Can Induction Strengthen Inference to the Best Explanation?

by

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AUTHOR'S DECLARATION

I hereby declare that I am the sole author of this thesis. This is a true copy of the thesis, including any required final revisions, as accepted by my examiners.

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Abstract

In this paper I will argue that the controversial process of inferring to the best explanation (IBE) can be made more coherent if its formulation recognizes and includes a significant inductive component. To do so, I will examine the relationship between Harman's, Lipton's, and Fumerton's positions on IBE, settling ultimately upon a conception that categorically rejects Harman's account while appropriating potions of both Lipton's and Fumerton's accounts. The resulting formulation will be called *inductive-IBE*, and I will argue that this formulation more accurately describes the inferential practices employed in scientific inquiry. The upshot of my argument, that IBE contains a significant inductive component, will be that any conclusion born from such types of inductive inference must be, at best, likely, and not a necessity. And, although previous accounts of IBE have accepted the defeasibility of IBE, I will argue that *inductive-IBE* is more descriptive because it tells us *why* this fallibility exists. That is, although the Liptonian conception of IBE acknowledges that IBE is fallible, my account specifically addresses this characteristic and, thus, is more descriptive and informative in this regard. I will use *inductive-IBE* to argue, contra van Fraassen, that IBE can be a legitimate form of inference that leads science to true theories and real entities.

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Introduction

Inference to the Best Explanation is a type of inference whereby we infer that the best explanation of a phenomenon is the true explanation.[1] For example, if I see a particular type of footprint in the snow, I infer that it is the case that a person with snowshoes has recently passed this way because it is the best explanation of the phenomena.[2] At the outset, this appears to be a useful and benign rule upon which to ground our inquiry, for it allows us to assert the truth of a theory, or even the existence of an unobserved entity, simply because it provides the best explanation for observed phenomena. However, ever since Inference to the Best Explanation, (or IBE), was first articulated by Gilbert Harman in 1965[3], it has not escaped criticism and, thus, has many proponents[4] and detractors[5]. The former sought to employ IBE as a means to effectively assert truth and realism in science, without which, they have argued, science could not be regarded as a truth-seeking and rational endeavour. The latter failed to see any connection between our "best explanations" and truth in science, arguing that such inferential practices are insufficient to establish any true states of affairs with regards to unseen entities. Thus, the issue under discussion will be whether IBE can be improved in such a way as to avoid some of the criticisms and objections to its use.

In this paper I will advance what I believe to be a more coherent formulation of IBE, which I have termed *inductive-IBE*, in hopes of demonstrating that we have very good reason to employ such inference in the context of a complete and rational science, as well as everyday enquiry. The central

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[1] Although C.S Peirce articulated certain principles, such as abduction, which are necessary to to the theory of IBE, the term was coined by Gilbert Harman in his 1965 paper, "Inference to the Best Explanation," *The Philosophical Review*, 74.1 (January 1965).
[2] Lipton, Peter. *Inference to the Best Explanation.* London: Routledge, 1991, 1. Henceforth: "Lipton."
[3] Harman, Gilbert. "Inference to the Best Explanation." *The Philosophical Review* 74.1 (January 1965). Harman didn't actually coin the term IBE, but he introduced it into the literature.
[4] Proponents include Peter Lipton as discussed in his text *Inference to the Best Explanation* (1991), Gilbert Harman in his paper "Inference to the Best Explanation," (1965), and others such as Stathis Psillos and Igor Douven.

issue under discussion here is whether IBE can and should include induction as an essential constituent component, and if doing so can improve its ability to handle its opponent's objections. Of these opponents, van Fraassen is the most significant, and I will be addressing his critique in this paper. To do so, I will consider a variety of philosophers, and ultimately propose a combination of Peter Lipton's and R.A Fumerton's[6] positions, arguing that IBE contains many significant inductive components and that, for this reason, any conclusion born from it should also be treated as an inductive conclusion, that is, as a likelihood. I will then argue that van Fraassen's opposition to the employment of IBE is, in part, reducible to the problems with induction in general, and that his objections are vitiated, or at least assuaged, as a result of my formulation. This formulation, called *inductive-IBE*, will be expressed as follows: IBE is a method of inference whereby we infer that the best explanation is likely to be true.

In the first chapter I will introduce induction and deduction, as well as C.S Peirce's pragmaticism[7], in order to establish the nature of the debate. In the following chapter, we will familiarize ourselves with the different approaches to IBE including Lipton's, Harman's, and Fumerton's, as well as my own formulation, *inductive-IBE*. This will establish the various attempts to account for IBE, and I will outline why I believe my own formulation to be superior. In Chapter Three, I will demonstrate how, specifically, *inductive-IBE* can help to mitigate van Fraassen's objections to the employment of IBE. The fourth and final chapter will be devoted to the virtues and problems of my formulation.

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[5] Detractors include Bas van Fraassen who critiqued IBE in *The Scientific Image* (1984); *Laws and Symmetry*, (1989); as well as James Ladyman and other anti-realists, who will not be addressed in depth in this paper.
[6] Specifically, his paper, "Induction and Reasoning to the Best Explanation," *Philosophy of Science*, 47.4 (December 1980): 589-600
[7] Peirce, Charles S. *Collected Papers Volume V: Pragmatism and Pragmaticism*. Cambridge, Mass: The Belknap Press of Harvard University Press, 1935, in which Peirce's pragmatism forms the basis for IBE.

Chapter 1

Scientific Inquiry and Inductive Inference

In this chapter, I will briefly introduce induction, deduction, as well as Peirce's position on IBE. This will provide a useful framework from which to begin our discussion on IBE in the following chapter. I will explain here why induction is a problematic basis for scientific theory, and how Peirce's conception of *abduction* more accurately describes our current practices. I will also explain that induction, because it yields likely conclusions, will aid us in formulating *inductive-IBE*.

1.1 Induction, Deduction, and the Scientific Method

To begin, we will need to set out what exactly is meant by induction and deduction, and how we are putatively thought to employ these methods of inference. It will then be made clear, in the forthcoming section on Peirce, that there is yet another type of inference termed *abduction*, which is more appropriate in describing the scientific practice than is simple induction.

Firstly, deductive inferences are arguments in which the conclusion is guaranteed by the premises, (in most cases). In this instance, there is no new information added; rather, information is rearranged and, if this is done according to logical rules, this can be said to be truth-preserving.[8] These are purely logical, *tautological transformations* that leave nothing to empirical testing.[9] For the purposes of this paper, deduction can be thought of as forming part of Fumerton's theory of IBE.

[8] Salmon, Merrilee, *Introduction to Logic and Critical Thinking*, 5th edition, Belmont, CA: Thomson Wadsworth, 79.
[9] Popper, Karl. *The Logic of Scientific Discovery*, New York: Science Editions Inc., 1959, 28.

Inductive reasoning is generally thought to be the logic of science: we infer from singular statements and observations, to generalizations and theories.[10] This type of inference will be of highest importance in my argument, and will be referred to frequently. Inductive reasoning can take several forms, the most basic of which involves making a generalization based upon a sample, using extrapolation. Thus, if all *observed* swans are white, that allows the scientist to infer that *all* swans are probably white. The validity of such arguments depends upon other factors, such as sample size and other empirical factors. It is clear that this method of inference involves outside information, unlike the purely logical method of deduction.

Now, there are problems with employing this type of inference, *per se*, in science, according to Peirce and Popper. Laplace, explains Peirce, was of the opinion that affirmative particulars, such as observing more white swans, impart probability upon the hypothesis that "all swans are white."[11] This was generally thought to be the prototypical model of scientific discovery and, while one may be easily persuaded that an increasing number of affirmative observations can increase probability, this is only true to a very slight extent; probability might increase slightly, but no matter how many instances of white swans there are, we are still not justified in claiming that *all* swans are. This is because there is still the possibility that a recalcitrant particular, such as a black swan, exists; it has simply not been observed yet. This specific problem with induction can be applied to any instance in which we generalize from a sample because, no matter how many affirmative particulars may be observed, we cannot rule out the possibility of a recalcitrant observation simply by virtue of the inductive generalization.

Another problem with induction is elucidated by Peirce, who holds that induction cannot furnish us with new information. Like deduction, it deals with values and probabilities, he asserts, inferring from

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[10] Ibid, 27.

one particular sample, or number, to a larger, generalized value, but at no time does it bring us new information. This, he explains, is only brought about by abduction. "The only thing that induction accomplishes is to determine the value of a quantity. It sets out with a theory and it measures the degree of concordance of that theory with fact. It never can originate any idea whatever. No more can deduction. All the ideas of science come to it by the way of Abduction."[12] I will say more on this in the coming section devoted to Peirce's conception of induction and scientific inquiry.

Hume's Problem of Induction will also be important for the forthcoming arguments: this is a perennial problem for philosophers and scientists and should be familiar to most readers. David Hume first proposed the problem in his *Treatise on Human Nature*[13] as well as his *Enquiries Concerning Human Understanding*[14] which set out the basic difficulty with justifying the use of induction. We have no external justification for induction, apart from induction itself; that is, the only thing that can justify an inductive prediction or generalization is that induction has worked in the past. This itself is inductive.[15] To justify induction, we would have to assert that "objects in nature always continue to display similar causal characteristics," or that "nature acts uniformly across time," but we cannot assert the truth of these statements without appealing to induction. For example, how do I know the sun will rise tomorrow? I know this because it has done so in the past, supporting my belief that it will continue to do so. And how do I know it will continue to do so? Because it has done so in the past. One might object here that we know that the sun will rise, not because it has always done so, but because of the rotation of celestial bodies and the dissemination of light through the atmosphere. But

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[11] Peirce, Charles S. *Collected Papers Volume V: Pragmatism and Pragmaticism*. Cambridge, Mass: The Belknap Press of Harvard University Press, 1935, 104.
[12] Peirce, 90.
[13] Hume, David. *Treatise on Human Nature*, (Reprinted from 1739, ed. Ernest G. Mossner) Toronto: Penguin Books, 1969.
[14] Hume, David. *Enquiries Concerning Human Understanding And Concerning the Principles of Morals*" (Reprinted from the 1777 edition, eds. Selby-Bigge, L.A.) Oxford: Clarendon Press, 1975, §4.
[15] Ibid, §4, Part II, 37-9.

here again we must question whether we are justified in believing that such astronomical and scientific characteristics will continue to be exhibited. We only predict that they will because they have been exhibited in the past.

Thus, we can see that the Problem of Induction confounds the very basis of all scientific understanding and knowledge in everyday contexts. Hume explains that it is only through habit and custom that we are able to function properly in the world, for if we were entirely rational beings, we would not rely upon induction as a sound rule of inference.[16] We would not assume that fire will burn us or that the sun will continue to rise. This is a controversial conclusion, however, and I will not discuss it further.

Regardless, there are many problems with the logical application of induction in science and everyday situations. This is a very problematic type of inference but, nonetheless, it must be understood that it is also the most useful to us. Even though we may not be entirely justified in employing induction, it continues to produce results and is, thus, instrumentally indispensable. However, it is very important to note, at this point, that induction has a powerful characteristic that will be very useful for my argument in the coming chapters: any conclusion emanating from an inductive argument is likely to be true. Now, it has become clear that induction is not a strong logical inference for its conclusions cannot be certain, but nevertheless, inductive conclusions have continued to prove themselves to be accurate in most cases. To be sure, this statement, that inductive inferences have been correct in the past and that, therefore, they will continue to be correct, is an inductive statement and, hence, problematic. Either way, it is important to understand that induction does, notwithstanding its logical stigma, produce *likely* conclusions, and this will form the basis for my argument *contra* van Fraassen. We will return to these aforementioned concepts shortly.

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[16] Ibid, §5.

1.2 Peirce and Pragmaticism

Charles Sanders Peirce was the first to give a name to a method of inference that had been in use for thousands of years. He observed that there is an entire class of inference, which he called *abduction* or *retroduction*. This type of inference was the result of his pragmatic approach to the sciences in which a *hypothesis* is an inference to an explanation. That is, hypotheses and, indeed, any truths in science, are functions of our own powers of inquiry and scientific method. Thus, truth is accessible to us through our inferences. Peirce considered abduction to be the only way by which the scientist can arrive at new knowledge. That is, rather than pure deduction – which seeks only to rearrange, but not introduce, information – abductive inference allows us to function logically in scientific practice, granting us a framework by which to manage and include new information in a systematic and reasonable manner. Notice that, in deductive entailment, we assert that, "A entails B. A. Therefore B." Abduction works in the opposite direction, claiming, "B. A entails B. Therefore A." This is generally thought to be fallacious but, upon closer examination, and as will become clear, this method can actually build the base of a complete and coherent science.

Peirce was well aware of the strength of such abductive powers and attempted to account for them as pragmatic. "If you carefully consider the question of pragmatism," he explains, "you will see that it is nothing else than the question of the logic of abduction."[17] New knowledge is only created through abduction, and abductive validation is the process of asserting that a given hypothesis is the correct one if it is the best explanation.

So what exactly is Peirce's characterization of abduction? Well, this type of inference fastens explanation and inference in a novel way. Consider the following form:

[17] Peirce, 121

> The surprising fact, *C*, is observed.
>
> But if *A* were true, *C* would be a matter of course.
>
> Hence, there is reason to suspect that *A* is true because it provides the best explanation for the occurrence of C.[18]

Thus, we can see that A's being the best explanation for the occurrence of C makes it reasonable to infer that it is the case, according to abduction. To be sure, we must take into account all available information to reach this conclusion, but it is nevertheless a reasonable method that illustrates the importance of *explanatory hypotheses*, something which logicians recognized long before Peirce articulated his abductive theory.[19] As Peirce puts it, abduction is the process of "studying the facts and devising a theory to explain them."[20] Abduction is unique in that it makes use of explanatory considerations, rather than a strict move from antecedent to consequent; from premise to conclusion.

Moreover, abduction does not require justification, since it merely offers suggestions of things that *may be*.[21] To be sure, these suggestions are well-founded, calculated, and explanatory, but they are not bound by the same rules as are induction and deduction; they do not require the same level of justification that we demand from deduction and induction. "Deduction proves that something *must be*; Induction shows that something *actually is* operative; Abduction merely suggests that something *may be*."[22] This statement is not uncontroversial, but this *suggestive* nature of abduction will be an important component of my theory of *inductive-IBE* because, as we will see, I hold that abduction yields *likely* results. Thus, this likeliness is commensurate with Peirce's position that abduction is merely suggestive and not absolute or certain.

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[18] Ibid, 117
[19] Ibid.
[20] Ibid, 90
[21] Ibid, 106.
[22] Ibid.

Now, it is important to note here that abduction is very similar to IBE. The difference is that IBE is the modern version as characterized by the likes of Harman and Lipton. Lipton, for example, offers a full account of IBE and how it should be conceived, but at their root, Peirce's abduction and the modern forms of IBE are both explanatory inferences that yield hypotheses. The purpose of this chapter, therefore, was to introduce the basic logical inferences, and explain how Peirce characterized the original formulation of IBE. This will provide the background information necessary to understand the upcoming discussion. As well, this section has illustrated the distinction between induction and abduction. This will be important in the coming sections – especially in my own formulation, which includes elements of each.

Chapter 2

Various Approaches to Inference to the Best Explanation

In this chapter, I will be expounding IBE by discussing Lipton, Harman, and Fumerton's positions, as well as "loveliness," scientific realism, theories of explanation, and the relation between inference and explanation. I will also explain the benefits of IBE, as conceived of by Lipton, and conclude that these benefits provide ample reason to accept IBE as a reasonable and fitting method of scientific inference. Finally, I will formulate a conception of IBE as a combination of both Fumerton and Lipton's notions and position myself to address van Fraassen in the third Chapter. I call this formulation *inductive-IBE* to highlight the inductive element contained within IBE.

2.1 Lipton's Inference to the Best Explanation

Now that abduction has been explained, we are better positioned to examine IBE. Firstly, we must be aware that the two are essentially the same; the difference is that IBE is the modern version put forward by Harman in his 1965 paper "Inference to the Best Explanation."[23] I focus on this version for the remainder of this paper. I will first explain Lipton's conceptions of IBE, followed by an explanation of its virtues. We will also examine the most salient feature of the current debate concerning IBE: its ontological implications concerning real entities, given that explanatory hypotheses in IBE often include terms that purport to refer to entities that are not mentioned in the descriptions of the observed phenomena needing to be explained.

So how does IBE work, and why is it so important to our science? Well, IBE is an ampliative inference that goes *beyond* what can logically be inferred from a set of data. According to Thagard,

"…inference to the best explanation consists in accepting a hypothesis on the grounds that it provides a better explanation of the evidence than is provided by alternative hypotheses."[24] Thus, rather than a simple, linear inference from premises to conclusion, IBE makes a leap from a set of data to the assertion of a true explanation, by way of what Lipton calls *loveliness*. I discuss this concept in the next section. As mentioned in the preceding chapter, IBE functions in the opposite direction to deduction for, rather than A necessarily entailing B, we assert A because it explains B, given that B is the case. In basic deductive logic, this is known as the fallacy of affirming the consequent,[25] but since IBE, as explained by Peirce, does not need to adhere to any strict deductive schema, it is free to make such a deductively fallacious maneuver. The validity, rather, is found in the acceptability of the possible explanation and is contingent upon external factors and explanations. Thus, we utilize an explanatory feature to prop up our inference, and it is this unique feature that allows abduction to give us new information.

IBE is more than a simple device for postulating *possibilities* and *suggestions*; rather it selects the *best* of all available explanations, going beyond mere speculation, and postulating truth and entities.[26] Thus, we can infer that A is the explanation for B because it is the best, given all the available evidence. So we can see emerging the unique attribute of IBE over other forms of inference: IBE is in fact guided by our explanatory powers, and not simply entailed in a strict logical formula. This is a powerful model by which to propose hypotheses, and this is what allows us to obtain new information, as Peirce indicated.[27]

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[23] Harman, Gilbert H, "Inference to the Best Explanation." *The Philosophical Review*, 74.1 (January 1965).
[24] Thagard, Paul. "The Best Explanation: Criteria for Theory Choice." *The Journal of Philosophy* 75.2 (February 1978): 77.
[25] Salmon,315.
[26] Lipton, 56
[27] Peirce, 121.

Let us look at some basic examples: the detective infers that the butler did it because this is the best explanation of the data before him. The doctor infers that his or her patient has the measles because this is the best explanation of the symptoms. The astronomer infers that there exists an entity called a black hole, because it is the best explanation for the astronomical data observed.[28] One of the most significant examples of IBE at work is Darwin's theory of evolution, in which he proposed that evolution provides the best explanation for the various phenomena observed in the natural world.[29] These examples are important for our purposes, for they illustrate IBE's ability to postulate theories or unobservable entities. Thus we can see a move from best explanation, to true explanation. This truth-seeking quality is unique among other methods of inference because it makes use of contextual considerations, rather than strictly logical considerations. That is, although all inference is truth-seeking, IBE must be conceived of differently for the reason that it includes notions of best-ness, likeliness, past success, etc. To be sure, we can be mistaken about such inferences, either because of misleading evidence, insufficient information, or a mistake in logical form, and van Fraassen's critique of IBE is based, in part, upon these shortcomings. However, it can be argued, that with the right "pool" of potential explanations, with adequate information, research methods, and good reason, we may be justified in using IBE to make this "educated guess," or leap, from best explanation to truth. Ontologically speaking, IBE allows us to move from *inferring* that a proposition is the best hypothesis, to asserting the existence of the proposed entity or the truth of the theory, as was seen in the example of the black hole.

The concepts underlying IBE have been remarkably useful as well, having allowed science to progress to an extraordinary extent. It is not difficult to see that IBE is an important tool for both scientific endeavours and common use, without which we would, arguably, have a more shallow

[28] Lipton, 56.
[29] Thagard, 77.

ontology. There is a distinct utility to this form of inference and, though the philosophical aspects of it are hotly contested, this usefulness is not. It can be argued that much scientific progress would not have been possible without the employment of IBE and, furthermore, without the ontological ability to postulate true theories and existing entities. I concede that this assertion is subject of debate, for both the realist and the anti-realist can account for IBE's success, as will be discussed later. van Fraassen, for example, claims that IBE can only lead us to empirical adequacy, whereby the conclusions are adequate for our scientific purposes, but say nothing of true states of affairs. It must be clear that this paper is not concerned with advancing instrumentalist reasons for the acceptance of IBE, but such utility must nevertheless be pointed out in any exposition on IBE. Moreover, the realist asks, how could science claim to be a rational pursuit if it didn't assert the truth of its theories or the existence of its entities? To be sure, past success is not indicative of truth, especially when considering Hume's problem of induction, which will be explored later, but there is certainly a sense in which "helping ourselves" to such unobservable entities, by way of IBE, has proven utterly indispensable to our science.

2.2 The Role of Loveliness and Truth in IBE

Now that the nature of IBE has been established, we can turn to Lipton's conception of "loveliness." This term was coined by Lipton and encompasses a variety of considerations that, when combined, produce IBE's ability to make the move from *best* explanation to *true* explanation. These considerations came about in response to Mill's Method of Difference in which the true explanation lies in the differences in the causal histories of the potential explanation.[30] That is, "a cause must lie in the differences between a case where the effect occurs and an otherwise similar case where it does

[30] Lipton, 117.

not."[31] Now, there are many problems with Mill's principles as outlined by Lipton, and I will not touch upon them here. Regardless, the considerations of loveliness, which I will outline below, are a vast improvement over the problematic difference principle expounded by Mill. The correct explanation lies not in Mill's differences, but in Lipton's conceptions of loveliness, argues Lipton.

Notions of loveliness are supposed to bring us understanding of scientific theories and explanations and these notions can, in turn, be indicative of an explanation's truth value. Loveliness speaks not only of understanding, but can also allow our inferences to be truth-tracking, according to the proponent of IBE. To paraphrase Lipton, we need an account of likeliness that is accessible to us, and, since likeliness is not directly accessible to us, we must examine the symptoms of likeliness: that is, loveliness.[32] Loveliness, therefore, is indicative of likeliness. Loveliness must here be distinguished from *likeliness*, although, ultimately, we will see that the two can converge or diverge at different points. Likeliness, clearly, is directly indicative of truth; the more *likely* an explanation is, the closer it is to being true. Loveliness, on the other hand, provides us with *reasons* to hold that an explanation is likely, and also indicates that the proposition has the potential to offer greater understanding.[33] Let us now examine the characteristics of loveliness, as proposed by Lipton, so as to better understand this correlation.

According to Lipton, the first characteristic of a lovely explanation is simplicity: the simpler an explanation, the more conducive it is to our understanding. This is fairly easy to comprehend because a simpler explanation contains fewer, less complex, propositions and we are more apt to grasp an explanation with such qualities. However, simplicity becomes truth-indicative when we consider that the fewer assumptions and inferences that must be made, the less possibility for error. This is similar

[31] Lipton, Peter. "Contrastive Explanation and Causal Triangulation." *Philosophy of Science* 58 (1991): 692
[32] Lipton, 62.
[33] Ibid, 59.

to Ockham's Razor[34] where simplicity is often the mark of a true theory or explanation. Thus, the simpler explanation is not simply more lovely, but it is also more likely. This is a point of contention for van Fraassen, whose objections I examine in the next chapter.

The next consideration is success and instrumental value. Perhaps an explanation is lovely because it has worked and proven itself useful in the past. Such success provides understanding because it allows an explanation to cohere with our background knowledge and thus produces familiarity by applying past experience to current explanations. Moreover, past success is also indicative of likeliness because, given the same antecedent conditions, it seems likely that a past event will repeat itself in a similar manner, increasing likelihood and the potential for truth. Thus, past success increases likeliness as well as loveliness and understanding.[35]

Finally, we will consider unification. We are more likely to accept and understand a theory with greater explanatory breadth rather than one that explains only a slight number of phenomena. Thus, a lovely explanation would, ideally, have the ability to account for a multitude of phenomena and, as a result, would be simpler as well. It is no stretch to see how unification can increase loveliness by giving us greater understanding, and it can also give us likeliness. This is brought about for the same reason that simplicity brings us likeliness. According to Friedman, "a world with fewer independent phenomena is, other things equal, more comprehensible than one with more."[36] Moreover, the fewer

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[34] Ockham's Razor is a tenet proposed by William of Ockham (c.1285-1349), which states, *entia non sunt multiplicanda praeter necessitatem* or: entities are not to be multiplied beyond necessity. This tenet has been employed by reductionists and nominalists to express the view that an explanation need not be made complicated because the simplest is often correct, or that the simplest explanation is often the best. From: Blackburn, Simon. *Oxford Dictionary of Philosophy*, 2nd Edition," New York: Oxford University Press, 2005.

[35] It should be noted here that this is in itself an inductive inference. That is, Lipton is using enumerative induction to make the claim that past success (a sample) is *likely* to repeat itself (a generalization). Thus, as we will see shortly, Lipton disagrees with Harman with regards to induction and its role in IBE. According to Lipton's loveliness, and indeed, his account of IBE, enumerative induction and IBE are, in fact, distinct inferences, and induction is indeed required to justify IBE.

[36] Friedman, Michael. "Explanation and Scientific Understanding," *The Journal of Philosophy* 71.1 (January 17, 1974): 15.

propositions taken into account, the less chance of superfluous assumptions and predictions making their way into the phenomena to be explained. And finally, the testing of a unified theory is facilitated when approached from many different angles because there are more instances of the theory in the observable world, as opposed to a multitude of different, unrelated phenomena needing to be explained under different theories. For example, Newton's theory of gravitation can be observed in many areas of research and, thus, confirmed in a variety of manners.

The above-outlined considerations combine to form Lipton's concept of "loveliness" and are, as explained, intertwined in such a way as to provide understanding *and* likeliness. However, an explanation can be likely without being lovely, and the opposite is also the case. Consider an example: it is very *likely* that opium puts one to sleep because of its dormative powers, but, as Lipton explains, "this is the very model of an unlovely explanation."[37] A conspiracy theory, on the other hand, can be lovely because of its ability to unify a multitude of seemingly unrelated phenomena in an elegant and simple manner. But we cannot claim that this explanation is *likely*. Now consider Newtonian theory: this theory was once held to be both lovely *and* likely because of its powers of unification, unifying many putatively-unrelated phenomena. However, with the advent of Einstein's theory of Special and General Relativity, Newton's theory has become *less likely*, but arguably no less *lovely*. (In fact, many argue that it is more so than relativity theory). Thus we can see, although loveliness and likeliness are often related, they can diverge at different points. Nevertheless, it is safe to say that, when duly considered, the two provide support for making the leap from *best available explanation*, to postulating real entities and true theories.

[37] Lipton, 59.

Throughout the last two sections I have given an overview of the salient features of IBE, as categorized by Lipton,[38] as well as Paul Thagard.[39] To be sure, there is more to discuss in this matter, including Lipton's conception of causal inference and contrastive explanation, but an examination of these is outside the scope of this paper. What I wish to do now, rather, is to outline some of the benefits of IBE and how they relate to scientific realism. This will be followed by a discussion of Gilbert Harman's position regarding IBE and enumerative induction so that we may be in an adequate position to analyze both Lipton and Harman's positions in section 2.4.

2.3 Scientific Realism and the Benefits of Employing IBE

At this point we should have a clear idea of IBE, and it should be understood that it is a useful and important scientific tool. Furthermore, as indicated, Peirce holds that this type of inference is indeed the only one that can bring us new knowledge of the world, for it allows us to step outside of the rigid form of deduction and induction, and consider other information that might hold explanatory value. It is this step, however, that is in contention, and I will explain some of the virtues of IBE, as I see it, in this section.

It seems that, to be a proponent of IBE, one must be open to a realist account of science. That is, one must be willing to admit some form of physicalism or realism if they are to accept the non-instrumental benefits of IBE. In a 1993 paper, Nicholas Maxwell put forth a theory called *aim-oriented empiricism*, claiming that, in order to accept such a theory, "science must presuppose, even

[38] Lipton, Peter. *Inference to the Best Explanation*. London: Routledge, 1991, 56
[39] Thagard, Paul. "The Best Explanation: Criteria for Theory Choice." *The Journal of Philosophy* 75.2 (February 1978): 77.

in the context of justification, that the universe is comprehensible in some way or another."[40] Accepting this proposition would be to hold that there are, in principle, explanations that can account for phenomena, such as causal histories or general theories; we may not have access to sufficient information regarding such phenomena, but there is nevertheless a *truth* of the matter. This basic assumption is also held by Lipton who claims, "I am assuming that a goal of inference is truth, that our actual inferential practices are truth-tropic, i.e. that they generally take us towards this goal, and that for something to be an actual explanation, it must be (at least approximately) true."[41] Thus, in order to give IBE a reasonable consideration, whether one is pro- or contra-IBE, is to delve, if only slightly, into the realist/anti-realist struggle. Although I take it that this basic realist assumption is not acceptable by all, I argue that most could at least entertain it, and this provides a good basis for examining IBE in a somewhat charitable light. To hold the strict anti-realist view that there is no truth of the matter would surely put an end to our discussion about the merits of IBE. Holding such a position would immediately eliminate any need for truth-tracking methods of inference, because all the anti-realist must do is look for van Fraassen's "empirical adequacy," or some other instrumentalist justification, while saying nothing about truth. What I wish to do in this section is superficially examine the debate and make it clear that IBE, working upon the assumption that we are open to realist, is a strong method of inference.

If the aim of science is indeed to arrive at truth, then the inferences utilized in science must reflect, and further, this basic aim. In my view, IBE is commensurate with this aim, fitting nicely with a realist model of scientific progress, allowing the scientist a certain amount of ontological "room." This allows the scientist to assert not only the possibility of an entity, but the actual *existence* of an entity and this is arguably the first step toward an intelligible science. Now, van Fraassen does not

[40] Maxwell, Nicholas, "Induction and Scientific Realism: Einstein Versus van Fraassen Part One: How to Solve the Problem of Induction." *British Journal for the Philosophy of Science* 44.1 (March 1993): 62.

actually agree with the position that IBE is required for science to be rational, and I discuss this in Chapter Three. I simply wish to explain that realism is the basic assumption that we must make in order to accept the benefits of IBE in science. IBE is an important step that we must allow the scientist to take; it allows the scientist to make ontological *suggestions* that may or may not be revised in the future. It is to give such entities the ontological "benefit of the doubt" by subjunctively asserting their existence before we are utterly certain. This allows science to progress toward some sort of *end* or goal, i.e., truth, without having the truth to begin with. That is, if the aim of science is to arrive at the truth, but there are many truths which cannot be ascertained with certainty, then the next best thing is to grant truth to our best hypotheses and work from there, in the hopes of eventually confirming them. This subjunctive property of IBE has been advocated by Lipton, and I will discuss it later. What I argue in this paper is that, if we are to be realists about science, then we would do well to accept IBE as a viable option for inquiry. Moreover, my formulation is wholly commensurate with this truth-tropic aim of science.

It must be clear that IBE can be rejected by the realist as well, for even a realist might not wish to concede that our best explanations are true. The purpose of this paper is, therefore, to find a formulation of IBE towards which the anti-realist would be more charitable, while still being attractive to the realist. As we will see, this will entail moving away from the realist proponent of IBE, and accepting van Fraassen's arguments as important considerations. That is, I will move slightly closer to the anti-realist than would the typical formulation of IBE. I will argue, in Chapter Three, that my *inductive-IBE* formulation can address objections contra IBE and lead us to a less problematic method of scientific inquiry.

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[41] Lipton, 59.

IBE requires realism as an initial assumption, but it also supports realism. In other words, IBE must begin with openness to realism, but it can also serve as an argument for accepting realism. Firstly, this is because the act of simply employing IBE asserts that the scientist believes that there is indeed a truth for which we are searching, regardless of whether we are correct in our hypothesis or not and whether we can ascertain the truth or not. IBE, therefore, is not concerned with that which is empirically adequate and that which fits nicely into our scientific model; it is not compatible with a fictionalist or instrumentalist account of science. Secondly, consider Lipton's position where he states that IBE can be used to support arguments for realism: in response to the skeptic, IBE entitles us to "believe in the external world since hypotheses that presuppose it provide the best explanation of our experiences." [42] It is possible that it is all a dream, but these are *less good* explanations of our experiences, so we are rationally entitled to our belief in the external world. And finally, Lipton asserts that later theories tend to have better predictive power than earlier theories, and that the best explanation for this improvement and progress is that the later theories are *closer to the truth*. He argues that we ought to infer scientific realism as the best explanation because we are clearly making predictive progress by coming up with better approximations of the world's true state of affairs. The anti-realist must account for this progress. This discussion is beyond the scope of this paper, but it should be clear that, as an advocate of IBE, I hold an openness to the realist assumption. I do not wish to argue *for* realism, because this is another undertaking altogether. Rather, I hope to elucidate the nature of IBE's relationship to realism, and how it presupposes realism as well.

At this point, it is important to indicate that there are two types of anti-realist with respect to IBE. The first type denies, outright, the ability of IBE to establish the truth and existence of entities, such as mice in the wainscoting and the fact that *the butler did it*. This is a difficult position to maintain for it seems almost absurd to disallow such inference to the existence of a mouse in the kitchen. That is,

[42] Ibid, 72.

if there are tiny footprints, crumbs of cheese, mouse droppings, and a hole in the wall, then the best explanation is that there was a mouse present. And, if all the evidence is *as if there were a mouse in the kitchen,* then it is no stretch of reason to conclude that this is so. Indeed, as I will argue in the third chapter, any objection to this method is simply an objection to the inductive enterprise as a whole. There are always other possible explanations, (such as hoaxes or pranks being played upon the scientist), and this will be explained in the next section on Harman, but the same goes for all enumerative induction.

The second, weaker type of anti-realist includes van Fraassen, who holds that IBE can indeed be used to postulate mice in the kitchen and other *observable* entities, but not to postulate unobservables, such as black holes and electrons. Although this position is more difficult to argue against because it allows the use of IBE in certain circumstances, it also has its problems. For example, if we allow this argument form for a particular set of phenomena, (mice), then is it really such a stretch to apply it to unobserved phenomena, (such as black holes)? It seems, in this case, that the only difference is our own power of observation and familiarity. But perhaps we already have sufficient, pre-existing concepts that can adequately form and establish new entities. Perhaps, given what we understand regarding light, gravity, and x-ray radiation, black holes are not such foreign postulations, but rather the logical instantiations of our already-formed prior knowledge. If this is unacceptable, where does one draw the line between adequate knowledge and unobservable entities? I will not continue to surmise in this manner, except insofar as to say that it seems that there are many problems with such objections to IBE. As I will argue, many of these objections can be seen as problems with induction in general.

Finally, returning briefly to Peirce, we can see that his pragmaticism fits nicely into this realist/IBE model of scientific inquiry. Peirce is a type of realist, but accepts that many truths are unattainable in practice. He is also a naturalist, rejecting lofty metaphysics in favour of a down-to-earth conception of

inquiry based upon rigourous experiment and reason. Truth must include people and is found within our very practices of inquiry and, although we cannot attain certain objective *truths* of the world, they are nevertheless attainable *in principle*.[43] I contend that this pragmaticist model of truth and inquiry is entirely compatible and reconcilable with IBE. Abduction, that is, embodies Peirce's notions of inquiry because it begins with the same basic assumption: that truth is attainable *in principle*, and then postulates truth based upon *our own* methods of experiment and reason. Thus, if there is a truth, and if truth is indeed to be found within our own methods of science, then an *inference to the best explanation* would encompass this notion by taking all facets into account (experiment, empirical data, reason, causal history, &c.) and assert the *best* as being true.

So far, I have briefly touched upon IBE's association with realism, pragmaticism, and truth as a means to demonstrate that IBE heavily supports realism, and is heavily supported *by* realism. These arguments will be assessed more thoroughly in Chapter Four, but as a preliminary thought on the matter, perhaps a way to refute IBE would be to strongly argue for an anti-realist account of science, thus negating any need to employ truth-seeking inferences such as IBE. Perhaps our discussion on van Fraassen will illuminate this notion. Ultimately, I will propose a formulation of IBE that makes strong use of the concept of *likeliness* and induction.

Finally, could the anti-realist ever be persuaded that there is more to an explanation than empirical adequacy? Is an explanation simply a proposition that fits nicely into our fictionalist account of the world? For, if this is so, it seems arbitrary in that an explanation can be *anything*, as long as it is the best fit in our web of belief. This is a somewhat disquieting possibility. Nonetheless, I hope it has become clear that IBE and truth support each other, and that there is a complex interplay between IBE

[43] My acknowledgements go to Professor Cheryl Misak, whose talk at the Perimeter Institute for Theoretical Physics (May 20, 2008) provided information for this aspect of Peirce's realism.

and realism as well. I also hope to have demonstrated my position for the employment of IBE in science and will continue to argue along these lines in the third chapter.

2.4 Harman's Account of IBE and Enumerative Induction

Gilbert Harman introduce the term "IBE" to the literature in his 1965 paper "Inference to the Best Explanation"[44] in which he proposed that all enumerative induction is actually abduction or, as he called it, *inference to the best explanation*. I have already given an overview of enumerative induction, Peirce's articulation of abduction, and Lipton's account of IBE, so we are well-positioned to examine Harman's argument that "enumerative induction should not be considered a warranted form of nondeductive inference in its own right."[45]

Let us begin with a synopsis of Harman's principal argument. He argues that, even if one accepts enumerative induction as one form of nondeductive inference, one will have to allow for the existence of "the inference to the best explanation." Then he argues that all warranted inferences which may be described as instances of enumerative induction must also be described as instances of the inference to the best explanation. So, "either (a) enumerative induction is not always warranted or (b) enumerative induction is always warranted but is an uninteresting special case of the more general inference to the best explanation."[46]

Some examples from Harman allow one to better understand his argument: when the detective determines that the butler did it, he is reasoning that there is no other explanation which better

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[44] Harman, Gilbert H., "The Inference to the Best Explanation," *The Philosophical Review*, 74.1 (January 1965).
[45] Harman, 88.

accounts for the evidence at hand. Similarly, the scientist who infers the existence of subatomic particles reasons that they most adequately account for the observed data. These examples are obvious cases of IBE, but consider the following: if we infer that a witness to a crime is telling the truth, our reasoning includes several inferences: we infer that the witness says what he says because he *believes* what he is saying, and we infer that he believes what he is saying because he actually *did* witness the crime. Thus, even in the relatively simple case of witness testimony, there are many background inferences that must be implicitly made before we can assert that the speaker is telling the truth. Our confidence in the testimony would disappear, however, if we thought there was another explanation which more adequately accounted for the testimony. For example, the witness may be lying for personal gain.[47] Harman also explains that IBE has an advantage over enumerative induction because it exposes these intermediary assumptions, or lemmas, whereas induction conceals them and fails to account for them.[48] Thus, his theory purports to be more descriptive and illuminating.

Consider another example given by Harman: based upon an individual's external behaviour, we infer certain facts about his or her mental state.[49] If a subject touches a stovetop and recoils, exclaiming, "Ouch!" we infer that their mental state is that of *pain*. Now, if this were pure enumerative induction, we would simply accept that, since people have always recoiled from hot stovetops because of pain, that this is yet another case of such a phenomenon, and this is a fairly reasonable claim, assuming that conditions are similar in this circumstance. However, Harman makes the argument that we are actually inferring that the subject's behaviour comes from a mental state, pain, because this is the best explanation of the observation. The subject, for all we know, could actually *enjoy* such an experience, but still reacts by exclaiming ouch and pulling his hand away. Or

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[46] Ibid.
[47] Ibid, 89.
[48] Ibid, 91.
[49] Ibid, 89.

perhaps the stovetop is a trick stovetop and does not actually heat up. In this case, the subject would not be feeling pain, but would simply be *pretending* to be in pain. These alternative explanations may be absurd, but they illustrate that, logically speaking, there are *always* alternative hypotheses and explanations, regardless of the observed data. Now, granted that if there are always alternative explanations, no matter how absurd they may be, then we must nevertheless take them into account and deem that *one of them is the best*. Harman has attempted to demonstrate that putative enumerative induction is actually a string of implicit inferences, each with its own set of possible explanations, of which we choose the best. Ultimately, however, I will disagree entirely with Harman, and side with Fumerton and Williams.

To further clarify Harman's position, we must understand that enumerative induction tells us that an explanation, E, is correct because it has been so in the past, and that there is ample reason to assume that, under similar conditions, E will hold in the future. Harman's account, conversely, tells us that these cases actually indicate that E is the *best* explanation, and not just a universal generalization of an observed sample. This is similar to Peirce's position on induction where he states that induction is simply a strict inference of numbers, in which we infer from a small number, or sample, to a large generalization.[50] Notice, however, that Peirce and Lipton differ from Harman with regards to the distinction between induction and abduction. At the outset, it may seem that Harman makes no distinction, where Lipton and Peirce certainly do. However, I argue that Harman, conceptually, accepts the distinction, yet he argues that induction *reduces* to IBE. He makes it clear that he is not refuting the existence of induction; rather, he hasn't yet found an instance of induction that cannot be reduced to IBE. Thus, Harman too accepts the distinction, but claims that all induction is IBE, in practice.

[50] Peirce, 90.

Finally, let us look at the example of white swans in order to solidify Harman's point. This is supposedly a very pure instance of enumerative induction where a scientist will conclude that all swans are white, because all the observed swans were white. In this case, a sample of white swans provides increasing support for the hypothesis that all swans are white. However, Harman would contend that this is not the way we reason. We actually reason, he would hold, that *all swans are white* is the best explanation for the observations because there could have been other explanations: for example, the white swans could have been a cruel trick played by the scientist's friend whereby all the black swans were intentionally removed from the sample area. To be sure, this is indeed a possible explanation for the phenomena, but IBE will tell us that it is not the *best* in this circumstance. Similarly, in the case of the mouse in the wainscoting, the crumbs of cheese, the footprints, and the hole in the wall could have been a trick played by my mischievous neighbour, but it is more than likely that there was, in fact, a mouse. Thus, there are always alternative hypotheses and, although they may be absurd, Harman's account attempts to demonstrate that, nevertheless, we choose the *best* explanation to be the true explanation, rather than simply generalizing based upon a sample.

Now, Harman makes it clear that he has not, and cannot, conclusively refute the possibility that enumerative induction is used. There may, in fact, be a legitimate use of induction that does not reduce to IBE. However, he has instead attempted to demonstrate that, *as far as he can tell*, enumerative induction is reducible IBE, and this is meant to shift the burden of proof to those who defend induction in this way.[51] Now, it is an open debate as to whether Harman is correct in his claim that all enumerative induction is IBE, for there may well be some special cases where this is not true. However, it seems that in *most* of the typical cases, this argument can be made. (But as well will see shortly, I do I not agree with Harman's position.) He concludes this branch of his argument by asserting that, if we *are* to use enumerative induction, we must employ some other sort of

nondeductive inference, IBE, to justify our claim. However, the virtue of IBE is that no such requirement need be placed upon IBE. That is, if we employ IBE, we do not need enumerative induction as a separate form of inference because it would be superfluous.[52]

Let us briefly explore this facet of Harman's argument. Enumerative induction, he argues, is of the form "All observed A's are B's, therefore all A's are B's, (or the next A will probably be a B.)" In practice, there is always more information available than the mere fact that *all A's are B's*, and we must take the totality of evidence into consideration, he says. Thus, depending on the evidence, we are sometimes warranted in making our inference, and sometimes we are not; so the question posed by Harman is, under what conditions is one permitted to make an inductive inference?[53] Induction, he argues, would provide an unsatisfactory answer to this question because of its narrow scope, whereas IBE is able to give us an account of when we have a warrant. For this, Harman introduces notions of simplicity and plausibility, claiming that, "one is warranted in making this inference whenever the hypothesis that all A's are B's is (in light of all the evidence) a better, simpler, more plausible (and so forth) hypothesis than is the hypothesis, say, that someone is biasing the observed sample in order to make us think that all A's are B's." He then claims that if, on the other hand, the total evidence indicates that some other explanation is more plausible, simple, &c, then we are warranted in accepting that hypothesis instead.[54] This is similar to Lipton's more developed notions of loveliness and likeliness, which we examined in section 2.2 of this chapter.

To conclude this section, I have outlined Harman's position on induction and IBE. I will proceed now to argue that it is problematic and should be rejected based upon considerations from Fumerton and Williams. Upon my rejection of Harman, I will formulate my modified version of IBE.

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[51] Ibid. 90.
[52] Ibid.
[53] Ibid.
[54] Harman, 91.

2.5 R.A Fumerton's Analysis of IBE and Refutation of Harman

R.A. Fumerton's paper "Induction and Reasoning to the Best Explanation" argues against Harman's claim that "there is a legitimate process of reasoning to the best explanation which can serve as an alternative to either straightforward inductive reasoning or a combination of inductive and deductive reasoning."[55] Fumerton contests Harman's position that all instances of induction are reducible to IBE and argues the opposite: all instances of putative IBE are reducible to induction. In opposition to Harman, Fumerton states "that when the suppressed premises are made explicit we have all the premises we need to present either a straightforward inductive argument or an argument employing both induction and deduction."[56] To illustrate this view, he argues that we always employ hidden premises when we infer to the best explanation, and that these premises are often of the form, "X is usually the case under a set of conditions, C." Thus, we are not inferring to the best explanation; rather, we are carrying out a set of inductive (and sometimes deductive) inferences that can account for putative IBE.

Let us look at one of Fumerton's examples: the best explanation for footsteps on the beach, says Fumerton, is that a human walked there. According to Harman, this would be an instance of IBE, because we are picking this explanation over any number of other possible explanations – identical robot or alien footprints, for example. Thus, the explanation that a human walked on the beach is true because it is better than any other possible explanation. Fumerton, conversely, argues that there are

[55] Fumerton, R.A. "Induction and Reasoning to the Best Explanation." *Philosophy of Science* 47 (1980): 590.
[56] Ibid, 589.

underlying premises, necessary to draw the inference, that Harman fails to acknowledge. In our example, this premise is that, "in the vast majority of known cases, footprints on the beach are caused by men." Therefore, under normal circumstances, it is likely that these prints were caused by a human unless we are given reason to believe otherwise.[57] This hidden premise inductively supports the conclusion and there is no need to employ problematic notions of "best-ness." To be sure, the problem of induction remains as problematic as ever, but this is not our concern here. Moreover, it is important to note, for the purposes of my main argument, that the conclusion of such an inductive inference is only *likely*, and not necessary.

Fumerton also addresses notions of loveliness, and demonstrates how these notions are inductively justified. To do this, he takes Thagard's conceptions of *consilience*, simplicity, and analogy, (the latter will be explained in the next chapter). For our purposes, these can be taken to be similar to Lipton's conception of loveliness. Fumerton states that, "if employment of such criteria is justified, the justification is inductive, i.e. rests on our independently determining that, for the most part, correct theories satisfy these criteria."[58] That is, the only way we know that simplicity and explanatory breadth are indicative of a "good" theory is because this has been the case in the past.

Therefore, it seems that Fumerton's position on inferring to the best explanation is as follows: IBE is actually just a string of inductions (and perhaps deductions), and there is no need to employ problematic notions of better and best. Moreover, notions of loveliness are also inductively justified. That is to say, they require a "meta-level" of justification in order to function properly for IBE. Therefore, IBE is heavily, if not entirely, dependent upon induction. In my own formulation of *inductive-IBE*, I will specifically outline the details.

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[57] Ibid, 592.
[58] Ibid, 597.

One question that may be raised is the following: why is Fumerton's analysis more compelling than Harman's? Harman claims that all induction is really abduction, whereas Fumerton claims that all abduction is really induction. But which approach is better? Why couldn't Harman simply reply that Fumerton's inductive premises are actually just the best explanations? And conversely, Fumerton could simply reply that Harman's best explanations are inductive by nature. Well, firstly, Fumerton explains that notions of best-ness and loveliness are, themselves, inductively justified. We are justified in trusting simpler theories, for example, because they have turned out to be successful in the past.[59] Thus, induction is required to justify our notions of best-ness and loveliness. Moreover, van Fraassen has made it very clear that there are severe problems with notions of best-ness and loveliness, and these will be discussed in depth in the third chapter.

However, there is another important argument to be made that illustrates the attractiveness of Fumerton's position that IBE implicitly relies on induction. Consider the following example: the scientist, who infers that all swans are white, based on a sample of white swans. Harman tells us that "all swans are white" is the best explanation because it is *better* than the explanation that the scientist's friend, for example, biased the sample as a prank. To make this manoeuver, Harman has employed various notions of best-ness and loveliness, but I argue that, in reality, this process is implicitly contingent upon induction, as Fumerton has argued. For consider the following modification: perhaps, in the past, the scientist's friend has frequently played tricks on the samples in order to bias the results. In this case, it would be no stretch to seriously consider this as an explanation, because of the inductive premise that this is a regular occurrence. I argue that Harman is, in fact, basing his approach upon inductive premises that are always implicitly considered. In the typical case of the swans, the premise upon which Harman must rely is that, "this sort of prank has never happened and will probably not happen, unless we have reason to suspect it." It is not notions

[59] Ibid, 596.

of best-ness that form our conclusions; rather, it is the conformity to inductive premises inherent in the IBE. Thus, it is clear that the conclusions borne from Harman's IBE hinge heavily upon inductive premises of the form described by Fumerton. I find this argument to be significantly more compelling than Harman's, and it is for this reason that I side with Fumerton in formulating my own version of IBE. The best explanation, I argue, is *precisely that* which inductively conforms to such tacit premises.

Finally, and perhaps most importantly for our position against van Fraassen, Fumerton aptly sums up his stance on unobservables, as well as my own view on the matter, in the following quotation:

> Reaching a conclusion inductively based on one's *entire* body of evidence usually involves countless inductive arguments, the premises of which confirm countless hypotheses to varying degrees of probability… What one is justified in believing with such an evidential framework depends on the result of weighing a great many probabilities. Though I don't really know, I suspect that the scientist has perfectly good *inductive* evidence justifying his belief in the existence of many of the theoretical entities to which he is committed (where the existence of such things is understood precisely as the realist would have us understand it.) *Though theoretic entities are themselves unobservable, their defining properties may be such that we have observation of other things having those properties*.[*sic*.][60]

I have included the above quotation in this section on Fumerton, but it will become clear in the third chapter as to why exactly it is significant for our intents and purposes contra van Fraassen.

To conclude, I argue that Lipton's notions of loveliness, as well as Thagard's similar notions of consilience, simplicity, and analogy, are only justifiable because they have proven correct in the past; they are indicators of successful theories, and are therefore inductively justified. We also see a compelling attack upon Harman's position for it seems that there is, in fact, a strong argument that enumerative induction is employed in IBE. Although Harman does make a distinction between

[60] Ibid, 599.

induction and abduction, he holds that all induction is really abduction, and I have argued that Fumerton's analysis, that all abduction is really induction and deduction, is more accurate. However, in my formulation I will preserve the abductive component of IBE, unlike Fumerton, and will illustrate this concept in the following sections.

2.6 Comparisons and Contrasts

We have now sufficiently familiarized ourselves with the key approaches to IBE from Peirce, Lipton, Harman, and Fumerton, as well as notions of loveliness, Likeliness, and truth. As a normative claim I have outlined the many benefits of employing IBE, and have sided with Lipton in that IBE is also descriptive of the manner in which we conduct inquiry. In this section, I will briefly conclude by outlining some differences between Lipton's and Harman's positions. I shall ultimately reject Harman's argument concerning enumerative induction and, rather, combine Fumerton and Lipton to formulate *inductive-IBE* in 2.7.

Naturally, Lipton gives a much more substantive account of IBE than does Harman. Presumably this is because, whereas Lipton's project was to fully expound and put forth a coherent account of IBE, Harman wished only to demonstrate that pure induction is actually IBE, without going into any great depth as to how exactly IBE should be cashed out. Thus there are many areas of indeterminacy where we cannot conclusively contrast the two positions and, I surmise, Harman would likely be in agreement with much of Lipton's "filling in the blanks." However, there is a very important dissimilarity with regards to the role of induction in the two philosophers' positions, for Lipton (and Peirce) both accept that induction is independently warranted in its own right, whereas Harman's position is that induction is reducible to IBE.

Lipton claims that IBE is an important inferential tool that is employed in science and everyday enquiry, but that it exists alongside other independent forms of ampliative inference, such as induction. Harman, conversely, claims that IBE is the basis for all enumerative induction; that (almost) all induction is reducible to IBE. Lipton does not touch upon this argument in his book, but it is clear, from his thorough articulation of IBE, that he holds that there is a sharp distinction between the two kinds of inferences, as does Peirce. Both philosophers advance accounts of abduction, or IBE, which make no reference to a Harmanian reduction of induction to IBE. That is, although they are not explicit in this regard, it becomes clear from these philosophers' works that induction is an entirely separate type of inference that is not reducible to IBE. In this section, I will argue that Harman's position is problematic, and that it would be beneficial to formulate a new conception of IBE that sits between Lipton and Fumerton.

In Harman's book, "Thought," he outlines his position with the Gettier example[61] of a subject who believes there is a candle twelve feet in front of him[62]. Indeed the subject is correct insofar as there is a candle in front of him, but he cannot be said to have knowledge because, as the example is described, the candle he is actually viewing is a reflection in a mirror of a candle behind him, (and there just happens to be another candle twelve feet in front of him, hidden behind the mirror). As mentioned, Harman accounts for this by use of lemmas, claiming that, "this Gettier example is accounted for if the reasoning instantiator ascribes to the man in question reasoning that infers that things look the way they do… because there is a candle twelve feet in front of him."[63] Thus, once this

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[61] A Gettier problem is meant to illustrate that the traditional analysis of knowledge (i.e. that knowledge is "justified true belief"), is insufficient to account for what knowledge really is. Such problems often include a subject who "knows" something accidentally. For example, I may know that there is a sheep in the field when, what I actually saw was a sheep dog. However, unbeknownst to me, there is an actual sheep behind the sheep dog. Thus, I cannot be said to have knowledge, and yet I have a belief that is justified and true. (Gettier, Edmund L., "Is Justified True Belief Knowledge?" *Analysis* 23 (1963): 121-123).

[62] Lipton, 60. Referring to Harman's example of the candle in the mirror from Harman's 1973 book, *Thought*, Princeton: Princeton University Press, 22-3.

[63] Ibid.

lemma is falsified, we can see why it fails to pass the knowledge test. That is, Harman is able to solve the Gettier problem by claiming that the subject employed a false lemma, and this is the reason he cannot be granted the status of *knowing* the candle was twelve feet in front.

However, as Michael Williams asks, upon what grounds do we select the appropriate lemma to ascribe to the subject?[64] How do we know what the subject *really thought*, and why could we not have used another lemma? For example, we could have ascribed, to the subject in Harman's example, the reasoning that, "he is generally a reliable observer, i.e., whenever there looks to be a candle in front of him, there usually is one; it looks as if there is a candle in front of him now; there is no reason to suspect any unusual circumstances; so there is a candle in front of him."[65] In this case it becomes clear that Harman is basing his lemmas and, thus, his theory of IBE, upon assumed, almost arbitrary, grounds. The above inference given by Williams involves no false lemmas, and Harman would be happy to accept this as a warranted inference in other contexts, but he would be hesitant, presumably, to accept that the subject who reasons in this way would *know* that there is a candle in front of him.[66]

One may disapprove of Williams' account by asking how it is more compelling than Harman's. Harman may also object here by claiming that his own lemma (that things look the way they do because there is a candle in front of him) is simply the best explanation, and that Williams' lemma (that the subject is generally a reliable observer) is not. On this reasoning, Harman's lemmas are not arbitrary at all and, rather, they are simply the best explanations. I do not accept these objections, however. Williams' lemma, that "the subject is generally a reliable observer" is equally as good as Harman's explanation in this case. That is, why would we ever be hesitant to accept Williams' lemma under, what we assume to be, normal circumstances? And why would Harman's lemma, (that

[64] Williams, Michael. "Inference, Justification, and the Analysis of Knowledge." *The Journal of Philosophy* 75.5 (May 1978): 253.
[65] Ibid, 259.
[66] Ibid.

“generally when I see an object in front of me, it is so,”) be better? Indeed it does seem that the lemmas involved can be tailored to suite Harman’s argument and, for this reason, are somewhat arbitrary. Moreover, the issue is not whether Williams’ lemma is better; rather, the very fact that one can easily propose other, equally compelling lemmas is reason enough to cast some doubt upon Harman’s position.

This is important to my position on IBE because I hold that Harman’s premises that constitute “best” explanations are in fact dependent upon other inductive premises, such as Williams’ premise. As mentioned, my position is that Harman’s lemmas for best-ness are actually dependent upon inductive premises, and this is Fumerton’s position as well. Best-ness is a function of the inductive premises inherent in the inference, and I will explain how.

Consider Fumerton's position: I could easily label Harman's lemma, “that things appear the way they do because the object, X, is in front of the subject,” as an inductive premise. That is, Harman's lemma, in this case, is dependent upon the simple inductive premise that, “usually things appear a certain way, X, because they are a certain way, X,” and that, therefore, the best explanation is commensurate with this premise. In the same manner, Williams' lemma, that the subject is generally a reliable observer, can also be easily conceived as a premise in a simple inductive argument. Thus, I agree with Williams that Harman's lemmas are arbitrary. And not only are they arbitrary but it seems that they can succumb to Fumerton's formulation of IBE: they are reducible to simple premises in an inductive argument and, thus, should be treated as such. Harman’s premise is believed for inductive reasons, whereas Fumerton’s premise is a premise in an inductive argument. Either way, however, induction is required for justification. This serves to further support my own formulation of *inductive-IBE* whereby any inference to the best explanation necessarily contains a set of inductive inferences and/or premises. Also, recall my example, outlined above, about the scientist’s friend who often plays tricks upon the experiments. If this were a regular occurrence, then, inductively, a prank would be a

good explanation, even though it does not exhibit Harman's qualities of best-ness. Rather, it relies upon implicit inductive inferences.

Also consider Lipton's position where he often speaks of induction as a wholly separate type of inference. For example, he claims that IBE is better than induction because it accounts for the fact that we are often not willing to extrapolate from a given sample. Induction always extrapolates from sample to general cause, regardless of whether it is warranted or correct. *Per* Lipton: "Inference to the Best Explanation does not always sanction extrapolation, since the best explanation for an observed pattern is not always that it is representative."[67] Lipton allows for the use of pure induction in science as a separate form of inference and this is certainly a difference between his position and that of Harman. To be sure, Harman does allow for the distinction between induction and abduction, as does Lipton, but Harman does not believe there exists a case where enumerative induction is not reducible to IBE. Rather, he holds that all putative induction is reducible to IBE, and this is certainly at odds with Lipton.

Fumerton and Williams have highlighted important problems and objections to Harman's view that all enumerative induction is IBE. Moreover, Lipton, though not explicitly contesting Harman, is also of the view that there is a distinction to be made. Finally, we cannot forget Peirce who, like Lipton, maintains a sharp divide between pure induction and abduction. This is evident from our discussion in the first chapter. I will now conclude this chapter with the final formulation of IBE that we will carry into the next chapter on van Fraassen

[67] Lipton, 60

2.7 Inductive-IBE

I call this formulation *inductive-IBE* because I wish to highlight the fact that induction is a significantly constitutive part of IBE, as argued by Fumerton. From the above discussion, it should be clear that abduction is fraught with instances of inductive premises and that it requires inductive justification. I*nductive-IBE* sits between Fumerton and Lipton and holds that the term "IBE" is actually a combination of abduction (including Lipton's *inference to the best explanation* and his notions of loveliness), and Fumerton's set of inductive premises. My formulation of *inductive-IBE* is as follows: IBE is a type of inference that makes use of both induction and abduction. It contains a set of inductive arguments, as outlined by Fumerton, a well as notions of loveliness as characterized by Lipton. So not only is there a strong inductive component built into this formulation of IBE, but I have preserved the abductive and explanatory nature of IBE. The upshot, and this is how I will address van Fraassen, is that any conclusion born from an argument so heavily dependent upon induction is *likely*, at best. This likeliness will be the focus of Chapter Three.

However, there is a very important caveat of which to be mindful: it should be clear that my formulation of IBE should not be taken as a *new* variety of inference to the best explanation. That is, I am not proposing a new theory under which we should conduct our inquiry. Rather, I am attempting to elucidate a *descriptive* characterization of IBE that more accurately accounts for the manner in which we currently employ IBE. As mentioned, *inductive-IBE* is so called because it highlights the inductive nature of IBE. It must be understood that Lipton, and indeed other advocates of IBE, agree that IBE can be fallible, even when correctly applied. My formulation differs because it *accounts* for this level of certainty more adequately than do the other formulations; it makes it clear that, because of the inductive constituents inherent in IBE, this method of inference leaves a margin of error.

To begin, I will quickly demonstrate *inductive-IBE* using the example of the footprints on the beach. According to my formulation, a man walking on the beach is the best explanation of the phenomena because this explanation has been the case in the past and, under similar conditions, should be the case at the present. However, the employment of induction *per se* is insufficient, because there are many other considerations that must be addressed. For example, it could be the case that there is a better explanation that, while dependent on induction, also requires other notions of best-ness. For example, if a human-like robot has recently escaped from a factory, then straight induction would be insufficient because it would not tell us when to extrapolate. That is, it would ignore the contextual evidence that tells us when to generalize from a sample. Moreover, notions of explanatory breadth and simplicity are not addressed by pure induction either. Perhaps the best explanation is one which explains the disappearance of the robot *and* the footprints on the beach. I will not continue in this manner because it should be clear that *inductive-IBE* is composed of induction and notions of best-ness, simplicity, breadth, etc.

So how does my view differ from Lipton and Fumerton? Well, unlike Lipton, I admit that there must be inductive premises that play a significant role in IBE. Lipton, although he admits that IBE is fallible, says nothing about induction's role in IBE, and this is why I choose the term *inductive-IBE*: in order to highlight this important aspect. However, I do wish to appropriate Lipton's notions of loveliness, and there are a few reasons for this: firstly, without notions of loveliness, the very nature and essence of an inference to the best explanation would be lost. That is, without such explanatory considerations, we would be left with pure induction, and this is a different matter altogether. Now, I do not use induction here in a pejorative sense, but, as indicated, a problem with pure induction is that it does not tell us when to extrapolate.[68] There are other explanatory considerations that are required to make contextual, relevant, and well-informed inferences in scientific inquiry and I believe that

Lipton, and indeed proponents of IBE in general, are correct in placing value upon these explanatory notions. I certainly wish to preserve them in my formulation; however, they are not exclusively constitutive of IBE.

Unlike Fumerton, I do not believe that all IBE is reducible to induction. For Fumerton, all IBE is reducible to a set of inductive and deductive inferences, and this is precisely contrary to Harman's position. I differ from Fumerton because I allow for "Liptonian" explanatory considerations such as simplicity, analogy, breadth, *as well as* his inductive component. Fumerton essentially tells us that all IBE is induction, and I argue that IBE *contains* induction, but explanatory considerations as well.

Until now we have discussed how my position differs from Lipton and Fumerton. I will now explain why my formulation is more compelling. *Inductive-IBE* is better than Lipton's version because it accounts for the inherent inductive nature of IBE. That is, Lipton's theory does not account for the significant role of induction in IBE, when it certainly should. Thus, because Lipton's formulation is too heavily dependent upon explanatory considerations, loveliness, and best-ness, this opens himself up to criticism from Van Fraassen, as well as Fumerton. I have already outlined, in detail, exactly why induction is an important part of IBE.

Moreover, *inductive-IBE* does a better job of describing the manner in which scientists actually conduct their inquiry and inferences. Scientists do not assert the truth of theories and the existence of theoretical entities with certainty, but Lipton's account seems to indicate that IBE provides the scientist with a much more certain conclusion than is the case. *Inductive-IBE*, on the other hand, is clearly *less certain* in its conclusions, simply because it admits that IBE is heavily dependent upon induction. Now, many theories and entities, such as Evolution and black holes, can become absolute certainties after lengthy processes of experimentation, but I argue that theoretical postulations are

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[68] Ibid.

initially treated as likelihoods, or strong possibilities. Thus, my formulation is *less certain* than Lipton's because I allow induction to play a central role in the inference. This is a more accurate portrayal of the level of certainty exhibited in scientific inquiry.

Fumerton's conception of IBE is that it contains inductive inferences. I certainly agree with this proposal, but I take issue with the fact that he does not allow for other explanatory considerations. He states that, "when the suppressed premises are made explicit, we have all the premises we need to present either a straightforward inductive argument, or an argument employing both induction and deduction."[69] I argue, rather, than we should preserve the fundamental nature of IBE, as held by Peirce, as an inference guided by explanatory considerations. These considerations provide contextual relevance, and, as Lipton argues, tell us when to extrapolate. Induction, on the other hand, is often left unaffected by such important considerations. Peirce, as mentioned, argued that induction is simply a process of numbers in which we generalize from a sample, but this says nothing about the role of explanation in our inferences.

Thus, not only do I argue that we *should* employ such explanatory considerations, for the reasons listed above, but also that this is more *descriptive* of the manner in which scientists conduct inquiry. That is, scientists do not consider generalizations from samples, *per se*. Rather, they employ a variety of explanatory considerations and notions of loveliness as well, and my formulation better accounts for this than does Fumerton's.

It is for these reasons that I situate myself between Fumerton and Lipton, appropriating half of each philosopher's argument to create *inductive-IBE*. I believe that *inductive-IBE* better describes scientific practice, and that it better withstands Van Fraassen's criticism. Finally, my position is simply more reasonable than other conceptions of IBE. This is because it is simple logic that any conclusion born

[69] Fumerton, 589.

from a correctly executed inductive inference is *supported* and *likely*, but not a certainty, as it would be in a deductive argument. (Of course, deductive and inductive validity are dependent upon correct premises and valid argument forms. I am assuming that this is the case when I indicate that induction provides support, and deduction provides necessity).

Now, the upshot of all this is that the conclusions of such inductive inferences are, at best, only *likely*. I wish to argue that the same should be said regarding IBE: that inference to the best explanation can infer only a *likely* hypothesis. There are three reasons for this assertion:

i. As per Fumerton, IBE contains premises that are grounded in inductive evidence, which infer likeliness by supporting the conclusion.

ii. Abductive notions of loveliness are indicative of likeliness, but not certainty. They too provide support for the hypothesis or explanation.

iii. These notions of loveliness are, themselves, inductively justified. We know that simplicity and breadth, for example, are the marks of strong theories, because they have been so in the past. Once again, this inductive justification leads to likeliness, but not certainty.

Naturally, due to these overwhelming and significant inductive components, the conclusion of an inference to the best explanation should be a likelihood, and nothing more. Lipton would agree in this regard, because he too holds that IBE leads to *likely* conclusions. However, the reasons mentioned above demonstrate that this likeliness comes about by virtue of the inductive component inherent within the inference. Thus, by reformulating IBE from "an inference that the best of the available potential explanations is an *actual* explanation"[70] to "an inference that the best of the potential explanations is likely to be true," we can mitigate some of van Fraassen's objections to IBE, as well as formulate a more informative notion of this inference. Perhaps van Fraassen's concerns would be alleviated if IBE were put alongside enumerative induction in that its conclusions are *likely* and, thus, no worse off than most of our inductive inferences. To be clear, van Fraassen disputes the use of

induction as well, and this is indeed an important aspect to note. However, I wish to demonstrate that IBE should be no worse off, in the eyes of van Fraassen, than induction. Therefore, the result of my argument may not be a formulation that van Fraassen can agree with; rather I hope to propose a conception of IBE towards which he would be less hostile and which takes into account his objections with IBE.

I have already explained precisely how my formulation differs from Harman, Lipton, and Fumerton, but before I proceed to demonstrate how my *inductive-IBE* stance addresses van Fraassen's arguments, I will explain how I am at variance with other proponents of IBE. I differ from Lipton in the following manner: Lipton states that the best potential explanation is an *actual* explanation, whilst I hold that the best potential explanation is only a *likely* explanation. Thus, I go further and argue that even Lipton's characterization of IBE would make us too good at our inferential practices and epistemic access, and I propose a slightly weaker formulation of IBE. This stance may not be as well received by proponents of IBE, who wish to maintain IBE's ability to assert the existence of non-observables. However, my intention is to come to a formulation that is more readily acceptable to both advocates and opponents of IBE. Thus, I am at variance with proponents of IBE because they may wish to propose a stronger formulation of IBE. I will say more on this subject shortly.

To conclude this chapter, I have outlined the various approaches to, and formulations of, IBE, including my own. I have also outlined how each philosopher compares and contrasts in their views of the matter. Ultimately, I have rejected Harman's position and settled somewhere between Lipton and Fumerton. In the next chapter I will explain why my own approach is ideal. I will start by addressing van Fraassen's critique of IBE.

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[70] Lipton, 60.

Chapter 3

Van Fraassen and *Inductive-IBE*

To begin this chapter I will delve immediately into the important objections with IBE's ability to bring us new information and to postulate unobservable entities. I will then explain how my approach, *inductive-IBE*, mitigates the severity of these objections.

3.1 van Fraassen's Position on Inference to the Best Explanation

Regarding IBE, Bas van Fraassen asks, "Will this pattern of inference lead us to belief in unobservables? Is the scientific realist simply someone who consistently follows the rules that we all follow in more mundane contexts?"[71] Clearly, he has objections to this notion. van Fraassen considered IBE in his works including, "The Scientific Image" and "Laws and Symmetry." His basic misgiving with IBE is that there is nothing contained in the inference that justifies the ontological postulation of the existence of entities. Instead, he holds that IBE, when dealing with unobservables, can lead only to "empirically adequate" hypotheses, and that we don't necessarily care as to whether they are truthful or not.[72] To be sure, they have a truth-value, but this is beyond our scope and concern. This is what he calls *constructive empiricism*, and it states that explanations about observables are "true," but when dealing with unobservables, an explanation is only instrumental; it succeeds at *doing work*, but its truth component is irrelevant for our intents and purposes. According to constructive empiricism, theories and explanations about unobservables are only empirically

[71] van Fraassen, *The Scientific Image* Oxford: Clarendon Press, 1980, 20.
[72] van Fraassen, *The Scientific Image*, 18.

adequate. “A theory is empirically adequate,” says van Fraassen, “exactly if what it says about the observable things and events in this world, is true – exactly if it ‘saves the phenomena.’”[73]

In *Laws and Symmetry,*[74] and *The Scientific Image*,[75] van Fraassen argues against IBE’s ability to establish unobserved entities, claiming that there is no valid connection between ampliative inference and truth. Is the realist simply someone who follows rules of inference? he asks. Can such inference lead us to postulate unobservable entities?[76] We see that van Fraassen is attempting to deny any link between our rules of inference and real, but unobservable, entities. One such argument is what Lipton calls “Voltaire’s Objection.” It expresses van Fraassen's anti-realist position that we have no reason to suspect that the *loveliest* explanation has any bearing on truth. Empiricism states that our only source of information is through firsthand experience and, thus, any inborn or instinctive expectations are only “lucky if they lead us alright, and not like lemmings into the sea.”[77] For why should we believe that we inhabit the loveliest of all possible worlds?[78] (Notice, a realist too can be sympathetic to Voltaire’s Objection.)

Our abductive powers in science are certainly important, van Fraassen claims, but they do not lead us to a true picture of the way the world really is. Rather, when we employ such practices we can attain only *empirical adequacy*, because this is all such ampliative inferences are meant to do. Why go beyond the power of such inferences if all they do and, indeed, all they *need* do, is get us at an instrumental virtue of empirical adequacy? Moreover, considerations of loveliness and likeliness are not truth-indicative; they are simply indicative of empirical adequacy. Loveliness is, for all of science’s intents and purposes, and indeed for our everyday enquiry, adequate, but real entities are not

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[73] Ibid, 12.
[74] van Fraassen, Bas, *Laws and Symmetry*, New York: Oxford University Press, 1989.
[75] van Fraassen, *The Scientific Image*, Oxford, 198
[76] Ibid, 20.
[77] van Fraassen, *Scientific Image*, 144.
[78] Lipton, 74.

the sort of thing that can be postulated by way of empirical adequacy because, as Stathis Psillos puts it, empirical adequacy and truth do not coincide.[79] It seems difficult, in light of van Fraassen's objections, to accept that our own understanding and inference has any bearing on the way the world really is. This possibility would seem antithetical to the notion that truth is independent of us.

Furthermore, van Fraassen questions the "pool" of possible explanations from which we pick the best, asking how a true explanation can be guaranteed from a limited pool of explanations. Psillos calls this position the "argument from the bad lot,"[80] and I will use this term throughout this paper. The argument from the bad lot expresses a straightforward concern with our epistemic limitations, claiming that we cannot be justified in postulating truth simply because the explanation is the *best we currently have*, given our pool of possibilities.[81] van Fraassen's argument, in its essence, is this:

> Let us grant that scientists have effected an ordering of a set of theories $T_1,\ldots, T_n$, all of which offer potential explanations of the evidence *e* and that they have sorted out which is the best explanation of *e*, say T_1. In order for them to say that T_1 is the approximately true account of *e*, they must make 'a step beyond the comparative judgment that [T_1] is better than its actual rivals.' They must make 'an ampliative step.' This step involves belief that the truth is already more likely to be found within the lot of theories available to them, than not. But our best theory may well be the 'best of the bad lot.' So, in order for the advocate of IBE to argue that IBE leads to truth, he must *assume* the Principle of Privilege. That is, he must assume that 'nature predisposes us to hit on the right range of hypotheses.'[82]

That is, in order to assume that our best explanation is true, we must take an ampliative step beyond the "comparative judgment" that our theory is better than its rivals. Naturally, van Fraassen argues that this ampliative step is unwarranted. What if the true explanation was not epistemically available to us? What if our pool was incomplete? What if our science hadn't progressed to the point that it

[79] Psillos, 33.
[80] Ibid, 36.
[81] Ladyman, 306.

could provide sufficient information for potential hypotheses? In van Fraassen's view it is important to realize that IBE is only an inference to the *best* explanation *available* to us, but this says nothing about true states of affairs. That is, the fact that an explanation is the best one we have does not indicate truth; rather, it simply expresses the fact that it is indeed *our best explanation.*

To press this point further, having a *best* explanation is only a relative and comparative proposition, rather than a truth-indicative one. Our inference can only be deemed *good* or *bad*, (or *worst* or *best*), relative to the other explanations in the pool. Therefore, *best* only means *better than the other possibilities,* and to claim any sort of objective truth or to postulate the existence of an entity would be problematic, given that our best explanation is only *the likeliest of available explanations.* Now, these two points are quite simple to grasp. I concede that van Fraassen has illustrated some basic conceptual problems with the rigourous employment of IBE where the ontology of unobservables is concerned. To be sure, he certainly allows that IBE is important for science, and indeed allows for significant progress and advancement insofar as observable objects are concerned. But he cautions, on the basis of the preceding objections, against taking our inference too far.

van Fraassen also argues that, for any "best" explanation that we may come to, there may be many other equally good explanations that we have not discovered and that, because of this uncertainty, IBE should not be employed to indicate likeliness. Using Psillos' terminology once again, this is called the "argument from indifference."[83]

According to van Fraassen:

> Let us grant that we have chosen the theory *T* that best explains evidence *e*. A great many of unborn hypotheses inconsistent with *T* explain *e* at least as well as *T*. Only one theory, either *T* or one of the hitherto unborn theories, is true. All the rest are false. Since

[82] van Fraassen, *Laws and Symmetry*, New York: Oxford University Press, 1989. p 142-3.
[83] Psillos, 43.

> concerning *T* we know nothing with respect to its truth-value other than that it belongs to the (probably) infinite class of theories that explain *e*, we must treat it as 'a random member of this class.' But then we may infer that *T* is very improbable.[84]

This is similar to the "argument from the bad lot," in that it questions our epistemological access to the possible, available explanations, claiming that we are not in the privileged position of knowing *all* the possible explanations. van Fraassen is attempting to prove that, since there can be an infinite number of other, equally good, explanations, that our chosen theory is arbitrary and, in all likelihood, wrong. Thus, we are not warranted in postulating truth or the existence of unseen entities simply because it is our best theory; rather, we can only attain empirical adequacy and remain agnostic with regards to the truth.

I concede that these arguments cut quite deeply into the sustainability of IBE. However, there are many other considerations that mitigate these objections. Ultimately, the problems of IBE stem from deeper epistemological problems that stem from our inability to access true states of affairs. Basic difficulties such as the Problem of Induction, disagreements over theories of knowledge, the realism-anti-realism debate, and general skepticism all lie at the crux of our predicament. It is only by understanding the significance of these perennial problems that we can conclude that IBE is the best approach available if we are open to realism. Not only is this a normative claim about how we ought to conduct our science, but it is also a descriptive claim about the manner in which we currently conduct our science. And, given that we employ IBE in science all the time, with much success, Lipton's theory – that realism is the best explanation for our success – seems attractive. I do not wish to address this argument, however, because the realist/anti-realist debate is beyond the scope of this paper.

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[84] van Fraassen, *Laws and Symmetry*, 146.

3.2 van Fraassen and *Inductive-IBE*

In this section I will construct what I believe to be cogent arguments in support of IBE that alleviate van Fraassen's concerns. If one can argue that IBE is heavily reliant upon induction, then perhaps we can mitigate many of van Fraassen's concerns. As indicated, I call this *inductive-IBE*, where the term "inductive-IBE" denotes an inference comprising abduction (and all its implied explanatory inferences, loveliness, likeliness, &c.), as well as induction. This approach highlights the significance of induction in IBE, which is important for my argument *contra* van Fraassen.

I will now consider each of van Fraassen's main objections to IBE and explain in detail how my formulation can help to mitigate these problems. This will be followed by a discussion of how *inductive-IBE* manages unobservables, for this appears to be the particularly troublesome aspect of IBE, as far as constructive empiricism is concerned. I will first touch upon van Fraassen's "argument from the bad lot," which throws the adequacy of the pool of potential explanations into doubt. In Fumerton's example of the footprints on the beach, (where we infer them to be made by humans, from the hidden premise that, "in the vast majority of cases, human-like footprints on the beach are caused by humans,") Fumerton and I have argued that we are actually employing enumerative induction to pick from the pool of potential explanations. That is, rather than relying upon the possibly vague notions of *best* and *loveliest* exclusively, we are also employing a simple generalization to shape and select our pool, and this generalization is based upon past observations. The vast majority of human-like footprints on beaches are caused by humans and, therefore, the *best* and loveliest explanation is one which is commensurate with this premise, under normal conditions. (Recall that the loveliness of an explanation consists in the potential explanation's having past success and, thus, its likelihood of repetition is increased. This itself is an inductive inference.)

Accordingly, I argue that these inductive premises shape the pool of potential explanations, helping us to select from a more refined and already-likely list. In the beach example, induction narrows and adds precision to the pool of explanations by claiming that the majority of past, similar cases have been caused by humans and that, therefore our pool of explanations should reflect this observation. I argue that these sorts of premises increase the reliability of the pool of explanations in such a way that van Fraassen's argument, that the pool may be incomplete, is lessened. As well, I agree with Lipton's position that "a reasonable version of Inference to the Best Explanation does not require that we always infer the best of the available explanations... The best must be good enough."[85] Thus, using reason, induction, and a thorough scientific framework of investigation, the pool is not the Harmanian mixture of near-infinite possibilities, nor is it the inadequate, raw, and poorly thought-out pool as conceived by van Fraassen. Inductive inferences are a very powerful tool when combined with proper scientific investigation and experiment.

But, van Fraassen would counter, these inductive premises are unwarranted, because induction is an unwarranted form of inference. However, like all inductive premises, I maintain that they simply add *support* to the conclusion, and this is all they need do. To be sure, there is still a "pool" of potential explanations, and we cannot be sure that this pool is complete or adequate, but it is shaped and selected for by these inductive premises, such as, "in the majority of cases, X is the cause," which provide support for their conclusions. According to Lipton, IBE includes two filters: "one that that selects the plausible candidates, and a second that selects from among them."[86] I argue that induction is heavily in use at both these stages and, in both cases, serves to support the conclusion.

Furthermore, we employ other ampliative inferences such as analogy. An analogy allows us to infer that it is *likely* that our current, potential explanation is analogous to another correct explanation,

[85] Lipton , 177.

given analogous circumstances. As well, using simple induction, (it happened in the past, therefore, under similar conditions, it is likely to happen again,) we can further increase the likelihood that our pool of potential explanations is adequate. This addresses van Fraassen's argument that "…in order for the advocate of IBE to argue that IBE leads to truth, he must *assume* the Principle of Privilege. That is, he must assume that 'nature predisposes us to hit on the right range of hypotheses.'"[87] I argue here that nature does not predispose us to hit upon an adequate range of hypotheses; rather, our inductive powers allow us to shape an adequate pool of potential explanations that, for our scientific intents and purposes, are supported by ampliative inferences. Once again, I submit that inductive inferences are problematic for a variety of reasons, but I hope to have shown that IBE is no worse off, because it is contingent upon the application of such inferences.

Perhaps van Fraassen's misgivings with the pool of potential explanations still hold; perhaps we can never know infallibly that our pool is complete or adequate. But what is certain, under the *inductive-IBE* formulation, is that these misgivings can be no worse than those put forth by Hume's Problem of Induction. Be that as it may be, at a certain point, we are forced to admit that we, as scientific inquirers, are at a disadvantage; we are unable to always have a complete pool of potential explanations for, if we *were* to always have a complete pool, this would make us too good at our inquiry. In fact, inquiry would not be necessary if this were the case. But a complete pool is an impossibility in most scientific inquiry and, in the absence of a complete pool, the best we can do is observe statistics and make generalizations. We must include premises, as posited by Fumerton, of the form: "the majority of the time, X is the case. Therefore, a likely explanation is one that does not, under normal circumstances, diverge from this premise."

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[86] Ibid, 61.
[87] van Fraassen, *Laws and Symmetry*, 142-3.

Let us look at another objection from van Fraassen: that having the *best* explanation is only a relative assertion. “Best,” here, only implies “better than the rest,” and says nothing about truth, he argues, and should therefore not act as a guide to real entities. What can we make of this objection in light of my current line of argument? Well, it seems that this objection can be mitigated if we understand that our “pool” of potential explanations does not include aliens and other absurdities; rather, our pool consists of only the likely, potential candidates, of which we then pick the likeliest. As mentioned in my previous argument, our pool is not composed of *any* explanation, but rather a few potential explanations selected for by inductive (and other explanatory) considerations. That is, I argue against Harman's position that our pool of potential explanations is anything and everything; that even the most absurd explanation is a potential explanation. I hold, rather, that our pool of potential explanations is already a selection of the likeliest and loveliest. We must remember to conduct science within common reason, and rule out such explanations that postulate aliens, robots, and vicious pranks, (unless such explanations are contextually relevant, or inductively justified). Thus, if we select the *best* of the *likeliest* explanations, we are invariably left with none other than the likeliest explanation.

I will admit, however, that the best of the likeliest is contingent upon having an adequate pool of explanations. For, if our *likeliest* explanation is only the likeliest of a bad lot, then it cannot be the likeliest explanation, all things considered. This is why I argue that IBE must function within a complete and thorough science that takes all evidence and experiment into account. In this manner, our pool has a greater likelihood of adequacy and, consequently, of producing the best candidates for truth. That is, if our pool is *initially likely* to contain the correct explanation, then the best of these must be the likeliest.

To conceive of this concept in another manner, consider the prototypical, Liptonian formulation of IBE in which we move from *best* explanation to the *actual* explanation. The problem with this

formulation is elucidated well by van Fraassen because it does in fact seem that this is an unwarranted leap, from *best* to *actual*. However, under my formulation, we are no longer making the problematic manoeuver of moving from *best* to *actual*; rather, we are making the more benign step from *best of the likeliest explanations* to *an explanation likely to be true*, something towards which, I believe, van Fraassen would be less hostile. To be sure, van Fraassen may reject even this proposal, for he may be unwilling to admit that it is likely that unobservable entities even exist, but I believe that *inductive-IBE* begins to address and alleviate some of these concerns.

Another virtue with my account is that it accords with the majority of Lipton's conception of IBE. To be sure, Lipton conceives of IBE as being an inference from *best* potential explanation to the *actual* explanation, but he also makes it clear that we are, indeed, dealing with loveliness as an indicator of *best*-ness, and I agree in this regard. It does indeed seem that Lipton has it right with regards to loveliness as an indicator of best-ness. However, like van Fraassen, I am unwilling to make this leap from best to actual, simply on the grounds of likeliness and loveliness, and this is where Lipton and I diverge. Rather, I prefer to preserve the characteristic of a "likely" explanation by claiming that it is nothing more than that: a likely explanation. Perhaps it is the likeliest *and* the best, but this cannot lead us to certainty.

Finally we will examine van Fraassen's "argument from indifference." This argument is very similar to the "argument from a bad lot" and it holds that, for any explanation we may agree upon, there are probably equally good explanations that have not yet been discovered or proposed. That is, IBE fails to take into account other undiscovered explanations, by postulating that our current explanation, X, is correct. It is not difficult to see how *inductive-IBE* avoids this concern, and it does so in the same way that it deals with the "argument from a bad lot." That is, if we conceive of IBE as an inference to the likeliest explanation, then the fact that there may be better explanations, yet to be discovered, does not detract from the current explanation's still being *likely*. Once again, my

manoeuver allows for the possibility of recalcitrant future evidence, and for better explanations that may come along in the future. As I discuss in the final chapter, an important virtue of my *inductive-IBE* formulation is that a better explanation that may arise in the future does not cast doubt upon the enterprise of IBE in general, because of the margin of error inherent in inductive-IBE. The same cannot so easily be said about the prototypical account of IBE.

Furthermore, as I have argued, we are not dealing with an infinite class of possible explanations (as both Harman and van Fraassen hold). Recall, van Fraassen says: "Since concerning *T* we know nothing with respect to its truth-value other than that it belongs to the (probably) infinite class of theories that explain *e*, we must treat it as 'a random member of this class.' But then we may infer that *T* is very improbable."[88] However, upon my formulation, if we are indeed employing a variety of inductive practices to select our pool, then we are no longer dealing with an infinite class of (possibly absurd,) potential explanations. Rather, we have narrowed our choices to a select few *likely* explanations, the best of which is arguably the likeliest.

Another objection to this argument may be that since there is a potential for better, future explanations, the likeliness of our current explanation *is* less than we think it is. That is, even if we formulate our explanation as only being *likely*, this likeliness can still be lessened if there are potentially many, better explanations. I take this to be one of van Fraassen's most important concerns. After all, a theory cannot be likely if no other theories have been taken into consideration. My counter-argument is that, given the intents and purposes of science, and given the application of proper inferences and procedures, and given that all evidence is weighed appropriately, we may still assert likeliness. This is contingent upon the requirement that *inductive-IBE* must function within a thorough and complete science that considers all evidence and experimentation. It is only in this

[88] van Fraassen, *Laws and Symmetry*, 146

manner that we can be sure that there is not a significant number of potentially better, unconsidered explanations. Once due consideration and care has been exercised in scientific pursuits, it is no stretch to assert that our best theories are also likely, especially if they continue to be verified and established using the above-mentioned scientific method. Science must consider all available evidence and it must function upon the application of many procedural operations that attempt to ensure the accuracy of its proposed theories. This includes the correct application of ampliative inferences such as induction and analogy. Therefore, I argue that this fact serves to grant, at least, *likeliness* to our best theories, even if there may exist better, yet undiscovered, theories for, once again, my formulation allows for error.

Let us now consider how *inductive-IBE* addresses unobservables, for this is where van Fraassen's opposition towards IBE is most prominent. Remember, constructive empiricism does not dispute the use of IBE with regards to observables, such as mice in the kitchen and footprints on the beach,[89] but van Fraassen does oppose the use of IBE to postulate unobservables such as electrons and black holes, and he also questions whether we are justified in believing theories that postulate such unobservable entities. For example, if IBE leads us to a theory that states that black holes are actual entities, then this is problematic for a variety of reasons: as van Fraassen would state, black holes have never been observed and cannot be observed. Accordingly, we should remain agnostic regarding their existence, and not even grant *likeliness* to such theories. I disagree for, although we may not be certain about such entities, and although there may be potentially better, undiscovered explanations, we can also be in a position to assert *likeliness*.

That is why I argue that changing IBE's formulation to asserting *likeliness* rather than *actuality* is important. For, even van Fraassen would likely concede that there is *something* causing the observed

[89] van Fraassen, *The Scientific Image*, 18.

phenomenon; there must be *something* that is emitting X-ray radiation; there must be *something* that is bending the light of nearby stars. The similarity, I argue, between mice in the kitchen and black holes, is that both produce phenomena that require explaining, regardless of whether we can see them or not. Now, van Fraassen does agree in this regard: he holds that there are facts of the matter that can be explained, even in the case of unobservables. However, he disagrees that our explanations can make these theories of unobservables any more *likely* or truth-indicative. What I wish to communicate is that there are objects, both observable and otherwise, that need explaining, and this is the goal of science. Using my formulation of *inductive-IBE*, it does not seem so problematic to assert that it is *likely* that the exhibited phenomena are caused by a black hole, or some object which exhibits the required characteristics, whatever we wish to call it. This likeliness is supported for a variety of reasons: black holes, for example, fit nicely with Einstein's General Theory of Relativity, and they also unify and explain a wide variety of observed phenomena. They are also commensurate with our background knowledge of other phenomena, such as light and x-rays. As well, the theory of black holes has come about because of rigourous and continuous scientific experiment and observation, and in such a framework of a thorough and careful science, it should not seem problematic to assert the likeliness of our *best* theories. Thus, if IBE is composed of a set of inductive and deductive inferences, as argued for by Fumerton, then it seems no stretch to allow for the *likelihood* of a particular ontology. That is, although we cannot be certain about the existence of unobserved entities, we can nevertheless give *support*, from a variety of angles, to the conclusion that they exist, because they are the best explanations for the evidence at hand.

One objection from van Fraassen may be that Fumerton's argument regarding induction should not extend to unobservables because such entities are generally unprecedented and, thus, not well-suited for inductive or statistical generalization. Unlike the footprint example where we can easily assert a premise, (such as, "generally, footprints on the beach are caused by humans,") there is often no such

generalization to be made about first-time, unique entities. We cannot, for example, propose that "generally, x-ray radiation of this nature is emitted by black holes," and that, "therefore, it is likely that these particular x-rays are emitted by a black hole." I have two replies to this objection.

Firstly, even though we are dealing with unprecedented entities and theories, there is still a wealth of background beliefs and knowledge that comprises hypotheses and theories. Recall the quotation from Fumerton: "*Though theoretic entities are themselves unobservable, their defining properties may be such that we have observation of other things having those properties.* [*Fumerton's emphasis.*]"[90] For example, using the already-understood concepts of x-ray emission, the life cycle of stars, and of light curving, we can employ inductive inference within IBE of the form, "X is generally caused by an object that exhibits the characteristics A, B, and C." So, although we are unable to make the simple enumerative induction of, "generally, strong gravitational fields are caused by black holes," we are nevertheless in an adequate position to claim, "generally, light only bends because of a strong gravitational field, or a bending of space-time," or "the life cycle of a star can often end when the star's mass becomes so dense that it collapses upon itself due to gravity." Therefore, we can come to the intermediate conclusion, or lemma, that it is likely that there is a bending of space-time, or an object causing this bending. As well, this proposition is corroborated by other accepted theories – in the case of black holes, General Relativity. We can also employ similar inductive inferences with regards to other observed phenomena, such as X-rays, claiming, "in general, X-rays can only be emitted from an object that exhibits the qualities of our proposed 'black hole.'" From this variety of lemmas and inferences from pre-existing knowledge, the scientist can then surmise as to the *likeliest* explanation that would account for the observed evidence. We do not, after all, conduct science in a vacuum, oblivious to pre-existing facts, observations, and analogy. Rather, we make a series of innumerable inductive inferences that, when finally combined, can lead to accurate scientific

projections. Recall, finally, Fumerton's position: "Reaching a conclusion inductively based on one's *entire* body of evidence usually involves countless inductive arguments, the premises of which confirm countless hypotheses to varying degrees of probability."[91]

Secondly, when dealing with unprecedented entities, explains Fumerton, we employ analogies between our current knowledge and the proposed explanation or entity. This analogical reasoning is a type of inductive reasoning and serves to increase likelihood. As Fumerton states, "One explanatory theory T1, then, will be better supported by analogy than another theory T2 if we find that T1 is analogous to established explanations… while T2 is not."[92] Though theoretic entities are themselves unobservable, their defining properties may be such that we have observation of other things having those properties.[93] Once again, analogical reasoning is not certain, but it does provide support for a conclusion, further increasing likeliness.

Thus I hope that we can understand that IBE is comprised of an inference to the best explanation (loveliness and likeliness), as well as induction, deduction, and analogy. Not only is the scientist employing straightforward abduction to postulate new entities, but he or she is also employing induction and analogy based upon current background knowledge about the universe. Thus, the conclusions born from this string of inferences are normally well-supported and can, at least, be *likely*. I argue that van Fraassen should be able to accept this possibility and, as mentioned, this requires at least an *openness* to scientific realism concerning both observable and unobservable entities. I will not delve into the realism debate any further than what has already been explained in Chapter Two and, as to whether van Fraassen is open in this regard, I will not surmise. However, I still remain committed to Lipton's position whereby realism is the best explanation of our current

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[90] Fumerton, 599.
[91] Ibid.
[92] Ibid, 595.
[93] Ibid.

predictive success, and is therefore a likely conclusion. Nevertheless, I hope to have demonstrated that the employment of IBE, under my formulation, is a very *reasonable* method to employ in science, for the aforementioned reasons. It provides support for its conclusions, as does induction, and should therefore be considered as having the same strength, (and weakness), as induction. Finally, it should be noted that most, if not all scientific inquiry, hinges heavily upon inductive inferences; why should IBE be any different? *Inductive-IBE* is wholly commensurate with this conception of science.

Chapter 4
Virtues, Problems, and Concluding Remarks

To sum up, I have examined the prototypical formulations of IBE as proposed by Lipton and others, and argued that it contains a significant inductive component. The difference between my account and Fumerton's is that I allow for abductive considerations of loveliness and best-ness *as well as* induction. I differ from Lipton in that I also acknowledge the significance of induction in IBE. I then proceeded to explain that, for this reason, IBE should be treated similarly to induction, that is, as providing support and inferring *likely* conclusions. Using this corollary, I argued against van Fraassen's position that there is no justified reason to accept that IBE can postulate correct explanations regarding unobservable entities. Indeed, I agreed with van Fraassen in that there are severe problems with the employment of IBE, but I then demonstrated that IBE could be modified into a more coherent form, while taking his considerations into account. To conclude now, I will briefly specify some of the virtues, as I see them, of my account, followed by potential problems.

There are three important virtues to my account of IBE: firstly, *inductive-IBE* is more *descriptive* of the manner in which we conduct science. Secondly, *inductive-IBE* mitigates van Fraassen's arguments because it is, in fact, closer to his position than the original formulation. And thirdly, *inductive-IBE* does not commit us to a problematic method of inquiry that opens up the entire enterprise of IBE to falsification on the grounds that it has failed in the past. (I will clarify this notion shortly.)

Inductive-IBE is more descriptive of scientific inquiry than is the prototypical, Liptonian conception. Thus far, we have understood that Lipton's position is not only normative, but also descriptive of the manner in which we conduct our inquiry. However, I argue that the *inductive-IBE* formulation more accurately describes our actual practices than does Lipton's. To be sure, Lipton

acknowledges the defeasibility of IBE, as do I, but I argue that my theory more adequately accounts for this defeasibility. That is, I have demonstrated *exactly why* IBE can only lead to likely conclusions and this, I take it, helps to describe the level of certainty among scientists. A scientist does not initially assert the existence of theoretical entities with certainty; this much is clear. But I argue that other formulations of IBE don't account for this level of scientific certainty as effectively as *inductive-IBE*. To be sure, most accounts of IBE accept that this type of inference is fallible at times, but I have demonstrated that this is due to its intrinsic inductive nature. Accordingly, I argue that my formulation better reflects the level of certainty exhibited in the scientific field, where most scientists would likely agree that theories and unobservables are subject to constant revision and improvement, even with the proper application of IBE. Admittedly, after years of experiment and success, a scientist may be certain that a theory or entity is the case, and this has certainly been exhibited with Darwinian theory and electrons, for example. However, at the outset, and upon the initial hypothesis, most scientists do not display such a high level of certainty as normal IBE tends to suggest. To Lipton's credit, however, I still contend as strongly as ever that IBE, in general, more accurately describes our scientific practices than does constructive empiricism. Scientists actually do believe in the existence of electrons and black holes and van Fraassen fails to account for most scientists' ontological commitments and convictions.

Secondly, my account withstands some of van Fraassen's criticisms by positioning itself more closely to van Fraassen himself. That is, my formulation of IBE is further away from the realist camp, as well as from the proponent of IBE. Instead, I have recognized the problems indicated by van Fraassen, and devised a modified version that takes into account his considerations, and that better withstands his objections. To be sure, he would likely deny even *inductive-IBE*, but would, I think, be more charitable and receptive towards it than he would be towards the other formulations.

Thirdly, the *inductive-IBE* formulation does not commit us to the truth of the theories it postulates and, therefore, is not open to falsification on the grounds that a newer, better explanation has arisen. We do not, upon exercising *inductive-IBE*, assert the certainty of its conclusions and commit ourselves to the existence of black holes, for example. Thus, scientists may be free to revise a theory because of future considerations. In the prototypical formulation of IBE, conversely, this is problematic because, if the scientist is found to be wrong after the correct application of IBE, this is a serious blow to the enterprise of IBE in general. Consider Newtonian theory: this theory is an excellent example of IBE at its best, for it exhibited unification, elegance, simplicity, likeliness, as well as bringing us greater understanding. However, with the advent of Einstein's Special and General Relativity, Newtonian theory was cast into doubt. Thus, the anti-realist could claim that IBE, though employed perfectly, failed to arrive at the correct picture of the world, and this simple failure of the prototypical, Liptonian conception of IBE can be devastating to its overall case.

However, if a scientist, upon my account, correctly applies the rules of *inductive-IBE* and is later found to be incorrect, as is the case with Newton, the anti-realist cannot proclaim the failure of IBE *in general*. This is because there is a margin of error, by virtue of *inductive-IBE*'s significant inductive component. IBE can still remain the dominant scientific tool, simply by concluding that the explanation is *likely* to be true, and if its conclusion is disproved at some later time, this does not significantly harm the value of this type of inference.

Now I will discuss some of the problems that may present themselves to my account. Chief among them is that it is entirely conceivable, even after our discussion, that van Fraassen would still not be willing to grant that IBE provides support for belief in unobservables. That is, his view is that IBE has no truth-tracking value whatsoever and that no amount of support and evidence for unobservables has any bearing on the theory's truth, or even likelihood. So, regardless of whether we wish to assert the absolute existence of an unobservable entity, or only the *likelihood* of an entity, van Fraassen may

object outright to this possibility, opting instead for empirical adequacy. In reply to this objection, I would grant two options: either we accept that there is a truth of the matter with regards to unobservables, or we do not. In the former, realist position we are faced with uncertainty and the problem of induction; in the latter, we simply strive for empirical adequacy, fictionalism, or instrumentalism. As indicated, I argue for the former proposition, but this is an entirely divisive question that will not be addressed further. I hope to have shown, instead, that there is a less problematic characterization of IBE that can adequately address the arguments of my opponents.

On a similar note, another objection to this type of application of IBE and, indeed, any type of IBE, would be to argue strongly for scientific anti-realism, instrumentalism, or fictionalism. This maneuver would immediately eliminate any need for a realist-type inference, such as IBE, that attempts to track the truth. Thus, as per my discussion on realism in Chapter Two, my formulation requires at least an openness to realism, if not a full endorsement of it.

4.1 Conclusion

In conclusion, it seems that we must be prepared to admit that many truths or facts are not open to our direct acquaintance, given our limited position as human observers. Black holes, for example, cannot be seen using visible light because they are, by definition, an absence of light. The same goes for electrons. In all these cases of unobservables, the best we can hope for is to observe the effects, evidence, and observable phenomena that are *indicative* of the existence of these entities. Granted this, if one is a realist, then he or she will admit that there is, in fact, an entity or truth of the matter, and that we can attain it *in principle*. I do not believe that we should be forced to remain agnostic with regards to the ontology of unobservables, simply because we, as human observers, have a limited capacity. Rather, we should attempt as best we can to track the truth of the matter, for this is arguably

the goal of science. Inference to the Best Explanation is the ideal candidate for such truth tracking and is the best practice we have thus far. To be sure, there are many problems as outlined by van Fraassen and others, but I see these as obstacles to be overcome, and I argue that my formulation of IBE is a good attempt at executing an acceptable and charitable notion of IBE for scientific inquiry.

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