Jonathan Vogel. *BOOK REVIEWS*

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*INFERENCE TO THE BEST EXPLANATION*. By PETER LIPTON. New York: Routledge, 1991. Pp. x, 194.

According to the positivist view of science, as articulated by Carl Hempel, the explanatory success of a theory can contribute to its confirmation. For, roughly, a theory explains what can be deduced from it, and the theory is confirmed if what can be deduced from it is observed to be true. These days, perhaps, fewer people would accord deducibility the all-important role given it by Hempel. Still, it is widely maintained that explanatory success provides a good reason to believe a theory, and, further, that a theory which gives better explanations than its competitors is thereby worthy of choice.

Peter Lipton's *Inference to the Best Explanation* is a sustained articulation and defense of this view. Lipton begins by distinguishing the task of describing our inferential practices from the project of justifying those practices in the face of some kind of skeptical challenge. The descriptive task is Lipton's primary concern, and he tries to spell out how explanatory considerations enter into the way we evaluate hypotheses. He helps to clear up some confusions about what inference to the best explanation really comes to, so that it becomes neither trivially true nor trivially false that we engage in such a practice (59–65). According to Lipton, we often gauge the *likeliness* of a theory's truth by its explanatory *loveliness*, but likeliness and loveliness are distinct notions (61). Simplicity and the unification of phenomena are said to make a hypothesis lovelier (71, 119), although these particular explanatory virtues are not discussed in detail.

Lipton does provide a very interesting general account of what explanation involves and how it works. In many cases, to explain is to identify a cause, and often (if not always) what we explain is why one thing is so rather than another. An explanation of P rather than Q needs to satisfy the "Difference Condition": "we must cite a causal difference between P and not-Q, consisting of a cause of P and the absence of a corresponding event in the history of not-Q" (43).

If explanation is contrastive, and inference is inference to an explanation, one will expect to find an important contrastive component in inference itself. According to Lipton, we proceed by noting a difference between otherwise similar situations. We then search for (or posit) a cause present in one case and absent in the other that would explain the contrast. The need to give a contrastive explanation governs the way we generate hypotheses and the way we test hypotheses by experiments. As an illustration of this model, Lipton offers Semelweis's investigation of child-bed fever. Semelweis sought to explain why the incidence of fever in one

hospital ward was higher than another. By controlling for one difference rather than another in the wards (that is, by creating a range of contrasts), Semelweis was able to infer that patients were being infected by the medical students who examined them. Thus, we have an example of successful inference to a contrastive explanation.[1]

Lipton attempts to meet various powerful objections to the view that we (properly) infer to the best explanation. One such criticism is that explanatory standards vary too much from person to person to serve as a guide for theory choice; another complaint is that it would be foolhardy or dogmatic to assume that explanatory adequacy and truth coincide. In answer to this second concern, one might argue that inference to the best explanation has earned our confidence through its success in the past, but Lipton is quite critical of this response. He also addresses other important topics, including Hempel's Paradox of the Ravens and the question whether successfully predicting data gives a theory more support than accommodating data already on hand.

It is to Lipton's credit that, throughout, he manages to be sensible yet controversial. I imagine that few people will agree with everything he says, and some will disagree strenuously with much of it. For my own part, I have some reservations concerning Lipton's formulation of the Difference Condition (quoted above). Suppose that it rains at noon, and we ask why it was raining at noon rather than sunny. A reasonable answer would be that a cold front came in a bit earlier, causing the rain. However, it is difficult to see how Lipton's account is supposed to apply in cases of this sort. First, we need to identify an event (state?) that is the negation (absence?) of its being sunny at noon. The Difference Condition then requires that there be no event in the actual history of the nonoccurrence of sunshine "corresponding to" to the coming of the cold front that caused the rain; this "corresponding" event would have brought about sunshine, had it not rained (see 44). But as I imagine the present example, conditions sufficient to cause sunshine actually do occur. These conditions would have produced sunshine, had the *additional* presence of the cold front not made it rain instead. We ought then to say either that some event "corresponding" to the cold front's arrival does actually occur, or that the notion of a nonactual "corresponding" event can be given no content here (making the Difference Condition vacuous in this case). Thus, a seemingly

[1]Lipton notes and discusses the close connection between his model and John Stuart Mill's Method of Difference. The account Lipton offers is meant to accommodate inference to unobserved causes and the selection of a single hypothesis where multiple differences are observed; to that extent it is superior to Mill's (114–16). Lipton also discusses in detail how his views on explanation and confirmation compare with Hempel's.

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impeccable contrastive explanation fails to meet the terms of Lipton's account.[2]

Although *Inference to the Best Explanation* may not have completely resolved the issues here, or in some other connections, it is nevertheless an excellent piece of philosophy. Lipton makes valuable contributions with respect to any number of important questions in epistemology and philosophy of science. Anyone with a more than passing interest in these fields will find his book indispensable.[3]

JONATHAN VOGEL

*Amherst College*

[2]Given that it actually was rainy rather than sunny, it will of course be true that before noon there was some event that caused the rain, and no event that caused sunshine. In that sense, there is no event in the history of the nonoccurrence of sunshine that "corresponds to" the arrival of the cold front. But if the Difference Condition required no more than this, it would be trivial: in general, if P occurs and Q doesn't, then no event actually stands in a causal relation to Q, although some event stands in a causal relation to P. Lipton has informed me that he no longer holds to the Difference Condition exactly as stated.

[3]I am grateful to Phillip Bricker and Peter Lipton for helpful conversations.

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*KNOWLEDGE IN PERSPECTIVE: SELECTED ESSAYS IN EPISTEMOLOGY*. By ERNEST SOSA. Cambridge: Cambridge University Press, 1991. Pp. xi, 298.

Ernest Sosa's articles over the last two decades have helped define the current state of epistemology. *Knowledge in Perspective* collects a number of these articles, affording a much needed overview of Sosa's accomplishments. Despite the span of time and the diversity of occasions for which these pieces were written, there is an impressive unity to Sosa's epistemological thought. A brief report of his views is bound to strip them of their greatest strengths, their subtlety and attunement to the complexity of epistemological phenomena. Nevertheless, I would describe Sosa as at bottom a reliabilist who takes coherentism and perspectivism seriously. I will begin with Sosa's criticisms of traditional foundationalism and coherentism and then turn to his perspectivist version of reliabilism.

Sosa's main complaint about traditional foundationalism is that it multiplies principles. A foundationalist like Chisholm or Pollock offers a list of principles—of the form "If conditions *C* obtain for *S*, then *S* is justified in