Experimental Equality and Under Determination

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Abstract: During this century, there emerged from the philosophical analysis of scientific theories two results invested with broad epistemological significance By the 1920s, it was widely supposed that a perfectly general proof was available for the thesis that there are always empirically equivalent rivals to any successful theory. Secondly, by the 1940s and 1950s, it was thought that - in large part because of empirical equivalence - theory choice was radically underdetermined by any conceivable evidence. Whole theories of knowledge (e.g. , W. V. Quine's) (Fraassen. 1976) have been constructed on the presumption that these results were sound; at the same time, fashionable recent repudiations of the epistemic project (e.g., Richard Rorty's) have been based on the assumption that these results are not only legitimate, but laden with broad implications for the theory of knowledge. In this paper, we reject both the supposition of empirical equivalence and the inference from it to underdetermination. Not only is there no general guarantee of the possibility of empiricalk equivalent rivals to a given theory, but empirical equivalence itself is a problematic notion without safe application. Moreover, the empirical equivalence of a group of rival theories, should it obtain, would not by itself establish that they are underdetermined by the evidence. One of a number of empiricalk equivalent theories may be uniquely preferable on evidentially probative grounds. Having, argued for these conclusions in the first two sections, respectively, we shall propose, in section III of this paper, a diagnosis of the difficult that has impeded their recognition, and extract in attendant, positive moral for the prospects of epistemology.
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1. Introduction

This paper is concerned with the venerable dispute between realism and antirealism. In this paper we discuss that the aim of science is to discover truths about observables and unobservable which is "scientific realism." On this view, a proper reconstruction of scientific reasoning interprets accepting a scientific theory as believing it true. Science confirms the truth of claims concerning entities too small to be observed, such as subatomic particles or genes, or entities that are unobservable despite being large, such as the electromagnetic field, component forces, and my superego, just as it confirms the truth of predictions about observables that haven't yet been observed and perhaps never will be.

In this paper it is discussed that there are many varieties of antirealism. An anti-realist influenced by early logical positivism might believe that any scientific claim apparently purporting to describe unobservable is actually made true or false by observable facts alone and so we may be justified in believing it true. Alternatively, instrumentalism holds that a scientific claim or theory apparently purporting to describe unobservable has no truth-value. Rather, it is merely a piece of conceptual machinery - a device, made of cognitively meaningless marks, for generating empirical predictions from observations. In contrast to these non-literal construal of scientific theories, van Fraassen's version of scientific antirealism takes theoretical claims literally that is, as purporting to describe unobservable. An antirealist might contend that empirical evidence can never justify our believing in the truth of claims about unobservable - perhaps because many theories, disagreeing in what they say about unobservable, are empirically equivalent. Bellarmine intended to remind Foscarini of this argument for antirealism by me ntoining epicycles and eccentrics- different arrangements of the crystalline spheres posited as carrying the planets around the Earth. The ancients knew that these different arrangements generate exactly the same observables. This is the sort of argument for antirealism that Laudan and Leplin criticize. Some realists argue that to decide among empirically equivalent theories, scientists employ criteria of theory choice such as those discussed in the present paper. Van Fraassen defends antirealism without contending that it would be irrational to believe in the truth of claims about unobservable. He denies the realist view that it is rationally compulsory for scientists to believe in the approximate truth of the theories that they accept. Moreover, according to van Fraassen, a scientist's belief in a theory's truth - insofar as it goes beyondher belief in its empirical adequacy and her commitment to using that theory to deal with new phenomena and to set her scientific agenda plays no role in her *scientific reasoning.* Belief in the theory may cause a scientist to come up with theoretical innovations that lead to greater empirical adequacy and may be important to a scientist's extra scientific pursuits. Van Fraassen's view that accepting a theory involves being committed to using the theory's resources in connection with future scientific tasks is taken even further by Sellars, who regards theoretical terms as observational terms in waiting. Furthermore, van Fraassen seems to believe that although rationality permits us to believe in the truth of various claims about unobservable, a genuinely empiricist attitude which isn't rationally compulsory would lead us to regard such belief as unworthy.

To forgo such belief and be agnostic about the reality of the unobservable posited by our best theories seems the fittest thing for the empiricist philosopher. Having looked at some varieties of antirealism, I would now like to examine briefly how realists and antirealists conceive of the structure of scientific theories. Hempel begins with the standard view of a scientific theory as consisting of a deductively closed, axiomatizable set of sentences forming three levels. At the bottom level arc reports of particular observable facts. The middle level consists of generalizations expressed entirely in the observational vocabulary Vu - the terms of which were well understood prior to any such theories being formulated. Interpretive sentences, a.k.a. "correspondence rules," "bridge principles", mediate inferences from sentences in Vb to sentences containing the theoretical vocabulary Vt, the terms of which apparently purport to refer to unobservable posited by the theory, and vice versa. The top level consists of theoretical laws mediating inferences between Vt sentences. On this layer-cake view, claims about unobservable predict and explain observations only indirectly: by entailing empirical generalizations that explain them directly. We establish the middle layer's uniformities by induction from our observations, logically prior to positing any unobservable. In short, only the cake's middle layer, not its top layer, touches the bottom. In contrast to Hempel's syntactic conception of scientific theories, this takes theories to be collocations of sentences, that is nonlinguistic entities representing all the possible worlds where the theory is true.

2. I Problems with Empirical Equivalence

A. Inducements to skepticism

The idea that theories can be empirically equivalent, that in fact there are indefinitely many equivalent alternatives to any theory, has wreaked havoc throughout twentieth-century philosophy It motivates many forms of relativism, both ontological and epistemological, by supplying apparently irremediable pluralisms of belief and practice. It animates epistemic skepticism by apparently underwriting the thesis of underdetermination. In general, the supposed ability to supply an empirically equivalent rival to any theory, however well supported or tested, has been assumed sufficient to undermine our confidence in that theory and to reduce our preference for it to a status epistemically weaker than warranted assent.

Specifically, this supposed ability is the cornerstone of arguments for the inscrutability of reference and the indeterminacy of translation, which together insulate the epistemic agent by challenging the objectivity of criticism on which an entire philosophical culture has depended. It has spawned prominent, contemporary versions of empiricism, including those of Quire, Bas van Fraassen, and J. D. Sneed, which belie the promise of science to deliver theoretical knowledge. It encourages conventionalism in geometry through Hans Reichenbach's invocation of universal forces. It questions the possibility of ordinary knowledge of other minds through the contrivance of the inverted spectrum. It blocks inductive generalization through the stratagem (it fashioning artificial universals to vie with natural kind, as, in Nelson Goodman's "grue" paradox, reducing the taws of apparent laws to mere entrenchment .

B. An argument against empirical equivalenece

We find the pervasiveness of this influence out of proportion to the conceptual credentials of the basic idea of empirical equivalence. by connecting three familiar and relatively uncontroversial theses, we can construct a simple argument to cast doubt on empirical equivalence in general, as a relation among scientific theories (and, by parity of reasoning, between any rival perspectives).

On the traditional view, theories are empirically equivalent just in case they have the same class of empirical, viz., observational, consequences (Laudan, 2000).A determination of empirical equivalence among theories therefore requires identifying their respective empirical consequence classes. As the empirical consequences of any statement are those of its logical consequences formulable in an observation language, these classes are (presumably proper) subsets of the logical consequence classes of theories. Central, therefore, to the standard notion of empirical equivalence are the notions of observational properties, the empirical consequences of a theory, and the logical consequences of a theory. We shall show that, when these concepts arc properly understood, the doctrine of empirical equivalence loses all significance for epistemology.

Cur three familiar theses are these:

Familiar thesis 1, the variability of the range of the observable (VRO):

Any circumscription of the range of observable phenomena is relative to the state of scientific knowledge and the technological resources available for observation and detection.

In particular, entities or processes originally introduced by theory frequently achieve observable or "empirical" status as experimental methods and instruments of detection improve. Such variability applies to any viable distinction between observational and theoretical language (Leplin, 1996).

Familiar thesis 2, the need for auxiliaries in prediction (NAP):

Theoretical hypotheses typically require supplementation by auxiliary or collateral information for the derivation of observable consequences.

While direct derivability of statements bearing evidentially on theory is not in principle precluded, auxiliaries are generally required for the derivation of epistemically significant results (Laudon et al, 2006).

Familiar thesis 3, the instability of auxiliary assumptions (IAA):

Auxiliary information providing premises for the derivation of observational consequences from theory is unstable in two respects: it is defeasible and it is augmentable.

Auxiliary assumptions once sufficiently secure to be used as premises frequently come subsequently to be rejected, and new auxiliaries permitting the derivation of additional observational consequences frequently become available.

Our argument against empirical equivalence now proceeds as follows. As VRO makes clear, the decision to locate a logical consequence of a theory outside its empirical consequence class (on the grounds of the former's nonobsercational status) is subject to change. That class may increase, coming to incorporate an ever greater proportion of the theory's total consequence class (Laudon et al, 2007). This result already shows that findings of empirical equivalence are not reliably projectable, since we cannot reliably anticipate which of a theory's now unobservable consequences maybecome observable. But the problems with empirical equivalence run deeper than the inconstancy of the boundary of the observable. For even if it were possible to circumscribe the range of the observable relative to a state of science, we shall see that it would still be impossible so to circumscribe the range of auxiliary information available for use in deriving observational consequences.

By NAP, a theory's empirical consequence class must be allowed to include statements deducible from the theory only with the help of auxiliaries. One can distinguish the broad from the narrow class of a theory's empirical consequences, where the narrow class contains only observational statements implied by the theory in isolation from other theories and hypotheses. But ANP shows that it is the broad class, containing as Ncell statements deducible only if the theory is conjoined with such auxiliaries, that matters epistemologicalk. Regardless whether holists are right in contending that the narrow class is empty, it is a class of little epistemic moment. It is by the complement of the narrow with respect to the broad that theories are primarily tested, and a characterization of empirical equivalence limited to the narrow would have no such epistemological consequences as we are concerned to contest.

It follows by IAA that, apart from shifts in observational status, a theory's empirical consequence class may increase through augmentations to the theory's total consequence class. As new auxiliary information becomes available, new empirical consequences derived with its help are added. Of course, conditionals connecting the auxiliary statements newly used to the empirical statements newly derived were already present among the theory's logical consequences. But the detached empirical statements are not present until the auxiliaries on which their deducibility depends become available. So long as we include within a theory's empirical consequence class statements derivable from the theory only via auxiliaries, so long as we construe that class broadly - and we have argued that it must be so construed to reflect the realities of theory testing - the theory's logical consequence class will be augmentable in virtue of containing the empirical consequence class as a subset. The empirical consequence class can also diminish, again by IAA, as the rejection of needed auxiliaries discontinues the derivability of some of its members (Pratt et al, 2005). Therefore, any determination of the empirical consequence class of a theory must be relativized to a particular state of science. We infer that empirical equivalence itself must be so relativized, and, accordingly, that any finding of empirical equivalence is both contextual and defensible.

C. Response to anticipated objections

The response we anticipate to our argument is a challenge to its assumption that empirical consequence classes must be identified for their equivalence to be established. Can there not be a general argument to show that classes must be the same independently of determining their membership? An obvious suggestion is that logically or conceptually equivalent theories must have the same consequence class, whatever that class is. As we do not question the empirical equivalence of logically equivalent theories, we ignore this suggestion and assume henceforth that theories whose empirical equivalence is at issue are logically and conceptually distinct.

One approach to constructing a general argument is to invoke the Lowenheim-Skolem Theorem. This theorem asserts that any firstorder, formal theory that has a model at all has a denumerable model. A standard proof uses terms involving individual constants indexed by the natural numbers as the domain of a model. But if the domain need only be a set of terms, it could just as well be any denumerable set whose members are proposed as the referents of those terms. So, in principle, such a theory has an infinite number of models.

Another approach is to construct an algorithm for generating empirical equivalents to a given physical theory , such as the lowenheim - skolem theorem fails to do for formal theories. For example, there exist instrumentalist algorithms for excising the theoretical terms of a theory without empirical loss. Whether such algorithms are in fact successful is rendered highly dubious by the premises of our argument. It is by no means clear that a theory's instrumentalized version can match its capacity for empirical commitment, once the role of auxiliaries in fixing such commitment and the variability of the range of the observable are acknowledged. At most a theory's instrumentalized version can be held empirically equivalent to it relative to a circumscription of the observable and a presumed or intended domain of application. But while theories fix their own intended interpretations, they do not fix their own domains of application, nor the resources for detection of entities they posit. Algorithmically excised references may pick out entities that become detectable. -New applications may arise with changes in collateral knowledge. Indeed, it is a measure of a theory's success when posited entities acquire a technological role, and applications for which the theory was not designed become possible (Cudd et al, 2003).

The only other approach we know of to establishing empirical equivalence without identifying empirical content is to argue from cases. We propose an example, inspired by van Fraassen's in The Scientific Image, as representative. Let TN be Newtonian theory. Let R be the hypothesis that the center of mass of the expanding universe is at rest in absolute space. Let V be the hypothesis that the center of mass of the universe has constant absolute velocity v. Consider the claim that TN + R is empirically equivalent to TN + V (Fraassen et al, 1997).

This claim is based on the common TN component of the theories. It is Newtonian theory itself that assures us that unaccelerated absolute motion has no empirical consequences of a kind encompassed by the theory; that is, no consequences within mechanics. We can therefore bring two lines of criticism against the claim of empirical equivalence: either there is some other kind of consequence not envisioned within mechanics, or the underlying Newtonian assurance is wrong. The question is whether conceivable developments in scientific knowledge enable us to distinguish the theories empirically on one of these bases.

We can construct an extension of TN+V which agrees with TN+R in not predicting basons . Let TN + W be TN + V plus the hypothesis that there is a velocity w such that basons appear if and only if and to the extent that v>w .

Then the absence of basons establishes only that v does not exceed w ; it does not require R. The presence of besons still refutes TN+R, but TN+R can be supplemented to allow basons ; perhaps they arise spontaneously . TN + R then lacks an explanation of bason production , such as TN + V provides. Something in the way of explanatory parity is achievable by adding to TN + R the hypothesis that what absolute motion produces is antibasons, which immediately annihilate basons. So the presence of basons is explained by the lack of absolute velocity. Still, TN + R does not explain the frequency of bason detection, as TN + V does. The observed frequency must simply be posited, as a constant determined by experiment, and this procedure is an admitted disadvantage relative to TN + V. But this comparison does not affect empirical equivalence.

The appeal to nonmechanical, differentiating phenomena can be defeated, because, if empirical equivalence holds within mechanics, it continues to hold for any extensions of mechanics in which the presence or absence of additional, nonmechanical phenomena is made to depend on the value of a mechanical property. This seems to be a general result. If theories T1 and T2 are equivalent with respect to properties P1, . . . , pn they have equivalent extensions for any enlarged class of proper ties p1 , … , pn , q1, … , qm ; where properties are functions of pl,..., pn. On the other hand, if q1 ,..., qm, are not functions of p they cannot be used to discriminate between T1 and T2 (James, 1996).

3. *II Underdetermination*

We have argued that the thesis that even empirically successful theory has empiricalk equivalent counterparts is precarious, at best. Bu, for now let us suspend our incredulity about empirical equivalence and suppose that the thesis is sound. We wish to explore in this section what, if anything, then follows from the existence of empirically equivalent theories for general epistemology.

A number of deep epistemic implications, rouehh collectable under the notion of "under-determination," have been alleged for empirical equivalence. For instance, it is typical of recent empiricism to hold that evidence bearing on a theory, however broad and supportive, is impotent to single out that theory for acceptance, because of the availability or possibility of equally supported rivals. Instrumentalists argue that the existence of theoretically noncommittal equivalents for theories positing unobservable entities establishes the epistemic impropriety of deep-structure theorizing, and with it the failure of scientific realism. Some pragmatists infer that only nonepistemic dimensions of appraisal are applicable to theories, and that, accordingly, theory endorsement is not exclusive nor, necessarily, even preferential. One may pick and choose freely among theories whatever works for the problems at hand, so that the distinction between theories and models is lost. In a phrase, the thesis of underdetermination, denying the possibility of adequate evidential warrant tior any theory, has become the epistemic corollary to the presumptively semantic thesis of empirical equivalence (Hacking, 1993).

Against these positions, we shall argue that underdetermination does not in general obtain, not even under conditions of empirical equivalence. As we have seen, empirical equivalence is chiefly seen as a thesis about the semantics of theories; underdetermination, by contrast, is a thesis about the epistemology of theories. It has been supposed that, if theories possess the same empirical consequences, then they will inevitably be equally well (or ill) supported by those instances. We shall contest this supposition and, with it, the reduction of evidential relations to semantic relations, on which it rests. We dispute the ability of semantic considerations to resolve epistemic issues. But even allowing the epistemic dimension we have discerned in empirical equivalence, we shall find that the relative degree of evidential support for theories is not fixed by their empirical equivalence (Hacking, 2003).

A. Evidential results that are not consequences

We begin by noting that instance, of a generalization may evidentially support one another, although they are not consequence, of one another. Previous sightings of black crows support the hypothesis that the next crow to he sighted will be black, although that hypothesis implies nothing about other crows. Supposing this evidential connection to be uncontrocersial, we ask why, then, in the case of universal statements it should be supposed that evidential support is limited to logical consequences. Is it that the evidential connection admitted to hold among singular statements is at best indirect, that it connects those statements only via a general statement that they instantiate? The thesis would then be that direct evidential support for a statement is limited to its logical consequences, and singular statements instantiating the same generalization support one another only in virtue of directly supporting that generalization. In short, where there appears to be evidential support for a statement, s, outside the range of s's logical consequences, such support is parasitic on support of a general statement, m, which entails s, from m's logical consequences (Boyd, 1979).

We believe this to be an unperspicacious way of accounting for what goes on in singular inference. Often the evidential link between singular statements is stronger than the support available for a general intermediary, whose identification can, in any case, prove elusive. But even if this account worked, it should be noted straightaway that allowing a statement to accrue indirect empirical support in this fashion already undermines the claim that statements are confirmable only by their empirical consequences. This result alone suffices to establish that the class of empirical consequences of a statement and the class of its prospective confirming instances are distinct.

We began this discussion with the hackneyed case of black crows in order to show that the possibility of inferences of even the most mundane sort (from particular-to-particular) depend upon demin, the thesis that evidential support accrues to a statement only via its positive instances. This claim becomes even clearer when one considers the manner in which real scientific theories garner empirical support. Consider, for instance, the theory of continental drift. It holds that every region of the earth's surface has occupied both latitudes and longitudes significantly different from those it now occupies.

A number of points are to be noted about such examples. First, by dating them we emphasize that they are not dismissable by invoking auxiliaries via which the evidence is derivable. One could not in the 1890s represent Thomson's results as consequences of electrical laws by making electroatomism an auxiliary. Despite Ludwig Boltzmann's pioneering work, statistical mechanics was too speculative in 1905 to qualify as an available auxiliary. Even taking an ahistorical view, it would be casuistical to represent evidence as a consequence of a hypothesis from which it is derivable via auxiliaries, if it is the auxiliaries rather than the hypothesis that really fuel the derivation. If a formal criterion is wanted, we may stipulate that a hypothesis be ineliminable from the derivation of what are to qualify as its consequences (Boyd, 1979).

Second, the more general theory via which the evidence supports a hypothesis of which it is not a consequence need not be very precise or specific. For example, the statistical mechanics that Brownian motion supported was more a program for interpreting phenomenological thermodynamics probabilistically than a developed theory. There can be good reason to believe that conceptually dissimilar hypotheses are related such that evidence for one supports the other, without possessing a well worked out or independently viable theory that connects them. Perhaps a theory that connected them has been discredited without the connection it effected being discredited. In this respect, nonconsequential evidence for general statements approximates the case of singular statements for which the inferential link proved elusive.

Third, we need not fear running afoul of familiar paradoxes of confirmation in taking evidence to confirm a hypothesis in virtue of supporting a more general statement that implies the hypothesis. The intuition that what increases our confidence in a statement thereby increases our confidence in what that statement entails is fundamentally sound. The difficulties that Carl Hempel, for example, extracted from his "special consequence condition" depend on a certain logical form for general laws and a simplistic criterion of confirmation - Nicod's criterion - which requires, in opposition to the position we have undertaken to defend, that all positive consequences be confirming. Much sophisticated reasoning in the natural sciences would be vitiated by restricting evidence relevant in assessing a theory to the entailments (via auxiliaries) of the theory. And any singular prediction would be so vitiated as well.

Finally, we need to acknowledge and take into account a subtlety of confirmation that might appear to challenge the force of nonconsequential evidence for our argument. There is an obvious way in which a statement not entailed by a theory can be evidence for the theory. The statement might imply another empirical statement that is entailed. Suppose, for example, that the theory entails a - perhaps indefinitely extendable - disjunction of which the statement is a disjunct. By implying a statement that is a consequence, the evidence, though not itself a consequence, fails to discriminate between the theory and any empirically equivalent theory. So showing that there can be evidence for a theory that is not a consequence of the theory does not suffice to show that empirically equivalent theories can be differentially supported.

B. Empirical consequences that are not evidential

Establishing that evidential results need not be consequences is already enough to block the inference from empirical equivalence to underdetermination. But it is instructive to make the converse point as well. Suppose a televangelist recommends regular reading of scripture to induce puberty in young males. As evidence for his hypothesis (H) that such readings are efficacious, he cites a longitudinal study of 1000 males in Lynchburg, Virginia, who from the age of seven years were forced to read scripture for nine Nears. Medical examinations after nine years definitively established that all the subjects were pubescent by age sixteen. The putatively evidential statements supplied by the examinations are positive instances of 1-1. But no one other than a resident of Lynchburg, or the like-minded, is likely to grant that the results support H.

This example has a self-serving aspect. That the televangelist has a pro-attitude toward H on grounds independent of the purported evidence he cites is already enough to make one wary; one need not recognize the flaws in the experimental design of the longitudinal study. In a case without this feature, a person hypothesizes that coffee is effective as a remedy for the common cold, having been convinced by finding that colds dissipate after several days of drinking coffee. The point here is that the very idea of experimental controls arises only because we recognize independently that empirical consequences need not be evidential; we recognize independently the need for additional conditions on evidence (Niven, 1990). No philosopher of science is willing to grant evidential status to a result e with respect to a hypothesis H just because e is a consequence of I{. That is the point of two centuries of debate over such issues as the independence of e, the purpose for which H was introduced, the additional uses to which H may he put, the relation of H to other theories , and so forth .

4. III Formal Constraints on Epistemology

If the identification of empirical consequences with evidential support is so implausible, how has it managed to gain such a foothold? We suggest that a more persuasne, less readily dispelled confusion is ultimately responsible. That confusion, as we have intimated, is to misunderstand the relationship between semantics and epistemology, bringing the largely technical and formal machinery of semantics improperly to bear on epistemic issues.

Specifically, we wish to reveal and challenge the widespread - if usually implicit - conviction that epistemic relations are reducible to semantic relations. It is commonly supposed either that truth and meaning conditions just tire justification conditions, or, at least, that they can be made to double as justification conditions (Niven, 1990). Either way, epistemology is made the poor relation of a family of interconnections among semantic, syntactic, and epistemic concepts, and is left to make do with tools handed down from semantics. It seems to us that distinctively epistemic issues are left unresolved by such a presumed reduction, and that epistemic theses depending on it - such as the underdetermination thesis - are wrongheaded. We will first explain and illustrate the confusion we have diagnosed, then trace the mistaken assimilation of support to empirical consequences to it.

The problem originates in foundationalist epistemology - especially in Descartes' image of a mathematically rigorous, deductive structure for knowledge - and thus is not confined to empiricism. If the evidential relation is deductive, the evidence on which a knowledge claim is based must bear semantic relations to the claim sufficient to permit the deduction.

Perhaps the best modern illustration is the attempt by the logical empiricists to demarcate science by semantic means. Both "verifiability" and "falsifiability," the prime concepts in terms of which scientific status has been delimited, are tools of semantics. Demarcation criteria proposed by the Vienna Circle or its positivist disciples, and by the Popperians, are alike in depending on semantic analysis and syntactic form of statements. What is required for classification as verifiable or falsifiable is basically that a statement satisfy constraints as to logical form and be couchable in observation language. It was assumed on all sides that such conditions suffice to identify the class of statements that are properly the objects of scientific inquiry.

One might think to defend the adequacy of semantic tools for the intended distinction by arguing that the relevant notion of "science" to be demarcated is not that of what passes muster by scientific standards, but merely that of what is up for grabs in scientific inquiry. After all, statements falsified by scientific inquiry are yet to be classed as scientific. But not only is a distinction between what qualifies as scientific and what does not basically epistemic; so too is a distinction between what is worthy of investigation or entertainable by scientific means and what is not. It is basically what we have already found it reasonable to believe that decides these things.

The demarcation problem of the logical empiricists arose as a variant on the logical positivists' program for distinguishing cognitive significance from emotive uses of language misleadingly given propositional form. Already at this level one may discern the assimilation of evidential to semantic relations. For the evaluative force of, e.g., ethical pronouncements that led positivists to disqualify them as genuine propositions is also present in epistemic pronouncements, and, derivatively, in science. Epistemology, it is now commonly recognized, is value- laden (Lycan, 1985). But science was the logical positivists' paradigm of cognitive significance, its propositional status, the ideal to which ethics, religion, and metaphysics futilely spired. If epistemology, and science in particular, was to be salvaged, then epistemic evaluation would have to rest on semantic relations as the only actual alternative to value-free empirical relations.

Of course, this does not make the semantic concept sufficient for the epistemic one, but further developments tended to elevate semantics and syntax over the notions of evidential warrant and rationality of belief used in other truth conditions for knowledge attributions. The incompleteness of the list of truth conditions was manifested in a curious asymmetry between the truth and evidence conditions. If the truth condition is not met, no bolstering of the evidence is sufficient for knowing. But inconclusive evidence that leaves open the possibility of error can be sufficient for knowing, if only, as a matter of fact (or happenstance), the world cooperates. In many celebrated paradigms of knowing, the evidence needed does not seem all that strong (Lycan, 1985). Thus, attention focused more on the truth condition than the evidence condition - more on semantic than epistemic issues. Ironicallc, the recent emergence of reliability theory, which re-emphasizes the justificatory component of knowledge in the tradition of Gettier's challenge, underscores the paucity and defeasibility of the evidence on which ordinary knowledge relies. Add to this asymmetry the success of Tarski's theory of truth in contrast to the sorry state of theories of evidential warrant, and one has the makings of a semantic and syntactic orientation for epistemology.

Given this orientation, it was natural to approach the problem of warranting a hypothesis - the problem of testing-by attending to statements that bear syntactic and semantic relations to the hypothesis - to its instantiations. At least this was natural for empirical generalizations, whose instantiations are empirical statements. This approach then created so many internal problems and tasks - Hempel's paradoxes of confirmation across logical relations; Goodman's problem of projectibility - that the possibility of warrant provided by statements syntactically and semantically independent of the hypothesis was lost sight of. Instantiations of theoretical hypotheses are not empirical, but an assimilation of support to consequences was somehow extrapolated for them, by supposing them in principle recastable in observational terms or, perhaps, by supposing their testability reducible to the testability of empirical generalizations. Such was the hold of the resulting picture, that the assimilation of support to consequences exceeded the confines of logical empiricism to capture the format of textbook characterizations of scientific method itself. Although written by a philosopher, Hempel (Landau, 2006) the following passage will strike every reader as stereotypical of standard accounts of empirical inquiry:

5. Results

Results that test a theory and results that are obtiainable as empirical consequences of the theory constitute partially nonoverlapping sets . being an empirical consequence of a theory is neither necessary nor sufficient to qualify a statement as providing evidential support for the theory . because of this , it is illegitimate to infer from the empirical equivalence of theories that they will fare equally in the face of any possible or conceivable evidence (Laudan, 2005). The thesis of underdetermination , at last in so far as it is founded on presumptions about the possibility of empirical equivalence for theories – or " systems of the world " – stands refuted .This ubiquitous assimilation of a theory's test cases to its logical consequences in an observation language, as we have argued above, wrongly ignores some of the more salient ways of testing theories. Worse, it generously greases the slide from empirical equivalence to underdetermination and epistemic parity. Ironically, the limitation of a statement's justification conditions to its truth conditions represents a striking break with the traditional empiricist project. Prior to the emergence of neopositivism in the 1920s, the general idea about theory testing and evaluation was that there was a range of "phenomena" for which any theory in a particular field was epistemically accountable. (In planetary astronomy, for example, these phenomena would be observations of positions of the planets, sun, and moon.) A theory's success or failure was measured against these phenomena, and decided by the theory's ability to give an account of them. A theory was, of course, responsible for its entailments, but it was held equally accountable for all the relevant, established phenomena, and could not evade this responsibility by failing to address them. For a Newton, a Ptolemy, or a Mach, "saving the phenomena" meant being able to explain all the salient facts in the relevant domain (Leplin, 1909). With the rise of neopositivism, the epistemic responsibilities of theories were radically reinterpreted. Theories became liable only for what they entailed. Failure to address relevant phenomena, or at least to be indirectly applicable to them, now emerges as a cheap way of protecting such success as a theory does achieve, rather than as a liability. Where empirical adequacy formerly meant the ability to explain and predict all the salient phenomena, it now requires only possession of none but true empirical consequences. Recall the passage lately quoted from van Fraassen. The radical character of the shift we are describing becomes immediately clear there when one notes his identification of "empirical adequacy," saying only true things about observable features of the world, and "saving the phenomena." Prior to our time, no one would have supposed, as does van Fraassen, that saving the phenomena amounts only to possessing an observable model. No one would have supposed, as does van Fraassen, that a theory is to be judged only against the correctness of its own observational commitments (be those commitments expressed in model-theoretic or propositional form), irrespective of the comprehensiveness of the class of such commitments, irrespective of the theory's applicability to problems independently raised. It is testimony to the pervasiveness of the thesis that epistemic assessment is reducible to semantics that van Fraassen's conflation of the hitherto quite disparate notions of empirical adequacy and saving the phenomena has gone unnoted.Much epistemology in our day is arbitrarily and unreasonably constrained by these developments. Our concluding, positive moral is that epistemic warrant unfettered by semantics has rich and varied sources yet to be exploited.

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