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ABSTRACT

In this paper, I explore Larry Laudan's and Jarrett Leplin's recent claim that empirically equivalent theories may be differentially confirmed. I show that their attempt to prise apart empirical equivalence and epistemic parity commits them to two principles of confirmation that Hempel demonstrated to be incompatible.

In a recent article, Larry Laudan and Jarrett Leplin launch a powerful attack on the underdetermination of theory by data thesis, concluding ambitiously, 'the thesis of underdetermination, at least in so far as it is founded on presumptions about the possibility of empirical equivalence for theories... stands refuted' (Laudan and Leplin [1991], p. 466). They maintain (a) that there is no reason to suppose all theories to possess genuine empirically equivalent rivals, and (b) even if two theories *are* empirically equivalent, it does not follow that they are equally well confirmed: there could be rational grounds for choosing between the two. It is this second part of their argument that I am concerned with here. Following Laudan and Leplin, I take the *empirical equivalence* (EE) of two theories to mean that their entailed observational consequences are identical. By the *epistemic parity* (EP) of two theories, I mean the relation of being equally confirmed by all available evidence, and therefore equi-credible. The key question is then this: is there a valid inference from EE to EP or not? Laudan and Leplin insist that the answer is 'no': empirically equivalent theories, they argue, may be differentially supported. However, I believe their central argument to be crucially flawed.

The standard realist manoeuvre for blocking the inference from EE to EP appeals to so-called 'extra-empirical virtues' of theories, such as simplicity, mathematical elegance, etc. A pair of empirically equivalent theories may differ on point of simplicity, and if simplicity is a guide to truth, that provides a rational basis for preferring one over the other. The standard instrumentalist response is to challenge the realist to justify his conviction that extra-empirical virtues *are* epistemically relevant, and it is hard to see how that challenge can be met. Laudan's and Leplin's central argument tries to drive a wedge between EE and EP *without* uncritical appeal to simplicity, etc., and is therefore not

susceptible to this standard rebuttal. To do this, they target a doctrine they call 'consequentialism', according to which 'hypotheses are to be tested exclusively by an exploration of the truth status of those empirically decidable statements which they entail' (ibid., p. 470), a doctrine which sustains the inference from EE to EP, but which Laudan and Leplin insist is false. By contrast, they argue, 'being an empirical consequence of a hypothesis is neither necessary nor sufficient for being evidentially relevant [to it]... these conclusions will establish that theories identical as to empirical consequences may be differentially supported' (ibid., pp. 460–1).

'Consequentialism' is clearly a close relative of hypothetico-deductivism, and Laudan and Leplin do well to highlight the relevance of assumptions about confirmation to the underdetermination issue. It is not implausible that much of the recent support for the underdetermination thesis has stemmed from implicit acceptance of a crude hypothetico-deductivism. For it is a simple fact about logical implication that a given set of sentences will be entailed by more than one mutually incompatible set, and combined with a hypothetico-deductive account of empirical support, this simple fact generates underdetermination. Laudan's and Leplin's arguments against consequentialism use a combination of historical examples and a priori considerations. Their historical examples purport to be instances of theories receiving positive evidential support from data they do not entail. If this situation can occur, it *seems* as if the door is open for prising apart EE and EP.

Laudan's and Leplin's first and most detailed example concerns the empirical support that accrued to the theory of continental drift (TC) in the 1950s and 1960s from studies of remnant magnetism. Call the entirety of this evidence 'e'. Now the continental drift theory, according to Laudan and Leplin, 'holds that every region of the earth's surface has occupied both latitudes and longitudes significantly difference from those it now occupies' (ibid., p. 461). It is 'committed' to two general hypotheses, H_1 and H_2, they continue, which are as follows:

> H_1: There has been significant climatic variation throughout the earth, the current climate of all regions differing from their climates in former times.
>
> H_2: The current alignment with the earth's magnetic pole of the magnetism of iron-bearing rock in any given region of the earth differs significantly from the alignment of the region's magnetic rocks from earlier periods.

I take it that describing TC as 'committed' to H_1 and H_2 means that TC *entails* both H_1 and H_2. Laudan and Leplin now argue as follows. The data from remnant magnetism, e, supports H_2. However, e supports H_1 as well, they insist, though H_1 obviously does *not* entail e. This occurs, they maintain, because by supporting H_2, e confirms the general drift theory TC, and therefore its consequence H_1. So H_1 is confirmed by e, though e is not a logical

consequence of it. To assess this argument, it helps to layout the logical structure of the confirmation relations Laudan and Leplin discern in more detail.

Premises:
(a) $TC \Rightarrow H_1 \wedge H_2$
(b) The following deductive relations obtain between TC, H_1, H_2 and e:
$TC \Rightarrow H_1$; $TC \Rightarrow H_2$; $H_1 \not\Rightarrow e$.
(c) *Ex hypothesi*, the following inductive relation obtains: e supports H_2.

Laudan and Leplin argue as follows:

(1) e supports H_2 (premise (c))
$\therefore$ (2) e supports TC, since $TC \Rightarrow H_2$
$\therefore$ (3) e supports H_1, since $TC \Rightarrow H_1$
$\therefore$ despite $H_1 \not\Rightarrow e$, e confirms H_1.

Consider the inference from line (1) to (2). Why should it be valid? One way of making it valid is to accept the general principle that if evidence confirms a hypothesis, it confirms anything that entails the hypothesis: evidence 'flows up' the entailment relation. Call this principle A. Now consider the inference from line (2) to (3). Why should it be valid? Its validity is assured if we accept the principle that if evidence confirms a hypothesis, it confirms anything that the hypothesis entails. Call this principle B. Notice that A is Hempel's 'converse consequence condition' (CCC) and B his 'special consequence condition' (SCC) (Hempel [1945]). Laudan and Leplin explicitly endorse the SCC, as did Hempel. They *appear* to use the CCC too, for the only reason they offer for taking e to support TC, i.e. for inferring (2) from (1), is that TC entails H_2 and e supports H_2. But in that case, the problem with their argument is clear. For Hempel demonstrated that one cannot, on pain of absurdity, maintain both the special and the converse consequence conditions as ubiquitous constraints on confirmation. The absurdity that results is this: every statement supports every other one. For consider any statement S. Every statement confirms itself, so S confirms S. By converse consequence, S confirms $[S \wedge T]$, since $[S \wedge T] \Rightarrow S$. By special consequence, S confirms T, since $[S \wedge T] \Rightarrow T$. This result holds for arbitrary T, and must therefore be regarded as a *reductio ad absurdum* of the simultaneous use of the special and converse consequence conditions.

Hempel's result shows that confirmation cannot *in general* be subject to the two consequence conditions. One could of course maintain that *in certain cases* evidence can flow up and down the entailment relation, without holding SCC and CCC to be generally valid. Examples can no doubt be found where our intuitions tell us very firmly that either or both of Hempel's consequence conditions are instantiated. I do not doubt Laudan's and Leplin's historical

claim that the case of continental drift and the other real scientific examples they allude to constitute examples of this sort. But the confirmation relations *cannot* then obtain in virtue of the purely logical relations between hypotheses and evidence. Hypothesis H_1 in the example above may indeed have been supported by e, but this cannot be due solely to the deductive relations that Laudan and Leplin describe, for Hempel's two incompatible conditions would then be being invoked. So Laudan and Leplin could escape my objection to their example above by adducing specific features of the historical situation, or appealing to the unificatory power of TC (for example) to explain why both of the consequence conditions can be instantiated in this case. But they do not do this. On the contrary, having argued their central point—that entailed consequences do not coincide with supporting data—they apply the point to the question of empirical equivalence, and claim to have discovered a *general exemplar* for showing how EE and EP may come apart. I quote the relevant passage in full.

> What, then, *is* the connexion ... between nonconsequential evidence and differential support of empirically equivalent theories? We propose the following exemplar. Theoretical hypotheses H_1 and H_2 are empirically equivalent but conceptually distinct. H_1, but not H_2, is derivable from a more general theory T, which also entails another hypothesis H. An empirical consequence e of H is obtained. e supports H and thereby T. Thus, e provides indirect evidential warrant for H_1, of which it is not a consequence, without affecting the credentials of H_2 (ibid., p. 464).

The structure of this argument is as follows:

Premises: (a) $T \Rightarrow H_1$; (b) $T \not\Rightarrow H_2$; (c) $T \Rightarrow H$; (d) $H \Rightarrow e$; (e) e supports H; (f) $H_1 \not\Rightarrow e$; (g) $H_2 \not\Rightarrow e$.

Argument: (1) e supports H (premise (e))
$\therefore$ (2) e supports T (since $T \Rightarrow H$—requires CCC)
$\therefore$ (3) e supports H_1 (since $T \Rightarrow H_1$—requires SCC)

Again, the inference from (1) to (2) requires the CCC, and the inference from (2) to (3) the SCC. Since we are now dealing with an abstract exemplar rather than a specific historical example, the defence available in the case of the theory of continental drift—that (1) implies (2) but *not* in virtue of purely logical relations—is obviously unavailable: all we are told are the logical relations between theory, hypotheses, and evidence. There is thus an important difference between Laudan's and Leplin's continental drift example and their 'general exemplar' for illustrating how EE and EP may diverge. In the former case, Laudan and Leplin might deny invoking the CCC—they might argue that TC is supported by e in virtue of TC entailing H_2, e supporting H_2 *and* some extraneous non-logical factors, such as the absence of any serious theoretical alternative to TC, or TC's unifying power, for example. But in the exemplar,

where *all* we are given are the deductive relations between T, H, H_1, H_2 and e, no such defence is available. I see no way of avoiding the conclusion that Laudan and Leplin are employing the SCC and the CCC together. And Hempel showed that one cannot do that.

Laudan's and Leplin's error is all the more remarkable since they explicitly say:

> 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 difficulties that Carl Hempel ... 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 ... defend, that all positive consequences be confirming (ibid., p. 463).

This is wrong on two counts. *One* of Hempel's difficulties—the paradox of the ravens—does indeed depend on the logical form he attributes to general laws, but the incompatibility of the two consequence conditions is entirely independent of the logical form of either hypothesis, evidence statement or theory. Secondly, Nicod's criterion does *not* take all positive consequences to be confirming. On the contrary, according to Nicod's criterion, the law $\forall x$ [$Rx \rightarrow Bx$] ('all ravens are black') is confirmed by the evidence statement [$Ra \wedge Ba$] ('this is a black raven'), which is *not* a logical consequence of the law, but a positive instance of it. Throughout their article, Laudan and Leplin confuse positive instances and logical consequences. They appear to take Hempel's 'instance-confirmation', developed in 'Studies in the Logic of Confirmation' to be a version of hypothetico-deductivism, which it clearly is not. (Hempel explicitly rejects a H-D account, early in that article).

Laudan's and Leplin's faith in Hempel's SCC is remarkable, given Carnap's and Salmon's demonstration that the principle does not generally hold on the Bayesian definition of relative confirmation (Carnap [1950], pp. 474–5; Salmon [1975]). Laudan's failure to appreciate this last point is evident from his treatment of the same issue in a 1995 paper. He offers an example structurally identical to the continental drift case discussed above. His claim is this: experimental evidence for Galileo's law of free fall also confirmed Kepler's laws of planetary motion, via confirming Newtonian mechanics. Remarkably, Laudan makes the further claim that this is a consequence of demanding that one's degrees of belief conform to the probability calculus: 'the empirical support flows upwards from observation to Galileo's law and then to Newton's theory and from there downwards to Kepler's laws. As I become more confident about Galileo's laws, I must—*on pain of violating the probability calculus*—become more confident about Kepler's laws—given Newtonian theory as a bridge' (Laudan [1995], p. 32, my emphasis). But this is

totally untrue. Conformity with the probability calculus does not require either SCC or CCC, let alone their conjunction. Whether or not you should become more confident about Galileo's laws, in Laudan's example, depends entirely on your assessment of the relevant priors and likelihoods, and is not guaranteed by the requirement of synchronic consistency alone.

To conclude, Laudan and Leplin do well to question the inference from EE to EP, and to highlight the relevance of confirmation-theoretic considerations to the underdetermination issue. A general explanation of how EE and EP may diverge would be a welcome philosophical result, but Laudan's and Leplin's attempt to provide it appeals to an incoherent notion of confirmation, as I have shown, and therefore fails.

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