How explanatory reasoning justifies pursuit: a Peircean view of IBE

1. Introduction

The notion of an *inference to best explanation* (IBE) has attracted much attention in philosophy of science since Gilbert Harman (1965) first coined the phrase.[1] Most proponents of IBE (e.g. Psillos 1999, Lipton 2004, Douven 2011) take it to be a distinctive mode of non-deductive inference where *explanatory reasoning*, i.e. considerations concerning what would be a good or the best explanation of one or more phenomena, is used as a guide to theory choice. This form of reasoning, they hold, plays a crucial role in scientific practice, in both a normative and a descriptive sense. On the descriptive side, historical examples are used to argue that scientists often rely on explanatory reasoning when choosing between theories or hypotheses.[2] This descriptive point is then used to justify the normative claim that the explanatoriness of hypotheses can be used to justify or give reasons for choosing between them, and that this is generally a reliably, if fallible, guide to the truth.

The idea of an explanatory inference pre-dates its current popularity by at least a hundred years. From 1865 onwards, C.S. Peirce promoted an inference, which he called *abduction*, proceeding from the premise that a given hypothesis, if it were true, would make an otherwise surprising fact “a matter of course” (CP 5.189).[3] Recent scholarship has however emphasised that Peirce's mature account of abduction differs significantly from the contemporary notion of IBE.[4] Contemporary discussions usually assume that explanatory reasoning, at least in the form of IBE, can justify *accepting* hypotheses as (approximately) true. They thus regard it as a species of inductive or ampliative inference. While Peirce agreed that abductions should guide our choices of hypothesis, he only understood this in the sense of choosing which hypotheses to investigate further. Peirce held that only empirical investigations can justify accepting a hypothesis, insisting that abductions give us no reason to regard it as true, except insofar as these lead to subsequent empirical testing. He did regard abduction as a form of inference – something which involves giving reasons (whether good or bad) – and not, for instance, a mere heuristic for discovery. However, these are reasons for *courses of action*, viz. subjecting hypotheses to empirical testing,

1 IBE is also often invoked outside of philosophy of science, for instance to spell out the notion of coherence (e.g. BonJour 1985) or as the methodological basis for metaphysics (e.g. Lewis 1986). See Day & Kincaid (1994) and Minnameier (2004) for further examples and discussion.

2 Since the term 'theory' is often used ambiguously in philosophy of science (cf. Vickers 2013b) I shall generally prefer the term 'hypothesis', in the sense of a fairly definite claim about some phenomenon or part of the world – e.g. “the mass of the electron is approximately 1/1836 the mass of the proton”, “the extinction of the dinosaurs was caused by a meteor”, “the structure of H_2O is H-O-H”.

3 Following standard conventions, references to Peirce (1932-1958) are abbreviated as CP [volume].[paragraph number].

4 The following interpretation is defended especially clearly by McKaughan (2008). Cf. Also Kapitan (1992, 1997), Hintikka (1998), Minnameier (2004), Paavola (2006), Campos (2011), and Plutynski (2011). Niiniluoto (1999) gives a chronological review of the development of Peirce's views on abduction.

rather than matters having to do with belief or acceptance (McKaughan 2008: 450 & 454).

In this paper I defend a view of the justificatory role in science of explanatory reasoning in general, and IBE in particular, along the lines of these Peircean insights.[5] Specifically, drawing on the distinction between acceptance and pursuit (Laudan 1977; Franklin 1993a, 1993b), I propose to see explanatory reasoning as first and foremost providing justification for *pursuing* a hypothesis, as opposed to justification for *accepting* it. Call the latter view *explanationism* for short. The Peircean view defended here, I argue, enjoys two advantages over explanationism.

The first concerns what Peter Lipton (2004) calls *Voltaire's Objection* to explanationism: why should we regard a hypothesis as any more likely to be *actually* true just because it would be a better explanation if it *were* true? The Peircean view side-steps this problem, since it requires no general connection between explanatoriness and truth. Furthermore, the Peircean view faces no analogous problem either. As I shall show, there is a simple and straightforward connection between good explanations and justification for pursuit, based on the kinds of "economical" considerations Peirce emphasised as being crucial to abduction. I introduce Voltaire's Objection in section 2 and explain why it poses a problem to explanationism. In section 3, I present a general account of pursuit and then, in section 4, show how explanatory reasoning can justify pursuit.

Second, I argue that once the Peircean view is on the table, this undercuts several of the empirical arguments for the reliability of IBE proposed by explanationists. Firstly, it challenges the argument that, since scientific practice relies extensively on explanatory reasoning and science generally leads to (approximately) true theories, IBE is generally a reliable form of inference. For this effectively commits a fallacy of composition, ignoring the possibility that explanatory reasoning play a justificatory role different from indicating the likeliness of hypotheses being true. Secondly, the Peircean view provides an equally or more descriptively adequate account of many of the historical case studies that are used to motivate explanationism. Thus, the Peircean view is at least as well supported by these case studies as explanationism. I lay out these criticisms in sections 5 and 6.

2. Voltaire's Objection

The slogan that one should infer "the best" explanation conceals an important distinction. For there are at least two senses in which an explanation can be better than its competitors, and these should be kept separate when evaluating explanatory inferences (Lipton 2004: ch. 4). In the first case, a

5 I do not claim that what I here call "the Peircean view" is the most plausible interpretation of Peirce's considered views on abduction, much less that it captures everything Peirce ever wrote about it (he discussed abduction extensively throughout his career, often modifying or rejecting his previous views – cf. McKaughan 2008; Niiniluoto 1999). Rather, I simply use it as a name for the view, inspired by Peirce, which I here defend in the context of the contemporary debate.

hypothesis may be the *likeliest* explanation relative to the other competing hypotheses considered. The likeliness of an explanation has to do with truth – i.e. it is the explanation which we regard as most likely to be true, or closest to the truth. In the limiting case scientists may be able to rule out, or make highly improbable, all plausible alternative explanations in light of the available evidence and accepted background theories. Here, the remaining explanatory hypothesis would be the likeliest available explanation, and in this sense the best. Since scientists generally aim to discover good explanations, if a hypothesis *H* is the likeliest available explanation of some otherwise surprising phenomenon, they would be justified in accepting *H*. At least as far as I am concerned this is a perfectly cogent inference and nothing I say in this paper aims to challenge it. As Lipton is careful to point out, IBE is only interesting as an inductive inference to the extent that it goes beyond merely being an inference to the likeliest explanation.

The sense of "best explanation" that is of interest to explanationists concerns how good an explanation we would deem a hypothesis *H* to be, if it were true. Let us say that the *explanatoriness* of *H* depends on the amount and quality of the explanations *H* would provide, if it were true. Or, since the "goodness" of explanations is usually taken to concern how much understanding they give us, the explanatoriness of *H* can also be seen as the amount of understanding it could potentially afford us.[6] Assessing this requires subjunctive reasoning, i.e. reasoning about what would be the case – viz. how much understanding it would provide – if *H* were true. We can call this kind of reasoning *explanatory reasoning*. What explanationists claim, then, is that explanatory reasoning can give us some additional or independent reason to accept a hypothesis as true (or approximately true). In other words, they regard the explanatoriness of a hypothesis as a guide to its likeliness.

This claim is however also what makes the explanationist account of IBE controversial. One question concerns what "good explanations" means. There are many different accounts of explanation (causal, unification, etc.), and proponents of these variably emphasise certain virtues (being simple, unifying, coherent, elegant, quantitatively precise, specifying a mechanism, etc.) as characteristic of good explanations. Many explanationists (e.g. Lipton 2004: ch 4; Psillos 2009: ch. 10) prefer to stay neutral on what defines good explananations. Since my argument in this paper will not depend on any particular view of explanation or of how they give us understanding (however we conceive of this), I am likewise happy to stay neutral on these matters.

Explanationism however faces a more pressing problem – what Peter Lipton (2004, ch. 9) calls *Voltaire's Objection*. As critics have pointed out, the fact that a hypothesis would be a good explanation of something, if it *were* true, does not, *prima facie*, seem to have any implications for

6 Explanatoriness is my preferred term for what Lipton calls the "loveliness" of an explanation.

whether it is *actually* true.[7] Indeed, this seems worryingly close to a form of wishful thinking. So why should this give us any additional reason to accept the hypothesis?[8] Of course, like all inductive inferences, IBE would be fallible, and so explanationists should not be expected to *guarantee* its success. Nonetheless, they ought to provide some reason to think that explanatoriness is generally a reliable guide to likeliness, i.e. that it generally tends to take us closer to the truth.

Douven (2011: sec. 3.2) mentions that few, if any, explanationists would think the reliability of IBE can be defended on a priori grounds. Consequently, they have instead proposed empirical arguments, based on historical case studies where explanatory reasoning seems to have played an important role in scientific practice. Since these practices have generally been reliable, explanationists argue, we have a good reason to think IBE is itself a reliable inference. I examine some variants of this arguments in section 5. First, I want to argue, in the following two sections, that the Peircean view of explanatory reasoning avoids Voltaire's Objection and, furthermore, faces no analogous problem.

3. Pursuing hypotheses and justifying it

In his astute exegetical study of Peirce's views on abduction, Daniel McKaughan (2008) distinguishes three general interpretations: the *Generative Interpretation*, the *Justificatory Interpretation*, and the *Pursuitworthiness Interpretation*. The Justificatory Interpretation corresponds to explanationism, where abduction is taken to provide justification for accepting hypotheses as true or approximately true. This view is typically contrasted with the Generative Interpretation, associated with Hanson (1958).[9] Hanson argued that it is a significant philosophical task to analyse the processes through which scientific theories are formulated, generated or discovered, promoting Peirce's abduction as such an analysis. Popper (1959/1935) and the positivists, he argued, were mistaken in restricting philosophy of science to questions of how evidence justifies the acceptance of theories, relegating all other issues to empirical sociology, psychology or history.[10]

McKaughan argues that these two interpretations overlook an important step in the process of inquiry between the initial formulation of a hypothesis and its acceptance or rejection as part of established scientific knowledge. Apart from formulating and developing hypotheses to investigate, scientists, in order to prioritise their time, resources, and efforts, furthermore need to make

7 In Lipton's Voltairean phrase: why should we think that we live in the loveliest of all possible worlds?

8 E.g. van Fraassen (1980), Cartwright (1983); see Barnes (1995) for a sustained criticism along these lines directed specifically at Lipton (2004), and Roche & Sober (2013) for a recent probabilistic formulation of the problem.

9 E.g. Paavola (2006).

10 The Hansonian view received much attention by post-Kuhnian historically minded philosophers. Nickles (1980a, 1980b) contains several papers by these "friends of discovery".

decisions regarding which of these to investigate or develop further. In other words, scientists need to make decision regarding which hypotheses are most worthy of further *pursuit*.[11] As McKaughan shows, this was a dominant theme especially in Peirce's later discussions of abduction – thus, the Pursuitworthiness Interpretation. It is this aspect of Peirce's views on which I draw in the following.

The distinction between *accepting* and *pursuing* a hypothesis was first proposed by Larry Laudan (1977: 108-114, 1980: 174). Laudan noticed that, historically, scientists have often chosen to work on scientific theories despite these having have major empirical and conceptual problems relative to the dominant views, citing, amongst others, Copernicanism, the atomic theory, and quantum mechanics in their early stages. By distinguishing between pursuing and accepting, Laudan argued, we can make sense of why it was rational for scientists to pursue these theories even though they had strong reasons to accept competing theories.[12] More recently, Allan Franklin (1993a, 1993b) has argued that certain episodes in particle physics are best understood as cases where physicists chose to pursue hypotheses before they had reasons to accept them. Indeed, some of the physicists involved were quite convinced of their falsehood whilst pursuing them.[13]

Franklin's examples are especially suggestive for present purposes, since these concern hypotheses that were pursued exactly because of their potential for explaining otherwise puzzling phenomena. For example, Franklin (1986, ch. 1) discusses the rejection by particle physicists of the so-called principle of parity conversation. The puzzling phenomenon physicists faced was this: for a particular set of decay patterns, the principle that each particles has a unique mass indicated that they stem from a single particle, while the principle of parity conversation ruled this out. When the physicists T.D. Lee and C.N. Yang in 1956 proposed that parity conversation may be violated in weak interactions, and suggested experiments to test this hypothesis, it sparked an intense experimental interest. It should be noted, first, that the same hypothesis had earlier been suggested as a logical possibility, but without being proposed as a solution to the above puzzle and without arousing much interest (Franklin 1986: 29f). Second, many of the physicists involved were quite convinced that the experiments would falsify the hypothesis.[14]

Apart from the descriptive point that scientist often actually do make and argue for decisions about which hypotheses to pursue, there are also normative reasons why scientists ought to justify

11 Pursuing a hypothesis is generally taken to involve at least two aspects: (i) subjecting it to empirical testing and (ii) developing it theoretically, e.g. clarifying it, resolving conceptual problems, or removing apparent tensions with other accepted theories (Laudan 1977, Whitt 1990). I mostly focus on (i) in this paper.

12 Laudan was here using the distinction to defend the rationality of science against challenges from Feyerabend.

13 One of Franklin's concerns is to show, *pace* Pickering (1984), that these episodes were guided and decided by evidential considerations.

14 Franklin reports (1986: 24) that Richard Feynman bet Norman Ramsey $50 to $1 that the experiments would fail to show parity violation – and ended up paying!

such choices.[15] The reason is pragmatic: the resources available to scientists are scarce but human imagination is abundant. In Peirce's words:

> Proposals for hypotheses inundate us in an overwhelming flood, while the process of verification to which each one must be subjected before it can count as at all an item, even of likely knowledge, is so very costly in time, energy, and money—and consequently in ideas which might have been had for that time, energy, and money, that Economy would override every other consideration even if there were any other serious considerations. In fact there are no others. (CP 5.602)[16]

In other words, scientists need to justify which hypotheses are worth investigating in order to optimise the epistemic output of their resources.

Let me emphasise, however, that although it concerns practical and pragmatic factors, justification for pursuit is not wholly detached from epistemic matters. On the contrary, choosing which hypotheses to pursue concerns how to maximise the epistemic output of science.[17] This also makes it slightly misleading to characterise the distinction as one between justification and pursuit. Although the two are sometimes conflated, the distinction between (justification for) accepting and pursuing hypotheses cuts across the much discussed distinction between context of discovery/context of justification.[18] Choices regarding which hypotheses to accept as well as which to pursue can and ought to be justified. The difference is that acceptance concerns which hypotheses are more likely to be to true, given our background knowledge and evidence, whereas the justification for pursuing hypotheses involves practical reasoning about which *courses of action to follow*, given our resources, overall goals and available information.[19]

How should we characterise justification for pursuit, then? McKaughan summarises Peirce's answer as follows: "If we estimate that testing the hypothesis will be *easy*, of potential *interest*, and *informative*, then we should give it a high priority" (2008: 457). Peirce himself mentions "cost, the value of the thing proposed, in itself; and its effect upon other projects" (CP 7.220; cf. McKaughan 2008: 467, note 12). Similarly, Franklin (1993a: 122) observes from his case studies that "[t]he decision to pursue an investigation seems to depend on a weighting of at least three factors; the

15 Further case studies of pursuit are discussed by Whitt (1990, 1992), Achinstein (1993) McKinney (1995), McKaughan (2008), Šešelja & Weber (2012), and Patton (2012).

16 Peirce frequently connects "economical" considerations to his account of abduction; see McKaughan (2008: 452ff) for further references.

17 Šešelja, Kosolosky, & Straßer (2012) distinguish justifying pursuit relative to purely epistemic goals as opposed to widers set of social, moral, and epistemic goals. Kitcher (2011) is an example of someone who discusses justification for pursuit in this broader sense. I here focus on the epistemic aspects of justification for pursuit.

18 Laudan (1980: 174) characterises the context of pursuit as a "nether region" between discovery/generation and (ultimate) justification. In my view, the "context" terminology is still somewhat misleading: these are not separated phases or contexts of scientific inquiry, but often co-occuring or interweaving. See furthermore Schickore & Steinle (2006) on the context-distinction.

19 McKaughan (2008: 454); cf. Kapitan (1992, 1997).

interest of the hypothesis, its plausibility, and its ease of test". He also mentions (1993b) more purely pragmatic concerns such as "recycling expertise" or being able to continue already ongoing research programmes.

Building on these remarks, I characterise justification for pursuit as consisting in weighing and ranking the salient competing hypotheses in terms of pragmatic factors such as:

- the expected cost of testing the hypotheses (in terms of time, money, energy, computational power, etc.),
- how easy it would be to carry out these tests,
- how likely we think it is that these tests will give us clear evidence for or against the hypotheses,
- how interesting the hypotheses are, including (crucially for my purposes) whether they would allow us to explain otherwise puzzling phenomena,
- whether carrying out these tests are likely to reveal other interesting facts about the world, and
- the effects on other project, e.g. whether pursuing a given hypothesis will allow scientists to extend experimental paradigms already used in many other contexts.

Inferences which produce justification for pursuit, on this conception, are ones which change our estimates of these factors, thus changing the ranking of the hypotheses considered.

It can be helpful to think of proposal along the lines of a simple, slightly formalised model, where the connection, stressed by Peirce, to "economical" considerations is more explicit. When deciding between a set of possible courses of action, in order to best use our resources we ought to choose that course which optimises the expected outcome relative to the expected costs of following that course of action. For the present case, then, let E(*pursue*(*H*)) abbreviate the *expected epistemic outcome* of pursuing a hypothesis *H* and let C(*pursue*(*H*)) be the expected costs of pursuing *H*. The expected epistemic outcome can computed as follows. Let $P(O_i \mid pursue(H))$ be the probability of the outcome O_i obtaining given that *H* is pursued, and let $V(O_i)$ be the epistemic value associated with that outcome obtaining. Then the expected epistemic outcome can be defined as $E(pursue(H)) = \sum [P(O_i \mid pursue(H)) \cdot V(O_i)]$. Given this, we can construe the justification for pursuing a hypothesis, JP(*H*), in terms of the quantity

$$(1)\ JP(H) = \sum [P(O_i \mid pursue(H)) \cdot V(O_i)]/C(pursue(H))$$

Within this model, inferences producing justification for pursuit will be ones that change the ranking induced by (1) on a set of competing hypotheses.

Let me stress that this model is both very idealised and abstract. I do not suppose that it is generally possible to make anything but rough estimates or comparisons of these quantities. Furthermore, the estimates of individual scientists, as well as what they take the most important epistemic outcomes of science to be, will probably vary significantly. I do not have any comprehensive account of these matters. Finally, scientists obviously do not always conform to or even approximate this model in their deliberations about which hypotheses to pursue; nor do I claim that it would be better if they did. Nonetheless, I find that this model can provide a useful framework for expressing and clarifying issues regarding justification for pursuit.

To illustrate, consider two salient outcomes of pursuing a hypothesis: learning (or getting strong evidence) either that it is true or false. One way to interpret the claim that a hypothesis *H* is easily tested is that the probability of learning that *H* is true or false, given that one pursues *H* – i.e. that P(*learn*(*H*) | *pursue*(*H*)) or P(*learn*(~*H*) | *pursue*(*H*)) is significantly above zero. Reasoning which shows this to be the case would, all things being equal, increase JP(*H*), since it would presumably always be more epistemically valuable to learn either that a hypothesis is true or that it is false, than not learning anything about its truth. So when Peirce, for instance, claims that

> the best hypothesis … is the one which can be the most readily refuted if it is false. This far outweighs the trifling merit of being likely (CP 1.120)

this would be case where P(*learn*(~*H*) | *pursue*(*H*)) is sufficiently high, and C[*pursue*(*H*)] sufficiently low, to rank *H* above the other candidate hypotheses.

4. How explanatory reasoning justifies pursuit

I claim that the Peircean view avoids Voltaire's Objection. But consider the following objection: the justification to pursue a hypothesis, at the very least, involves showing the hypothesis to be minimally plausible or probable. Indeed, Peirce sometimes says that abductions give us "reason to suspect that [the hypothesis] is true" (CP 5.189) or reasons "regarded as lending the hypothesis some plausibility" (CP 2.511, footnote) and that "[c]ertain premises will render an hypothesis probable, so that there is such a thing as legitimate hypothetic inference [i.e. abduction]" (*loc. cit.*). But if this is the case, the Peircean view would also require *some* connection between explanatoriness and likeliness (or plausibility). Even if it would be a weaker one than that required by explanationism, this is still sufficient for a version of Voltaire's Objection to apply to the Peircean view as well.

The premise of this objection is mistaken. Justification for pursuit need not stem from showing the hypothesis any more probable or plausible than before. In particular, this is not how explanatory inferences, on my account, justify pursuit.[20] Now, it might be argued that a hypothesis needs some minimal degree of plausibility in order for it to be worth pursuing at all. If this is correct, then *one* way of justifying the pursuit of a hypothesis is to show that it is more plausible than previously thought. However, this is not the only way. For one thing, it could equally be argued that a hypothesis is only worth investigating if it is not completely trivial or obvious. Thus, one can equally justify pursuing a hypothesis by showing that there is more reason to doubt it than previously thought.

More importantly for my argument, however, that a hypothesis is neither completely implausible nor completely trivial is not sufficient to justify pursuing it, at least not if the costs of doing so are non-negligible.[21] To see this, focus again on the outcomes of learning whether *H* is true or false. Now, as remarked above, one factor in evaluating this outcome is how probable one takes it to be that pursuing *H* will reveal whether it is true or false. This depends, in part, on its plausibility, but also on how difficult it would be to test it. The other relevant factor is how epistemically valuable it would be to learn whether *H* is true or false: if the hypothesis is sufficiently uninteresting, this may well outweigh the costs of pursuing it. This is where I want to argue explanatory reasoning comes into the picture. My claim is that by showing that *H* is more explanatory than previously thought – e.g. by showing that if it were true, *H* would be able to explain an otherwise surprising or puzzling phenomenon – we show that it would be more valuable than previously thought to learn whether *H* is true, thereby raising the justification for pursuing it.

To spell out this argument a bit further, notice first that the epistemic goals of science concern more than simply knowing as many truths as possible. As Phillip Kitcher (1993: 94) puts the point:

> Tacking truths together is something any hack can do. … The trouble is that most of the truths that can be acquired in these ways are boring. Nobody is interested in the minutiae of the shapes and colors of the objects in your vicinity, the temperature fluctuations in your microenvironment, the infinite

20 As for Peirce, he eventually came to regard his earlier writings as having mixed up induction and abduction (Niiniluoto 1999: S441; McKaughan 2008: 453f).

21 This is probably not even necessary. As Franklin (1993a: ch. 3) points out, physicists sometimes pursue experimental work on a hypothesis after they regard it as conclusively falsified. Experimental research have other uses beyond merely generating evidence for or against some particular hypothesis – they have "a life of their own" (Hacking 1983). Correspondingly, my model allows for other valuable outcomes of pursuing hypotheses besides learning whether they are true or false.

> number of disjunctions you can generate with your favorite true statement as one disjunct, or the probabilities of the events in the many chance setups you can contrive with objects in your vicinity. What we want is *significant* truth.

There are plenty of truths out there that could be discovered and at much lower cost than the questions actually pursued by scientists. The value of scientific knowledge depends on other factors beyond merely the amount of truths known, no matter how certain.

Now, what these additional factors are – what other "epistemic goods", as we might call them, are important in science – is not something we need to give a general account of here. However, most philosophers of science, and explanationists in particular, seem to regard explanation and understanding as being among them.[22] So one way a hypothesis can be more epistemically valuable than merely being true is by being a good explanation, i.e. by increasing our understanding of the phenomena scientists investigate. Philosophers may disagree about why explanation and understanding are epistemically valuable – maybe they are intrinsically valuable, or maybe they are only valuable as a means to achieving other important epistemic goals. However, all I need for the present argument is that explanation/understanding is in fact epistemically valuable.

Consider again the premise of an IBE: that the hypothesis *H* would provide the most understanding out of a set of rival explanations, if it were true. Thus, if we were to learn that *H* is actually true, this would be an epistemically valuable outcome. Indeed, learning that the most explanatory hypothesis is true would be the optimal epistemic outcome as far as explanation and understanding are concerned. What if we instead learn that *H* is false? As mentioned earlier, this is still more valuable than not knowing whether *H* is true or false. Furthermore, it does seem more valuable to learn that the most explanatory salient hypothesis is false, than learning the falsity of a less interesting hypothesis. For one thing, learning that the most attractive option is closed off will help us redirect our resources to other worthwhile projects that are actually achievable. One might also argue that there is something intrinsically valuable about knowing that the most appealing explanation is actually false. (All I need for my argument, however, is that it is not *less* epistemically valuable to learn the falsity of the most explanatory hypothesis). Suppose, then, that everything else is held equal between a set of rival hypotheses: the costs of pursuing them are the same, we regard it as equally likely that pursuing them will allow to us learn whether they are true or false, all other expected epistemic outcomes of pursuing them are equal, and so on. In this case, given the account of justification for pursuit outlined above, scientists would be justified in pursuing the most explanatory hypothesis.

22 For instance, Kitcher (1993: 105ff) discusses "Explanatory Progress" as one of the goals pursued by science beyond mere truth.

This, in a nutshell, is my account of how IBE justifies pursuit. The argument just presented furthermore applies to explanatory reasoning more generally: showing that a hypothesis *H*, if true, would explain an otherwise surprising fact raises the epistemic value associated with learning whether *H* is true or not. This, all else being equal, increases the justification for pursuing *H*.

Let me close this section by emphasising the generality of my argument. It only rests on the premise that it, all else being equal, is more epistemically valuable to know whether more explanatory hypotheses are true or false than less explanatory ones. In particular, it does not rely on any specific account of explanation or of why explanations are valuable. Combined with the account of justification for pursuit outlined in section 3, I hope to have convincingly shown that there is a simple and straightforward connection between IBE, explanatory reasoning, and justification for pursuit. I conclude that the Peircean view faces no obvious analogue of Voltaire's Objection.

5. Empirical arguments for explanationism

As mentioned in section 2, explanationists often cite historical case studies where scientists seem to have relied on explanatory reasoning. They argue on the basis of these that IBE provides a descriptively adequate account of this aspect of scientific reasoning, which is in itself significant if we want to understand how scientific reasoning actually proceeds.[23] On the normative side, many explanationists furthermore hope to answer Voltaire's Objection by deriving an argument for the reliability of IBE from this descriptive point.

In this section and the next I argue that the Peircean view challenges and potentially undermines this strategy for defending explanationism. The problem is that the Peircean view provides an alternative interpretation of explanatory reasoning in science which is usually not taken into account in these arguments. If the Peircean view turns out to be more descriptively adequate than explanationism, this would undermine the empirical premise on which these arguments rely. Of course there are many possible ways to argue empirically for the reliability of IBE, and I cannot address all of them here. Instead, I examine a few influential variants of this argument, showing how each of them relies on the descriptive premise and how the Peircen view challenges it. In the next section, I argue that the Peircean view indeed provides a plausible interpretation of several of the case studies often cited by explanationists.

Consider first a rather general line of argument which connects the reliability of IBE closely to the question of scientific realism. Here is how Douven (2011: sec. 3.2) summarises the crucial

23 Lipton (2004) explicitly claims this as the main virtue of his account of IBE. Douven (2011, sec. 1.2) also emphasises the “ubiquity” of explanatory reasoning in both everyday, scientific, and philosophical contexts.

step in Boyd's (1980, 1983) argument (Douven here uses "abduction" to refer to IBE):

> Boyd then argues that the reliability of scientific methodology is best explained by assuming that the theories on which it relies are at least approximately true. From this and from the fact that these theories were mostly arrived at by abductive reasoning, he concludes that abduction must be a reliable rule of inference.

Lipton (2004: 148) similarly connects the normative and descriptive ambitions of explanationism:

> My is hope is rather that by this stage you are convinced of the descriptive merits of explanationism, so insofar as you believe that our actual practices are reliable, you will tend to discount Voltaire's objection.

Now, many philosophers have questioned whether the historical evidence actually favours scientific realism.[24] Furthermore, some take this argument to beg the question against anti-realists, since arguments for scientific realism tend to rely on a form of IBE.[25] But since my aim here is not to challenge scientific realism I am willing to grant these points for the sake of the argument. Rather, I argue that even when granting this, the argument fails to support its conclusion.

To see this, notice that the argument is supposed to run from the premises

> (*Realism*) Scientific methodology relies on theories that are mostly true or approximately true.

and

> (*IBE in scientific practice*) These theories were mostly arrived at through IBEs (or at least by explanatory reasoning).

to the conclusion

> (*Explanationism*) IBE is generally a reliable rule of inference.

But stated this way, the argument clearly commits a fallacy of composition. For the most that can be concluded from the fact that science generally leads to true hypothesis is that scientific methodologies *taken as a whole* are reliable. It does not follow that any particular inferential pattern in science *on its own* is reliable as well. As illustrated by the Peircean view, explanatory reasoning may play an important justificatory role in science (e.g. choosing which hypothesis to *pursue*)

24 See e.g. Saatsi (2012) and Vickers (2013a) for recent discussion of the relation between historical evidence and the scientific realism debate. The classical statement of this objection is Laudan (1981).

25 For discussion see Psillos (1999: ch. 4) and Lipton (2004: ch. 11).

without being a reliable guide to truth. Thus, one can grant the premises of this argument while holding that IBE is wholly unreliable when divorced from subsequent empirical testing.

A slightly more subtle way of spelling out Boyd's argument (discussed e.g. by Psillos 1999: ch. 9, Lipton 2004: 148ff) emphasises the point that explanatory reasoning itself relies on the theories we already accept – e.g. for generating plausible candidate explanations, or for evaluating their explanatoriness. So if we regard these background theories as generally true, the argument goes, we should regard the methods that rely on them as being generally reliable. But this argument presumes that explanatory reasoning aims at choosing the likeliest hypothesis. If the Peircean view captures the role played by explanatory reasoning, however, this is simply not the purpose of explanatory reasoning. Supposing it does rely on background theories, there is still no reason to think that this would make IBE reliable with regards to something it does not aim at accomplishing.

Consider finally the more direct strategy for testing the reliability of IBE suggested by Douven (2002, 2005). Douven argues that we can, under certain circumstances, regard cases where a hypothesis inferred through an IBE is confirmed by other empirical methods as evidence of the reliability of IBE.[26] For instance, if we infer through IBE that a certain microbe exists, and we subsequently observe this microbe in a microscope, this convergence provides evidence both of the reliability of IBE and of microscopes.[27]

One objection that could be raised to this argument is whether we should expect there to be a completely domain-general argument for the reliability of an abstract inferential schema, such as IBE.[28] Even if we bracket this concern, notice furthermore that the kind of cases that could lend support to explanationism on this account would exactly be cases where we have some independent empirical test of the hypothesis. In these cases the Peircean account suggests a relevant alternative interpretation, namely that the explanatory reasoning merely justified pursuing the hypothesis and only the empirical testing justifies our accepting it. Again, if a given case study is more plausibly interpreted along Peircean lines, this would undermine its support for explanationism. Furthermore, while explanationism can only be supported by cases where we have empirical reasoning to believe the hypothesis is true, the Peircean view can also account for those cases where empirical testing falsifies or fails to support it. In both cases the Peircean can argue that explanatory reasoning justified pursuing the hypothesis. (I illustrate this point in the next section).

26 Douven (2002) relies on Glymour's (1980) bootstrapping procedure, while Douven (2005) develops a similar argument on the basis of Bayesian confirmation theory. Douven takes his formal accounts to be related to similar arguments found in Harré (1988), Bird (1998), and Kitcher (2001).

27 This example draws on Hacking's (1983: ch. 11) argument for the reliability of microscopy.

28 Saatsi (2009) raises this objection to empirical arguments for the reliability of IBE, drawing on Norton's (2003) “material theory of induction”.

6. The descriptive adequacy of the Peircean view

The empirical arguments for the reliability of IBE will be undermined or at least seriously challenged if it can be shown that explanatory reasoning primarily plays the role of justifying pursuit rather than acceptance of hypotheses. I do not here have the space to argue that Peircean view is in general the most plausible interpretation in all historical cases involving explanatory reasoning. To illustrate its descriptive potential, nonetheless, I examine a number of the case studies usually cited by explanationists, arguing that in these cases the Peircean view provides an at least as plausible interpretation.

6.1. Semmelweis

Consider first the case of Ignaz Semmelweis' investigation of childbed fever, a fatal disease affecting many women giving birth in one of the maternity wards where he worked in Vienna in the 1840s. Lipton (2004: ch. 5) draws extensively on this case in order to support his descriptive claim that scientific inferential practices often proceeds via explanatory considerations. Lipton stresses that at several points in his investigations, Semmelweis considered what would explain the differences in fatality rates at the two wards. Going through a number of potential explanations, and testing these, Semmelweis in the end hypothesised that the infections were caused by “cadaverous particles”. The crucial difference between the two wards, according this hypothesis, was that medical students, who had just performed autopsies, delivered children in the first ward but not in the second. Disinfecting the hands of the students seemed to lower the infection rates, thus vindicating Semmelweis' hypothesis.

As Sami Paavola (2006) argues in detail, Semmelweis' explanatory reasoning in this case only seems to play the role of suggesting hypotheses to Semmelweis, which he then went on to test empirically. The Peircean can thus hold that it was empirical testing, and not explanatory reasoning *per se*, which justified accepting Semmelweis' hypothesis.[29]

Furthermore, Dana Tudlockziecki (2013) has recently challenged the standard version of this story. As she shows, Semmelweis failed to convince many of his contemporaries, simply because his reasoning was not as flawless as many philosophers seem to suppose. Semmelweis' crucial disagreement with his contemporaries was that childbed fever had one and only one cause (most were willing to agree that cadaverous particles played some role in causing the disease). Tudlockziecki (2013: 1074-5) concludes that:

29 Paavola uses this case study to argue in favour of a Hansonian, i.e. generative, view of abduction over explanationism. He does not consider the pursuitworthiness interpretation adopted here. As far as I can tell, Paavola's analysis supports the pursuitworthiness interpretation at least as well as the generative interpretation.

> Semmelweis simply did not provide any convincing reason to subscribe to the monocausality thesis. When Semmelweis was, reasonably, asked to perform certain experiments that could have supported his thesis, he declined, and, in addition, it was pointed out that the monocausality thesis failed to explain several salient phenomena associated with childbed fever that could be explained on a multicausal view

In this case then, to the extent that explanatory reasoning was successful, it seems to have been in proposing hypothesis for further testing. It was exactly when Semmelweis strayed from this strategy that his arguments lost their force. Notice that the monocausality view is both more unifying and simple explanation of childbed fever than a complex multiplicity of causes. On an explanationist view, this would seem to provide some reason to favour the hypothesis. But in fact, the explanatory potential of the monocausal hypothesis seems to bear little relevance to the objections raised by the critics. This is of course exactly the conclusion that the Peircean would come to: no amount of explanatoriness can outweigh the requirement of empirical testing. Of course, I do not claim that any defenders of explanationism would hold otherwise. Nonetheless, explanationism would require some additional account to accommodate this aspect of the Semmelweis case. In contrast to this, the core tenets of the Peircean view account directly for the weaknesses of Semmelweis' arguments.

6.2. Experimental reasoning

Another class of cases sometimes invoked by explanationists is the establishment of experimental results. For instance, Douven (2011: sec. 1.2) cites the reasoning J. J. Thomson employed to argue that cathode rays consist of "charges of negative electricity carried by particles of matter". According to Douven,

> The conclusion that cathode rays consist of negatively charged particles does not follow logically from the reported experimental results, nor could Thomson draw on any relevant statistical data. That nevertheless he could "see no escape from the conclusion" is, we may safely assume, because the conclusion is the best—in this case presumably even the only plausible—explanation of his results that he could think of.

However, if this was the only plausible explanation available to Thomson, this is only an instance of inference to the *likeliest* explanation – i.e. a case where all plausible alternative explanations are ruled out or made highly unlikely by the evidence – and not a case where explanatoriness functions as an additional indicator of likeliness. As pointed out in section 2, the Peircean view does not challenge the former type of inference, so this case is of little help to explanationists.

This analysis of experimental reasoning applies most similar cases. Consider for instance the series of experiments that Pieter Zeeman conducted in order to demonstrate what is today know as the Zeeman effect (Arabatzis 1992): that the spectral lines emitted by a given source, such as the vapours from a piece of heated sodium, can be caused to split by placing the source in a magnetic field.[30] During 1896 Zeeman carefully went through a series of gradually more complicated experiments to show that the action of the magnetic field was the only plausible source of the observed effect. For instance, Zeeman was worried that the effect might be due to differences in the density vapours in his test tube. To eliminate this effect, he reproduced the experiment with a smaller tube which he furthermore rotated around its axis in order to achieve an equal distribution of the gas. Of course, Zeeman's experiments did not *entail* this conclusion, and in this sense one could say that he merely inferred the best explanation. But given Zeeman's background knowledge and the results of his experiments, he was justified in regarding the action of the magnetic field as the only plausible explanation of the effect. Thus, also in this case we are only dealing with an inference to the likeliest, rather than the most explanatory, hypothesis.

More generally, Franklin (2009: sec. 1.1.1; cf. 1986: ch. 6, 1990: ch. 6) has summarised a number of epistemological strategies commonly employed by experimental physicists to establish the veracity of their results. Nowhere on Franklin's list does typical explanatory virtues (unification, elegance, etc.) figure as reasons to trust experimental results. It does however include (item 3) the strategy of eliminating plausible sources of error and alternative explanations – what Franklin calls "the Sherlock Holmes strategy". As mentioned in section 2, explanatory reasoning in Franklin's case studies seem only to play a role in suggesting hypotheses for further testing, but not in establishing them as (probably) true or false.

6.3. Neptune and Vulcan

A final case, which Douven (2011: sec. 1.2 & 3.2) explicitly cites as a successful application of IBE, involves the discovery Neptune. The story here is, briefly, the following: astronomers had observed that the orbit of Uranus around the sun showed irregularities which were not predicted by the standard Newtonian theory of gravitation. In response, astronomers John Couch Adams, in Cambridge, and Urbain Le Verrier, in Paris, independently produced calculations showing how this anomaly could be explained by a hitherto unobserved planet beyond Uranus. Shortly after, in 1846, astronomers observed this planet, which came to be named Neptune, where the calculations had predicted it to be.

30 See also Arabatzis (2006: ch. 4) for an account of the theoretical and experimental work that led to the discovery of electrons, including Thomson's and Zeeman's experiments.

Notice that the astronomical community only seems to have widely accepted the existence of Neptune *after* it had been observed. Thus, this case is also open to the Peircean interpretation that Adams' and Le Verrier's calculations only justified the *pursuit* of the Neptune-hypothesis. In this case, the fact that the calculations provided concrete predictions of where to find this planet seems crucial, since this in effect provided guidelines for where on the night-sky to look for a planet capable of causing the anomaly in Uranus' orbit. This greatly raised the probability that pursuing this hypothesis would lead to astronomers learning whether it in fact existed or not. As argued in section 4, this need not involve any additional reasons to believe that the planet actually exists.

The aftermath of the discovery of Neptune provides further support for the Peircean analysis. Encouraged by his previous success, Le Verrier analysed the orbit of Mercury, arguing on the basis of similar calculations that there is an additional planet between the Sun and Mercury, which was dubbed Vulcan. Astronomers began the search for the planet, and although there were some reported observations, the hypothesis in the end produced too many failed predictions to convince a majority of astronomers of its existence. (The ability of Einstein's General Theory of Relativity to explain this anomaly later came to be regarded as one of the theory's early empirical successes).

This case is particularly relevant to Douven's direct strategy for establishing the reliability of IBE. Since Le Verrier used fairly similar patterns of reasoning to predict the existence of both Neptune and Vulcan and had very little prior observational evidence of their existence, this pair of cases seems to provide an important test of the reliability of explanatory reasoning. However, since it only successfully predicted the existence of a planet in one case, this at best shows IBE to be of limited reliability. The Peircean view, on the other hand, can regard both cases as instances where explanatory reasoning successfully fulfilled its function of suggesting hypotheses for empirical investigation.

7. Conclusion

I have argued in this paper that the Peircean view developed here provides both a normatively and a descriptively more adequate account of scientific explanatory reasoning than explanationism. Firstly, the Peircean view avoids Voltaire's Objection and faces no analogous problems of its own. Secondly, it is capable of accounting at least as well for several of the case studies usually cited by explanationists, thus challenging empirical arguments for the reliability of IBE.

I do not claim that explanatoriness can *never* be a guide to likeliness or a reason for accepting a hypothesis. I do for instance not rule out that we in specific domains may have knowledge allowing us to reasonably infer that certain explanatory virtues will also be truthtracking.[31] But, firstly, if this is sometimes the case, our reliance on IBE would still depend on independent empirical knowledge about specific domains of inquiry. Secondly, and more substantially, the argument of this paper shows why we need not *generally* assume explanatoriness to be a reliable guide to the truth: the Peircen view nonetheless allows us to account for the central justificatory role of explanatory reasoning in science.

31 As suggested by Saatsi (2009); cf. Kuipers's (2002).

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