1

MUST EVIDENCE UNDERDETERMINE THEORY?

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According to the underdetermination thesis, all evidence necessarily underdetermines any scientific theory. Thus it is often argued that our agreement on the content of mature scientific theories must be due to social and other factors. In this chapter, I will draw on a long-standing tradition of criticism to argue that the underdetermination thesis is little more than speculation based on an impoverished account of induction. I will argue that a more careful look at accounts of induction does not support an assured underdetermination or the holism usually associated with it. Finally, I will urge that the display of observationally equivalent theories is a self-defeating strategy for supporting the underdetermination thesis. The very fact that observational equivalence can be demonstrated by arguments brief enough to be included in a journal article means that we cannot preclude the possibility that the theories are merely variant formulations of the same theory.

Some will be quite comfortable with these claims. Others will not. Recalling that science is a human activity, they will expect that, one way or another, social, cultural, political, ideological, and a myriad of other human engagements can become enmeshed in the final product of science. These factors may be peripheral in that they affect only the external conditions under which science is practiced or scientific questions will be pursued. Or they may become involved in the very content of science itself.[1] In the latter case, the real questions are not whether this happens, but when does it happen: how much do these factors determine the content of scientific theories; and how does it come about that they do? Given the very great complexity of scientific practice and the premium scientists themselves place on eradicating intrusion of external factors into the content of their theories, it would seem safest to answer these questions by direct study on a case-by-case basis. However, many scholars in science studies have proven unable to resist an easy answer to the question that assures a place for these factors in all mature sciences in advance of any direct study.[2] This easy answer will be the subject of my chapter, and I will call it "the gap argument":

> Step 1. No body of data or evidence, no matter how extensive, can determine the content of a scientific theory (underdetermination thesis). But there is universal agreement on the content of mature scientific theories. Therefore, there is a gap: at least a portion of the agreement cannot be explained by the import of evidence.
>
> Step 2. My favorite social, cultural, political, ideological, or other factor is able to account for what fills the gap. Therefore, my favorite factor accounts for a portion of the content of our mature scientific theories.

The second step of the argument has obvious problems. It is hard enough in particular cases to establish that these social or other factors played a role in deciding the content of a theory. If it happens, the scientists themselves are eager to hide it, so the point must be made in contradiction with the scientist's overt claims. Thus one might despair of finding a properly grounded way to enlarge the claim so that it covers all of science at once. Or at least it is daunting, if the claim is to say anything more than the banality that something other than data, we know not what, seems to be involved.

This second, dubious step of the argument is not my concern in this chapter. My concern is the first step. The underdetermination thesis has long been a truism in science studies, accepted and asserted with the same freedom that philosophers now routinely remark on the impotence of logic to provide us with a finite axiomatization of arithmetic. Yet the underdetermination thesis enjoys no such status in philosophy of science, where it is hotly debated. If you consulted a recently written companion to philosophy of science you would find the following synopsis: The thesis "is *at the very best* a highly speculative, unsubstantiated conjecture. Even if the thesis can be expressed intelligibly in an interesting form, there are no good reasons for thinking that it is true" (Newton-Smith 2001, 553, emphasis in original). My goal will be to display why I think this is a fair assessment. I do not intend to give a survey of the now expansive literature on the underdetermination thesis.[3] Rather, I plan to assemble two strands of criticism from it that I find especially cogent and put them in a strong and simple form.[4]

In the following section, I will state what I take the underdetermination thesis to assert and also what I believe it does not assert. I will also summarize three strategies that have been used to support the thesis: those based in Duhemian holism, Quinean holism and an argument based on the possibility of displaying pairs of observationally equivalent theories. This last argument is inductive, since the pairs displayed are supposed to illustrate in examples what is claimed to happen universally. In the subsequent section, I will lay out the first principal objection to the thesis: it depends essentially on an impoverished and oversimplified account of the nature of inductive inference. A brief survey of major accounts of inductive inference will show that they support virtually none of the properties of inductive inference presumed in the context of the underdetermination thesis. For example, in most accounts of induction, if two theories are observationally equivalent, their common observational consequences might still supply differing evidential support for the two theories. Then I will address the displaying of observationally equivalent theories as a support of the underdetermination thesis. I will suggest that such displays are self-defeating. If it is possible to establish that two theories are observationally equivalent in argumentation compact enough to figure in a journal article, they will be sufficiently close in theoretical structure that we cannot preclude the possibility that they are merely variant formulations of the same theory. The argument will depend upon a notion of physically superfluous structure. Many observationally equivalent theories differ on additional structures that plausibly represents nothing physical. I will also introduce the converse notion of gratuitous impoverishment; some artificial examples of observationally equivalent theories are generated by improperly depriving structures in one theory of physical significance.

The Underdetermination Thesis

The underdetermination thesis asserts that no body of data or evidence or observation can determine a scientific theory or hypothesis within a theory. This simple assertion requires clarification in two aspects. First, if the thesis is to be relevant to the use of data or evidence in science, the data must bear on theory by way of induction or confirmation relations, for this is how data or evidence bear on theory in science. Second, underdetermination is intended to convey the notion that the adoption of a particular theory cannot be based solely on evidential considerations. This may happen merely because the theory in question is one for which no strong evidence may be found. The underdetermination thesis is intended to have universal scope, so it must also apply to cases in which there does appear to be strong evidence for the theory. In both cases, underdetermination is explicated as the assured possibility of rival theories that are at least as well confirmed as the original theory by all possible data or evidence. The rival theories cannot be better confirmed, or else they would become our preferred choice. So the underdetermination of theory by data or evidence means that rival theories exist that are equally well confirmed by all possible evidence or data.

What the Thesis Does Not Assert

(Merely) De Facto Underdetermination The thesis does not merely assert that the data or evidence actually at hand *happens* to leave the theory underdetermined. That certainly is possible, especially when new theories are emerging. In other cases, sometimes real, sometimes contrived, it may be very hard to procure the requisite evidence. The underdetermination thesis is much stronger. It asserts that in all cases, no matter how long and ingeniously evidence collection may proceed, underdetermination will persist.

(Merely) Sporadic Underdetermination The thesis also does not merely assert that cases can arise, either contrived or natural, in which some part of a theory transcends evidential determination. Such cases clearly can arise. Lorentz insisted that there was an ether state of rest, even though Einstein's work in special relativity made it clear that no observation could determine which it was, of all the candidate inertial states of motion. The underdetermination thesis is much stronger; it asserts that all theories are beset with this problem.

Humean Underdetermination The thesis is also distinct from the most famous of all philosophical problems, Hume's problem of induction, or simply *the* problem of induction. This is the purported impossibility of providing a noncircular justification of induction. No matter how often we have seen that bread nourishes and fire burns, we cannot infer that they will continue to do so. We beg the question, Hume told us, if we ground our conclusion in the assertion that patterns in the past have continued into the future, for that assertion itself presumes the tenability of induction on patterns. If one accepts Hume's skepticism, no evidence from the past on bread or fire will determine their behavior in the future through justifiable inference. This form of underdetermination has been called "Humean underdetermination" (Laudan 1990, 322–24). It is distinct from the underdetermination of the underdetermination thesis since it denies the possibility of induction outright, as opposed to addressing a failure of the determining power of induction. This form of the thesis trivializes underdetermination. If one denies that induction is possible, a fortiori one must deny it any interesting properties, such as a power of unique determination.[5]

Underdetermination by Grue In Goodman's (1983) celebrated example, our past observations of green emeralds confirm the hypothesis "All emeralds are green" and also equally the hypothesis "All emeralds are grue," where "grue" means "green if examined prior to some future time *T* and blue otherwise." The problem of grue can be given a narrow or a broad reading. Neither coincides with the underdetermination thesis. In the narrow reading, grue reveals an underdetermination in the import of evidence within a particular class of syntactic theories of confirmation: those that allow an *A* that is a *B* to confirm that all *A*s are *B*, where we are free to substitute anything grammatically valid for *A* and *B*. This is a narrow problem for a particular class of theories, solved by various restrictions on the confirmation theory. Quine (1970b), for example, solved it by restricting *A* and *B* to natural kind terms. My own feeling is that the problem requires a deeper reappraisal of the nature of induction (Norton 2003). In the broader reading, the problem of "grue" is just the problem that any pattern can be projected in arbitrarily many ways, with none supposedly distinguished by the earlier history. This broader reading of the problem of "grue" just makes it a version of Humean underdetermination, which I have argued above is distinct from the underdetermination thesis.

Arguments for the Thesis

I will develop three arguments that have been used to justify the underdetermination thesis: local, global, and inductive. Before turning to them, I should mention one basis for the thesis that withstands little scrutiny: the thesis has become such a commonplace that it has entered that elusive body of knowledge labeled "what everyone knows." That is, it is justified by the fallacy, *argumentum ad populum.*[6]

Duhem's Local Justification An important justification for the underdetermination thesis is derived from Pierre Duhem's celebrated analysis of the logic of hypothesis testing in science (1954, chap. 6). Duhem remarked that a hypothesis cannot be tested in isolation. A hypothesis must be conjoined with others before observationally testable conclusions follow from it. Consider one of Duhem's examples (§6.2), the hypothesis that the vibration in a ray of polarized light is parallel to the plane of polarization. If an observable consequence is to be deduced from it, the hypothesis must be conjoined with many other hypotheses in geometric and wave optics, and even with the fact that the darkening of a photographic plate measures the intensity of light. But what if the observable consequence fails to obtain? We might discard the original hypothesis or we might protect it from refutation by discarding one of the other hypotheses. So Duhem was led to ask "Are certain postulates of physical theory incapable of being refuted?" (§6.8). The gap between Duhem's analysis and the underdetermination thesis is easy to close. If we can accommodate our theory to recalcitrant observations in many ways, then we have many distinct systems of hypotheses that accommodate the observations. Thus—and this is the step I shall return to below—the observations are powerless to decide between the systems. It is not clear whether Duhem himself would have wanted to follow the modern literature in taking this last step. He completed his discussion by describing how the good sense of physicists eventually would lead to a strong decision in favor of one of the competing theories (§6.10).

Quine's Global Justification The underdetermination thesis entered the modern literature in the conclusion of Quine's "Two Dogmas of Empiricism" as a quiet by-product of Quine's project of dismantling the venerable analytic-synthetic distinction and of the notion that meaningful statements are reducible by logical construction to those about immediate experience. In the resulting account, experience was allowed only to place looser constraints on our conceptual system. Quine's view[7] was most typically and most fully described in suggestive but elusive metaphors, such as this celebrated passage from "Two Dogmas" (1951, §6).

> The totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure

> mathematics and logic, is a man-made fabric which impinges on experience only at the edges. Or, to change the figure, total science is like a field of force whose boundary conditions are experience. A conflict with experience at the periphery occasions readjustments in the interior of the field. Truth values have to be redistributed over some of our statements. Reevaluation of some statements entails reevaluation of others, because of their logical interconnections—the logical laws being in turn simply certain further statements of the system, certain further elements of the field. Having reevaluated one statement we must reevaluate some others, which may be statements logically connected with the first or may be the statements of logical connections themselves. But the total field is so underdetermined by its boundary conditions, experience, that there is much latitude of choice as to what statements to reevaluate in the light of any single contrary experience. No particular experiences are linked with any particular statements in the interior of the field, except indirectly through considerations of equilibrium affecting the field as a whole.

Duhem and Quine's Justifications Compared There are obvious similarities between the Duhemian and Quinean paths to underdetermination. Both depend on the idea of some logical distance between hypothesis and experience that leaves the latter unable uniquely to fix the former. There are also significant differences that I have tried to capture with the labels "local" and "global." In the local approach, we generate observationally equivalent sets of hypotheses by adjusting individual hypotheses in some given system. The resulting equivalent systems will differ at most locally, that is, in just the hypotheses that were altered. In the Quinean approach, however, one posits ab initio that our total system of knowledge, including science, ordinary knowledge, and even mathematics and logic, is not fixed by experience, which can only impinge on it from the periphery.

When the full weight of this difference is felt, one sees that Duhem and Quine really have very different conceptions. Duhem's approach is narrowly focused on the confirmation of scientific hypotheses by scientists in actual scientific practice. The underdetermination Quine envisages permeates our entire conceptual system, extending to physical objects, Homer's gods, subatomic entities, and the abstract entities of mathematics (1951, §6). These radically different, alternative total systems envisaged by Quine are not the sort that could be generated by multiple applications of Duhemian adjustments. Indeed Quine finds such an extension implausible (1975, 313–15; see also Hoefer and Rosenberg 1994).

Exactly because Quine's underdetermination extends all the way through to the abstract entities of mathematics themselves, it is not clear that his version of underdetermination is properly presented as a limitation of the reach of evidence in the context of the establishment of scientific theories. Is the relation over which the underdetermination prevails the relation of confirmation of inductive inference? This relation is not usually invoked in fixing the abstract entities of mathematics. Once we fix the abstract entities of our conceptual system, might Quine's underdetermination no longer affect the determining power of evidence in a scientific theory, if ever it did? Quine's statements about underdetermination are so brief and metaphorical as to preclude answers and, in my view, even a decision as to the precise nature of his notion of underdetermination and whether it is well supported.

Intermediate positions are available. Longino (2002, 126–27; 1990, 40–48) argues that data has no evidential import, in the absence of background assumptions. While the context invoked is not as broad as that invoked by Quine, she has in mind something grander in scope than merely the other scientific hypotheses Duhem envisaged; the background assumptions may include substantive methodological claims, such as the assumption that correlations are attributable to common causes. That evidence does not uniquely determine theory is in turn traced to the lack of unique determination of these background assumptions.

Inductive Justification: Instances of Observationally Equivalent Theories

The underdetermination thesis amounts to the claim that for any theory, there is always a rival that is equally well supported by all possible evidence. The credibility of the thesis is often supported inductively by displaying such pairs of theories. They are usually labeled "empirically equivalent theories" or "observationally equivalent theories," and this is usually taken to mean, especially in the latter case, that the two theories have identical sets of observational consequences. Observational equivalence would appear to be a necessary condition for theories to be empirically equivalent; any difference in observational consequences could direct us to the possible evidence that would distinguish the theories. (I will urge below that it is not sufficient.)

This justification of the underdetermination thesis is inductive. We see a few examples of how a given theory may be paired with an observationally equivalent rival. We are to conclude that this will always be possible, which result is just the underdetermination thesis, as long as we judge that evidence cannot decide between observationally equivalent rivals.

There are not many examples. I find it helpful to group them with labels inspired by pearls: natural, cultured, and artificial.

Natural Pairs These are pairs that have arisen in the real development of science. The best-known examples are these. In Newton's original version of mechanics, there was an absolute state of rest which transcended observational specification. We form an observationally equivalent pair by taking versions of Newtonian mechanics with different absolute states of rest. Einstein's special theory of relativity and a suitably adjusted version of Lorentz's ether theory agree on all that can be observed about the slowing of moving clocks and shrinking of moving rods, yet they disagree on the theoretical account behind it.[8] In the 1920s, Cartan and Friedrichs showed how to construct a space-time theory that returned all the same motions as Newton's theory of gravitation. Yet their theory did not represent gravitation as a field, but as a curvature of space-time, modeled after Einstein's general theory of relativity. Finally it is a standard part of the lore of quantum mechanics, that two distinct quantum theories emerged in the 1920s, the matrix mechanics of Heisenberg, Born, and Jordan, and the wave mechanics of de Broglie and Schrödinger. They were soon shown to be equivalent.[9]

Another pair that is often mentioned in this context is the nonrelativistic quantum mechanics of particles and Bohm's (1952) hidden variable theory, although they are not strictly observationally equivalent. The former theory assigns no definite position to a quantum particle in most of its states; the position is brought to be by the act of measurement. The latter theory always assigns a position to the particle and it just becomes manifested on measurement. In both cases there is a probability distribution associated with the resulting positions and they will agree in all ordinary circumstances.[10]

Cultured Pairs These are instances of credible scientific theories contrived by philosophers specifically for philosophical ends. The best-known examples come from geometry. The geometry of a surface can be recovered from the behavior of measuring rods transported over its surface. Following an earlier, related example of Poincaré, Reichenbach described how one could conform almost any desired geometry to a given surface. If the behavior of transported rods did not conform to the desired geometry, one posited the existence of a universal force field whose sole effect was to distort the measuring rods away from the true lengths of the geometry to those that were observed. The same observables would be compatible with many suitably adjusted pairs of geometries/universal force fields. Glymour (1977) and Malament (1977) have described observationally indistinguishable space-times. An observer can receive light signals only from that portion of space-time encompassed by the observer's past light cone. Since space-times can be conceived in which this observable portion is only a small part of the total space-time, even for observations made over an infinite lifetime, a full specification of that observable portion fails to fix the entire space-time. Newton-Smith (2001, 535–36) reports another intriguing example. We routinely represent the continua of space and time by the real continuum. No measurement of finite precision can distinguish that continuum from one merely consisting of rational numbers.

Artificial Pairs These are instances of incredible scientific or quasi-scientific theories contrived by philosophers specifically for philosophical ends. In van Fraassen's constructive empiricism, we are enjoined to accept the observational consequences of a theory but not to assent to the truth of its theoretical claims. Kukla (1998, chap. 5) employs a variant of this view to generate empirically equivalent but logically incompatible pairs of theories. We start with any theory T and construct T' as that theory which asserts the truth of the observational consequences of T but the falsity of all of its theoretical claims (where van Fraassen merely withheld judgment). Kukla's proposal is immediately identifiable as the latest in a venerable lineage of tortured narratives that portray a world in which we would be mistaken to believe that things are really just the way they seem. Descartes wrote of a deceiving demon purposefully planting false beliefs. More recently we hear of the world created a few thousand years ago, complete with its ancient fossil record; or of ourselves created five minutes ago with a lifetime of memories in place; or the nightmarish fable of the evil scientist who has placed our brains in vats of nutrients where they are fed perfectly realistic sensations of a world that is not there. All these deliver revisionist theories, logically distinct from the standard theories from which they stem, but with identical observational consequences.

The Underdetermination Thesis Neglects the Literature in Induction and Confirmation

An Impoverished Hypothetico-Deductive View of Confirmation

This is probably the most common of all objections leveled against the underdetermination thesis and it has been raised in many guises. The concern is quite straightforward and quite fundamental. Since the underdetermination thesis makes a claim about inductive or confirmatory relations, one would expect the thesis to be supported by the very long tradition of work in induction and confirmation. Yet the expositions of the underdetermination thesis seem to make only superficial contact with this literature and the arguments for the thesis, notably the three sketched above, seem to depend entirely on a flawed account of the nature of induction. That is, they invoke an impoverished version of hypothetico-deductive confirmation, through which

> Evidence *E* confirms theory *T*, if *T*, possibly with auxiliaries, logically entails *E*.
>
> If two theories *T* and *T'* are each able logically to entail *E*, then *T* and *T'* are confirmed *equally* by *E*.

One sees immediately that this account of confirmation is troubled by an excessive permissiveness. The standard illustration of the concern is frivolous conjunction. If some theory *T* entails evidence *E*, then so does the strengthened theory *T'* = *T* & X, where X is any hypothesis, no matter how odd or frivolous. *T* & X then enjoys the same level of evidential support as *T*.

It is exactly this permissiveness that renders impoverished hypothetico-deductivism an uninteresting option in scientific practice and in the induction literature. Yet it is exactly this permissiveness that the arguments for the underdetermination thesis seek to exploit. For this impoverished version of hypothetico-deductive confirmation appears to be the relation routinely invoked in accounts of the underdetermination thesis. Empirically equivalent pairs of theories, in this literature, typically turn out to be pairs of theories that have the same observational consequences. They are automatically read as being equally confirmed by these consequences. Or we are to generate a rival theory by making compensating adjustments to the hypotheses of the original theory. The rival is equally confirmed by the common observations only if all that matters in the confirmation relation is that the two theories have identical observational consequences. In his discussion of empirical underdetermination in "On Empirically Equivalent Systems of the World," Quine (1975, 313) wrote: "The hypotheses [that scientists invent] are related to observation by a kind of one-way implication; namely, the events we observe are what a belief in the hypotheses would have led us to expect. These observable consequences of the hypotheses do not, conversely, imply the hypotheses. Surely there are alternative hypothetical substructures that would surface in the same observable ways." And surely we are intended to conclude that these alternative hypothetical substructures are worthy rivals to the originals.

What We Learn From the Literature in Induction and Confirmation

There are many accounts of induction and confirmation, and I will say a little more about them below. First, however, I want to collect a number of generalizations about what is to be found in that literature:

> Most accounts of induction and confirmation do not admit any simple argument that assures us that evidence must underdetermine theory.

The impoverished hypothetico-deductive confirmation relation is exceptional in admitting such an argument.[11] Most accounts do not give us the opposing result, however. Excepting cases in which a fairly rich framework is presumed, most accounts of induction do not assure us that evidence can determine theory. As a matter of principle, most accounts of confirmation leave open whether evidence determines or underdetermines theory.

> Most accounts of confirmation do not allow that theories with identical observational consequences are equally confirmed by those consequences.

One might wonder how we could ever have taken seriously an account of confirmation that tells us otherwise. Consider a revisionist geology in which the world is supposed created in exactly 4004 BC complete with its fossil record. Standard and revisionist geologies have the same observational consequences, but we surely do not think that the fossil record confirms a creation in 4004 BC just as strongly as the ancient earth of standard geology. In standard geology, detailed analysis points to precise datings for the standard geological eras. What in the fossil record points exactly to 4004 BC as the date of creation of revisionist geology and not, say, 8008 BC or last Tuesday? Indeed the fossil record would seem to strongly disconfirm the 4004 BC creation.

> Many accounts of confirmation do not restrict evidence to deductive consequences of hypotheses or theories.

As Laudan and Leplin (1991) illustrate, there are many cases of evidence that is not a logical consequence and logical consequences that are not confirmatory. The latter arises when both a hypothesis and all credible competitors entail the same consequence. Laudan and Leplin's example is the hypothesis *H* that regular reading of scripture induces puberty in young males and the consequence

E of the onset of puberty in some particular young males who read scripture (1991, 465).

> Many accounts of confirmation allow evidence to bear directly on individual hypotheses within a theory rather than merely supporting the entire theory holistically.

This is not to dispute Duhem's remark that hypotheses often need the support of auxiliary hypotheses if observational consequences are to be derived from them. It does dispute the idea that the obtaining of these consequences bears inductively solely on the conjunction of all the hypotheses. Consider the observation of spectral lines in sunlight corresponding to the helium spectrum. The inference from the presence of helium in the sun to the observed spectrum requires numerous additional assumptions about the optics of cameras and spectrographs. But the observed helium spectrum is strong evidence for the presence of helium in the sun and at best tangential evidence for the optical theory of the camera.

Three Families of Theories of Inductive Inference

The general conclusions above about accounts of induction derive from the existing literature of criticism of the underdetermination thesis and from a synoptic survey that I have developed of theories of induction (Norton 2005, 2003, §3). Below I sketch some general themes from the survey relevant to the underdetermination thesis. Accounts of induction can be grouped into three families, each driven by a basic principle with the variant forms of the theories generated by the need to remedy weaknesses in the principle. They are inductive generalization, hypothetical induction, and probabilistic induction.

Inductive Generalization The first family is based on the principle that an instance confirms its generalization. The simplest and best-known form is just enumerative induction: that some *A*s are *B* confirms all *A*s are *B*. The weakness of this account is the very limited reach of the evidence. Accounts that have sought to extend its reach include Hempel's satisfaction criterion of confirmation, which expands the logic used from syllogistic to first-order predicate logic, Mill's eliminative methods, and Glymour's bootstrap. The last two seek to extend the reach of inductive generalization by allowing insertion of new terms in the induction relation. Mill's methods allow us to reinterpret conditions of necessity and sufficiency as causal relations; Glymour's bootstrap allows us to employ hypotheses from the theory under test to convert evidence in an observation language into instances of hypotheses in the language of theory. These accounts are struggling with a restriction antithetical to underdetermination. The import of the evidence in this family is neither vague nor diffuse; it is precise and narrow—so narrow that much effort is expended in seeking to widen it. This narrowness of inductive generalization has allowed Glymour to emphasize that his version of the relation both contradicts holism, in that it allows evidence to support particular hypotheses within a theory, and contradicts the underdetermination thesis in that the relation allows evidence to accord different strengths of support to observationally equivalent theories (1980, chaps. 5, 9).

In inductive generalization, an instance confirms the generalization. It will often be the case that the generalization entails the instance; but there is no principle *simpliciter* that anything that entails the instance is confirmed by it. One readily finds cases in the family in which evidence confirms hypotheses that do not entail the evidence. Laudan and Leplin (1991, 461) report the simplest case, known since antiquity as "example": that this crow is black confirms that an as yet unexamined crow is also black. (Laudan and Leplin proceed to display many more examples of cases of evidence that are not consequences.) As a result, this family of accounts of confirmation tends to be immune to arguments for underdetermination that depend upon theories being confirmed by their deductive consequences.

Glymour's bootstrap allows us to use auxiliary hypotheses to infer from evidence to instances of the hypothesis to be confirmed. In an extension of this idea, the inference from evidence to hypothesis is rendered fully deductive by using suitably strong auxiliary hypotheses. The resulting approach goes under many names, including "demonstrative induction," "eliminative induction," and "Newtonian deduction from the phenomena." What makes the approach nontrivial is that we may find that we have already strong, independent evidence for the needed auxiliary hypotheses, so that the inference from evidence to hypothesis can be made deductively without taking any further inductive risk. Most striking is that, through this scheme, the evidence determines the hypothesis supported; it is deduced from it. I have described how this approach has been used in a number of cases. For example, it was used shortly after 1910 to force quantum discontinuity on the basis of the evidence of observed thermal radiation spectra in a climate in which researchers were disinclined to accept the result (Norton 1993, 2000).

Hypothetical Induction The second family of accounts of induction is based on the idea that a hypothesis is confirmed by its deductive consequences. This idea in its rawest form is just the impoverished account of hypothetico-deductive confirmation that lies behind the underdetermination thesis. Exactly because the idea is so weak as to admit rampant underdetermination, the account is never actually used in its raw form. It is always used in a form that tames its indiscriminateness by the addition of further requirements. These additional requirements generate the various accounts of induction in the family.

For example, for evidence *E* to confirm *H*, *H* must not only entail *E* but, in what I call "exclusionary accounts," it must also be shown that if *H* were false *E* would very likely not have obtained. Satisfying this additional condition automatically excludes the rivals envisaged in the underdetermination thesis. The additional condition proves fairly easy to secure. Mayo (1996) has described how it arises in the context of traditional statistical hypothesis testing and has shown how it can be extended to other cases through her notion of a severe test. Exclusionary accounts supply numerous examples that contradict the underdetermination thesis. In a controlled study, test and control groups are randomized so that the only systematic difference between them is that the test group only is administered the treatment. It then follows that any subsequent difference between the groups is evidence just for the hypothesis of the efficacy of the treatment and does not confirm any other rival hypothesis that proponents of the underdetermination thesis might envisage. There are other more indirect ways of securing the exclusionary requirement that if *H* were false then *E* would very likely not obtain. One of the most elegant arises in the "common cause" or "common origin" arguments (Salmon 1984, chap. 8; Janssen 2002).

Other natural augmentations of the impoverished hypothetico-deductive confirmation relation have the same effect of deflecting underdetermination. One popular approach is to demand that the hypothesis being confirmed is simple, with complicated rivals thereby precluded.[12] In another, "inference to the best explanation," the hypothesis supported by the evidence must also be the best explanation of the evidence (Lipton 1991). In another that I have called "reliabilism" (Norton 2003, §3), the hypothesis confirmed must in addition be produced by a method known to be reliable. The demand underpins the dismissal of what appear to be empirically adequate hypotheses as ad hoc. The complaint is that they were not generated by a reliable method, but by artful contrivance and are thus not candidates for evidential support.

Probabilistic Accounts The third family of theories of induction, probabilistic accounts, is based on the idea that scientists carry degrees of belief that are governed by a calculus and that the import of evidence propagates through the belief system by the rules of a calculus. The inspiration for the view came from the theory of stochastic processes—originally games of chance—for which the probability theory was created. The probability calculus remains the favored calculus and the dominant approach is called "Bayesian" after the theorem used centrally in propagating belief.

These probabilistic accounts are the least hospitable to holism and the underdetermination thesis. In them, the import of any item of evidence E throughout the entire system of belief can be traced, hypothesis by hypothesis. To see its import for any hypothesis H, one merely compares the posterior probability $P(H|E)$ with the prior probability $P(H)$. The dynamics of the resulting redistributions can contradict starkly with the natural suppositions of Duhemian holism. As Salmon (1975, §4) showed, one can have the following circumstance: A hypothesis H and auxiliary A entail some observation O that fails to obtain. While the falsity of O refutes the conjunction H & A, it may confirm H and may also confirm A! (See also Howson and Urbach, chap. 4.)

Standard within the lore of Bayesianism are limit theorems that describe how the accumulation of evidence forces convergence of belief onto the correct hypothesis. The theorems work very well in the contexts in which they apply, but they do require nontrivial assumptions to delimit those contexts. For example, if we have two hypotheses with the same observational consequences, no accumulation of observations can alter the distribution of belief between them. That is set in advance by the prior probability distribution.

Probabilistic accounts also give us the most general understanding of the possibility that hypotheses may be confirmed or disconfirmed by evidence that is not a deductive consequence. It arises whenever a hypothesis H is not probabilistically independent of the evidence E. That relation is expressed formally as the inequality of $P(H|E)$ and $P(H)$, which immediately asserts that the evidence E confirms or disconfirms H.

Probabilistic accounts, in their Bayesian formulation, provide the most extensive and versatile of all accounts of inductive inference. The view they provide of inductive relations is quite antithetical to both holism and a necessity of underdetermination of theory by evidence. Even if one thinks that they give a partial account of inductive relations that works well only in some domains, they still preclude any unqualified claims of holism and underdetermination.

The Display of Observationally Equivalent Theories Is Self-Defeating

Observationally Equivalent Theories as Variants of the Same Theory

We have seen that the possibility of displaying observationally equivalent theories is taken as evidence for the underdetermination thesis. One can of course generate observationally equivalent theories on the cheap by merely permuting terms. To use Quine's (1975, 319) example, we could merely switch the words "electron" and "molecule" in modern science. It is generally agreed, as Quine asserts, that the resulting pair of theories is not distinct in a sense relevant to the underdetermination thesis. They are merely notational variants of the same theory; they are merely the same theory dressed in different clothes. Quine (1975, 322) judges this to be so also in the case of Poincaré's alternative geometries. A concern frequently expressed in the literature on the underdetermination thesis is that all the commonly displayed examples of observationally equivalent theories are defective in some way like this and that this masks some deeper failing (see, for example, Newton-Smith 2001, 532–33). If just one or two examples turned out to be suspect, that would be a curiosity. But when all do, we have a phenomenon that needs to be explained. In the following, I will try to make this concern more precise and explain why we should expect all the examples developed in the literature to be suspect. I will urge that the difficulty with this inductive justification of the underdetermination thesis runs far deeper than the obvious concern that a powerful, universal conclusion is being grounded in very few instances. I will conclude that that the display of observationally equivalent theories is a self-defeating strategy for establishing the underdetermination thesis.

Every Well-Established Example Must Be Suspect

I shall seek to establish the following thesis:

> If it is possible for us to demonstrate the observational equivalence of two theories in a tractable argument, then they must be close enough in theoretical structure that we cannot preclude the possibility that they are variant formulations of the same theory.

Two aspects of the thesis must be stressed. First, it does not apply to all observationally equivalent theories. It is specifically restricted to those whose observational equivalence can be demonstrated in the sort of compact argumentation that can appear in a paper in the philosophy of science literature. That extra restriction is essential to the argument that establishes the thesis, for the ease of demonstration of observational equivalence will be used to ground a deeper similarity. Second, the thesis does *not* assert that the relevant observationally equivalent theories *must* be variant formulations. All that is asserted is that this is a strong possibility in every case in which we can demonstrate observational equivalence. But that doubt in the special cases is sufficient to defeat the inductive justification of the underdetermination thesis. For such an induction must proceed from instances known to be observationally equivalent, that is, those whose observational equivalence can be asserted in the literature with proper warrant. This induction now turns out to be suspect, for an induction from instances all of which are suspect is itself suspect.

A General Argument

The basic idea of the argument for the above thesis is that the manipulations needed to demonstrate observational equivalence are only likely to be tractable when the two theories are very close in structure, since the theoretical structures are our only practical way of circumscribing the observational consequences of a theory. The argument is developed in a series of steps:

1. If we are to demonstrate that two theories have identical observational consequences, then we must have a tractable description of their observational consequences. The description may be oblique or indirect. But it must be possible, otherwise, we would have no way to prove something about the observational consequences.

2. The description of the observational consequences of the first theory will most likely make essential use of the central theoretical terms of the first theory. It might happen that the observational consequences of the theory could be described compactly without recourse to these theoretical terms. But then we would have a very odd theory indeed—one whose entire observational content can be compactly described without its theoretical structure. Such an odd theory would be ripe for excision of superfluous theoretical structure!

3. For the same reason, the description of the observational consequences of the second theory will most likely make essential use of the central theoretical terms of the second theory.

4. If we are to be able to demonstrate observational equivalence of the two theories, the theoretical structures of the two theories are most likely very similar. While it is possible that they are radically different, if that were the case, we would most likely be unable to demonstrate the observational equivalence of the two theories. For the theoretical structures are what systematizes the two

sets of observational consequences and a tractable demonstration of observational equivalence must proceed by showing some sort of equivalence in these systematizing structures.

5. The two sets of theoretical structures may be interconvertible without loss; or they may not be. In the latter case, there would be additional structures present in one theory but not in the other. However any such additional structure will be unnecessary for the recovery of the observational consequences. That follows since the additional structure has no correlate in the other theory, yet the other theory has identical observational consequences. Thus any additional structures will be strong candidates for being superfluous, unphysical structures.

6. We conclude that pairs of theories that can be demonstrated to be observationally equivalent are very strong candidates for being variant formulations of the same theory.

Physically Superfluous Structure

Consider once again the natural and cultured examples of observationally equivalent theories listed above. (I will return to the artificial examples below.) They are not manifestly variant formulations of the same theory. Lorentz's ether theory is physically distinct from Einstein's special relativity; Lorentz's theory posits an ether state of rest, whereas Einstein's does not. Indeed, we saw above that observationally equivalent theories are not generally equally confirmed by their observational consequences. That conclusion would be unsustainable if observationally equivalent theories were mere variant formulations, for then they must be equally confirmed or disconfirmed by all evidence.

How are we to reconcile this? The answer is that step 5 of the argument above asks us to take a particular view of certain structures in pairs of observationally equivalent theories. In some cases, it can happen that the observationally equivalent theories are intertranslatable without loss. Matrix mechanics, in suitably rich formulation, can be converted into wave mechanics; and wave mechanics can be converted into matrix mechanics, without loss of structure in either direction. Fully intertranslatable theories are automatically strong candidates for being variant formulations of the same theory. More typically, we have a more complicated situation in which there is a loss of structure. Lorentz's theory can be converted into Einstein's by the simple expedient of asserting that the results of time and space measurements with slowed, moving clocks and shrunken, moving rods are the true measures of time and space. But we must also discard the ether state of rest, since it has no counterpart in Einstein's theory. According to Einstein's theory, it is superfluous structure corresponding to nothing in the physical world.

Step 5 of the above argument enjoins us to treat all such superfluous structure as representing nothing physical. And it does so with good reason. For we have two theories with a common core fully capable of returning all observations without the additional structures in question. Moreover, in some of the examples to be visited in a moment, the additional structure may lack determinate values. For example, as stressed by Einstein famously in 1905, the ether state of rest of Lorentz's theory could be any inertial state of motion. Because of the perfectly symmetrical entry of all inertial states of the motion in the observational consequences of Lorentz's theory, no observation can give the slightest preference to one inertial state over another. So its disposition is usually understood not to be an unknowable truth but a fiction.

Observationally Equivalent Theories Must Not Be Judged Variants of the Same Theory

I want to emphasize here that nothing compels us to accept that observationally equivalent theories are merely variant formulations of the same theory. As long as the theories are logically distinct, one can always insist that differences of theoretical structure correspond to something physical, even if the differences are opaque to observation or experiential test. This situation arose with Lorentz's electrodynamics, which is observationally equivalent to special relativistic electrodynamics. Long after Einstein's celebrated work of 1905, Lorentz insisted upon the correctness of his theory and its distinctness from Einstein's special relativity: his theory incorporated an ether state of rest not to be found in special relativity. The mainstream of physics found Lorentz's view implausible and decided that his ether state of rest was physically superfluous structure, so that the two theories were really just variants of the same theory.

Natural and Cultured Pairs Similar stories play out for the other examples. The possibility of discounting the physical reality of superfluous structure leaves the physical distinctness of the pairs in question. Among the natural pairs, consider the many observationally equivalent formulations of Newton's mechanics, each with a distinct inertial state of motion designated as the true, absolute state of rest. A Newtonian can insist that the formulations differ on a matter of fact: the true disposition of absolute rest. However the overwhelming modern consensus is that this is the wrong way to understand Newtonian mechanics. The absolute state of rest plays no role in the deduction of observational consequences and, because of its perfectly symmetrical entry into each theory, we have no basis in evidence to decide which inertial motion it coincides with. So it is routinely discarded as physically superfluous and all the formulations regarded as physically equivalent.

A similar analysis arises in the case of the standard formulation of Newtonian gravitation theory in terms of gravitational fields and the Cartan curved space-time formulation. One could insist that the two are distinct in that the former posits a background set of inertial motions not present in the latter. Following the model of general relativity, it is now standard to assume that this background inertial structure is physically superfluous. Indeed, in the special case of homogenous cosmologies, symmetry arguments essentially similar to those used in the case of Newton's absolute space and Lorentz's ether makes insistence on the physical reality of the background inertial structure unsustainable (see Norton 1995).

Consider quantum mechanics and Bohm's theory, setting aside again that they are not strictly observationally equivalent. Bohm's theory adds a definite, hidden position for the particle, always possessed by it at every moment, and our ignorance of its true value is expressed in a probability distribution. A Bohm theorist can insist that this is a physically real addition to the ontology, so that the Bohm theory is physically distinct from traditional quantum mechanics. A traditionalist can reply, however, that the particle position only becomes manifest at the moment of measurement, so that standard quantum mechanics can assert that the position and its probability distribution came to be at the moment of measurement. All a Bohm theorist has done is to project the position and associated probability distribution back in time to the initial set up—a superfluous addition since all the theoretical information needed to specify the actual measurement outcome is already fully encoded in the wave function. The debate over the proper attitude to take to the Bohm theory is ongoing, and I stress that I do not want to take a side here. All I want to point out is that there are readily available arguments for the physical superfluity of the additional structure posited by the Bohm theory, so the example is not an unequivocal instance for use in the induction to the underdetermination thesis.

Among the cultured pairs, the best known are the Poincaré-Reichenbach cases of multiple geometries. One could insist that Reichenbach's universal force field is a physically real field so that the different geometries are physically distinct. No one, neither Reichenbach nor his critics, wanted to take the differences seriously, physically, and all regard the multiple accounts as simply variant presentations of the same facts. Reichenbach thought the examples demonstrated the conventionality of geometry and his critics thought they demonstrated the artificiality of universal force fields (see Norton 1992, §5.2.4). I am less sure of what the correct analysis is for the remaining cultured examples, observationally equivalent space-times and continua with and without reals. However, exactly because the examples are highly contrived, I am not inclined to read much significance into them for the import of evidence in real science.[15] A finitist in geometry might well want to read the addition of reals to rational continua as physically superfluous structure, so the two representations of continua would be merely variant formulations of the same physical facts. Alternatively, since the rationals fully fix the real insofar as the reals can be defined as Dedekind cuts of rationals, the example might illustrate what I shall call below "gratuitous impoverishment."

Additional Structure Comes to Be Judged Physically Superfluous

While we can always insist that pairs of logically distinct theories represent distinct facts, the historical tendency has been to reinterpret additional structure not essential to deriving a theory's observational consequences as physically superfluous. Such was the case with Newton's absolute space, Lorentz's ether state of rest, and the background inertial structure of Newtonian gravitation theory. It is not the case with Bohm's theory, which seeks to reinstate a type of ontology discarded by traditional quantum theory.

Sometimes the recognition that two competing theories are really variants of the same physical facts can be a cathartic release from a protracted debate in which neither side seems able to display empirical evidence that decisively favors their view. In special relativity, for example, there was a long debate over how heat and temperature transform under the Lorentz transformation, with several incompatible candidates advanced. It was eventually resolved with the decision that the differences corresponded to nothing physical (see Liu 1994). There was a similar debate over the correct Lorentz transformation for the energy and momentum of stressed bodies in special relativity. It was resolved with the decision that the different transformations depended on differences in a definition hidden in the formalisms (see Janssen 1995).

Gratuitous Impoverishment and the Artificial Pairs

The natural and cultured pairs of observationally equivalent theories can be construed as variant formulations of the same physical theory if we regard the additional structures of one or other of the pair as physically superfluous. The artificial pairs are similar insofar as one member of the pair does accord physical significance to a structure whereas the other member of the pair does not. In these artificial cases, however, I want to urge that the second deprives this additional structure of physical significance improperly. That is, it represents what I shall call a "gratuitous impoverishment" of the second theory. Unlike the natural and cultured pairs, in artificial pairs the additional structure is essential to both theories' derivation of their observational consequences and is well confirmed by these observational consequences. Indeed, in most cases the additional structure is fixed by the observational evidence, whereas the superfluous structure of the natural and cultured pairs is usually not. If we discard this additional structure, we lose an essential part of the machinery of both theories and deny something for which we have good evidence—the academic equivalent of burying one's head in the sand.

For example, a revisionist geology must specify that the world was created in 4004 BC complete with the fossils that would have been formed if the earth had the ancient history presumed by traditional geology. So the theoretical structures of traditional geology are essential to the formation of the revisionist rival, but the ancient past of that geology is then denied. Yet our fossil record is surely strong evidence for that ancient past; it is certainly no evidence at all for a creation in 4004 BC. Similarly, our experiences of the ordinary world are surely strong evidence for the reality of the ordinary world; they are certainly no evidence at all for the supposition that our brains sit in vats of nutrients and are fed simulated experiences.

The same remarks apply to Kukla's algorithm for generating an observationally equivalent rival *T'* for any theory *T*: construct the theory *T'* with identical observational consequences as *T*, but with the negation of all of the theoretical claims of theory *T*. Kukla offers the procedure as an algorithm for generating empirically equivalent rivals for any nominated theory, so that it amounts to a proof of the underdetermination thesis. If we assume that the algorithm is applied to a well-formulated theory *T* whose theoretical structure is essential to *T*'s generation of observational consequences, then the construction of *T'* amounts to a gratuitous impoverishment of theory *T*, the denial of structures that are essential to the derivation of observational consequences and that are well confirmed by them.

Even if we set aside the concern of gratuitous impoverishment, the proof fails since it does not deliver an equally confirmed rival. That the world appears just as if theory *T* is true surely confirms more strongly the theoretical claims of *T* than it does their negations. Success of the proof requires us to suppose otherwise—that both the theoretical claims of *T* and their negations are equally confirmed in *every* case. Of the accounts of confirmation canvassed here only one, defective account can give us that.

The underdetermination thesis would prove to be something much less than we imagined if it merely asserts that the determining power of evidence is foiled by the utterly fantastic deception of these artificial pairs.

In conclusion, the underdetermination thesis offers a simple and profound restriction on the reach of evidence. Its appeal in science studies is strong, for it affords a simple argument that is both general and principled for the necessity of social and other factors in the determination of the content of scientific theories. Anything that easy and powerful seems too good to be true. And that is my principal claim: it is too good to be true; and the arguments in its favor have employed hasty stratagems that do not bear scrutiny. Our examinations of the nature of inductive inference have proven it to be sufficiently complicated to support no simple thesis that evidence must underdetermine theory—or, for that matter, that evidence must determine it. I see no easy escape. General claims on the relative weight of evidential and other factors in the determination of scientific theories will need to be supported by careful scrutiny of the particular cases at hand. There are no shortcuts.

NOTES

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1. Everyone has their own favorite examples of the latter. Mine is described in Norton (2000, 72).

2. For an entry into this literature, see Longino (2002, 124–28; 40–41), from whom I take the word "gap," and Laudan (1990, 321). Use of the gap argument need not always invoke the underdetermination thesis by name. Bloor (1982) in effect introduces it with the

claim, drawing on Hesse's work, that the world is unable to determine a classificatory system for use in science, but that the stability of our theoretical knowledge comes "entirely from the collective decisions of its creators and users" (280).

3. In addition to the works cited below, see Ben-Menachem (2001); Boyd (1973); Brown (1995); Earman (1993); Magnus (2003); and McMullin (1995).

4. Other strands of criticism include Grünbaum's (1959) challenge that there is no assurance that nontrivial, alternative auxiliary hypotheses will be available to protect any given hypothesis from refutation, and that sometimes they are assuredly not available. Laudan and Leplin (1991) and Leplin (1997) object that the empirical equivalence that underpins the thesis depends essentially on the false presumption of fixity of auxiliary hypotheses.

5. One might wonder if Quine, the wellspring of the modern underdetermination thesis, is really advocating this trivial version when he writes: "Naturally [physical theory] is underdetermined by past evidence; a future observation can conflict with it. Naturally it is underdetermined by past and future evidence combined, since some observable event that conflicts with it can happen to go unobserved" (1970a, 178–79).

6. Is Quine committing this fallacy when he continues the preceding quote: "Moreover many people will agree, far beyond all this, that physical theory is underdetermined even by all *possible* observation" (1970a, 179)? Or is it another snake intentionally hidden in his stylistic garden?

7. While this view is often properly labeled a version of "holism," Quine (1975, 313) insisted that the term be reserved for Duhem's view that hypotheses could not be tested against experience in isolation.

8. For details of the adjustments needed, see Janssen (1995).

9. I say "lore" here since the situation with the original theories and proofs was not so simple. See Muller (1997).

10. As Bohm pointed out in his original paper, since his theory does not collapse the particle's wave function unlike ordinary quantum mechanics, there remains an extremely small probability of some observable effect arising from the uncollapsed portion; it is "so overwhelmingly small that it may be compared to the probability that a tea kettle placed on a fire will freeze instead of boil" (Bohm 1952, n18). Curiously the exact same remark could be used to dismiss the practicality of observations that could distinguish a phenomenological thermodynamics from a kinetic theory of heat. Yet such observations proved feasible once we stopped looking at intractable cases likes kettles on fires. Other possible observable differences have been discussed. See Cushing (1994, 53–55).

11. The simple argument is (briefly) that compensating adjustments to different hypotheses in a theory yield an alternative theory with the same observational consequences and is equally confirmed by them.

12. I have argued that this requirement of simplicity is not the injection of a mysterious metaphysics of simplicity into induction relations. Rather it is merely a somewhat blunt way of adding in further factual conditions known to prevail in the case at hand and which are decisive in determining the import of the evidence (see Norton 2003, §3).

13. The space-time example is at the contrived extreme of the cultured examples since it is the observations made by just one observer that fail to determine the space-time; the totality of observations of all observers does not, although we have no way physically to

collect all the observations at one event. Also the cosmology presumed is somewhat impoverished in that it is assumed that an observer can only have evidence for the space-time structure at an event if the observer can receive a light signal directly from it. Standard cosmological theories are rich enough in additional theory to fix routinely the space-time structure at events in the cosmos outside our past light cone.

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