

Default Privilege and Bad Lots

Abstract: The underconsideration argument against Inference to the Best Explanation and scientific realism holds that scientists are not warranted in inferring that the best theory is true, because scientists only ever conceive of a small handful of theories at one time, and as a result, they may not have considered a true theory. However, antirealists have not developed a detailed alternative account of why explanatory inference nevertheless appears so central to scientific practice. In this paper, I provide new defenses against some recent objections to the underconsideration argument, while also developing an account of explanatory inference that both survives these criticisms and does not entail realism.

1. Introduction

The “underconsideration” or “bad lot” argument against scientific realism holds that scientists are not warranted in inferring that the best theory is also true, because they only ever conceive of a small handful of theories at one time, and as a result, they may not have conceived of a true theory. More precisely, the underconsideration argument challenges the use of Inference to the Best Explanation (IBE), for we clearly select the best explanation only from the pool of explanations that we have considered.

While Duhem (1954), Sklar (1981), and van Fraassen (1989) offer early variants for this argument, enthusiasm both for and against this argument has recently picked up, with Stanford (2006) and Wray (2008) developing the argument along several dimensions, including safeguarding it from challenges raised by realists, such as Lipton (1993; 2004: Ch.9); still other realists and explanationists (Iranzo 2001; Minnameier 2004; Psillos 1999: Ch.9) have taken their best shot at the bad lot without rebuttal.

I, too, am jumping on the underconsideration bandwagon, though not without putting on a few handbrakes. Specifically, I will take on the unenviable task of rehabilitating IBE in the

wake of the argument, while at the same time letting realism die its proper, underconsidered death. More precisely, I have two goals. First, I continue the critical, antirealist project of troubling the realist vis á vis the underconsideration argument, by supplementing arguments where needed (§2). While committed to the soundness of the underconsideration argument, I nevertheless think that antirealists have not been as forthcoming with a positive account of why scientists are not generally troubled by it. This brings me to my second goal, in which I argue that when IBE is viewed as a social practice of epistemically responsible rather than epistemically reliable reasoning, it can elide the difficulties of the underconsideration argument (§3). While this does not license realism, it preserves many of our intuitions about the rationality of theory choice.

2. Some considerations about underconsideration

To begin, we should get a better sense of what the underconsideration argument actually says. IBE is fairly construed as an inference of the following form:

p

Among the available explanations h_1, \dots, h_n of p , h_i is the best explanation of p .

So (probably) h_i .

Van Fraassen (1989: 143) has pointed out that, so construed, IBE “is a rule that selects the best among the historically given hypotheses,” and since “we can watch no contest of the theories we have so painfully struggled to formulate, with those no one has proposed...our selection may well be the best of a bad lot.” More precisely, van Fraassen’s challenge reveals the following lemma in IBE:

Privilege: A true explanation of p is likely to be found among h_1, \dots, h_n .

Using two broad strategies, antirealists ask their realist opponents to demonstrate their entitlement to *Privilege*. First, all else being equal, there are many more unconceived hypotheses than conceived ones, rendering *Privilege prima facie* improbable. Second, the history of science suggests that scientists consistently fail to consider theories that we now take to be true. In this paper, I will take these two strategies as complementary. Specifically, in addition to providing a distinct line of criticism, the historical evidence and illustrations of the second strategy help to render the purely logical considerations of the first more concrete and less conjectural¹.

Peter Lipton has characterized the underconsideration argument in terms of two premises. The *ranking premise* grants scientists reliability in making *comparative* judgments about the probabilities of the hypotheses being considered (h_1, \dots, h_n). More precisely, I take the ranking premise to state that if scientists rank h_i a better explanation of p than h_j , then the objective probability of h_i is higher than that of h_j . Lipton holds that the antirealist should grant this premise on pain of sliding into an uninteresting form of skepticism. The second is merely a denial of *Privilege*, which Lipton aptly calls the *no-privilege premise*. Given these two premises, the strongest thing that may be inferred from the premises of an IBE is that the best explanation is more probable than its competition. But this is quite different than inferring that the best explanation is probable in absolute terms, as IBE's devotees assert.

2.1 Privilege and ranking

¹ As Stanford (2006) argues, if underconsideration arguments are not to trade in merely idle skeptical possibilities (on a par with worrying that scientists are brains in vats), then these arguments should be historically grounded.

To meet this challenge, Lipton argues that *Privilege* must be true on pain of inconsistency with the ranking premise. Lipton takes the ranking premise to grant scientists reliability in ranking contraries. However, he then argues that this will entail *Privilege* on the following grounds:

Suppose ... that we wish to rank the contradictories T1 and not-T1. If we find a contrary to T1 (say T2) that is ranked ahead of T1, then not-T1 is ranked ahead of T1, since T2 entails not-T1 ... So it is not clear how to ban the ranking of contradictories while allowing the ranking of contraries.

In other words, ranking contraries entails ranking contradictories, and this permits scientists to reliably rank in absolute terms (Lipton 2004: 156). As we shall now see, there are both older and newer arguments by which to rebut this challenge.

The older rebuttal claims that such a gambit cannot punch the appropriate inference ticket. Specifically, Wray (2008: 323) claims that Lipton “has gained very little by collapsing the comparative evaluation of these theories into an absolute evaluation.” For example, if early modern astronomers were given specific evidence, e.g., that Venus orbits the sun, and were choosing between the Ptolemaic and Tyconic theories in early modern astronomy, then this evidence will certainly rule out the Ptolemaic system, but our astronomers could not infer from this that the Tyconic theory “in all its details is likely true,” for the evidence is compatible with many theories unconsidered at the time, e.g. the theories of contemporary astronomy.

While congenial to Wray’s criticism, let me further trouble this line of reasoning with an argument that shows how Lipton’s argument actually would make scientists *incoherent* rankers. Suppose, as Lipton says in the quote above, that we wish to rank both T1 and not-T1. If we find

a contrary to T1 that is ranked ahead of T1 (say T2), then according to the quotation, not-T1 is ranked ahead of T1. Now suppose that there is another contrary of T1, say T3, but that it is ranked **below** T1. Then, since T3 entails not-T1, once again invoking the quotation above, T1 is ranked ahead of not-T1. Therefore, it cannot be the case that there is simultaneously a contrary of T1 ranked above it while another is ranked below it, on pain of the contradiction that T1 is ahead of not-T1 and vice versa, i.e. that T1 is ranked both ahead and below of not-T1.

But the possibility of ranking some of T1's contraries ahead of it and some below it is a very basic feature of inductive inference—constitutive of its non-monotonicity and defeasibility. The ranking premise simply says that if a scientist thinks of these contraries, then he will rank the first a superior explanation to T1; the second, inferior to T1; and furthermore, that these rankings apply not just to explanatory merits, but also to the objective probabilities of T1 and its two contraries.

Furthermore, this is all it should say, for scientists frequently *do* rank contraries in this manner. For example, the largest hurdle to establishing the Weinberg-Salam electroweak theory (first formulated in 1967) was the low frequency of neutral currents, most notably in kaon decay. The Weinberg-Salam model's most distinguishing commitment was to the existence of the Z^0 boson. In contrast, the V-A theory, the chief competitor to the electroweak model as an explanation of weak interactions, posited no such boson, and predicted a low frequency of neutral currents, albeit a higher frequency than the experimental results. For most in the high energy physics community during the 1960's, the scarcity of neutral currents defeated the electroweak model but not the V-A model. However, when the Weinberg-Salam model was modified to include the Glashow-Iliopoulos-Maiani (GIM) mechanism and the

charmed quark concept in 1970, it made a more accurate prediction of the low frequency of neutral currents than the V-A model. At this point, the low frequency of neutral currents, coupled with the electroweak model and GIM mechanism, defeated the V-A model.

Assuming all of this can be driven by explanatory considerations, the ranking premise counsels us to grade the electroweak-plus-GIM theory (playing the role of T2 in our more abstract discussion) ahead of the V-A model (T1), which in turn should be ranked ahead of the electroweak-sans-GIM theory (T3). By contrast, Lipton's argument would prohibit us from tracking the scientists' judgments here, since this would lead to the contradictory result that, e.g. the V-A model should be ranked both ahead and below of its denial; or, to make the contradiction more manifest, it is both more probable that Z^0 bosons exist than not, and less probable that Z^0 bosons exist than not. So, either Lipton has to provide a reason why the ranking premise doesn't apply in this example, or he has to give up the claim that if scientists rank a contrary of T1 ahead of T1, then not-T1 should also be ranked ahead of T1. Pursuing the first strategy would be rather ironic, since it was exactly Lipton's aim to show that it was the *antirealist* who could not consistently grant both the ranking and no Privilege premises. This, of course, suggests that it is Lipton's views on ranking contraries that are flawed. Thus, Lipton's claim that we can collapse comparative ranking into absolute ranking faces not only Wray's objection, but the further objection that it would yield incoherent ranking by ignoring the non-monotonicity of inductive inference.

2.2 Privilege, ranking and background theories

However, the underconsideration argument is not out of the woods yet. Lipton offers a second argument for *Privilege*, once again invoking the ranking premise. In this version,

however, he claims that because we use background theories to rank our hypotheses, and because antirealists grant the ranking premise, they should also grant that our background theories are approximately true. Without this, it would be surprising (dare I say a miracle?) that scientists reliably rank their explanations. Furthermore, Lipton argues:

The problem for the argument from underconsideration then appears on iteration. These background theories themselves are the result of prior generation and ranking, and the best of the theories now being ranked will form part of tomorrow's background. Hence if scientists are highly reliable rankers as the ranking premise asserts, the highest ranked theories have to be absolutely probable, not just more probable than the competition (Lipton 2004: 157-158).

Wray and Stanford attack this argument by denying its initial premise: if the ranking premise is true and we rank hypotheses using background beliefs, it does not follow that those background beliefs are largely true. According to Lipton (2004: 157), background theories "influence the scientists' understanding of the instruments used in their tests, the way the data themselves are to be characterized, the prior plausibility of the theory under test, and bearing of the data on the theory," and these considerations influence ranking. So, if Lipton's argument is correct, then scientists could not rank reliably if they had a different understanding of their instruments and data.

In response, antirealists cite various incarnations of the pessimistic induction², of which Stanford's (2006: 53-60) is perhaps most apt. For example, studying biology at the turn of the 20th century, he shows that scientists have used such disreputable entities as vital forces to

² Wray (2008: 322) cites (Carrier 1991; Laudan 1984; Stanford 2006) as providing particular difficulties for Lipton's argument.

understand the instruments and data in fields such as microscopic anatomy, descriptive embryology, morphology, and paleontology, and suggests through historical analysis that biologists nevertheless reliably ranked successive theories of inheritance.

As before, I want to add insult to the realist's injury. In particular, other critics of underconsideration have pursued Lipton's argument in broad outline, but have suggested different relations between background theories and scientists' ability to evaluate hypotheses. Sadly for the realist, these proposals don't work either. Beginning in the abstract, all explanationists grant that we rank explanations on their simplicity and the variety of evidence they explain (consilience), but there is nothing contradictory about appreciating a theory's simplicity and consilience, ranking it accordingly, and having a false background theory. Since these are widely considered the two preeminent explanatory virtues, it is *prima facie* possible that background theory does not inform ranking.

As I see it, foes of underconsideration have three replies to this challenge; none work. The first reply exploits the fact that consilience (and more arguably simplicity) supervenes on a concept of explanation that is determined by background theory. For example, perhaps our background beliefs dictate that we should use causal explanations in one context, and deductive-nomological explanations in another (Day and Kincaid 1994: 282; Psillos 2002: 615). However, this is insufficient, for scientists have reliably ranked explanations that, as a genus, are dubious. For example, Ptolemaic astronomy was the most consilient species of an explanatory genus that required all suitable explanations to put the earth at the center of the universe, describe the motions of all celestial entities in terms of perfect circles, etc.

A second reply would claim that background theories form part of the analysis of concepts such as consilience and simplicity. For example, part of an explanation's consilience consists of its ability to explain background beliefs (Minnameier 2004: 80; Psillos 2002). This is more promising, since it seems that we would be unreliable rankers if our theories explained mostly false background beliefs. However, what seems most pivotal in this scenario is that the most consilient hypothesis explains previously acquired *data*, not background *theory*, so we still have not shown that reliable ranking depends on having an (approximately) true background theory. Rather, we could imagine an antirealist, particularly of an empiricist bent, claiming that we have merely saved more phenomena.

A third option for foes of underconsideration is to remind us that simplicity and consilience are not the only virtues in town. Conservatism, which states that a hypothesis that coheres with more background beliefs is a better explanation than one that coheres with less, is also an explanatory virtue and thus also influences ranking (Lipton 2004: 151). However, this overrates conservatism—most developed accounts of explanatory coherence accord it high defeasibility and a lesser role than consilience and simplicity, e.g., (Harman 1986; Lycan 1988; Thagard 1992). Indeed, Thagard explicitly denies it any role whatsoever.

Furthermore, so long as it is possible for simplicity and consilience to bypass background theories, it means that a highly simple and consilient explanation that fares poorly regarding conservatism may be the best but unavailable, thereby spoiling our lot. For example, Thagard (1992: 209) notes that, despite relativity theory's greater explanatory coherence, Einstein rejected the following background beliefs, most (if not all) of which were highly coherent ("in the center of the web") in classical physics:

(1) Time and space are absolute; (2) There is a luminiferous aether; (3) Objects have no maximum velocity; (4) Euclidean geometry adequately describes space; (5) There are instantaneous gravitational effects; (6) Light travels through space in straight lines.

To be sure, there was a good deal retained from prior physical theory, but the chief point is that the high ranks that relativity scored were not primarily a function of its conservative elements (i.e., its coherence with background theory); rather there were other explanatory virtues that did most of the justificatory work. If this argument is correct, then it is hard to see how the ranking premise's dependence on background theory entails that the latter must be true.

Thus, the underconsideration argument has several lines of defense against the idea that reliable ranking entails true background theories. Not only can we invoke variants of the pessimistic induction, but we can also show that several other relationships between background theories and the ranking premise fail to produce the realist's desired result. Background theories may determine criteria of explanatory relevance, but nothing about this dictates that those criteria are truth-tropic. Background theories may play a role in determining various explanatory virtues used to rank reliably, but these can be captured just as easily within an antirealist framework. Conservatism may be an explanatory virtue, but it does not appear central enough to secure the truth of background theories. So realists would do well to keep background theories out of the foreground when tackling underconsideration.

2.3 Privilege, hypothesis generation, and background theory

Yet another defense of *Privilege* takes background beliefs to help in the *generation*, rather than the *ranking* of explanations. Like the argument just rehearsed, arguments favoring the reliability of hypothesis generation also advert to the approximate truth of background

theories. Thus, any general arguments against the truth of background theories from the previous section can be applied here. Furthermore, unlike arguments involving the ranking premise, antirealists need not grant their opponents much in this line of argument. Owing to its issuing absolute (as opposed to comparative) probabilities, the claim that scientists reliably generate probable or approximately true explanations—begs far more questions in the realism debate than the ranking premise.

But as before, I think further criticism is possible by more closely scrutinizing the relationship between background theories and their intended target—in this case, hypothesis generation. In particular, just as background theories played specific ranking roles in §2.2, we can also identify specific generating roles for background theories. In particular, it has been proposed that (a) background beliefs determine the appropriate form of explanation, e.g., causal versus nomological (Day and Kincaid 1994: 282; Psillos 2002: 615); (b) background beliefs can narrow down the space of available hypotheses, sometimes to just one hypothesis (Psillos 1999: 217-219; 2002: 616); (c) background beliefs provide heuristics for generating hypotheses (Lipton 2004: 150-151). As I shall argue, none of these require antirealists to accept the reliability of scientists' powers of hypothesis generation.

The first, in which criteria of explanatory relevance are determined by background theories, admits of historical counterexamples identical to the ones rehearsed above. At one point, background theories suggested that Ptolemaic astronomy was the best in an explanatory genus that required all suitable explanations to put the earth at the center of the universe, describe the motions of all celestial entities in terms of perfect circles, and so on. At another point, they suggested vital forces, etc. Even invoking “mature” sciences fails us. For instance,

background theories in physics during the latter half of the 20th century, the paradigm of scientific maturity, helped to generate the V-A theory.

The second generating role, in which background beliefs narrow down the space of available hypotheses, also faces difficulties. Psillos, the chief proponent of this view, begins his solution to van Fraassen's bad lot argument with the claim that our background theories are approximately true (Psillos 1999: 215-222). From this, it follows that any explanation that would force us to contradict those theories would be improbable. Since scientists use background theories to "drastically narrow down the space in which hypotheses can provide a potential explanation of the evidence at hand," often to just one hypothesis (Psillos 1999: 218-219), they capitalize on this "background knowledge privilege." Since fit with background theory is widely regarded as an explanatory virtue, we have reason to believe that scientists' explanatory evaluations are likely to track the truth.

However, Psillos's argument runs into problems. First, Psillos may put too great an emphasis on conservatism, a problem we have rehearsed above. Second, while background theories certainly narrow the space of potential explanations, there appears to be a compelling pessimistic induction that raises serious questions about the *reliability* of this process. To use one of our earlier examples, the various background claims about the absoluteness of space and time, aether's existence, Euclidean space, etc. all limited the space of potential explanations, but clearly did so to the detriment of truth. Importantly, given their remoteness from direct observation, the antirealist takes the truth of claims exactly like these as dispensable to the aims of science.

Indeed, even if we grant (only for the sake of argument) that background theories are approximately true, this and other examples suggest that scientists often rely heavily on the false parts of the background theory to limit the range of potential explanations. This is bad news for realists, since it is this specific use of background theories in *hypothesis generation* that is supposed to thwart underconsideration arguments³. For example, Stanford points out that scientists regularly take certain theoretical entities, properties, etc. as necessary for explaining a particular class of phenomena, only to be proven profoundly wrong by their successors. For example, in direct engagement with one of Psillos's preferred examples, Stanford writes of Maxwell's view on the aether:

...he [Maxwell] insists that to avoid the patent (or at least inconceivable) absurdity of 'energy...existing in a point of space' we must recognize the existence of a material and mechanical medium filling the space between bodies, for energy 'cannot be contained in any vessel except the inmost substance of material things.' (Stanford 2006: 152)

Of course, this view about a material and mechanical medium has subsequently turned out to be false, so even leading scientists, such as Maxwell, can make incorrect judgments about what parts of the explanatory field should be eliminated by background theories.

This leaves the third and final generating role for background theories—Lipton's "background heuristics." Unfortunately, Lipton does little to state what these heuristics amount to, and some of the more promising ways of articulating this idea yield the same disappointment for the realist. For example, suppose that these heuristics involve drawing

³ Indeed, by way of *reductio*, with further argumentation, this could refute the idea that one must adopt a realist attitude towards background theories. This would have ramifications for Psillos's more global defense of the approximate truth of background theories, which rests on an abductive defense of IBE. Since this paper is only concerned with underconsideration, the details of this issue would lead us too far afield. (Iranzo 2008) critiques these further aspects of Psillos's argument.

analogies with previously successful explanations in our background beliefs (Thagard 1978, 1992). Similarly, analogies with failed explanations might serve as precautionary constraints in hypothesis generation. Unfortunately, this won't suffice. A "successful explanation" would already have to be construed realist-style in order for these heuristics to defuse underconsideration, and that begs the question.

So once again, realists have failed to meet the challenges of the underconsideration argument. As nice as it would be for us to be gifted enough to generate a true explanation, there is nothing so magical in our background theories to underwrite this. Whether background theories determine explanatory relevance, whittle down the playing field to just one hypothesis, or provide hypothesis-forming heuristics, theory construction is susceptible to underconsideration.

3. IBE unconsidered and reconsidered

Thus, we have seen that within a certain framework, the prospects of claiming *Privilege* are rather bleak. However, we cannot deny that there is *something* rational about surveying our most plausible theories, ranking their ability to explain a phenomenon, and forging ahead with the highest ranking member. Generally, antirealists take too much glee in the critical component of their enterprise while providing little insight into the alternative. It is not uncommon for there to be some antirealist hand-waving about how we must view scientific reasoning as more pragmatic than we might have initially thought, and then to say nothing more about what this intersection of inference and pragmatics amounts to. In the balance of this essay, I seek to replace some of this hand-waving with a more detailed antirealist proposal.

To that end, I believe that exactly what it means to *justify Privilege* requires profound reassessment in two ways. First, debates about externalism and internalism have led some epistemologists to distinguish two notions of justification (Engel 1992; Fogelin 1994; Kornblith 1983; Williams 2001, 2008). The first, epistemic *reliability*, is the objective probability of a belief-forming process yielding a true belief. The second, epistemic *responsibility*, is the justification credited to someone who fulfils her epistemological duties, e.g., seeking and properly evaluating all of the available reasons, where available reasons are typically context-sensitive or “personal.” These are not coextensive. An epistemically responsible agent may be unreliable when certain evidence is unavailable to her; an epistemically reliable agent may be irresponsible when certain evidence is not considered or properly evaluated—a phenomenon particularly common among animals and infants (insofar as they are agents at all).

Both friends and foes of underconsideration have taken reliability as the primary notion of justification. In what follows, I would like to give responsibility pride of place⁴. In doing so, I am not foreclosing the possibility of a tight connection between the two notions, only that a defense of *Privilege* does not rest on forging this connection, a point I develop below⁵.

My second departure from the previous scholarship on underconsideration introduces a distinctively *social* element to my treatment of *Privilege*. Specifically, I argue that even if the explanation that we accept is only the best of a bad lot, it is capable of playing an important

⁴ Another option (Kuipers 2004) is to retain a broadly reliabilist bent, while also avoiding commitment to *Privilege* and weakening the conclusions of explanatory inferences so that the best available hypothesis is inferred only as the available hypothesis that is closest to the truth. This still capitulates a good deal to the underconsideration argument, since it is consistent with the best available hypothesis being very remote from the truth, i.e. the best of a bad lot. As a result, I suspect that the “constructive realism” it licenses is more constructive than realist, though I certainly cannot argue for that here without going too far afield.

⁵ The majority position treats responsible epistemic behavior as only worth its salt if it is also reliable; though (Brandom 2000: Ch.3; Williams 2008) subvert reliability to responsibility.

social-epistemological role. If correct, we should no longer treat the realism/anti-realism debate as merely a debate about the logical relations between hypotheses and data, but must instead look at how such propositions are actually used in scientific practices.

Taken in sum, I will seek to show that a responsible scientist can sometimes be reasonable in accepting an explanation even if it is the best of a bad lot, and that the value of such responsible explanatory evaluation can be made clear by examining the ways in which other scientists are entitled to use that explanation in subsequent inferences. The tethering of epistemic responsibility to this kind of social epistemology is fairly natural, for in many other social practices, we measure the value of undertaking a responsibility by the entitlements reaped by such an undertaking. For example, undertaking the commitment to pay a cashier entitles one to the corresponding goods and services. Let me also underscore that this is a fairly radical departure from the positions we have surveyed thus far, for both realists and antirealists assume that accepting a hypothesis that is the best of a bad lot cannot be reasonable.

As a first step, we must clarify the kinds of responsibilities that scientists must undertake with respect to *Privilege*. Suppose a scientist, call her Anne, believes that h on the basis of IBE, and, a second scientist, call him Ben, challenges that inference on the following grounds, “The pool of hypotheses from which you inferred h may contain nothing but falsehoods,” or “There may be a hypothesis you didn’t think of that better explains the evidence.” In other words, Ben challenges Anne’s *Privilege*. Simply raising these skeptical possibilities is not sufficient reason for Anne to give up her belief in h . She could fairly ask,

“Why do you think that?” or “What do you have in mind?” and Ben’s failing to answer such questions would permit Anne to proceed in her belief that *h* undeterred⁶.

However, this isn’t to say that one’s *Privilege* can never be overturned. Ben can cite genuine reasons for querying *h*’s credentials, even if Ben agrees with Anne that *h* is the best explanation currently available. (Some physicists may have this attitude towards string theory). On an explanationist line, these reasons will concern *h*’s explanatory incoherence, e.g., phenomena it fails to explain, background beliefs it contradicts, that it requires as much explanation as it provides, etc. In this case, Ben would be justified in thinking *h* the best of a bad lot. Furthermore, Ben is entitled to deny that *h* is even the *best* of a bad lot in contexts where he can provide a new hypothesis that is more explanatorily coherent than *h*.

Thus, being a responsible endorser of hypotheses suggests far different criteria of adequacy than being a reliable ranker or generator of those hypotheses. First, the ranking premise in this new framework need not entail anything about objective probability; though certainly it can. Second, instead of having an obligation to provide *prior grounding* (something resembling sufficient reason) for *Privilege*, responsible endorsement consists of a *defense commitment*, i.e., answering challenges to the hypothesis that one accepts. These *challenges* are subject to a “put up or shut up” constraint, meaning that they themselves must be substantiated. As suggested above, this can even be given an explanationist gloss by cashing out these challenges in terms of *explanatory failures* that *lower h*’s ranking, thus respecting the constraints of the ranking premise. Finally, *Privilege* has *default justification*, meaning that one

⁶ Others have identified these features of explanatory justification, e.g., “very few beliefs can be warranted if the notion of warrant involves elimination of the possibility that the belief be false” (Psillos 1999: 216); “believing that there *may be* a better explanation than the best available one does not undermine justification” (Iranzo 2001: 83). However, they do not develop this point.

is justified in believing *Privilege* without providing positive reasons for it until an appropriate challenge, i.e., one satisfying the put up or shut up constraint, is raised.

The position that scientists have default justification for *Privilege* with an epistemic responsibility to satisfy defense commitments in the face of appropriate challenges shall be called the *Default and Challenge Model of Privilege* (DCP)⁷. While many claims enjoy default status, *Privilege* will be the only one that concerns us here. Furthermore, I assume that a person has met her defense commitments, i.e. is epistemically responsible, with respect to *h* only if she can answer all the appropriate challenges to *h* that have been entertained in her epistemic community at a given time, or at least can defer to someone else who can meet those challenges.

It will be useful to briefly examine how DCP handles some of the failures of the positions discussed in the previous section. First, unlike the realist positions discussed in §2.3, DCP does not make any claims about hypothesis generation or theory construction; instead, using a fairly conventional (albeit responsibilist) theory of justification (for us, explanatory coherence) and the default and challenge model of justification, it focuses on the *acceptance* of explanations. Second, unlike some of Lipton's and Psillos's proposals, DCP accords no special role to conservatism; it is just another theoretical virtue, and can be as defeasible or insignificant in this capacity as a sustained reflection on explanatory coherence sees fit. Third, DCP is compatible with non-monotonic rankings of contraries, a problem that plagued Lipton's account. Importantly, given the aforementioned difficulties concerning theory generation,

⁷ The moniker is based on a similar model of justification endorsed by (Brandom 1994; Williams 2001, 2008). Williams cites Carneades, Austin, Wittgenstein, and Harman as proleptic proponents of the default and challenge model. Rolin (2006; 2008) has also applied this model to issues in the social epistemology of science.

conservatism, and monotonicity, any account of explanatory evaluation should feature these features, regardless of additional issues concerning scientific realism.

Despite these advantages, DCP faces two large potential objections: (1) DCP arbitrarily assigns *Privilege* default justification; (2) if correct, DCP would render IBE vacuous. Allow me to defang these in turn.

3.1 DCP is not arbitrary

Those leery of DCP are liable to think that default and challenge approaches cheapen justification. We could in principle take any contentious claim and put the burden of proof on its critics by according it a default justificatory status. Thus DCP appears arbitrary and perhaps question-begging, for the critic of *Privilege* will not give it default status. As a first reply, it is worth noting that the most obvious candidates for default entitlements are propositions we are likely to take for granted in certain contexts, and this appears to be scientists' most common attitude toward *Privilege*⁸.

Closely related, the fact that our scenario with Anne and Ben provides a more natural description of scientific practice than one in which Anne must rebut bald skeptical possibilities that have no explanatory credentials certainly suggests that DCP is not arbitrary. Lest this be misunderstood, I am *not* claiming that DCP is justified simply by some crude appeal to scientific practice. Rather, I am responding to the particular worry that my position, i.e. that *Privilege* enjoys default justification, is a just-so story. If DCP truly were as ad hoc as this objection

⁸ Rolin (2006) provides additional examples of default entitlements, including middle range hypotheses and social and political values. Williams (2001) also notes that in many contexts, we have a default entitlement to deny skeptical hypotheses, e.g. I am not a brain in a vat.

intimates, my imaginary exchange between Anne and Ben would seem far more fanciful than it is.

But *Privilege's* critic may well challenge this, using pessimistic inductions to show that underconsideration is the norm and thus a more faithful description of scientific practice. For example, Stanford's (2006) recent work on "the problem of unconceived alternatives" suggests such a narrative. He argues that throughout the history of science, we have failed to conceive of theories that end up being equally well confirmed by the same evidence available to their predecessors.

This leads to a second reply, which hinges on the fact that DCP shifts the locus of justification from reliability to responsibility. The pessimistic induction works wonderfully in undermining the *reliability* of explanatory inference, but if such historical narratives are relevant to DCP, then scientists must be *epistemically irresponsible* when they infer the best explanation. However, not even IBE's staunchest critics go that far. For example, van Fraassen (1989), who has devoted considerable attention to the deontology of belief, simply urges that rational agents are *entitled* to deny the conclusions of an IBE even when they accept its premises. However, the only occasion in which he makes the stronger claim that using IBE is *irresponsible* is if it is used as *rule* in a Dutch Book scenario. So, even by van Fraassen's lights, IBE can be used responsibly when it is not used as a rule, and this is all we need for our current argument.

Furthermore, both van Fraassen and Stanford claim that putative IBE's often engender theoretical *commitments*. Clearly, commitments and responsibilities are close cousins, e.g., commitment to a marriage typically entails responsibility to be faithful to one's spouse. If DCP is

correct, then theoretical commitments involve responsibility to defend one's theory via explanatory considerations. Van Fraassen's view is highly congenial to this, for he takes commitment to be "exhibited in the person's assumption of the role of explainer, in his willingness to answer questions *ex cathedra*" (van Fraassen 1980: 12).

Admittedly, earlier fans of underconsideration have not tied explanatory considerations to *defending* a theory. However, this is because they do not explore the defense of theories at all. But this places the burden of proof on the critic of DCP; not for us—for clearly scientists *do* defend their theories, and it hardly seems a stretch to think that explanatory considerations might feature prominently in these contexts. Thus, the idea that we have default entitlement to *Privilege* appears non-arbitrary because it plausibly mirrors scientific practice.

3.2 The value of responsible reasoning

This last challenge showed just how many concessions DCP permits: IBE can be neither reliable nor a rule, and can be endorsed by antirealists who have leveled serious challenges to IBE, and yet can still yield justified conclusions. Optimistically, this means that we are well on our way to providing an antirealist account of when a scientist is justified in her explanatory evaluation. Pessimistically, it raises the question as to whether this kind of justification is worth anything. But things are not so bleak. First, we have not claimed that IBE is inherently unreliable; only that even if it were unreliable, scientists could still use it responsibly. Nevertheless, I think we would do well to see what value this responsibilist conception of justification has independently of reliability. Thus, in the event that IBE is unreliable, it would not be in vain.

To clarify the stakes of this discussion, consider that if a scientist *S* responsibly infers *h* via IBE, he has good reason to take *h* as true, but from an external, third-person perspective this should be weakened to the statement that *given S's epistemic capabilities, (probably) h*. Obviously, this would only be as valuable as *S's* epistemic capabilities are reliable. In contrast, if *S* is reliably justified in accepting *h*, then from a third-person perspective, we can assert *(probably) h* without qualification. Clearly, all else being equal, knowing that *h* is probable is better than knowing *h* is merely probable by *S's* lights.

To show that we are not merely making IBE an effective tool for reporting beliefs, but actually trading in epistemic goods, two social-epistemological features of DCP must be brought into relief. First, the account of DCP requires *S* to be quite good, since her defense commitments must cover all challenges to the explanatory coherence of *h* circulating in an epistemic/scientific community at a given time. So, we might wonder what entitlements come with these often formidable responsibilities. This leads to our second point: we can make sense of epistemic entitlements antecedently of truth. In particular, we can take *inference* as primitive, and explicate the value of responsible reasoning in terms of the inferences to which members of the scientific community would be entitled in light of satisfactory defense of the best explanation⁹.

From that angle, having fulfilled one's epistemic responsibilities concerning *h*—understood once again as answering the challenges operant in a community at a given time—authorizes all others to use *h*, undetached from its explanatory credentials, as a premise in any

⁹ Cf. (Brandom 1994; Rosenberg 1980; Sellars 1963) for more on the semantics informing this idea. Rosenberg and Sellars also invoke explanatory considerations in their epistemology, but do not invoke default and challenge; Brandom, the opposite.

subsequent inferences. Thus, the stronger conclusion to be drawn from hypotheses justified via DCP is that *anyone in S's epistemic community is entitled to use h as a premise in subsequent reasoning (until h fails a future challenge)*, and this, as before, is from an external, third-person perspective. Certainly, this is weaker than claiming that *h* is probable, but it meets the challenge of showing that “merely” responsible uses of IBE yield epistemically valuable conclusions.

With this, we can get a better sense of the alternative to realism available to us after we take underconsideration and DCP into account. Suppose that a contemporary scientist (a member of *our* epistemic community) responsibly endorses *h*, meeting all the appropriate challenges to its explanatory coherence. Then we are entitled to use *h* as a premise in our reasoning. On the “inference-first” view suggested above, truth is to be explicated in terms of its inferential role. Applied to the present context, *h is true* plays the inferential role as a consequence of *h*. So, we are entitled to infer that *h is true*—until *h* fails a future challenge. It is worth stressing that this is an inference that *we* (as members of the scientist’s community) are *entitled* (not obligated) to make, and that this entitlement is subject to revision in light of subsequent inquiry. Given its defeasibility, deflationism, internalism, dependence on epistemic communities, and eschewing of metaphysics, a realist should not be terribly satisfied with this result. However, as members of the scientist’s epistemic community, *we* can proceed unflinchingly.

4. Conclusion

In summary, I have shown that some prominent attempts to rebut the underconsideration or bad lot argument fall short. Sub specie reliabilism, *Privilege* cannot be gained by appeal to the truth of our background beliefs, whether they assist in ranking or generating the best explanation. Sub specie responsibilism, however, we can take on all reasonable challenges to the coherence of our favored hypothesis, and if we can meet those challenges, we will have earned a kind of privilege that will not necessarily give us metaphysical comfort, but that allows us to forge ahead, taking on new challenges, and garnering a stronger foothold in our explanatory and inferential undertakings.

But, surely those looking for a reliabilist justification—particularly realists who seek to provide it, but perhaps also antirealists who think it something unattainable—will not be satisfied. I leave it to their ingenuity to make new moves in these debates. But let me give them one further question to consider: Would there be anything wrong if the progress of science were “only” a story of humankind’s increasing epistemic responsibility in the face of ever greater challenges?

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