R E V I E W

PETER LIPTON
Inference to the Best Explanation
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The primary goal of this book is to provide a partial description of our non-demonstrative inferences. Lipton calls his description 'Inference to the Best Explanation' (IBE). Though many philosophers tout IBE as vital to both scientific and commonsense reasoning, articulations of just what IBE amounts to have been sparse. This excellent book endeavors to map the essential features of that philosophical terrain. Embracing a causal model of explanation, Lipton notes that just which parts of a causal story are explanatory will depend on our interests, namely, which particular contrast between fact and foil we desire to explain. Different foils can call for different explanations, which will pick out different elements of the given fact's causal history. Just how do we pick out the explanatory element? Integrating Mill's Method of Difference, Lipton proposes an answer: 'To explain why P rather than Q, 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 case of not-Q' (p. 42). Readily conceding that this account of explanation neither covers all explanations nor exhausts what is involved in the many cases where it does apply, Lipton further develops his description of inference. He emphasizes a distinction between the likeliest explanation, which is the 'most warranted,' and the loveliest, which, if true, would 'provide the most understanding' (p. 59). Insofar as non-demonstrative inference to E is an inference that E is likely, equating 'best' with 'likeliest' will provide an uninformative description of inference. For Lipton, IBE is rather the inference that our loveliest potential contrastive explanation is the likeliest, i.e. the most warranted candidate for being an actual explanation. Lipton's thesis is not merely that the endeavor to explain is important to science, but that concern with explanatory loveliness is a driving force in scientific inference.

While Lipton is particularly concerned to replace the hypothetico-deductivist account of method, he seeks to show that IBE is more encompassing than either Bayesianism or Mill's methods. Lipton confronts hypothetico-deductivism with an illuminating analysis of Semmelweis's search for the cause of puerperal fever. He challenges the requirement that evidence and theory be deductively linked. He argues, for instance, that what stands as counterevidence is not the strict falsification of deduced predictions but a failure to explain the contrasts of concern. He also indicates how, by way of explanatory considerations, IBE can handle notorious threats to hypothetico-deductivism. For instance, articulating the conditions under which a contrapositive instance can serve as a foil, Lipton makes salient how the contrastive model of inference can distinguish between non-evidential and evidentially probative contrapositives. Lipton challenges the claim that Bayesianism and IBE are in conflict, suggesting instead that they complement one another. Cases in which our reasoning conflicts with Bayesian mandates do not refute the descriptive adequacy of Bayesianism, but only reveal the difficulty we have following the dictates of the probability calculus. Although we may atypically be lead astray, we can nonetheless generally implement Bayesian conditionalization (or at least approach conclusions to which it would lead) by way of explanatory reasoning. Lipton denies that the close relation between Mill's Method of Difference and IBE makes the latter superfluous. For instance, in contrast with the former, IBE describes how we select from various unobserved and unobservable causes. More generally, Lipton seeks to show IBE's own explanatory breadth, arguing that it encourages, so accounts for, our quest for unification, coherence with background beliefs, and the description of explanatory mechanisms. Loveliness is not a property our theories just happen to have; rather, the quest for loveliness crucially directs us toward those theories.

The final three chapters focus on justificatory issues. Lipton argues that his appeal to loveliness does not render his account of inference non-objective. And, irrespective of whether we can assume the world is lovely, since IBE generally corresponds with, so obtains the strength of, Bayesianism and Mill's methods, it fares no worse than its competitors. Lipton contends that, insofar as one grants an ability to rank a theory as more likely than others we have devised, one will have to grant that a theory can likewise be ranked absolutely. More generally, he argues that many forms of intermediate skepticism are inconsistent or slip, by misstep, into the Humean extreme. Lipton applies IBE itself to the question of why we should prefer prediction over accommodation: it is only in the case of prediction that a theory's truth can be the best explanation for fitting the data; by contrast, the posit that a theory's author 'fudged' the theory to fit the data (generally) remains a competing explanation for accommodation. The scientific realism debate moves

to the fore in the final chapter. Lipton suggests that, although the skeptic may reject the miracle argument ('it would be a miracle were our successful theories not true') as question begging, it may still be useful for adjudicating debates about, for instance, proximity to truth. Lipton nonetheless takes a theory's truth to be a 'bad explanation' for its success. The truth of any one of the many competitors that would share our theory's success would also explain our theory's success. The unlikely proposal is not that our theory is false, but that the true theory is not in the set of competitors. Embracing the recent suggestion that the miracle argument is an instance of the base-rate fallacy, Lipton attributes its appeal to our inclination to overlook just how miraculous it would be to simply think up the true theory. While he rejects the miracle argument, Lipton proposes three alternative arguments for realism. Each attempts to show that a realist version of IBE is superior to an instrumentalist version that restricts inference to observable causes. First, the distinction between observables and unobservables does not mark the proper epistemic divide. 'The relevant distinction, if there is one, is between the observed and the unobserved' (p. 200). Second, denying that IBE justifies belief in a theory's unobservables, the instrumentalist has less reason than the realist to believe that theory's predictions. Third, observation involves inference and, as Mill puts it, 'this inference may have been erroneous in any one of the instances, but cannot well have been erroneous in all of them, provided their number was sufficient to eliminate chance' (quoted on p. 204). Hence, Lipton suggests, a theory that asserts our observations to hold can have greater warrant than the observations themselves.

   Lipton anticipates that the instrumentalist will remain unconvinced. The following points might underlie the instrumentalist's reservations. I suspect that the instrumentalist can defend an epistemic distinction between the unobserved observable and the unobservable   at least with respect to existential claims that assert further instances of what has already been observed. Regarding Lipton's third argument, one might grant Mill's limited point above, and even that a generalization restricted to what has been observed possesses greater warrant than do the inferences in each instance of observation. However, it does not follow that any of those unobserved observable instances of a broader generalization lend support to, let alone possess greater warrant than, the observations. Nor, without question begging, would it follow that unobservable posits do. And, embracing her own version of IBE, the instrumentalist need not grant that the unification of what has been observed supports a belief in unobservables. Finally, the instrumentalist will doubt that we can ever justifiably deny the existence of 'a remotely plausible alternative' (p. 204). She will contend, first, that we lack grounds to claim that not a single theory among the indefinitely many competitors is remotely plausible. Second, she will ask whether a single case can be cited where the scientific

community truly dedicated itself to finding an alternative but failed to ever find one. And, third, since only altogether successful theories are of concern, she will propose that the best explanation for possessing no alternative is that the scientific community has not dedicated its time and effort to developing one.

A few points might also be raised against Lipton's second argument, which pertains specifically to van Fraassen's constructive empiricism. Insofar as the belief that a theory's claims about observables are true entails the belief that its next prediction will be true, the constructive empiricist may, like the realist, have an extra reason   one not reducible to the theory's past success   for believing the next prediction. And while the realist will share that reason, given the possibility of an epistemic distinction noted above, the instrumentalist appears open to deny that belief in unobservables provides any extra (good) reason for believing a theory's claims about observables (and hence predictions). Finally, predictions are evaluative tools: scientists derive and test them. Requiring that scientists qua scientists also believe them, prior to a test, may well point to an oddity in both realism and constructive empiricism. (Was Poisson not being perfectly scientific when he predicted 'the white spot,' a prediction he apparently believed would not obtain?) On this note, I'd suggest that a non-realist position, not among the usual suspects, can be gleaned from the essentials of Lipton's descriptive account of IBE. Let's call this Liptonian Non-Realism, LNR. Accepting that the choice of at least some scientific theories/systems over their competitors did not require belief, LNR accepts that speculation about what scientists believe is irrelevant to understanding the activity of science. (Whether belief is crucial for the non-scientific use of technology is another question.) Science can be understood in terms of choices, which are instances of what LNR calls 'acceptance.'[1] Taking inference to lead to acceptance rather than belief, LNR accepts the principle 'employ IBE,' as well as Lipton's descriptive thesis. Lipton's second argument is a non-starter against LNR since LNR accepts that scientists qua scientists need not believe their predictions. More generally, I'd suggest that, while LNR accepts Lipton's general description of inference, it is a non-realism unscathed by his arguments for realism. These reflections, of course, diminish none of the significant clarity Lipton's map affords in the quest to understand our non-demonstrative inferences.

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[1]   Many take van Fraassen to claim that, when scientists accept a theory, they believe what it asserts about observables. LNR finds this an awkward empiricism, unable as we are to observe the beliefs of scientists.