premise 1. But if the proposal is to help the nomic realist denier of HS to get around our epistemological argument for HS, then it will have to include, or be combined with, a principle connecting evidential support with epistemic justification. The

needed principle is: Other things being equal, if (i) we are in a situation where we have narrowed down our options to believing T, believing T*, and just believing their disjunction, and (ii) T and T* share all the same logical and probabilistic relations to the available observation statements, then we are epistemically justified in believing T (rather than believing T*, and rather than resting content believing the disjunction of T and T*) to the extent that T has a greater measure of virtue V than does T*. (This principle is ambiguous: It could be taken to mean that in the circumstances described, we are epistemically *justified to degree D* in believing that T is true, where D is proportionate to T's measure of virtue V; or it could be taken to mean that in those circumstances, we are epistemically justified—period—in *believing to degree D* that T is true, where D is proportionate to T's measure of virtue V. We will leave it ambiguous because we don't think it matters which way it is disambiguated; we reject it either way.) Without some such principle connecting evidential support to justified belief, the fact that virtue V confers evidential support on T does not show that we can be justified in believing T.

This principle entails the following one: Anyone who reasons in accord with policy PV is epistemically justified in so doing:

Policy PV: When you are in a situation where your open epistemic options are limited to believing T, believing T*, and resting content with believing their disjunction, believe T (or, believe T to a higher degree than T*) when T has a greater measure of virtue V than does T*.

The question we wish to pose now is: Why should adopting policy PV result in epistemically justified beliefs? Equivalently: why should adopting policy PV result in forming beliefs that are justified qua effort at arriving at accurate beliefs about the world?

Our claim that those two questions are equivalent involves an important assumption, so it is worth pausing over it for a moment. There are other species of justification besides *epistemic* justification of course, and beliefs can be justified in other ways. We consider whether an appeal to pragmatic justification can be of help to the deniers of HS below in section 5. It seems fair in this context to characterize epistemic justification as we have done, viz, as justification qua attempt to arrive at true beliefs. Two caveats: First, we certainly have epistemic interests in things other than true beliefs; we want relevant and useful beliefs, and there are situations in which a useful but false approximation is better than a true but unusable belief. But in the circumstances that policy PV covers, the only issue is whether to believe T, T*, or just their disjunction. Here, there is no real issue as to what beliefs would be relevant; we have already narrowed down the question the interests us to that of whether L is a law. Furthermore, there is no real choice between a useful approximation and a useless truth; neither T nor T* is an "approxi-

mation” to the truth stated by the other in any interesting sense. So in the case at hand, forming an epistemically justified belief does seem to amount to forming a belief that is justified from the point of view of our interest in getting true beliefs. Second, in speaking of “true” beliefs, we do not assume an inflationary correspondence theory of truth; in the case at hand, our epistemic interest is simply our interest in believing T in case L is a law and X, and our interest in believing T* in case L is true but not a law and X.

The denier of HS who tries to get around our argument by appealing to the fact that T has some virtue V in greater measure than does T* must, if her strategy is to succeed, also appeal to the principle that beliefs formed by following policy PV are thereby epistemically justified. Our question is *why this should be so*. By this question, we do not mean to demand some further epistemic justification for any beliefs formed by following policy PV; that would be an inappropriate demand, since the proposal under consideration is that V is a *fundamental* epistemic virtue. In other words, the policy PV is a fundamental principle of epistemic justification (or, an instance of such a fundamental principle, for the case of the particular theories T and T*). What we demand is not an epistemic justification of the policy PV itself in terms of more fundamental principles of epistemic justification, but rather an account of what it is about PV that makes it confer epistemic justification on the beliefs that are formed in accord with it. An analogy may be helpful here: Arguably, there are derivative rules of chess. For example, it is a derivative rule of chess that if the white king is in check by a black bishop, then on white’s next move she must remove her king from check if possible. This, and other derivative rules of chess, can be derived from, and thus justified in terms of, more basic rules of chess. The most basic rules of chess, however, cannot be derived or justified in this way. It still makes sense, however, to ask what it is about those most basic rules of chess that makes them rules of chess. The correct answer, presumably, will appeal to the history of the conventions that define the game of chess. Perhaps it will refer to historical documents, standard rule books, the decisions of governing bodies of chess organizations, and so on. So the correct answer goes outside the principles of correct chess play as such, and adverts to the nature of the institution of chess itself. Similarly, a correct answer to our question will not appeal to further principles of epistemic justification, but rather to the nature of epistemic justification. Our question can be rephrased thus: “What is it about policy PV that makes it an *epistemic* policy? (As opposed to, say, a convention of politeness, a moral rule, a prudential maxim, a rule of chess, or a made-up policy of no particular interest.)”

If a correct answer to this question is forthcoming, it is clear what it must be. What makes PV an epistemic policy (if that is what it is) is that its function is to promote the formation of accurate beliefs. Epistemic justification is

supposed to be something we want our beliefs to have because we seek (among other things) to represent the world accurately. That is what epistemic justification is for; we want to get it because we want to get accurate representations. A policy governing those belief-forming procedures that lead to epistemically justified beliefs, then, must be a policy the function or purpose of which is to promote the formation of true beliefs.

What does it mean to say that the function of a policy like PV is to promote the formation of true beliefs? One thing this could mean is that, as a matter of fact, conforming to the policy is a reliable means to the end of forming true beliefs. We have already seen reasons, however, to think that this could not be the case; any inferential procedure that enabled us to choose between T and T* could not be reliable in any interesting sense. The alternative seems to be that PV (or, some more general policy of which it is an instance) is designed for the purpose of promoting accurate beliefs. How can we understand what it is for a policy like PV to be designed for some purpose? There are two initially plausible possibilities: Either the policy was intentionally designed by human beings, who acted with the purpose of thereby making a tool for the promotion of accurate beliefs, or that policy (or perhaps, our conformity to that policy, or that policy's intuitive attractiveness to us, or our disposition to obey the policy) has a selection history in virtue of which it makes sense to say that the function of the policy is to promote accurate beliefs. (We here neglect other logical possibilities, such as functions deriving from divine design and from irreducible Aristotelian final causes, which seem out of step with that wonderful though nebulous entity, the modern scientific world-view. But see Plantinga (1993) for an alternative view.) Neither possibility is really tenable, however, as we will now argue.

It is most implausible that any policy like PV was ever intentionally designed by human beings (or ur-humans) with the explicit goal of promoting accurate beliefs. At any rate, there are no records of any such intentional design. Epistemologists who favor inference to the best explanation appeal to our intuitions, rather than to the inventions of past or present methodologists. Furthermore, it is doubtful that we even could intentionally design a policy like PV in order to advance our epistemic interests. Doing so would require some way of recognizing that obeying PV would be an effective means to the end of getting accurate beliefs. This, again, is something it is very doubtful we could ever do in a non-question-begging way.

That leaves only the possibility that policies like PV have a natural function, constituted by their selection history, which is to promote the goal of getting accurate representations of the world. Let us here construe "selection history" very broadly, so that it can include the history of the working of all sorts of feedback mechanisms, operating with a variety of kinds of positive and negative reinforcements. These mechanisms can include natural selection,

in which case the reinforcements are differential reproductive success; they might also include the process of conditioning that an individual organism undergoes in its dealing with the world, in which case the reinforcements are success or failure at getting desires fulfilled; they might also include the kind of artificial conditioning involved in education, and in social systems of rewards and sanctions.

Suppose now that one of $\{T, T^*\}$ is true, and that we have narrowed down our options to just these two theories. (This is an unrealistic idealization, but things only get worse for the explanationist denier of HS if we weaken it.) It is conceivable that feedback mechanisms might be sensitive to the differences between these theories, and so might hone our inferential practices in such a way that, given some body of evidence, we tend to accept one of these theories—say, T —rather than the other. But it is easy to see that no feedback mechanism could be sensitive to the difference between choosing to accept the *true* theory and choosing to accept the other. For, the things we do that get positively or negatively reinforced (such as forming and then acting on beliefs, or publicly expressing the conclusions of our inferences) as well as the reinforcements themselves (such as having offspring, dying, enjoying success at building effective mousetraps, getting As on exams, winning Nobel prizes) all supervene on the Humean base. Moreover, there is a principled reason for expecting this to be true in general: If a kind of reinforcement was not itself reliably detectable, then it could have no reliable influence on our behavior, which is to say that it could not function as a reinforcement at all.

Given that one of $\{T, T^*\}$ is true, any feedback mechanism would operate in exactly the same way no matter which one of them was true. So no such mechanism could operate in a way that is sensitive to the difference between choosing the true theory and choosing the false one. The upshot is that any inferential practice that was designed in such a way that it is working properly when we choose T over T^* on the basis of its explanatory virtues could not have been designed in order to help us develop an accurate representation of the world; it was designed “with some other goal in mind,” so to speak.

Therefore, there is no plausible way in which the policy PV can be understood as having the function of promoting accurate beliefs. But in that case, it is difficult to understand why it should be considered an epistemic policy at all. In other words, it is hard to see how compliance with the policy could be related to epistemic justification. Compliance with policies like PV might well constitute some other sort of justification, in which case V might justly be thought of as some kind of virtue (see our discussion of pragmatic virtues in the following section). Our claim here is simply that whatever kind of virtue V is, enjoyed to a greater degree by T than by T^* , it could not be an epistemic virtue, and the policy of preferring theories that possess it to theo-

ries that do not is not a policy whose function is to promote our epistemic goals.

5. Two Objections to Premise 2

(i) *Pragmatic virtues can favor T over T* even if the evidence does not.*

One important objection to Premise 2 insists that, even if there is no empirical evidence that favors T over T*, it might still be the case that the empirical evidence suggests that either T or T* is true, and pragmatic factors can tip the balance in favor of T. We would then be justified in believing T, and thence justified in rejecting T*. The resulting justification of T would thus not be *purely* empirical, for it would be partly pragmatic. But this seems to be a petty complaint; it might not even make sense to say that the justification of some belief is one hundred percent empirical, with no admixture of the pragmatic, the logical, the semantic, the mathematical, or anything else. So this objection needs to be taken seriously.

The notion of a *pragmatic virtue* is rather tricky, but for present purposes we can define it as a quality a theory has in virtue of which it is (other things being equal) good for us to accept, because doing so is a good way to advance some goal we have other than representing the world accurately. (In contrast, a quality of a theory that is good for us to accept because doing so is a good way to advance our goal of representing the world accurately can be called an *epistemic* virtue.) One quick response to this move is to point out that a denier of HS who wants to uphold both semantic and epistemological realism about laws of nature should not be happy about resorting to it. If the only reason we have for preferring T to T* is that doing so is a good way of advancing some goal *other than representing the world accurately*, then this reason does not seem sufficient to preserve epistemological realism about theories positing laws.

Some philosophers (e.g. Quineans) might not like this reply, for they doubt that there is any very clear distinction between pragmatic and epistemic virtues, and they might complain that our phrase “representing the world accurately” is too obscure to found this distinction upon. So we offer a second reply. Consider the kinds of things that are paradigmatic examples of pragmatic virtues. Some of these are qualities of a theory in virtue of which it is easy to use, and capable of being used in the sorts of material applications we want to make of science; simplicity, computational tractability, precision, and broadness of scope are examples. Others are qualities that make a theory advantageous for the future development of science; e.g., conservativeness, and the Kuhnian virtue of setting up a field of tractable problems for future research. Others are qualities that make the theory appealing to us, for reasons having to do with our psychology; supplying readily visualizable models, coherence with deeply-held beliefs, and the ability to provide what

seem like intellectually satisfying explanations are examples. None of these virtues is likely to favor T over its rival T* to a degree sufficient for preferring it over the latter, as we will now try to show.

Consider first the virtues that have to do with practical applications of science. T and T* make exactly the same set of predictions for observable phenomena, and each involves the very same degree of logical and computational complexity (assuming that the “it is a law that” operator does not play any crucial, explicit role in any computations). It might be argued that T is simpler than T*, but in fact they are just about equal by any reasonable criterion of logico-mathematical or ontological complexity. In a certain sense, T describes the world as a simpler place than does T*, because it posits fewer “cosmic coincidences,” but to appeal to simplicity in this way is to voice a metaphysical prejudice for the view that *nature* is simple, rather than a pragmatic preference for *theories* that are simpler and thus easier to work with. In short, as far as this family of pragmatic virtues go, there seems to be nothing to recommend T over T*.

Next, consider the pragmatic virtues that have to do with the future development of science. It is hard to imagine how T could be preferable to T* in this way. If T sets up a field of tractable problems that involve fitting empirical phenomena into a framework structured by the idea that L is a law, then T* sets up a field of tractable problems that involve fitting empirical phenomena into a framework structured by the idea that L is true and hence all empirical phenomena actually conform to it. It might be thought that considerations of conservativeness can favor T over its rivals. But if this is so, then it must be because we seek to conserve prior beliefs about what the laws of nature are. Beliefs like that are exactly what our argument says are unjustified, if HS is false. If the best that can be said in favor of a certain hypothesis about the (non-supervenient) laws is that *we already accept* a similar hypothesis, then this is a rather weak reply to the kind of critical argument we are developing; it is the acceptability of such beliefs in the first place that is being called into question. One further possibility that comes to mind is that if we regard L as a law of nature, then we are not likely to devote time and resources to explaining it, whereas we are likely to do this if we regard L as a nomologically contingent regularity. But this is not very plausible; we do seek to explain some things we take to be (plausible candidates for) laws of nature in terms of more fundamental laws, and we realize that we have to accept some regularities as brute.

Finally, the third sort of pragmatic virtue considered above is the kind of pragmatic virtue that is aptly lampooned by Hacking (1982) as the virtue of “making our minds feel better.” This is surely not the kind of consideration that an epistemological realist about laws should stake her case on. We conclude that an appeal to pragmatic virtues, whatever its advantages in other

local disputes in the philosophy of science, cannot block the nomic underdetermination argument if HS is false. (Incidentally, this goes partway toward replying to Complaint 4.)

(ii) *The contextualist gambit*

Advocates of epistemological contextualism have argued that in order to be justified in believing that P, one need not have evidence (or grounds, or reasons) that favor P over every alternative. It is sufficient to have evidence (grounds, reasons) that favor P over every *relevant* alternative, where which alternatives are relevant is determined by features of the context. So for example, in the context of a medical examination, in order for a physician to be justified in believing that her patient has the mumps, she must have evidence that favors that diagnosis over the alternative possibility that her patient has the common cold, but she need not have evidence that favors her diagnosis over the alternative possibility that she is the victim of a Cartesian demon and has no patient at all. However, if the same physician does epistemology in her spare time, then in other contexts, she might well be required to offer evidence (or grounds, or reasons) against the Cartesian-demon hypothesis in order to count as justified in her ordinary empirical beliefs about recent events. The difference is that in a medical context, a narrower range of possibilities count as relevant than in an epistemological context. This context-dependence is a fact about the semantics of our epistemic vocabulary, which can be confirmed by empirical evidence about our linguistic practices. Or so contextualists argue.⁶

Perhaps a contextualist maneuver can be used by a denier of HS in order to evade our underdetermination argument. The contextualist maneuver might run as follows: "In contexts where scientists are evaluating a law-positing theory such as T, which is well-supported according to the ordinary standards of scientific inference, alternatives such as T* which differ from T only in that they call one or more laws posited by T nomologically contingent, are not relevant alternatives. Hence, it is not necessary, in order to be justified in believing T, to have evidence that favors T over T*. So Premise 2 of our epistemological argument for HS is false. Nomic realism and the negation of HS can stand together." Let us call this line of thought *the contextualist gambit*.

In order for the contextualist gambit to work, it must be supplemented with some principle for determining which theories are irrelevant in which contexts. (Otherwise, the contextualist gambit is nothing more than an ad hoc evasion of our argument.) Note that the relevant principle cannot be that all alternatives to a given theory that differ from it only in that, where the given theory says it is a law that P, they say that it is true but nomologically

⁶ See, e.g., DeRose (1995), Lewis (1996).

contingent that P, are irrelevant. For if it were, then the only relevant possibilities would be those that posit only lawful regularities. For example, the theory presented in Newton's *Principia* posits some laws as well as some nomologically contingent regularities, such as the regularity that all the planets move around the sun in the same direction. If someone proposed a competing theory—call it *Principia+*—on which this regularity is a law of nature, we would not want to say that Newton's own theory can be justly neglected as an irrelevant alternative! So we need some more nuanced principle for deciding which scientific theories are relevant and which are irrelevant. This principle should be fine-tuned so that it does not count Newton's own theory as an irrelevant alternative to *Principia+*, but it does count the theory according to which all of Newton's claims are true except that his second law of motion is only a nomologically contingent regularity as an irrelevant alternative to Newton's own theory.

How might such a fine-tuned principle be formulated? Here is a stab:

CG: In any scientific context in which:

- (i) a number of theories, all of which are equivalent so far as their claims about the non-nomic are concerned, all enjoy some empirical support;
- (ii) one of these theories clearly does a better job than the others at the task of unifying a diverse range of phenomena under a small and simple set of basic laws, while minimizing the number of posited coincidences;

all of the theories except the one that does best, as judged by the criteria in (ii), are irrelevant alternatives to that theory.

The principle CG is pretty vague as it stands. But intuitively, it seems to get the job done in the case we just considered: The alternative to Newton's theory on which it is a law that all the planets go around in the same direction has a larger and less simple set of basic laws than Newton's theory; the alternative on which the so-called second law of motion is just a contingent regularity posits a vast cosmic coincidence not posited by Newton's own theory. So they both count as irrelevant alternatives to Newton's own theory, and Newton's theory does not count as an irrelevant alternative to either of them. This is a woefully imprecise argument. But perhaps CG could be made more precise, and perhaps some distinct principle could be formulated which gets the job done.

It might be objected that any such principle must be supported by an argument that shows that we have some good reason to believe that the favored theory is more likely to be true than any of the alternatives that this

principle rules out as irrelevant. And, it might be further objected, the argument of subsection 4.3 strongly suggests that no such argument is forthcoming. This objection would miss the point of the contextualist gambit. The contextualist proposal is that our word “justification” has a semantics that is context-dependent, in such a way that the extension of “justified beliefs” includes any belief that is favored by the available evidence over all of the alternatives that are *relevant in the context*. It doesn’t matter if we have no evidence that supports our belief that T is more likely to be true than T*, if T* is irrelevant in the context at hand. It is a feature of the semantics of our word “justified” that you don’t need such evidence in order to be justified in believing T rather than T*. To deny that S’s belief that P in context C is justified unless and until some reason can be given for thinking that the alternatives to P that are irrelevant in context C are less likely to be true than P is to beg the question against contextualism.

The contextualist maintains, in effect, that our practices of evaluating beliefs as justified or unjustified involve a certain way of drawing a distinction between relevant and irrelevant alternative possibilities. It is natural to ask whether these practices themselves can be given any kind of defense or vindication. It would perhaps be odd to ask for an *epistemological justification* of these practices, since these practices themselves are supposed to determine what counts as epistemologically justified in the first place. But there are still other ways in which one might try to defend, or rationalize, or vindicate these practices. For example, Craig (1999) argues that the primary use of our practices of epistemic appraisal is in distinguishing between informants we should count on and informants that we should not count on. If we counted nightmarish skeptical scenarios (featuring Cartesian demons, brains in vats, etc.) as relevant alternative possibilities, then we would never count any empirical beliefs about the external world as justified. In that case, we would never count any informant as a good one; the very distinction between informants we should count on and informants we should not count on would be swept away. So, if Craig is right about the use of our terms of epistemic appraisal, then we have a good practical reason for neglecting all skeptical-nightmare scenarios as irrelevant. This argument does not provide us with any epistemic justification for believing that we are not brains in vats, but it does provide a different kind of justification for one aspect of the context-sensitivity of our concept of epistemic justification (as the contextualists understand that concept), namely for the fact that in ordinary contexts, skeptical scenarios do not count as relevant alternatives. It is natural to think of this as a pragmatic justification: It shows that, given the practical purposes that our epistemic concepts are designed to serve, it is useful for us to use a concept of justification that works in the way the contextualists think our concept of justification works.

This kind of pragmatic justification is not available for the principle CG, however. As we argued above, the various features that make it practically useful to prefer one theory to another fail to favor T over T*, so no general principle that directs us to neglect theories like T* in favor of theories like T will serve any useful practical end. So a contextualist who appeals to CG (or to any other principle that does the job CG is designed to do) will be able to offer neither an epistemic nor a pragmatic justification of this principle. CG (or whatever other principle is used in its place) will be “rock bottom.” The contextualist will have to claim simply that CG correctly describes the way we actually use the concept of epistemic justification, and that there is no higher court of appeal in which it can, or needs to be, ratified. But CG directs us to accept as justified certain beliefs about contingent matters of fact that, as argued above, go far beyond the reach of any possible empirical evidence. Thus, the contextualist gambit attributes a very strong sort of dogmatism to our practices of evaluating empirical beliefs. Moreover, the claim that CG is simply descriptive of our practices of epistemic appraisal and that these practices neither require nor admit of any higher-order justification, amounts to endorsing that dogmatism.

Someone sympathetic to the contextualist gambit might reply that there is, after all, a certain amount of empirical evidence for the descriptive claim that we do, in fact, classify beliefs as justified or unjustified in accord with CG. For example, no one really doubts that Newton was justified in believing that his laws of motion were really laws, just because there was an alternative available according to which they were just nomologically contingent regularities. No one seriously thinks that Maxwell was not justified in believing that his four equations were laws of nature, just because there are alternative possibilities in which some of them are accidentally true. We grant these observations, but insist that there is another way to accommodate them: If one embraces HS, then one can consistently maintain that Newton’s justified beliefs about the regularities among non-nomic facts were sufficient to justify his belief that his laws of motion were laws, because what it is to be a law of nature is to be a regularity that plays a certain role within the whole system of non-nomic facts and regularities that make up our world. The “alternative possibility” that Newton’s so-called laws of motion are really just nomologically contingent regularities need not be neglected as an irrelevant possibility; it can be rejected as no possibility at all, in the light of other beliefs about the non-nomic facts. This way of accommodating the facts about our practices of evaluating beliefs about the laws as justified or unjustified comes with a price: It requires us to reject certain apparent metaphysical possibilities as mere illusions of possibility. But the alternative offered by the contextualist gambit comes with a steep price of its own: As we have just seen, it requires us to attribute a severe form of dogmatism to scientific

practice, and to endorse that dogmatism; in addition, it requires complicating the semantics of our epistemological vocabulary by adopting a principle like CG, which seems an ad hoc maneuver without any independent motivation. We suggest that recognizing limitations on our ability to detect metaphysical possibilities by a priori means is the cheaper price to pay.⁷

6. The Five Complaints About Underdetermination Arguments

Above, we considered five complaints about the use of underdetermination arguments to combat realism. Each of these five complaints effectively undermines some underdetermination arguments, in our view, but none of them touches the one we have been elaborating here. Complaints 1 and 2 allege that genuine cases of underdetermination between genuine rival theories are not as widespread as anti-realists often claim. We agree, but our argument nonetheless identifies a real case of underdetermination: Assuming that HS is false, T* is a genuine rival to T, and it is empirically equivalent to T in a very strong sense.

Complaint 3 alleges that underdetermination arguments often place unreasonable restrictions on what can count as evidence. In subsection 4.1 we argued that our underdetermination argument works with a conception of empirical evidence that is liberal enough to evade this objection. Our conception of empirical evidence does place certain restrictions on what can count on empirical evidence; in particular it restricts veridical evidence statements to propositions that belong to the Humean base. But this restriction is based only on two assumptions, which seem unobjectionable: First, empirical evidence statements must be reports of observations, and unreliable observations do not supply legitimate, veridical empirical evidence. Second, observations are things made by finite, empirical creatures such as ourselves, so every observation procedure must be executable within a finite spatiotemporal region. These are substantive assumptions, but we submit that to allow exceptions to them would be to stretch the concept of empirical evidence beyond its usefulness. Once these assumptions are granted, it follows immediately that every veridical evidence statement must belong to the Humean base, as we defined it in Part I.

Complaint 4 alleges that empirical equivalence does not automatically make for epistemological underdetermination. This is true, but as we have argued, the standard ways of getting around underdetermination—including appealing to pragmatic and explanatory virtues, and ignoring irrelevant alternatives—do not work in the case at hand.

⁷ This way of putting the matter can make it look as if what we are called to do here is to make a judgment about matters of fact; in section 6, we will describe a way of thinking about the matter according to which what we are called to do here is make a decision, or take a stand.

This leaves Complaint 5: The essential logic of anti-realist underdetermination arguments must be flawed, because it proves too much. For one thing, underdetermination arguments can be used to force a choice between skepticism about the external world and phenomenalism. For, if there is an external world that fails to supervene on our sense data, then it fails to supervene on all possible evidence concerning it. Hence, we cannot be justified in believing anything informative about it. So either the external world does supervene on our sense data, or else we must accept external-world skepticism. (Similar underdetermination arguments purport to show that we are forced to choose between behaviorism and skepticism about other minds, and between instrumentalism about unobservable theoretical entities and theoretical skepticism.) Since this infamous old argument has the same basic structure as our argument for HS, it might be thought that the two arguments stand or fall together, which is bad news for us. But this is simply wrong. The infamous old argument for phenomenalism rests on a false assumption about empirical evidence, namely that all empirical evidence statements consist of statements about our sense data. Since our argument is based on a much less restrictive view of what can count as an empirical evidence statement (see above), it can stand while the argument for phenomenalism falls. (Similar remarks apply to the parallel cases for behaviorism and instrumentalism.)

But there is more to Complaint 5 than this. For there is an argument for inductive skepticism that seems to work in the same way as our argument for HS. We framed our argument so that it required that in order for it to be possible to be justified in believing that L is a law, it must be the case that whether L is a law supervenes on the whole Humean base. But why should we have been so generous as to allow supervenience on *the whole* Humean base? It is hard to see how the empirical evidence statements that we do not know about could have any relevance for what we are now justified in believing. So it seems that if our reasoning is sound, then it ought to remain sound if we replace the thesis of HS with the thesis that what is a law and what is not supervenes on that part of the Humean base that we are aware of. The result would be an argument that we cannot be justified in believing any law-positing theory the truth of which is not settled by the empirical evidence now in hand. That amounts to inductive skepticism about laws of nature. Since there seems to be nothing in the argument specifically geared toward laws of nature, a more general inductive skepticism would seem to follow. (To put the point differently: If any ampliative inferences are ever justified, then why shouldn't ampliative inferences from empirical evidence statements to non-supervenient laws be justified? Have we given any reason to doubt the latter that is not a reason to doubt ampliative inferences as such?) If this is all right, then it is very bad news for us.

The answer to this worry is that our argument would not work just as well if we replaced HS with the thesis that lawhood supervenes on the part of the Humean base we are aware of. For one thing, our argument for Premise 1 allows that empirical evidence can favor one hypothesis over another even if it is consistent with both hypotheses; this favor might take the form of an inequality in likelihoods, a difference in the severity of the tests passed by the two hypotheses with the given evidence, or even a difference in explanatory virtues of the two hypotheses, given our background knowledge. Our arguments that none of these would enable a given body of evidence to favor T over T* turned on the fact that T and T* are strongly empirically equivalent, in the sense that if one of them is true, then all of the observable facts match those of some possible world where the other is true. But more importantly, our argument for Premise 2 was based not what we called the No-Alternatives-Left-Standing Argument, but rather on the principle that the norms of science do not warrant pointless epistemic risks. If we revised the argument in order to make it an argument for the supervenience of lawhood on the available empirical evidence, then the revised version of Premise 2 would have to read: "If semantic realism about laws is true, and no *currently available* empirical evidence *does* favor T or T* over the other, then we cannot be justified on empirical grounds in believing that T is true." We could not defend this premise by appealing to the principle that forbids pointless epistemic risks; a stronger principle would be needed, which forbids taking any epistemic risks at all. Such a stronger principle would, of course, already impose inductive skepticism. So, our argument could be turned into an argument for global inductive skepticism only by substituting, for a principle we do accept, another principle we do not accept, which already entails inductive skepticism on its own. Any argument can be turned into an argument for inductive skepticism by this method, so it is no particular weakness of ours.

Still, one might wonder whether our argument does not commit us to some supervenience claim that is stronger than HS, though weaker than global inductive skepticism. Perhaps if we replaced HS by the thesis that what is a law and what is not supervenes on that part of the Humean base that it is physically possible for us to observe, then the argument would still go through. Something like this might well be true. But before the case can be made, it must be spelled out just what it means for something to be "physically possible for us to observe." Perhaps it is physically possible for us to observe X just in case there is a physically possible world in which we observe X. But, given our liberal treatment of observation, it would probably turn out that we can observe just about any portion of the Humean base. For it is physically possible (consistent with the laws of physics) for us to have evolved on planets far away in space and time; it is physically possible for us to have evolved different sense modalities; and so on. Perhaps more con-

straints should be placed on what it is “physically possible for us to observe”; perhaps it should be required that it is consistent with the laws of physics plus certain details of our history, location and physiology. Then the question becomes, Which details of our history, location and physiology? If we build in too many such details, then we risk ending up restricting what it is possible for us to observe to what we will in fact eventually observe. In that case, our defense of HS would have to be modified by adopting a principle forbidding any epistemic risks that will not, in fact, ever come to grief against recalcitrant evidence. Since we have no way of telling in advance which epistemic risks will in fact come to grief, it would not be possible to implement this principle, so it would be unreasonable to impose it. So it won’t do to build into our notion of “physically possible for us to observe” all of the contingent details about our species. We will have to pick and choose among them. Some principle for picking and choosing will have to be selected, and this would be a perilous task.⁸ But perhaps it could be done; if so, then it will be possible to co-opt our argument for HS for the purpose of defending a stronger supervenience claim. Our point here is that it is far from obvious what that stronger supervenience claim would look like, and whether it would be disastrous to accept it.

Finally, one might point out that our argument doesn’t depend crucially on the fact that we have formulated HS as a thesis about laws; if successful, it shows that no hypothesis we can be scientifically justified in accepting can fail to supervene on the Humean base. Further, one might worry that there are some hypotheses that fail to supervene on the Humean base as we have characterized it, but which do seem to play legitimate roles in scientific theorizing. For this worry to be compelling, we need a concrete example. It might seem that such examples must be very hard to come by, since we have defined the Humean base as liberally as we have. Since any reliably detectable fact can belong to the base, the base can include facts about the spins of electrons, the sequences of amino acids in protein molecules, and even the rate at which the sun emits neutrinos.

But there might be some examples. Malament (1977)⁹ has shown that if general relativity theory is the correct account of space-time structure and gravitation, then there exist pairs of observationally indistinguishable, physically possible space-times that differ on important global properties, such as

⁸ Should we perhaps require that “it is physically possible for us to observe X” mean that it is consistent with the laws of physics together with all of the essential properties of the human species that humans one day observe X? In that case, we will need some criterion of deciding which properties of our species are the essential ones. And we would need to be confident enough in this criterion to appeal to it in formulating a normative principle concerning which epistemic risks are allowed in science. This is not an attractive avenue to take; our epistemology of science should not be beholden to current views about the essence of humanity.

⁹ Drawing in part on Glymour (1977b).

connectivity and the existence of a Cauchy surface. In particular, what Malament shows is that there are pairs of space-times, conforming to the laws of general relativity, such that for every future-directed time-like curve without future endpoint in the first space-time, there is a future-directed time-like curve without future endpoint in the second such that the regions swept out by the past light-cones of points “moving up” these curves are identical, even though the two space-times differ on some interesting global property, such as that of being simply-connected, or having a Cauchy surface. This result is relevant for our thesis because observationally indistinguishable space-times have the same Humean base if what a reliable observation procedure can deliver in a general relativistic world are facts about the metric field that are detectable by a possible observer and if what an observer can detect is exhausted by what goes on in her own past light-cone.¹⁰ Hence, if our reasoning is correct, the undetermined global properties are beyond the epistemic reach of science. This would be an interesting result, but it is far from clear whether it would be an advantage or a disadvantage of our position. It depends on what the most plausible view of the epistemic prospects of physical cosmology is, a complicated issue that we will not pursue here.

7. Concluding Remarks: HS as a Stance

We have defined our Humean base in a way motivated primarily by epistemological concerns, with an eye toward avoiding contentious metaphysical commitments. Even so, our argument has important metaphysical and ontological implications. As noted in Part I, the standard counterexamples to the more standard formulations of HS are also counterexamples to our thesis; also, the influential theories of laws that conflict with HS are ruled out by our thesis. So, if we are right, then laws of nature are not irreducible relations of necessitation between universals (*a la* Armstrong (1983), Dretske (1977), and Tooley (1977)), or *sui generis* modal principles that are basic ingredients of the world (*a la* Carroll (1994) and Maudlin (unpublished)), or anything else that can vary independently of the particular local details of the history of the universe, bound only by logical consistency. Lange’s picture of the laws as “sprinkled like powdered sugar over the doughy surface of the non-nomic facts”¹¹ must be rejected, as disappointing as that may be to sweet-toothed metaphysicians.

However, we think it is possible to understand our position otherwise than as a theory of “metaphysics” in the pejorative and pre-Kantian senses of

¹⁰ The latter assumption is intuitively plausible, but it is not above question. And it is *not* entailed by general relativity. The constraint equations of general relativity constrain data on a time-slice. So particles can “feel” the tug of gravitational sources that never move through their own past light-cones. Examples are provided by cosmological models with particle horizons and event horizons.

¹¹ Lange (2000, p. 51).

that term. Van Fraassen (2002) has recently argued that much philosophical work that has gone under the name of “metaphysics” has been guilty of false consciousness¹², viewing itself as arguing for a substantive view of the world when in fact it has been advocating a *stance*:

[A] philosophical position can consist in something other than a belief in what the world is like. We can, for example, take the empiricist’s attitude toward science rather than his or her beliefs about it as the more crucial characteristic. ... A philosophical position can consist in a stance (attitude, commitment, approach, a cluster of such—possibly including some propositional attitudes such as beliefs as well). Such a stance can of course be expressed, and may involve or presuppose some beliefs as well, but cannot be simply equated with having beliefs or making assertions about what there is. (van Fraassen 2002, 47-48)

We have presented HS as a thesis about the world. But we suspect that we are really engaging in what van Fraassen thinks of as advocating a stance, and we wish to try to clear ourselves of the charge of false consciousness.

Note that our version of HS places very few constraints on what the facts in the Humean base are really like, metaphysically and ontologically speaking. What is important about them is that they are reliably detectable, given the laws of nature. Nor do we place many constraints on what the laws of nature really are, metaphysically and ontologically speaking. What is important to us is that the laws and the facts in the Humean base are both subject to a global constraint that applies to them both simultaneously. Our motivation is not a deep conviction about the ultimate nature of the natural world. Our motivation is two-fold: First, we maintain (against van Fraassen) that discovering laws of nature (and discovering that they are laws of nature) is a legitimate goal of science, and that a lot of extremely interesting and successful science would have to be thrown out if this were denied (see Earman 2003). Second, we maintain (with van Fraassen) that science as such does not license pointless epistemic risks; every risky move taken in science must involve the genuine risk of having to back down eventually. Each of these is an expression of an attitude about science, or if you like, a value. We say, “These putative achievements of science are genuine achievements and should be counted as such,” and we say, “One of the valuable things about the activity of science, which we ought to strive to preserve, is the commitment to taking only worthy epistemic risks.” The upshot of our epistemological argument for HS is that remaining true to these two value judgments is incompatible with admitting the possibility of violations of HS.

It might seem paradoxical or outrageous to allow one’s value judgments to lead one to accept HS in this way. “Whether HS is true or not depends on how it is with the world, and the world does not have a great track record of honoring our values. If our values lead us to wish it were so, that doesn’t

¹² van Fraassen (2002, p. 50).

make it so.” What this objection overlooks is that the concept of a law of nature is a cognitive artifact¹³, and the current philosophical uncertainty about the structure and boundaries of this concept do not indicate only philosophical ignorance about objective facts, but also that the concept itself is still under negotiation. We recommend HS as a constraint on all future negotiations, and there is no impropriety in letting values influence a recommendation like that.¹⁴

References:

- Armstrong, D. M. (1983). *What is a Law of Nature?* Cambridge: Cambridge University Press.
- Carroll, John (1994). *Laws of Nature*. Cambridge: Cambridge University Press.
- Cartwright, Nancy (1983). *How the Laws of Physics Lie*. Oxford: Clarendon Press.
- Craig, Edward (1999). *Knowledge and the State of Nature: An Essay in Conceptual Synthesis*. Oxford: Clarendon Press.
- DeRose, Keith (1995). “Solving the Skeptical Problem,” *Philosophical Review*, 104, pp. 1-52.
- Dretske, Fred I. (1977). “Laws of Nature,” *Philosophy of Science*, 44, pp. 248-268.
- Earman, John (1993). “Underdetermination, Realism, and Reason,” in P. French, T. Uehling, and H. Wettstein (eds.), *Midwest Studies in Philosophy*, Vol. XVIII. South Bend: University of Notre Dame Press, pp. 19-38.
- _____ (2003). “Laws, Symmetry, and Symmetry Breaking; Invariance, Conservation Principles, and Objectivity,” Presidential Address, 2002 Biennial Meeting of the Philosophy of Science Association, Milwaukee, Wisconsin.
- Feyerabend, Paul K. (1962). “Explanation, Reduction and Empiricism,” in H. Feigl and G. Maxwell (eds.), *Minnesota Studies in the Philosophy of Science*, Volume III. Minneapolis: University of Minnesota Press, pp. 28-97.
- Giere, R. (1999). *Science without Laws*. Chicago: University of Chicago Press.
- Glymour, Clark (1977a). “The Epistemology of Geometry,” *Noûs* 11: 227-251.

¹³ “Artifact” in the sense of *something humans made*, rather than *illusion produced by an instrument*.

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- _____ (1977b). "Indistinguishable Space-Times and the Fundamental Group," in John Earman, Clark Glymour, and John Stachel (eds.) *Minnesota Studies in the Philosophy of Science*, Volume VIII. Minneapolis: University of Minnesota Press, 50-60.
- Goldman, A. (1979). "What Is Justified Belief," in G. Pappas (ed.) *Justification and Knowledge*, Dordrecht: Reidel, pp. 1-24.
- Hacking, I. (1982). "Experiment and Scientific Realism," *Philosophical Topics*, 13, pp. 71-88.
- Harman, G. (1965). "The Inference to the Best Explanation," *Philosophical Review*, 74, pp. 88-95.
- Hempel, C. (1966). *Philosophy of Natural Science*. Englewood Cliffs: Prentice-Hall.
- Kelly, K. (1996). *The Logic of Reliable Inquiry*. New York: Oxford University Press.
- Lange, M. (2000). *Natural Laws in Scientific Practice*. Oxford: Oxford University Press.
- Laudan, Larry and Leplin, Jeplin (1991): "Empirical Equivalence and Underdetermination," *Journal of Philosophy*, 88, pp. 449-472.
- Lewis, David (1986). *Philosophical Papers, Volume 2*. Oxford: Oxford University Press.
- _____ (1996). "Elusive Knowledge," *Australasian Journal of Philosophy* 74, pp. 549-567.
- Lycan, William G. (1988). *Judgement and Justification*. Cambridge: Cambridge University Press.
- Malament, David (1977). "Observationally Indistinguishable Space-Times," in John Earman, Clark Glymour, and John Stachel (eds.) *Minnesota Studies in the Philosophy of Science*, Volume VIII. Minneapolis: University of Minnesota Press, pp. 61-80.
- Maxwell, Grover (1962). "The Ontological Status of Theoretical Entities," in H. Feigl and G. Maxwell (eds.), *Minnesota Studies in the Philosophy of Science*, Volume III. Minneapolis: University of Minnesota Press, pp. 3-27.
- Maudlin, Tim (unpublished). "A Modest Proposal Concerning Laws, Counterfactuals, and Explanation."
- Mayo, Deborah G. (1996). *Error and the Growth of Experimental Knowledge*. Chicago: University of Chicago Press.
- Musgrave, Alan (1985). "Realism versus Constructive Empiricism," in P.M. Churchland and C.A. Hooker, *Images of Science: Essays on Realism and Empiricism*. Chicago: University of Chicago Press, pp. 197-221.
- Nozick, Robert (1981). *Philosophical Explanations*. Cambridge, Mass.: Harvard University Press.

- Plantinga, Alvin (1993). *Warrant and Proper Function*. Oxford: Oxford University Press.
- Quine, W. V. O. (1951). "Two Dogmas of Empiricism." *Philosophical Review*, 60, pp. 20-43.
- Rescher, Nicholas (1970). *Scientific Explanation*. New York: Free Press.
- Sellars, Wilfrid (1956). "Empiricism and the Philosophy of Mind," in H. Feigl and M. Scriven (eds.) *Minnesota Studies in the Philosophy of Science*, Volume I. Minneapolis: University of Minnesota Press, pp. 253-329.
- Tooley, Michael (1977). "The Nature of Laws," *Canadian Journal of Philosophy*, 7, pp. 667-698.
- van Fraassen, Bas C. (1985), "Empiricism in the Philosophy of Science," in P.M. Churchland and C.A. Hooker, *Images of Science: Essays on Realism and Empiricism*. Chicago, University of Chicago Press, pp. 245-308.
- _____ (1989). *Laws and Symmetry*. Oxford: Clarendon Press.
- _____ (2002). *The Empirical Stance*. New Haven: Yale University Press.