VAN FRAASSEN AND A DEFENSE OF INFERENCE TO THE BEST EXPLANATION

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by

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The undersigned, appointed by the dean of the Graduate School, have examined the dissertation entitled

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presented by Darin Finke,

a candidate for the degree of doctor of philosophy,

and hereby certify that, in their opinion, it is worthy of acceptance.

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Claire Horisk

Christopher Pincock

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To my wife, Kristin, and to my Mom and Dad

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TABLE OF CONTENTS

CHAPTER ONE

Introduction

In this dissertation I present and rebut a series of arguments against the justification of inference to the best explanation (hereby IBE) presented by Bas van Fraassen.[1] In this way, then, my project is entirely negative—in the following chapters I show that van Fraassen's arguments fail to undermine the justification of IBE. But even if I am correct, this does not establish that IBE is in fact justified. Although I believe it, and although I believe there is empirical evidence in support of it, I do not intend to argue that IBE is a justified mode of inductive argument.

Some philosophers have alleged that certain arguments against scientific antirealism beg the question.[2] At first this sounds like it might be a legitimate criticism. Arguments within philosophy, and within other disciplines, may indeed beg the question. It is a serious worry because begging the question is a serious epistemic flaw. If an argument begs the question, then the argument is flawed and thus should not be persuasive. Usually the flaw consists in one of the following situations: (1) a premise in the argument is essentially claiming the same proposition as the conclusion, (2) the very best reason for believing that a premise in the argument is true is the conclusion itself, or, to a lesser extent, (3) a premise is used that would never be accepted by the person targeted by the argument and this is known ahead of time by the one providing the argument. But the arguments which are alleged to be begging the question against the scientific antirealist seem to be different from any of these situations. This is because the

[1] These can be found in van Fraassen's *Laws and Symmetry* (Oxford: Oxford University Press, 1989), 142-70.
[2] See for example Fine (1984; 1986; 1991) and Laudan (1981).

purported question-begging has to do with the form of the argument rather than the relationship between the statement of the premises and the statement of the conclusion. These philosophers object to the argument not because of the content of the argument, but rather because it is an instance of IBE. They do not accept these instances of IBE because they contend that the justificatory status of IBE is the very issue at stake! Thus they allege that to assume that IBE is justified, and to use an instance of IBE to support scientific realism (or to object to scientific antirealism), will actually beg the question against the scientific antirealist because it would assume an answer to the very question that is being debated. They do not accept IBE as a justified mode of inference, and thus any argument that objects to scientific antirealism or supports scientific realism, and that takes the form of IBE, is seen as question-begging and thus improper and unpersuasive. I find this to be problematic. This way of presenting the problem seems to conflate the issue of scientific realism with the issue of the justificatory status of IBE. These must be different issues. It cannot be part of the scientific antirealist's core position that IBE is unjustified. This is a claim needs to be argued for. Instead of alleging that these instances of IBE are question-begging, they need to provide good reasons to believe that IBE is not a generally reliable method for getting at the truth. If IBE can be shown to be unjustified, then we should seek to show it. This will be difficult because IBE has a proven track record of success. That IBE has been successful is an empirical claim and a contingent claim. Experience informs us that explanatory power is a guide to truth. Moreover, the mechanism which drives IBE is not mysterious. Presumptively, it looks as if our position ought to be that IBE is generally a reliable way to get at the truth. But if there were good reasons to believe otherwise, we should listen. Van Fraassen has

attempted to provide these reasons. He has presented us with a series of arguments that are meant to establish that IBE is not a justified mode of inference. In this dissertation I take them seriously and show that they fail.

In chapter two, I describe the general features of IBE and establish that the justificatory status of IBE is important for scientific realism. In my explication of IBE I draw mostly upon Peter Lipton's excellent work on IBE, *Inference to the Best Explanation*.[3] I also describe and explain several versions of scientific realism. I contend that a necessary condition of scientific realism (the core claim) is that scientists can formulate and have formulated true or approximately true (or highly likely to be true) scientific theories that posit the existence of unobservable entities. It is this condition which makes the justification of IBE necessary, for the best and perhaps the only way to justify this outlook is by acknowledging the connection between explanatory power and truth—the essence of IBE. In this chapter I also present the No-Miracle argument as a powerful argument supporting scientific realism. It is noteworthy because it may be reasonably construed as an instance of IBE. If this is so, then the very best reason to be a scientific realist relies on the assumption that IBE is a justified mode of inference.

In chapter three, I present van Fraassen's initial argument, the argument from the bad lot, and show that it fails. This argument assumes that the IBE user will compare a limited number of possible explanations of some phenomenon, identify the best one, and infer that this best one is true. Van Fraassen worries that the best one may be the best of a bad lot. In other words, the lot of possible explanations from which the IBE user is choosing may not contain the true explanation. Thus, the IBE user ought to be able to provide a good reason to believe that the true explanation is already within the pool of

[3] Lipton, P., *Inference to the Best Explanation,* 2nd edition (London: Routledge Press, 2004).

possible explanations before concluding that the best one in the lot is the true one. I construe van Fraassen's argument as suggesting both that the justification of IBE requires the IBE user to provide such a reason, and that the IBE user will never be able to provide it. In this chapter I argue that both of these claims are false. Regarding the first claim I argue that requiring the IBE user to provide a good reason to believe that the lot of possible explanations under consideration is a good lot is not a necessary condition for the justification of IBE when IBE is understood probabilistically. I then describe how this may be done. Regarding the second claim I argue that the background knowledge and approximately true background beliefs of the IBE user will significantly shape the pool of possible explanations being considered so as to provide, in many cases, a justification for believing that the pool contains or is likely to contain the true explanation. This will be referred to as the appeal to privilege. The idea that theory choice is significantly affected by background knowledge has been put forth and argued for by such philosophers as Richard Boyd, Peter Lipton, and Stathis Psillos. Additionally, it should be noticed that my responses to van Fraassen's two claims may be used independently of each other. For instance, the appeal to privilege which I am promoting is sufficient, by itself, to escape van Fraassen's objection without having to endorse any probabilized version of IBE.

In chapter four, I present van Fraassen's argument from indifference and show that it also fails. This argument intends to show that the conclusion of every IBE is highly likely to be false. I show that this conclusion relies on a dubious claim, namely that for every possible explanation there exists (or possibly exists) a very large number of rival alternative possible explanations which all would explain the phenomena just as

well, if they were true. Not only does van Fraassen not argue for this claim, there are good reasons to believe that the claim is false. Moreover, the availability and tenability of a probabilized version of IBE renders the claim impotent. I end the chapter with a discussion of Peter Lipton's characterization of van Fraassen's argument.

In chapter five, I present van Fraassen's Dutch book argument and show that it fails. This argument is a special instance of the more famous Teller-Lewis Dutch book argument which attempts to show that, given certain Bayesian conditions, anyone who violates the principle of conditionalization will be susceptible to being Dutch booked. Van Fraassen claims that anyone who adopts a probabilistic version of IBE will violate the principle of conditionalization and thus will be susceptible to being Dutch booked. Van Fraassen concludes that since Dutch book susceptibility is a mark of irrationality, the probabilistic IBE user is irrational. I point out two problems with his argument. Firstly, the probabilized version of IBE which van Fraassen uses is not the only version available. A better version exists, and here I draw upon the works of Gilbert Harman and others to illustrate this. Secondly, the example van Fraassen uses is actually a misapplication of the probabilistic version of IBE van Fraassen endorses. This renders the example ineffective, for it dissolves any support the example may have given to the claim that this probabilized version of IBE will inevitably lead to being susceptible to a Dutch book.

CHAPTER TWO

Inference to the Best Explanation and Scientific Realism

1. The Issue

IBE is a mode of inductive inference. The essence of this mode of inference is that explanatory considerations serve as a guide to determine the truth. However, IBE is controversial. The justification of IBE has been questioned, mostly by scientific antirealists, who obviously hold a position that is contrary to scientific realism. These views conflict over the nature of science and the proper attitude to take toward science. Both of these views have, and have had, many adherents, and consequently, many critics.[4] It makes sense, then, to expect many differing versions of both views. The focus of this chapter is scientific realism and its relationship to IBE. I presume that there are some things which are shared, or perhaps ought to be shared, by all scientific realists, namely, the core features of the view that make one a scientific realist, rather than something else. I contend that one of these features of scientific realism is the view that our most mature and successful scientific theories are true, approximately true, or highly likely to be true. This is a controversial aspect of scientific realism. Since some of these scientific theories posit the existence of unobservable entities, it follows that the acceptance of scientific realism may commit one to accept or believe that certain unobservable entities exist (or are very likely to exist).

[4] For thorough analyses of the scientific realism debate, see both Boyd, R., "The Current Status of Scientific Realism", in J. Leplin (ed.), *Scientific Realism* (Berkeley: University of California Press, 1984), 41-82; and Psillos, S., "The Present State of the Scientific Realism Debate", *The British Journal of the Philosophy of Science*, 51 (2000), 705-28.

There is an obvious connection here between IBE and scientific realism. IBE is an inductive argument type which sanctions inferences to conclusions that posit the existence of unobservable entities, and scientific realism is a thesis which (presently) may commit one to accept or believe, among other things, that certain unobservable entities exist. My point in this chapter is to show that scientific realism *needs* IBE. Without IBE, there is significantly less of a reason, or less of a motivation, to believe or adopt scientific realism. More specifically, the very best reasons to believe that scientific realism is true (or perhaps the very best reasons to become a scientific realist) rely heavily on the idea that explanatory power is a guide to truth. Consequently, any attempt to undermine IBE as a justified or reliable mode of inductive inference may be construed, plausibly and reasonably, as an attempt to undermine scientific realism itself. As we will see in the upcoming chapters, van Fraassen has provided a series of objections to the justification of IBE. Given that van Fraassen is not a scientific realist (indeed, he is credited with developing and defending one of the most sophisticated versions of scientific antirealism—constructive empiricism), one might easily assume that van Fraassen's motive is to undermine scientific realism itself by attempting to undermine the justification of IBE.

In this chapter I explain the nature of IBE, as a mode of inductive inference that I accept as justified. I also explain, but do not defend, the thesis of scientific realism. I then argue that the thesis of scientific realism requires IBE to be a justified mode of inductive inference; hence the question of the justificatory status of IBE is of the utmost importance, at the very least, to the scientific realist.

2. Inference to the Best Explanation

Within the history of philosophy, or of logic in particular, IBE has only been around, by name, for a relatively short amount of time. Although Gilbert Harman is often credited with first discussing this type of induction by the name of IBE, it has roots in the writings of William Whewell, Charles Peirce, and arguably many others as well.[5] Whewell, without using any name for the inductive inference he discussed, argued for inferring the truth of a theory if, given its truth, it would explain observed phenomena, especially if the theory exhibits explanatory virtues like simplicity and consilience.[6] Peirce argued that abduction stood on par with induction and deduction as legitimate inference types, where abduction involved specifically inferring a causal explanatory theory.[7] Harman initially argued that each instance of enumerative induction is actually an instance, and a special case, of IBE. Paul Thagard attempted to fill in some of the gaps left by Harman.[8] He argued that actual scientific cases provide the criteria for evaluating explanatory theories, and that these criteria provide an account of how scientific theories receive their justification. Later, William Lycan claimed that explanatory considerations play a significant role in every inductive inference, and that good reasoning is essentially explanatory reasoning that "aims at maximizing the 'explanatory coherence' of one's total belief system."[9] Each of these philosophers, along with numerous others, suggested or promoted the main idea behind IBE—that explanatory considerations can be used as a guide to infer what the case is. I have only

[5] Harman, G., "The Inference to the Best Explanation," 88-95. It could plausiby be argued that IBE has been brought up or discussed even earlier with the works of Leibniz, Descartes, and perhaps many others as well.
[6] Whewell, W., *The Philosophy of the Inductive Sciences,* vol.2, 46-95.
[7] See Peirce, C. S., *Collected Papers*, in C. Hartshorn and P. Weiss (eds.), (Cambridge: Harvard University Press, 1931).
[8] Thagard, "The Best Explanation: Criteria for Theory Choice," 76-92.
[9] Lycan, *Judgement and Justification*, 128.

touched upon the rich history of this kind of reasoning. Throughout the history of inductive logic, or of reasoning and argumentation in general, the appeal to explanatory power is often not emphasized or noticed. This might be a testament to its fundamental nature, and it also might account for the reason that, historically speaking, it took a relatively long time for IBE to gain mainstream status as a subject matter.

For some time, IBE had eluded a precise characterization. It was (and still is) often seen to be a mere slogan trumpeted, usually, by philosophical realists of all stripes. Peter Lipton was the first one to move past the slogan and attempt to fill in the rest of the gaps.[10] He suggested that IBE should really be understood as "Inference to the Loveliest Explanation," where a *lovely* explanation is one which would offer a great deal of understanding, if it were true. He contrasts this with a *likely* explanation, which is one that is probably true, given the evidence at hand. Taken together, he construes IBE as inferring that the loveliest explanation of the pool of possible explanations under consideration is in fact the likeliest explanation. This is a reasonable characterization. In its barest form, then, the user of IBE judges that the theory or possible explanation which would, if true, be a sufficiently good explanation of the available evidence *and* provide the best explanation of the available evidence is also the theory that is (likely or most likely to be) true.[11] A closer look reveals several parts to this inference. The first step is the pooling of possible explanations. However this is done, an IBE user must have available a pool of possible explanations. There are three points to make regarding this pool. Firstly, for any phenomenon that is in need of an explanation, there are an infinite

[10] See Lipton (2004).
[11] I will use the term "IBE user" interchangeably as someone who uses IBE and as someone who defends or assumes the justification of IBE.

number of logically possible explanations that would, if true, explain it.[12] The vast majority of these possible explanations, however, are absurd and thoroughly implausible; in fact we rarely, if ever, consider them at all. Secondly, it is important to notice that this is a pool of *possible* explanations, rather than a pool of *actual* explanations. I take it that only actual explanations ever explain anything; and actual explanations are the only explanations that are true. This is why, in the explication of IBE, the subjunctive mood is used along with a conditional auxiliary verb: if the possible explanation *were* true, it *would be* the best explanation. Finally, there may be no systematic method for coming up with a possible explanation for some phenomenon; but certainly, the pool of possible explanations that an IBE user draws upon is very often, if not always, severely shaped by background knowledge. Generally, this allows the IBE user to rule out the absurdly implausible possible explanations and focus on the few possible explanations that are the most plausible.

The second step, the *comparative step*, or the *ranking step*, is the determining of which explanation, if true, would be the best from among the possible ones in the pool. This process may take into account many factors, the most important being the usual explanatory virtues. These include, perhaps among others, simplicity, scope, fruitfulness, testability, conservatism and consilience. These virtues are not employed in a vacuum; they will always be played against one's background knowledge. Of course, it is also the case that a possible explanation which exhibits some, or even all, of these characteristics may still nevertheless be false. These are explanatory virtues, properties which are commonly had by good explanations, not foolproof indicators of truth.

[12] I argue for this claim in Chapter 4.

The last step, the *ampliative step*, is the inference to the best explanation, among the possible ones in the pool. Here one of the questions is how strongly to word the conclusion. Ought the IBE user to conclude that the possible explanation which is deemed best compared to the others in the pool is *the* one true explanation? This would be the strongest possible inference. Or ought she to conclude that the inferred explanation is merely highly likely to be true? Or perhaps the most likely to be true, of the ones in the pool? These possible conclusions vary in strength, but IBE is flexible enough to accommodate all of them. The essence of IBE is that explanatory power is and ought to be a guide to believing what is the case. The particular aspects of a particular instance of IBE should determine how strongly one ought to believe a possible explanation that is deemed best from among a pool of possible explanations; the most important determining factor being the quality and strength of the inferred explanation. Within the framework of one's background knowledge and with regards to the explanandum, the phenomenon in need of an explanation, the IBE user ought to judge the quality and strength of the inferred explanation both independently and in relationship to the other possible explanations within the pool being considered. This evaluative judgment will help inform the IBE user as to the probability of the inferred explanation, and thus as to the strength of the conclusion.

If IBE can admit probabilistic terminology such as "likely" or "most likely" in its conclusion, which I believe it can, then it is tempting to see whether a fully and sufficiently probabilized version of IBE can be formulated. This would require marrying IBE with some form of epistemological probabilism. Fortunately, there are good reasons to believe that, by combining the essential parts of IBE with the essential parts of

Bayesianism, a probabilistic version of IBE may be formulated.[13] This would have an obvious effect on how to interpret and understand IBE. A probabilized version of IBE entails, for instance, that the conclusion would be interpreted as a degree of belief, falling somewhere between 0 and 1. Obviously, one upshot of this version of IBE is that it would provide a natural and legitimate way for IBE users to express their conclusions with probabilistic terminology; within a Bayesian framework, asserting that a claim is highly likely to be true, or asserting that one claim is more likely than another, is a quantifiable matter and is easily accommodated.

There may be countless examples of the use of IBE, ranging from the mundane, the scientific, and even the philosophical. Sherlock Holmes might have used IBE to infer that Moriarty was the perpetrator of a crime. Reflecting on the possible explanations of the evidence at hand, Holmes may have determined that if Moriarty had committed the crime, it would not only be a sufficiently good explanation of the evidence, it would be the best explanation of the evidence. A paleontologist might have used IBE to infer the previous existence of dinosaurs. After discovering what appears to be fossils of animals or of traces of animals, a paleontologist might have determined that if these discoveries were actually fossils, rather than, say, ordinary minerals coincidentally in the shape of various bones, it would not only be a sufficiently good explanation of their appearance, it would be the best explanation of them. Our very best evidence for believing that dinosaurs once existed is plausibly the fossil record. Using the fossil record to infer that dinosaurs once existed is clearly an inductive inference, and one that plainly seems to be an instance of IBE. A nuclear physicist might have used IBE to infer the existence of an

[13] The formulation of a sufficiently probabilized version of IBE is very important for my reply to van Fraassen's objections to IBE. I lay out the basic scheme and the prospects for this formulation in Chapter 3 and in Chapter 5.

elementary subatomic particle called a neutrino.[14] While experimenting with the decay of the nucleus of an atom, a nuclear physicist notices a discrepancy in the amount of energy at the beginning of the event compared to the amount of energy at the end of the event. This observation suggests a violation of the law of conservation of energy. The nuclear physicist, however, might reason that the existence of a hitherto undetected particle, a neutrino, would not only be a sufficiently good explanation of the apparent discrepancy, it would be the best explanation of it. In fact, one of the gratifying qualities of this explanation is that, in this instance, it saves from violation the apparently indomitable law of conservation of energy. Finally, a philosopher might be tempted to use IBE as a way to thwart a skeptic of the external world. While listening to the skeptic explain how the world might be an illusion conjured up by an evil and powerful genius, the philosopher may note that he seems to both see and feel a book that appears to be on his lap. The philosopher might be able to respond to the skeptic by reasoning that if there were actually a book on his lap, rather than a mere illusion of a book, this would not only be a sufficiently good explanation of this appearance, it would be the best explanation of it. It is interesting to note that, in this case, the philosopher is choosing from among empirically equivalent possible explanations by recognizing the superior explanatory power of one over the other. That IBE is able to sanction an inference between empirically equivalent rivals is a noteworthy testament to its remarkable power.

Of these four instances of IBE, one of them may be regarded as significantly different from the rest. There is only one case, the case of the nuclear physicist, in which the IBE user infers an explanation which includes a reference to an unobservable entity,

[14] Detailed accounts of the history of the discovery of the neutrino can be found in both Sutton, C., *Spaceship Neutrino* (Cambridge: Cambridge University Press, 1992), and Caldwell, D. (ed.), *Current Aspects of Neutrino Physics* (Berlin: Springer, 2001).

the neutrino. As I have already indicated, one controversial aspect of IBE is that it appears to sanction inferences to conclusions that posit the existence of unobservable entities. Recognizing that a possible explanation would be both a sufficiently good explanation and the best explanation of the data, if it were true, and inferring that it is true or likely to be true on the basis of this, is a process that is not inherently restricted to possible explanations which posit the existence of observable entities.[15] This aspect of IBE is extremely important. It is made clear in the next section that this is the reason that scientific realists have, and should have, such an interest in the justificatory status of IBE.

3. Scientific Realism

It is not surprising to discover that there is disagreement among philosophers as to the precise definition of scientific realism. A good place to start a discussion of scientific realism is Stathis Psillos' "The Present State of the Scientific Realism Debate". In this essay, Psillos claims that scientific realism is constituted by three separate theses:

(1) *The metaphysical thesis*: The world which science is purporting to describe has, in relevant respects, a definite and mind-independent structure.

(2) *The semantic thesis*: Scientific theories are to be understood literally.

(3) *The epistemic thesis*: The best (i.e., the most mature and successful) scientific theories are well-confirmed and either true, approximately true, or highly likely to be true. [16]

Each of these theses is meant to distinguish the scientific realist from various versions of scientific antirealism. To better understand each of these theses, it is useful to discuss an

[15] Of course, the IBE user may wish to restrict the inference to the domain of only those possible explanations that posit the existence of observable entities. However, any allegation that IBE ought to be restricted to such a realm at all times requires a good reason for thinking that this should be so.
[16] Psillos, "The Present State of the Scientific Realism Debate," 705-28.

example of a well-confirmed, mature, and successful scientific theory and a scientific realist's attitude toward it. Currently, the atomic theory might reasonably be construed as a well-confirmed, mature, and successful scientific theory.[17] The atomic theory, in general, is a theory about the nature of matter; specifically it asserts that all matter is composed of discrete entities called atoms. It may even go further in specifying particular properties of an atom—its electric charge, its weight, its diameter, its constituents, its behavior in certain environments, etc. Thus, the atomic theory appears to make claims of and about certain unobservable aspects of the world. If scientific realism is constituted by the three theses described above, then a scientific realist takes the view regarding the atomic theory elaborated in the following paragraphs.

First of all, the metaphysical thesis describes the position of metaphysical or ontological realism. This position distinguishes the scientific realist from versions of idealism and phenomenalism. As Psillos points out, this thesis also separates scientific realism from various verificationist positions.[18] Unlike the positions just listed, the metaphysical thesis claims that the unobservable entities postulated by scientific theories, if they exist at all, exist in a manner that is relevantly independent of our capacity to detect or verify that they do. Thus, if, as the atomic theory seems to suggest, atoms exist in this world, they exist in a manner that is relevantly independent of us; that is, relevantly independent of our minds, or our ability to detect, verify, or even know that they exist.

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[17] For an interesting and relatively thorough history of the atomic theory, see Pullman, B., *The Atom in the History of Human Thought* (Oxford: Oxford University Press, 2001), translated by A. Reisinger.
[18] Psillos cites, as examples, the verificationist accounts of Putnam's and Dummett's, in his "The Present State of the Scientific Realism Debate," 706.

Secondly, the semantic thesis describes the position of semantic realism. This position distinguishes the scientific realist from the various accounts given by instrumentalists and empiricists. Thus, this thesis rules out the idea that the theoretical terms postulated by scientific theories (i.e., any term used to refer to some unobservable entity) are merely language markers that try to account for only observable phenomena. Moreover, it rules out the idea that these scientific theories should or can be interpreted as capturing fully and only what is happening in the observable world. Thus, these positions would either deny that these sorts of theoretical claims are even capable of being true or false, or they regard the truth values of these claims as a function of the observational discourse surrounding them. Regarding the atomic theory, the semantic realist claims that it ought to be understood literally; that is, the assertions made by this theory should be understood as a literal description of the way the world really is. Thus, the assertions made by the atomic theory are capable of being true or false. If they are true, then they are accurate descriptions of the way the world is. Consequently, as things stands now, this description will necessarily include unobservable aspects of the world. Furthermore, when this position is combined with metaphysical realism, the continued existence of atoms is relevantly independent of any particular person's thoughts, beliefs, or commitments he or she may have about them.

Lastly, the epistemic thesis describes the position Psillos calls epistemic optimism.[19] This position distinguishes the scientific realist from more skeptical and pessimistic accounts of science's aims, methods, and achievements; the most notable may

[19] Psillos, "The Present State of the Scientific Realism Debate," 707.

be van Fraassen's constructive empiricism.[20] The constructive empiricist claims that "science aims to give us theories which are empirically adequate."[21] This implies that science aims at the truth of the observable world, but not of the unobservable world. In contrast, the epistemic optimist claims that science not only aims at the truth of *both* the observable and the unobservable world, but that, for the most part, it succeeds. There are different variations of this thesis, with respect to its strength. For instance, it would be unusual for a scientific realist to give the strongest reading of this thesis and admit that all mature scientific theories are simply true, in that these theories get everything exactly right. Most commonly the scientific realist will resort to something like approximate truth when describing mature and well-confirmed scientific theories. Jarrett Leplin might be credited with providing the weakest reading of this thesis with his minimal epistemic realism.[22] André Kukla describes this thesis as claiming that "it's logically and nomologically possible to attain to a state that warrants belief in a theory."[23] Even though this thesis appears to be weak enough to be accepted by all, there are still those who dissent; van Fraassen (or any constructive empiricist), for instance, appears to deny it.

Because there are numerous present-day scientific theories regarding unobservable aspects of the world that may be rightfully considered well-confirmed, mature, and successful, these three theses entail that such theories are true, approximately

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[20] A good explanation and discussion of van Fraassen's 'new' epistemology can be found, among other places, in his *Laws and Symmetry*, specifically pp. 129-214, and in his "Empiricism in Philosophy of Science," in P. M. Churchland and C. A. Hooker (eds), *Images of Science* (Chicago: University of Chicago Press, 1985).

[21] Van Fraassen, *The Scientific Image*, 12.

[22] Leplin, *A Novel Defense of Scientific Realism*, 26.

[23] Kukla, *Studies in Scientific Realism*, 11. To be fair, Leplin seems to be referring to scientific theories regarding the unobservable aspects of the world. In his (1997), he claims that, according to minimal epistemic realism, it is possible to "warrant attributing some measure of truth to theories—not merely to their observable consequences, but to the theories themselves" (102).

true or highly likely to be true. Because the atomic theory seems to fit the criteria of being well-confirmed (perhaps as well-confirmed as any scientific theory governing unobservable aspects of the world), mature, and successful, the scientific realist will regard the atomic theory as either true, approximately true, or highly likely to be true.

Before Psillos' essay on the scientific realism debate, Richard Boyd wrote a similar essay nearly two decades earlier. In his statement of scientific realism, Boyd included not only versions of the three theses mentioned already, but a fourth thesis as well. He states his fourth thesis, a thesis about the progression of science, as the following:

> (4) *The historical thesis*: The historical progress of mature sciences is largely a matter of successively more accurate approximations to the truth about both observable and unobservable phenomena. Later theories typically build upon the (observational and theoretical) knowledge embodied in previous theories.[24]

As Boyd points out, this thesis distinguishes the scientific realist from empiricists like van Fraassen, who want to avoid committing to the possibility of scientific knowledge about the unobservable aspects of the world, and from constructivists like Thomas Kuhn, who want to admit of successive accumulations of scientific knowledge only within a general paradigm.

Both Larry Laudan and Ilkka Niiniluoto have provided similar but more in depth accounts of what they take to be scientific realism. Laudan presents scientific realism as a thesis with which he disagrees; after presenting his version of scientific realism, which he dubs convergent epistemological realism, he proceeds to argues against it. Here are the five claims that are constitutive of convergent epistemological realism (CER):

[24] Boyd, "The Current Status of Scientific Realism," 41-42.

(CER1) Mature scientific theories are typically approximately true, and, within the same domain, more recent ones are closer to the truth than older ones.

(CER2) Every term in a mature scientific theory, both observational and theoretical, genuinely refers.

(CER3) Successive theories in a mature science will preserve the theoretical relations and referents of earlier theories.

(CER4) Acceptable new theories do and should explain why their successful predecessors were successful.

(CER5) Theses (CER1) through (CER4) constitute the best, if not the only, explanation for the success of science, and the empirical success of science provides empirical confirmation for realism.[25]

Laudan admits that there may not be any scientific realist who subscribes to all five of these theses, yet he claims that this list might best capture the realism advocated by both Hilary Putnam and Richard Boyd. It is worth noting that with (CER5) Laudan, contra Psillos and Boyd, is presenting what appears to be an instance of IBE as a constitutive part of the thesis of scientific realism itself.

Niiniluoto defends what he calls critical scientific realism (CSR). His characterization of this position is delineated by the following six theses:

(CSR1) At least part of reality is ontologically independent of human minds.

(CSR2) Truth is a semantical relation between language and reality, given by the correspondence theory, and its best indicator is given by a systematic enquiry using the methods of science.

(CSR3) Scientific claims about the existence of theoretical entities have a truth value.

(CSR4) Truth is an essential aim of science.

[25] Laudan, "A Confutation of Convergent Realism," 20-21.

(CSR5) Although our best scientific theories can fail to be true, it is possible to approach the truth and to make rational assessments of such cognitive progress.

(CSR6) The best explanation for the practical success of science is that scientific theories are approximately true or sufficiently close to the truth in the relevant respects; thus, it is rational to believe that the use of the self-corrective methods of science in the long run has been, and will be, progressive in the cognitive sense.[26]

Once again, it is worth noting that Niiniluoto, in his last thesis, (CSR6), appears to include an instance of IBE in his description of scientific realism. By including an instance of IBE in their respective statements of scientific realism, both Laudan and Niiniluoto seem to view IBE to be so utterly essential for the scientific realist that they include it as part of their definition of the position.

4. Scientific Realism and IBE

My argument in this chapter establishes that scientific realism needs IBE. Although there appear to be many varieties of scientific realism, as I have shown, I nevertheless regard the most fundamental and essential tenet of scientific realism to be that scientists can formulate, and have formulated, scientific theories regarding aspects of the world which are unobservable that are true, approximately true, or highly likely to be true. I refer to this logically contingent claim, regarding the remarkable ability of scientists to successfully formulate such theories, as the *central claim of scientific realism* (CC).

CC: Scientists can formulate, and have formulated, scientific theories which posit unobservable entities that are true, approximately true, or highly likely to be true.

[26] Niiniluoto, *Critical Scientific Realism*, 10.

So far, the statements of scientific realism can be divided up between those that make an explicit reference to, or use of, IBE (Laudan's and Niiniluoto's), and those that do not (Psillos' and Boyd's).[27] If scientific realism is framed as a thesis that makes an explicit reference to, or use of, IBE, then it is easy to show that scientific realism requires IBE: scientific realism requires IBE because IBE is part of its definition. The justificatory status of IBE would be of the utmost importance to the scientific realist, for a successful argument against the justificatory status of IBE would be, without any further inferences, a successful argument against scientific realism itself. It is important to notice that these statements of scientific realism make use of IBE precisely because they are intending to establish that our best or most successful scientific theories (which will include ones that govern aspects of the world which are unobservable) are true, approximately true, or highly likely to be true. In other words, they are intending to establish CC by using an instance of IBE.

If, however, scientific realism is framed as a thesis that does not make an explicit reference to, or use of, IBE, then *either* it is framed as a thesis that simply accepts CC outright as a necessary condition of the view, *or* it is framed as thesis that does not accept CC at all. Psillos' and Boyd's statements of scientific realism do not make an explicit reference to, or use of, IBE, but they do accept CC. If scientific realism is a thesis that accepts CC, as it should, then, it will require the use of IBE, no matter what the details. This is established in the last section of this chapter.

Overall, my argument may be expressed as the following:

(1) Scientific realism is a thesis that *either* must explicitly reference or use IBE, *or* must accept CC (inclusively).

[27] Notice also that each one of these versions of scientific realism accepts CC.

(2) If scientific realism explicitly references or uses IBE, then IBE is required.

(3) If scientific realism accepts CC, then IBE is required.

(4) Therefore, IBE is required for scientific realism.

This is a valid argument; if the premises are all true, then the argument is sound. I argued that premise (2) is true, essentially by definition. The truth of (3) is established in a later section when I discuss the well-known No-Miracle Argument, and I argue the truth of (1) in the next section.

5. Scientific Realism as Merely an Aim of Science

There are alternative statements of scientific realism that neither make an explicit reference to, or use of, IBE, nor accept CC. This characterization of scientific realism emphasizes the aims of science over and above anything else. If the proponents of this characterization are correct, then the first premise of my argument is false. Fortunately, there are good reasons to reject this alternative.

In his statement of scientific realism, Niiniluoto includes the claim that science aims at truth (perhaps among other things). The previous statements of scientific realism which I have discussed do not explicitly bring up the aims of science; rather, they all seem to make an assumption about it. If scientific realism entails that mature science is successful (to the extent that our best mature and well-confirmed scientific theories are true, approximately true, or highly likely to be true), then the scientific realist surely must believe that this is what science was intending to accomplish, for the alternative seems to be to believe that this success was an accident or a coincidence. Other philosophers of science have offered drastically narrower versions of scientific realism; in particular, they

have explicitly identified the thesis of scientific realism to be mostly, if not entirely, about the aims of science. As Psillos indicates, perhaps this was initiated when Karl Popper suggested that science aims at providing satisfying causal explanations of the phenomena in which we are interested.[28] Simply describing what science aims to do, however, is a far cry from defining the thesis of scientific realism. Nevertheless, van Fraassen has defined scientific realism as a thesis that is solely about aims. He claims that the correct statement of scientific realism is the following (his italics):

> *Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true.*[29]

Alan Musgrave has also claimed that scientific realism is "first and foremost" a thesis about the aims of science, but it is not a thesis that is solely about the aims of science. He writes:

> [Scientific realism] says that the aim of a scientific inquiry is to discover the truth about the matter inquired into. This incorporates a semantic thesis (inquiry results in true or false statements about the world) and an axiological thesis (science aims for true statements).[30]

What is odd about these statements of scientific realism is that they omit entirely whether or not science has succeeded in their aim. It is consistent with van Fraassen's characterization of scientific realism, for instance, that science has failed to achieve its aim *in every respect*. In other words, van Fraassen is suggesting that scientific realism

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[28] Popper, *Realism and the Aim of Science*, 131-46. Psillos discusses this issue in his "The Present State of the Scientific Realism Debate," *The British Journal of the Philosophy of Science*, 51 (2000), 708.
[29] Van Fraassen, *The Scientific Image*, 8.
[30] Both quotes are from: Musgrave, A., "Realism, Truth and Objectivity," in R. Cohen, R. Hilpinen, and Q. Renzong (eds.), *Realism and Anti-realism in the Philosophy of Science* (Dordrecht: Kluwer Academic Publishers, 1996), 19.

could and would still be true even if science has never, throughout its entire history, formulated a true theory about the nature of the world in either its observable or its unobservable aspects. Moreover, the same can be said of the future of science: scientific realism could and would still be true, if van Fraassen is correct, even if science *will always fail* in its future endeavors to formulate true theories about the nature of the world. Both of these claims are absurd. First of all, scientific realists are optimists about science. The methods employed by science are not only the best available at finding out about the world, they work. Science helps us understand the world, as Psillos reminds us, by engaging in a reasoned and principled activity that pushes back the frontiers of ignorance.[31] The literature in this field is filled with philosophers of science who attribute to scientific realists the claim that certain scientific theories, mostly the mature ones, are at minimum approximately true.[32] If van Fraassen were correct, then all of these attributions would be incorrect. The problem is that van Fraassen's scientific realist may or may not accept that science succeeds—at least sometimes!—in formulating true theories about the nature of the world because scientific success is not an essential part of his statement of scientific realism. Secondly, within the field of the philosophy of science there have been criticisms leveled at scientific realists over the amount of success science is supposed to have enjoyed and what can or cannot be inferred from this success. One example of such a criticism is the pessimistic meta-induction. This criticism has been championed by Laudan, who argues that there is substantial inductive evidence for believing that the theoretical terms employed in our current best scientific theories fail to genuinely refer because of such cases that have occurred repeatedly in the past; namely,

[31] Psillos, "The Present State of the Scientific Realism Debate," 708.
[32] Including ones already discussed, even a partial list will include Psillos (1999), Boyd (1990), Niiniluoto (1999), Kukla (1998), Kitcher (1993), Newton-Smith (1989), Putnam (1975), and Laudan (1981).

the repeated existence of successful scientific theories that have been replaced. When this claim is combined with the view that non-referring scientific theories cannot be true, or even approximately true, as Laudan suggests, then it follows that there is substantial inductive evidence for believing that our current best scientific theories are not true or even approximately true. Laudan uses this argument against the scientific realist. If scientific realism was a thesis only about the aims of science, then Laudan's argument fails to be any criticism of scientific realism at all. Scientific realists would have ignored it and they would have been right to do so. On the contrary, scientific realists have generally taken Laudan's criticism very seriously and have responded extensively.[33]

6. The No-Miracle Argument

The most impressive, and the most central, argument for scientific realism has exploited the enormous successes of science. In its main version, this argument aims to establish that science's most mature and successful theories are true, approximately true, or highly likely to be true. Because the most mature and successful theories that science has to offer us currently include ones that attempt to govern unobservable aspects of the world and ones that posit the existence of unobservable entities, this argument also aims to establish the truth of CC. Although, as I point out, there are slightly different versions of the No-Miracle argument (NMA), the strongest versions clearly rely heavily on the idea that explanatory considerations serve as a guide to the truth—the core tenet of the IBE user. NMA is clearly meant to be part of the arsenal of arguments dispatched by

[33] A partial list of such responses includes: Hardin, C. and Rosenberg, A., "In Defense of Convergent Realism," *Philosophy of Science*, 49 (1982), 604-15; Worrall, J., "Structural Realism: The Best of Both Worlds?," *Dialectica*, 43 (1989), 99-124; Kitcher, P., *The Advancement of Science: Science without Legend, Objectivity without Illusions* (Oxford: Oxford University Press, 1993); and Psillos, S., *Scientific Realism: How Science Tracks Truth* (London: Routledge, 1999).

scientific realists. Thus, since NMA clearly intends to establish, perhaps among other things, the truth of CC, and since NMA is best expressed and understood as an instance of IBE, the importance of IBE in the scientific realist's quest in defending scientific realism and hence in defending CC is easily established. Here is the general formulation of NMA as an instance of IBE:

General Formulation of NMA:

(1) There are tremendously successful scientific theories which posit the existence of unobservable entities.

(2) If these theories were true (or at least approximately true), this would not only be a sufficiently good explanation of this success, it would best explain this success.

(3) Therefore, these scientific theories are true (or at least approximately true).

The first premise is referring mostly to the successful theories of a mature science; and a theory is considered successful when it not only accounts for, or accommodates, the relevant observable phenomena, but also when it generates novel predictions that are true. In this case, the explanandum is the empirical success enjoyed by scientific theories, and the explanans is that these scientific theories are true or approximately true. Of course, NMA also may be expanded to support not only a kind of epistemic optimism, as the above conclusion suggest, but also to support a more robust overall scientific realist attitude toward science. This less modest approach would call for a change in the wording of the argument. Although this reformulation would still be an instance of IBE, the premises would aim for a much broader conclusion in support of all aspects of scientific realism in general. Andre Kukla, for instance, claims that, rather than the

conclusion stated above, the rightful conclusion of NMA is that "we should all be scientific realists."[34]

As with any instance of IBE, the most plausible possible explanations are considered and the one deemed best is chosen. In the case of NMA, given how the chosen explanation seems to be quite obviously the best explanation, it may be difficult to even think of alternative possible explanations of the data. In fact there are some versions of NMA that simply assert the truth or approximate truth of certain scientific theories as the *only* reasonable explanation in existence. Hilary Putnam, for instance, famously provided a statement of this argument in 1975. Given Putnam's word usage, it is easy to see how the argument received its name.

> The positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle. That terms in mature scientific theories typically refer (this formulation is due to Richard Boyd), that the theories accepted in a mature science are typically approximately true, that the same term can refer to the same thing even when it occurs in different theories – these statements are viewed by the scientific realist not as necessary truths but as part of the only scientific explanation of the success of science, and hence as part of any adequate scientific description of science and its relations to its objects.[35]

Putnam is rightly suggesting that the enormous success of science is in need of an explanation; and the only reasonable explanation for this success is to take up a scientific realist attitude and accept that the theories themselves are at least approximately true. Most often it seems, Putnam is cited as being the author of this type of argument, but as Psillos indicates, the history of this type of argument may be traced back further, perhaps to the writings of Hans Reichenbach, Grover Maxwell, and J. J. C. Smart. For instance, Maxwell noticed the fact that there were scientific theories which posited the existence of

[34] Kukla, *Studies in Scientific Realism*, 12.
[35] Putnam, *Philosophical Papers, Vol. 1: Mathematics, Matter and Method*, 73.

unobservable entities and which were very successful. He also rightfully claimed that this success "cries out for explanation" and he concluded that, reasonably, there seems to be only one way to explain it:

> The thesis that theoretical entities are 'really' just 'bundles' of observable objects or of sense data, would, if true, provide an explanation; but it is not taken very seriously by most philosophers today—for the very good reason that it seems to be false. The only reasonable explanation for the success of theories of which I am aware is that well-confirmed theories are conjunctions of well-confirmed, genuine statements and that the entities to which they refer, in all probability, exist.[36]

Around the same time J. J. C. Smart produced a similar-looking argument, instead of referring to miracles, however, he refers to cosmic coincidences:

> If the phenomenalist about theoretical entities is correct we must believe in a *cosmic coincidence*. That is, if this is so, statements about electrons, etc., are of only instrumental value: they simply enable us to predict phenomena on the level of galvanometers and cloud chambers. They do nothing to remove the *surprising character* of these phenomena. …Is it not odd that the phenomena of the world should be such as to make a purely instrumental theory true? On the other hand, if we interpret a theory in the realist way, then we have no need for such a cosmic coincidence: it is not surprising that galvanometers and cloud chambers behave in the sort of way they do, for if there are really electrons, etc., this is just what we should expect.[37]

The miracle to which Putnam refers, and the cosmic coincidence to which Smart refers, would be that scientific instruments, like galvanometers and cloud chambers, behave exactly *as if* there were such things as electrons, protons, and ions. In other words, given that our scientific instruments behave exactly as if there were unobservable entities such as these, it is surely much more reasonable to believe that there *really* are such entities

[36] Maxwell, G., "The Ontological Status of Theoretical Entities," 18.
[37] Smart, *Philosophy and Scientific Realism*, 39, his italics.

populating the world—for the alternative is to believe in miracles or cosmic coincidences.

More recently, NMA was strengthened by Richard Boyd, who provided a more naturalistic bent. Boyd denies that the methods that ought to be used in the investigation of the nature of science are any different from the methods which science itself uses. Science is empirically successful, and for Boyd, this is a radically contingent fact. The success had by science is quite amazing since scientific practice, as Boyd points out, is so thoroughly theory-laden and dependent on background assumptions. The methods employed by science are instrumentally reliable, and this too is a radically contingent fact; there is no *a priori* justification for the inductive principles that are a part of the scientific methodology, and this presumably includes IBE. Boyd sees NMA not only as an instance of IBE, but also as a kind of defense of the reliability of IBE itself. Since IBE is so important within the scientific enterprise, and since IBE cannot be justified *a priori*, the empirical methods of science itself can be used to support the inductive inference pattern of IBE. Boyd makes this clear with the following passage:

> Central to the realist's argument is the observation that projectability judgements are, in fact, judgements of theoretical plausibility: we treat as projectable those proposals that relevantly resemble our existing theories (where the determination of the relevant respects of resemblance is itself a theoretical issue). The reliability of this conservative preference is explained by the approximate truth of existing theories, and one consequence of this explanation is that *judgements of theoretical plausibility are evidential*. The fact that a proposed theory is plausible in the light of previously confirmed theories is some evidence for its (approximate) truth. Judgements of theoretical plausibility are matters of inductive inference from partly theoretical premises to theoretical conclusions; precisely these inferences justify, and explain the reliability of, 'inductive inference to the best explanation.'[38]

[38] Boyd, "Realism, Approximate Truth, and Philosophical Method," 223-24, his italics.

Boyd suggests that IBE itself is reliable because such an inference, like all inductive inferences, depends heavily on background theories, and our background theories are at least approximately true. And he argues that our background theories are at least approximately true because this best explains the instrumental reliability and success of the methods employed by scientists. In this way, Boyd appears to understand NMA not only as an instance of IBE, but essentially as an instance of a *meta*-IBE. Individual scientific theories are judged to be approximately true, and these theories are supported and justified with the use IBE. Hence there is good reason to believe that IBE itself is a reliable inductive method for getting at the truth, for this best explains the success it has enjoyed in justifying individual scientific theories. Moreover, we can be confident that these scientific theories are at least approximately true because their approximate truth best explains the empirical success of science itself. Peter Lipton provided a clear summary of this point when he claimed that NMA "says we ought to infer first that successful theories are true or approximately true, since this is the best explanation of their success, and then that Inference to the Best Explanation is truth-tropic, since this is the method of inference that guided us to these theories."[39]

At this point, one may worry that Boyd's program appears to be circular or question-begging. In fact, critics of scientific realism have been quick to press this point. The most ardent critic has been Arthur Fine who has argued that NMA, and Boyd's version of it in particular, is viciously circular for it employs the one inferential method, IBE, whose justificatory status is precisely what is at stake.[40] Boyd alleges that his

[39] Lipton, *Inference to the Best Explanation*, 191.
[40] See for instance, Fine, A., "The Natural Ontological Attitude," in J. Leplin (ed.) *Scientific Realism* (Berkeley: University of California Press, 1984), 83-107; Fine, A., "Unnatural Attitudes: Realist and Instrumentalist Attachments to Science," *Mind*, 95 (1986), 149-179; Fine, A., "Piecemeal Realism,"

reasoning his immune to this criticism because he is offering not just a single argument but an entire scientific realist package.[41] The merits and pitfalls of this package in its entirety ought to be compared to those of competing alternative packages; and in the end, Boyd believes that, overall, his offering is better than any competitor. Whether Boyd succeeds in this defense is irrelevant to the overall goal of this section. For the purposes of establishing the truth of premise (2) in my overall argument—namely, that IBE is required for versions of scientific realism that accept CC—it is enough to point out, as I have, the extensive and essential use of IBE in the scientific realist's defense of their position with the use of NMA.

7. Conclusion

In this chapter I argued that scientific realism requires IBE. I explained the nature of IBE and I also explained the thesis of scientific realism. I alleged that the thesis of scientific realism must accept or entail what I dubbed the central claim of scientific realism (CC), namely that scientists can formulate, and have formulated, scientific theories which posit unobservable entities that are true, approximately true, or highly likely to be true. I suggested that the best way, and perhaps the only way, to justify this claim is by acknowledging that explanatory power is a guide to the truth; in other words, justifying this claim will require IBE. This line of reasoning is best captured in the form of an argument called the No-Miracle argument. I explained this argument in detail, providing several versions of it that have been given by a number of philosophers. The

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Philosophical Studies, 61 (1991), 79-96. Laudan also pushes this line of argument in his "A Confutation of Convergent Realism," *Philosophy of Science*, 48 (1981), 19-49.

[41] Apart from Boyd, the charge of NMA's vicious circularity has been taken up and defended by various other philosophers of science. While I am not convinced that NMA is viciously circular, attempting a defense of NMA against this charge is outside the scope of this chapter and this dissertation.

argument itself is sufficient to show that the thesis of scientific realism requires IBE to be justified. Hence, the scientific realist has a significant stake in the debate over the justificatory status of IBE.

CHAPTER THREE

The Argument from the Bad Lot

1. Introduction

In this chapter I present van Fraassen's first objection in a series of objections against the justification of IBE. This objection has been called, by others, the "Argument from the Bad Lot," and I shall stick with using this name.[42] It is not as straightforward as it may seem at first and so I begin by carefully presenting and analyzing van Fraassen's characterization of it. Once I establish what I consider to be a strong and charitable reconstruction of van Fraassen's objection as a two-premise argument, I allege that it fails to establish its conclusion of undermining the justification of IBE by arguing that each of the two premises of the argument is false.

2. The Problem

In order to properly refute van Fraassen's initial argument against the justification of IBE, it is necessary to first gain an adequate grasp of it. Below I examine and discuss several passages describing this objection, but I begin with the initial discussion of it, given by van Fraassen himself:

> . . . [IBE] is a rule that only selects the best among the historically given hypotheses. We can watch no contest of the theories we have so painfully struggled to formulate, with those no one has proposed. So our selection may well be the best of a bad lot. . . . [T]o believe the best explanation requires more than an evaluation of the given hypothesis. It requires a step beyond the comparative judgment that this hypothesis is better than its actual rivals. While the comparative judgment is indeed a 'weighing (in the light of) the evidence', the

[42] This objection seems to have no uniform name; in the literature, it has been called "the argument of the bad lot," "the argument from the best of a bad lot," and "the problem of the bad lot."

> extra step—let us call it the ampliative step—is not. For me to take it that the best of set *X* will be more likely to be true than not, requires a prior belief that the truth is already more likely to be found in *X*, than not.[43]

A bad lot or pool of possible explanations is a pool of possible explanations that does not contain the true explanation. Likewise, I shall call a good lot or pool of possible explanations one that does contain the true explanation.[44] In this passage, van Fraassen seems to worry that the explanation inferred by the IBE user just *might* be the best of a bad lot of possible explanations. I take this to be the crux of his objection. If the IBE user is inferring the truth of an explanation on the basis that, if it were true, it would be a sufficiently good explanation and the best one of the lot, what if the user were choosing from among a pool of possible explanations that were all false? Obviously, this would be a problem for the IBE user. If the IBE user is confident that the one possible explanation deemed best (and sufficiently good) from a relatively small pool of possible explanations is the true explanation, then the IBE user had better express similar confidence that the true explanation is already within the pool of possible explanations being considered. But, strictly speaking, this objection is ambiguous between at least two different meanings—two alternative interpretations of the objection.

If the objection is just that for every instance of IBE the IBE user *might* be using a bad lot, then there are two main ways the IBE user may quell this worry; I will refer to them as option (1) and option (2). Option (1) is for the IBE user to provide reasons to believe that the pool of possible explanations being considered is definitely not a bad lot, and option (2) is for the IBE user to provide reasons to believe that the pool of possible

[43] Van Fraassen, *Laws and Symmetry*, 142-43.
[44] This terminology is muddied up with the introduction of approximate truth. If an explanation can be approximately true, and if the best of a lot of possible explanations is merely approximately true, then the terms ‘good lot’ and ‘bad lot’ may be inadequate.

explanations being considered is unlikely to be a bad lot. These two options correspond to two different interpretations of van Fraassen's objection, interpretation (1) and interpretation (2).

> Interpretation (1): Van Fraassen is asking the IBE user to provide reasons to believe that, in each instance of IBE, a bad lot is not just unlikely, but that the true explanation is guaranteed to be among the ones being considered.
>
> Interpretation (2): Van Fraassen is asking the IBE user to provide reasons to believe that, in each instance of IBE, a bad lot is still possible but in fact it is unlikely.

If interpretation (1) is correct, then the IBE user must respond with option (1); and if interpretation (2) is correct, then the IBE user must respond with option (2).

If interpretation (1) is correct, then van Fraassen is claiming that the justification of IBE requires that the IBE user to rule out the possibility of working with a bad lot. This presumably means that the IBE user would have to provide some guarantee that the lot of possible explanations under consideration, for each use of IBE, contains the true explanation. This is a hefty requirement; perhaps too hefty. In fact, the demanding nature of interpretation (1) might be seen to lend support for the competing interpretation: interpretation (2). At the end of the above quote, van Fraassen alleges that every user of IBE must make an assumption that is unjustified ("a prior belief"), namely that it is more likely than not that the true explanation is contained within the pool of possible explanations under consideration. Or, in other words, this assumption, which is purported to be unjustified, is that it is more likely than not that the pool of possible explanations under consideration is not a bad lot. This is obviously a different demand from requiring every IBE user to rule out the possibility of starting with a bad lot; and a

less stringent demand. Applying interpretation (1) of van Fraassen's objection and thus defending option (1) is not only more demanding than the alternative, interpretation (2) and thus option (2), it will very likely require a different overall strategy to boot. Thus, understanding the apparent ambiguity of van Fraassen's objection and piecing out this apparent conflation between two different objections within van Fraassen's writing is extremely important. The goal is to reconstruct van Fraassen's objection in the most charitable way possible; and, later in this chapter, I propose that one interpretation constitutes a more reasonable objection than the other. Regardless of the interpretation, however, it is clear that in either case, van Fraassen is claiming that the IBE user must provide justification for an assumption (or "prior belief") that is being made in each instance of IBE; and that the IBE user will ultimately and inevitably fail to provide such a justification, no matter which assumption is correct.

Fortunately, the above quote of van Fraassen's is not the only time in which he discusses his objection. He teamed up with other empiricists in a later writing and purportedly spelled out his objection once again, using slightly different language, in the following passage. I suggest that this passage appears to support interpretation (1). They claim that

> …the potential explanations between which we can choose are the ones we have actually come up with. So to conclude that the best of these is true an additional premise is required, *viz*., that none of the possible explanations we have failed to come up with is as good as the best of the ones we have.[45]

Here the objection, again, amounts to an alleged unjustified assumption (or "additional premise") that every IBE user must make. This time, however, the assumption is stated

[45] Ladyman, Douven, Horsten, and van Fraassen, "A Defence of van Fraassen's Critique of Abductive Inference: Reply to Psillos," 306.

differently. The IBE user is required to assume that there is no possible explanation *that the IBE user has not considered* that is as good as the explanation inferred to be the best. In other words, the IBE user must assume that the explanation inferred is not only the best one of the pool of possible explanations currently under consideration, but also that it is the best one of all possible explanations, including ones that have yet to be considered. This different wording of the objection appears to support interpretation (1) for the following reason. The true explanation either will be located within the pool of possible explanations currently being considered or it will not. If it is, then there would not be any possible explanation that the IBE user has not considered that is as good as the one inferred with IBE. If it is not, then presumably there would be a possible explanation, the true one, that the IBE user has not considered that is as good as (and in fact better than) the one inferred with IBE.[46] Consequently, the claim made in this more recent passage—namely, that every IBE user must provide a reason to believe that there is no possible explanation that the IBE user has not considered that is as good as the explanation inferred with IBE—is essentially the same proposition stated in interpretation (1); namely, that every IBE user must provide reasons to believe that the true explanation is guaranteed to be among the possible explanations being considered.

Further in the same passage, however, van Fraassen, et al., attempts to explicate his criticism once more. This time, however, the explication seems to suggest interpretation (2). They write that:

> …what van Fraassen actually argues is that 'our best theory may well be "the best of a bad lot"', not that it is more likely to be than not. This suffices for the argument, since the connection between the best available explanation and truth is

[46] In this discussion I assume there is a unique best explanation; namely the true one.

> only assured (and then only probabilistically, of course) if it is more likely that the truth lies inside the range of hypotheses being considered.[47]

Here we are told that what suffices for the criticism is that the explanation inferred by the IBE user *might* be the best of a bad lot—a reiteration of his initial (ambiguous) statement of the objection. But the very next sentence alleges that every IBE user must assume (and thus provide reasons to believe) that it is more likely that the true explanation lies within the pool of possible explanations being considered. This is best understood with interpretation (2).

Before I argue for and settle on an interpretation and reconstruct van Fraassen's objection in an unambiguous manner (and subsequently argue against it), it will be helpful to understand a different perspective on the objection, one given by Stathis Psillos.

3. Psillos' Attempt at Reconstruction

The ambiguity of van Fraassen's objection has not gone unnoticed. Psillos, for example, considers three alternative ways van Fraassen's objection might be interpreted.[48] These three interpretations are meant, it seems, to exhaust the spectrum of possibilities: (1) that a bad lot is merely possible, (2) that a bad lot is possible but unlikely, and (3) that a bad lot is possible and likely. Psillos entertains but dismisses the first two interpretations, and regards the third interpretation as the only reasonable way to understand van Fraassen's objection. For the sake of thoroughness, it is worth considering each of these three interpretations.

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[47] Ladyman, Douven, Horsten, and van Fraassen, "A Defence of van Fraassen's Critique of Abductive Inference: Reply to Psillos," 306.
[48] Psillos, *Scientific Realism: How Science Tracks Truth*, 216-17.

The first is that van Fraassen, when claiming the best *might* be the best of a bad lot, intended his objection to be that it is *logically possible* that the lot of possible explanations under consideration is a bad lot—for each and every instance of IBE. And thus no use of IBE is justified if it is logically possible that the lot of possible explanations under consideration is a bad lot. This would mean that the IBE user is being asked to show that, when using IBE, it is not logically possible that the pool of possible explanations currently being considered is a bad lot. Asking the IBE user to guarantee this is asking quite a bit. Psillos rightfully contends that this is too large of a demand placed on the IBE user. Any user of IBE must be allowed the logical possibility that the true explanation is not contained within the pool of possible explanations being considered "for surely there is no a priori warrant that scientists will invariably have hit the truth."[49] Consequently, Psillos concludes that this is not the proper interpretation of van Fraassen's objection.

Within this interpretation, however, Psillos considers that perhaps van Fraassen intends his objection to be that the possibility of a bad lot must first be eliminated prior to establishing that there are good reasons for believing that the current lot is not a bad lot. He contends again that this is too demanding, and consequently, not what van Fraassen has intended. Firstly, he points out that if this were the objection then van Fraassen's own epistemology would be in danger.[50] And secondly, he argues that the position taken by this objection would actually lead to outright skepticism, a position that even van

[49] Psillos, *Scientific Realism: How Science Tracks Truth*, 216.
[50] Again, a good explanation and discussion of van Fraassen's 'new' epistemology can be found, among other places, in his *Laws and Symmetry*, specifically 129-214, and in his 'Empiricism in Philosophy of Science', in P. M. Churchland and C. A. Hooker (eds), *Images of Science* (Chicago: University of Chicago Press, 1985).

Fraassen wants to avoid: "very few beliefs, if any, can be warranted if the notion of warrant involves elimination of the possibility that the belief be false."[51]

However, there is distinction here that needs to be noticed. There may be two separate logical possibilities being discussed by Psillos. One is that it is logically possible that the current lot is a bad lot. And the other is that it is logically possible that the explanation inferred by the IBE user is false. These are different logical possibilities. Even if the current lot of possible explanations under consideration is not a bad lot, there is a logical possibility that the inferred explanation is false. For instance, it might be that this pool of possible explanations actually does contain the true explanation, but the IBE user infers incorrectly that another possible explanation, a false one, is the case. The IBE user may do this if the inferred (false) explanation exhibits many of the explanatory virtues that true ones tend to have.

This latter way of understanding the objection is tempting, but ultimately it fails. When van Fraassen suggests that the chosen explanation *might* be the best of a bad lot, this may be taken to mean that the chosen explanation *might* be false. And one way to interpret this is with logical possibility: if the chosen explanation might be false, this just means that it is logically possible that it is false. Given that IBE is an inductive inference, it is perhaps by definition that the conclusion of the inference *might* be false (assuming that the conclusion is not a tautology). And so the objection to IBE cannot be that the conclusion might be false. This would be like objecting to IBE just because it is inductive—a true allegation, but not a criticism.

The former way of understanding the objection is the one which Psillos does consider (and reject): the logical possibility of using a bad lot when using IBE. This

[51] Psillos, *Scientific Realism: How Science Tracks Truth*, 216.

interpretation is a stronger version of interpretation (1), above. Recall that van Fraassen's objection consists in pointing out the alleged unjustified assumption that every IBE must make. Under interpretation (1), this assumption is the lot of possible explanations under considered is guaranteed not to be a bad lot. If Psillos' interpretation is correct, then van Fraassen's objection amounts to a different assumption; namely that it is not logically possible that the lot of possible explanations under consideration is a bad lot. There are guarantees and then there are reasonable guarantees. Asking the IBE user to guarantee that the lot of possible explanations under consideration is not a bad lot is one thing, but asking the IBE user to guarantee that it is logically impossible for the lot of possible explanations under consideration to be a bad lot is most certainly something else. It is an absurd requirement. If this were the assumption that every IBE user needed to make in order for their inference to be justified, there would indeed be very few, if any, justified instances of IBE. The only way to show that a bad lot is not logically possible is to show that the existence of a bad lot, when using IBE, would be a logical contradiction, or entail a logical contradiction. In either case, this requirement is obviously unrealistic; and thus it is a mistake to interpret van Fraassen's objection in this way.

The next interpretation Psillos considers is whether van Fraassen's objection amounts to claiming not merely that coming up with a bad lot is logically possible, but that coming up with a bad lot is in fact unlikely. He quickly dismisses this interpretation, as this position might very well characterize an average scientific realist or IBE user, just the sort of person van Fraassen, presumably, is arguing against. Obviously, Psillos is right to dismiss this possible interpretation of the criticism; this is no criticism at all.

The third interpretation is the one Psillos pursues. According to it, van Fraassen's objection is that, for each use of IBE, it is likely (or more likely than not) that the lot of possible explanations under consideration is a bad lot. Here is Psillos on his take:

> Van Fraassen may mean that it is likely (or, more likely than not) that our best theory is 'the best of a bad lot'. This is, I think, the only reasonable interpretation of van Fraassen's argument. . . . So I take it that van Fraassen's point is that unless an unwarranted privilege is appealed to, *it is more likely that the truth lies in the space of hitherto unborn hypotheses*.[52]

The last sentence, in italics, is the interpretation of van Fraassen's objection that Psillos maintains. One may wonder just how different the following two claims are: (1) the probability of getting a bad lot is likely, and (2) the probability of getting a bad lot is more likely than not. In this passage, Psillos seems to write as if they are interchangeable. I will assume the difference is negligible, and consequently ignore Psillos' mentioning both claims in his interpretation and just focus on (2), as Psillos seems to do at the end.[53]

It appears, then, that if Psillos is correct, van Fraassen is arguing that if we cannot legitimately make an appeal to privilege, then the lot of possible explanations under consideration is more likely to be a bad lot than not. Incidentally, in this interpretation, Psillos mentions an "appeal to privilege." This is an important concept and it will be used in my case against van Fraassen's objection. I expound on this concept in a later section; for now, it is sufficient to point out that the appeal to privilege may be

[52] Psillos, *Scientific Realism: How Science Tracks Truth*, 217 (his italics).
[53] If (2) is true, then this would seem to entail (1), given no third alternative. But does the truth of (1) entail (2)? If "likely" is interpreted as having a probability of at least .51, then it seems so; but this is an unusual interpretation of the word "likely."

understood as a way for the IBE user to justify their assumption (or prior premise) that the lot of possible explanations under consideration is more likely than not a good lot.

It is important to note that Psillos appears to interpret van Fraassen's objection not as an unjustified assumption (or prior premise) that each IBE user must make, as van Fraassen appears to allege. Rather Psillos interprets van Fraassen to be making a positive allegation regarding the probability of generating a pool of possible explanations that contains the true explanation. He understands van Fraassen to be claiming that, in each instance of IBE, the true explanation will likely not be within the pool of possible explanations under consideration (unless the IBE user is privileged in generating the pool). And because of this, Psillos criticizes van Fraassen for not providing any good reasons for believing that the true explanation will always be more likely to fall outside of the pool of possible explanations considered by the IBE user (unless an appeal to privilege is made). But this is a contentious way of interpreting the objection. Van Fraassen himself disagrees with Psillos' characterization, calling it a misrepresentation, and rebuffs the idea that the burden of proof falls onto van Fraassen's shoulders. Van Fraassen, et al., writes:

> So whereas Psillos challenges van Fraassen to show that it is more likely that the truth is outside the range [of possible explanations being considered], van Fraassen need only ask the proponent of IBE for reasons for believing that the truth is inside it.[54]

Once again, van Fraassen states his objection, and once again, it is stated in an ambiguous fashion. By asking the IBE user to provide reasons to believe that the true explanation is included within the pool of possible explanations being considered, it is

[54] Ladyman, Douven, Horsten, and van Fraassen, "A Defence of van Fraassen's critique of abductive inference: Reply to Psillos," 306.

not clear whether van Fraassen is demanding that the IBE user provide reasons to believe that the true explanation is *most definitely* within the pool of possible explanations being considered (interpretation (1)), or whether he is demanding that the IBE user provide reasons to believe that the true explanation is merely *likely to be* within this pool (interpretation (2)). It is interesting that Psillos alleges van Fraassen to have the burden of providing reasons to believe that the true explanation will always be more likely to fall outside the pool of possible explanations considered by the IBE user, but yet Psillos himself proceeds to provide reasons to believe that the IBE user will likely be privileged—and thus, will be able to provide reasons to believe that the true explanation is likely to be among the ones considered. This may just be an instance of Psillos covering all of his bases: first allege that van Fraassen has the burden of proof to establish that for each instance of IBE a bad lot is likely, but then go on the offensive in order to establish that for each instance of IBE a bad lot is unlikely.

4. The Argument from the Bad Lot

Even if I am correct about the ambiguity of van Fraassen's objection—that there are two ways to interpret it—there are good reasons to believe that one of the interpretations is a more charitable, and hence a more reasonable objection, than the other. Aside from the textual evidence, there are two reasons to prefer interpretation (2) over interpretation (1). These reasons are closely related to one another. The first is the demanding nature of interpretation (1). It is entirely unreasonable to demand that the IBE user guarantee that the true explanation will lie within the pool of possible explanations each and every time IBE is used, and then to object to the justification of the inference

when no guarantee is given. Moreover, by what kind of process can the IBE user guarantee that the true explanation is among the possible explanations being considered? I am not aware of any process that could provide such a guarantee. Certainly, if this process existed, then those who are interested in attaining knowledge of the world would be extremely keen to adopt it. Expecting an IBE user to employ a process that always, without exception, generates a pool of possible explanations which contains the true one is entirely unrealistic and thus entirely unreasonable. This is the second reason.

With this in mind, I offer the following two-premise argument to characterize van Fraassen's objection, the argument from the bad lot:

(1) An instance of IBE is justified only if the IBE user has a good reason to hold that the lot of possible explanations under consideration is likely not a bad lot.

(2) The IBE user can never have a good reason to hold that the lot of possible explanations under consideration is likely not a bad lot.

(3) Therefore, no instance of IBE is justified.

This is a valid argument; the premises, if true, guarantee a true conclusion. I allege that the conclusion is false, and I argue that both premises are false. I begin by undermining the second premise. This is the easier of the two tasks. This premise will be shown to be false by making an appeal to privilege; that is, background knowledge and approximately true background beliefs may provide the IBE with sufficiently good reasons for holding that the lot of possible explanations under consideration is likely, or even highly likely, to be a good lot. In the next section I argue that the first premise is false. The key to this argument will be the tenability of a sufficiently probabilized version of IBE.

5. Undermining the Second Premise

One way to understand premise (2) is with the concept of privilege. Van Fraassen and Psillos both discuss the appeal to privilege, albeit somewhat differently, in relationship to the proposition stated in premise (2). Even though van Fraassen and Psillos appear to have a different notion of what the appeal to privilege amounts to, it nevertheless may be understood as a way for the IBE user to justify their assumption (or prior premise) that the lot of possible explanations under consideration is likely a good lot; and hence may be understood as a way of undermining premise (2).

Van Fraassen anticipates the IBE user making an appeal to privilege as a response to his objection. He explores the idea behind it and concludes that the IBE user will not be able to justify being privileged. For van Fraassen, to appeal to privilege is "to glory in the belief that we are by nature predisposed to hit on the right range of hypotheses."[55] Thus, the appeal to privilege consists in the ability to be disposed to come up with a lot of possible explanations that contains the true one. He contends that justifying such privilege must take one of two forms, each stemming from the epistemological doctrines of naturalism and rationalism, respectively. The naturalist justification of privilege, he claims, would be an evolutionary one. Our evolutionary successes must point to a certain aptitude that we possess. The possession of this aptitude, or privilege, would explain the successes we have had. Van Fraassen argues that this reaction fails, as he rightly points out that evolutionary successes do not guarantee an aptitude of coming up with the truth. Simply put, our evolutionary successes are consistent with our not being privileged: one may be successful, practically speaking, without having a grasp or understanding of the truth. And thus, our evolutionary successes can be explained without any appeal to

[55] Van Fraassen, *Laws and Symmetry*, 143.

privilege. For example, it could be that we have evolved to have the proper mechanism, or mechanisms, that would enable us to eliminate the theories with observable consequences that turn out to be false. Perhaps the scientific method is one such mechanism. Using this mechanism then would tend to leave untouched the theories with observable consequences that turn out to be true. Consequently, our successes would be explained.[56] And this may be done without any appeal to the truth of the theory, and thus without any appeal to privilege.

The other attempt at justifying our privilege, according to van Fraassen, has roots in epistemological rationalism. This is the idea that we have certain bits of knowledge that are either due to our rational nature (as in knowledge that is *a priori*) or that are attained through our intuition (or deduced from such propositions). Our privilege would then be a product of our rational nature, our intuition, our innate ideas, or of something similar. In his discussion van Fraassen likens this approach to both Descartes' argument for the correspondence of our ideas to reality and to Plantinga's theistic epistemology. In arguing for the claim that our ideas do in fact generally correspond to reality, both Descartes and Plantinga ultimately appeal to God, or perhaps, more specifically, to God's *characteristics*, or, to be even more sure, to our *beliefs* about God's characteristics; namely, God as correspondence guarantor. Van Fraassen's only critique of such an approach is to point out rightly that the existence of God, or of certain characteristics of God, or of the characteristics we believe God to have, do not guarantee that we would be privileged in all of the relevant areas. With this rather short argument, van Fraassen dismisses the rationalist justification of privilege.

[56] Van Fraassen offers this Darwinian type of explanation for the successes of our current scientific theories; see van Fraassen, *The Scientific Image*, 40.

It is unclear why the appeal to privilege needs to be characterized in the way van Fraassen claims. Psillos' characterization of the appeal to privilege appears to get the job done; and in fact, his characterization of the appeal to privilege sufficiently undermines van Fraassen's second premise.

For Psillos to be privileged is to work within a network of background knowledge or approximately true background beliefs that help narrow the field of reasonably plausible possible explanations to be considered. He argues that IBE users, scientists in particular, are privileged in this sense.[57] Psillos refers to it as the "appeal to background knowledge privilege" and he claims that this idea has been argued for by others as well, namely by Richard Boyd and Peter Lipton.[58] All of them have suggested that it is seldom in science, if ever, that IBE is used without involving a great deal of background knowledge and approximately true background beliefs. When scientists choose among competing hypotheses that all may, for example, be empirically equivalent, they have at their disposal a wealth of background knowledge and approximately true background beliefs which may guide the choice, or inference, in various ways.[59] He claims:

> …it is at least dubious and at most absurd that theory choice operates in such a knowledge vacuum. Rather, theory choice operates within and is guided by a network of background knowledge.[60]

And it is this network of background knowledge and approximately true background beliefs that provides the privilege, for this network can severely cut down on the number

[57] Given that Psillos is interested in defending a version of scientific realism, he tends to keep the discussion of IBE limited within the realm of science.
[58] See for instance, Boyd (1984) and Lipton (2004).
[59] Others have argued likewise; see, for example, Boyd, R. "The Current Status of Scientific Realism", in J. Leplin (ed.), *Scientific Realism* (Berkeley: UC Press, 1984).
[60] Psillos, *Scientific Realism: How Science Tracks Truth*, 217.

of possible explanations to include in the pool. Moreover, when this network of background knowledge and approximately true background beliefs is combined with explanatory considerations, then the scientist has more than enough privilege to justify claiming that the true explanation is likely (or even highly likely) to be among the possible explanations being considered. He discusses an actual scientific episode to illustrate his claim. And he then concludes:

> The first aspect [of the background knowledge privilege] is that background knowledge can drastically narrow down the space in which hypotheses can provide a potential explanation of the evidence at hand. …The second aspect is that when the background knowledge does not suggest just one theoretical hypothesis, then explanatory considerations—which are part and parcel of scientific practice—are called forth to assist the selection of the best from among the hypotheses which entail the evidence. …I think both aspects of the 'background knowledge privilege' make it plausible that, contrary to van Fraassen's claim, scientists can have strong evidence for the belief that the best explanation is also the correct account of the phenomena.[61]

I believe this is all correct. The more relevant background knowledge and approximately true background beliefs, all of which can be and should be considered evidence at the disposal of the IBE user, the more likely the IBE user will begin with a pool of possible explanations that contains the true one. As science has progressed, as the batches of scientific knowledge have accumulated, there is more and more background knowledge, or evidence, had by the scientist community. This increases the sense that the scientist is privileged, and certainly provides a good reason for believing that the IBE user qua scientist is more likely than not going to begin an instance of IBE with a good lot.[62]

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[61] Psillos, *Scientific Realism: How Science Tracks Truth*, 218-19.

[62] There are philosophical questions regarding the nature of scientific progress and the meta-pessimistic induction that I do not discuss here. It might also be important to point out that Psillos' response seems to

All of this provides convincing reasons to believe that privilege, in Psillos' sense, is not only possible, but that privilege is obtained in a great number of cases. At this point, however, it is important to distinguish between being privileged in general and being privileged in particular cases. To be privileged in general, presumably, would be to have a general predisposition to include the true explanation among the pool of possible explanations to be considered for instances of IBE concerning broad areas. This would be the result of having a great deal of background knowledge and approximately true background beliefs covering a broad spectrum. I imagine this might be said (by some) about general groups of people, like scientists, for example. But it could be that the privilege is relative to different domains of study, different disciplines, or just different areas and levels of background knowledge. An example will illustrate this point. It could be that a medical doctor uses IBE to infer that her patient has chickenpox. If the justification of IBE depends on privilege, a good case could be made that this doctor is privileged regarding this particular diagnosis; that is, given the symptoms of the patient, the details of the case, and the doctor's extensive medical knowledge and experience, it just might be that the doctor is privileged in the sense of being sufficiently knowledgeable to come up a pool of possible explanations that contains the true one. And we could plausibly come up with good reasons to believe the doctor is privileged in this instance prior to the doctor's use of IBE. I do not see why the doctor in this case would not be able to adopt and follow IBE as a rational rule of inference *in cases of this type*. Or maybe even in *all* medical diagnoses. It is surely dependent on the background knowledge, the approximately true background beliefs, and thus, the general expertise of

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beg the question that the IBE user will be in possession of background knowledge and approximately true background beliefs. He anticipates this problem and responds to it. See Psillos, *Scientific Realism: How Science Tracks Truth*, 219-22.

the doctor, or of the IBE user, in question. Providing good reasons to believe that one is privileged with regards to a particular use of IBE will be, in most cases, tantamount to providing good reasons to believe that one's general expertise in the relevant areas is sufficient. Given the above discussion, having good reasons to believe that one is privileged, in this sense, is thus not only possible, it has been achieved and will continue to be achieved.

6. Undermining the First Premise

Van Fraassen also anticipates that the IBE user may allege that this entire objection has been incorrectly predicated on a faulty notion of IBE. Perhaps IBE is not the straightforward inductive inference that is portrayed in the objection. For van Fraassen, the IBE user who is making this type of allegation is "retrenching."[63] In general, to retrench is to declare that van Fraassen has set up the problem incorrectly. It is to suggest that the name "inference to the best explanation" is misleading. This mode of inductive inference is not suggesting that the possible explanation deemed best should be inferred to be utterly and completely true. In other words, IBE is not a mode of inductive inference which sanctions that the possible explanation deemed best from among a pool of possible explanations is the one we ought to deem true, end of story. Rather, the inference is, and should be, more modest. Perhaps the real rule of IBE, van Fraassen claims, merely suggests that "explanatory power is a mark of truth, not infallible, but a characteristic symptom."[64]

[63] Van Fraassen, *Laws and Symmetry*, 145-49.
[64] Ibid., 146.

This retrenchment may take two forms, according to van Fraassen. The first form, form 1, suggests that theories are made likely (or more likely) to be true based on their explanatory features. Consequently, the inference that is made when using IBE is one that simply utilizes this fact. If this is the case, then it is easy to see the misnomer, for when using the rule, IBE, we would not be inferring that the best explanation is flatly true, as in, for example, a logical entailment; rather we would be inferring that the best explanation is one that is likely (or more likely) to be true. Explanatory considerations, all things being equal, merely increase the probability of the hypothesis (or possible explanation) being true. Van Fraassen does provide an argument against the claim that the best explanation we have, given the possible ones under consideration, is likely to be true, but this argument, known as the argument from indifference, fails—as I show in the following chapter.

The second form of retrenchment, form 2, regards the explanatory considerations of theories as rationally required factors in updating one's beliefs in the face of new information or evidence. IBE becomes, then, a probabilistic rule for rational belief-updating in the face of new information or evidence. The explanatory success a theory experiences is accounted for, as a matter of rationality, when updating or changing one's beliefs. The only difference between form 1 and form 2 is that in form 2 the explanatory considerations are made to be part of the rational rule of inductive confirmation. Van Fraassen provides an argument against this approach as well, but this argument also fails—as I show in chapter five.

Fortunately, either form of retrenchment, form 1 or form 2, is sufficient to undermine the first premise of van Fraassen's argument. The best way for the IBE user

to retrench is to defend the tenability of a sufficiently probabilized version of IBE. This approach marries the essence of IBE with the merits of a form of epistemological probabilism.

One way to understand this approach is by noticing how modest or immodest the IBE user may be in inferring the truth of the conclusion. For example, a medical doctor may gather information about a patient and see that the patient has several symptoms of an illness; say, a fever, a headache, general discomfort, and a red rash. From a pool of possible explanations of these symptoms, the doctor may reason that if the patient had chickenpox, this would not only be a good explanation of the symptoms, it quite reasonably would be the best explanation of them. However, the doctor may also notice that the patient's symptoms are consistent with other diseases or conditions as well.[65] The doctor may realize that if the patient had smallpox, for example, no matter how improbable this may initially be, this would also explain the symptoms quite well. For this reason, the doctor may decide that the confidence she has in the patient's having chickenpox ought to be less than certainty. And this indeed may be the most, or only, reasonable position to take. Consequently, in using IBE the doctor may infer that the patient is very likely to have chickenpox, rather than inferring that the patient most certainly has chickenpox. The IBE user may infer that the best explanation is true, that the best explanation is very likely to be true, or that the best explanation is just likely to be true. The probability of the explanation in the conclusion is reflected by the modesty of the IBE user. The basic idea is that the IBE user need not fully believe that the conclusion of the inference is absolutely true. Accepting an explanation does not imply

[65] Obviously, further tests, further analysis, or new information about the patient may help trim down the number of diseases or conditions that are consistent with the symptoms, and consequently trim down the number of possible explanations in the pool being considered.

believing it with certainty. This kind of epistemological probabilism is best understood in the terms and conditions of Bayesianism.[66]

Briefly, Bayesians claim that belief is a matter of degree. This degree can be represented in terms of probabilities. The probability of a statement, *E*, may range between full belief that *E* is certainly true, P(*E*) = 1 (usually reserved for tautologies), full belief that *E* is certainly false, P(*E*) = 0 (usually reserved for logical contradictions), or any real number between 0 and 1. Thus my belief that there is a 90% chance of rain tomorrow may be interpreted as my assigning a probability to the statement that it will rain tomorrow, P(*E*) = .9. Bayesians also connect this probability statement to betting behavior. If I assign P(*E*) to .9, then perhaps I am reporting that I am willing to bet on rain tomorrow with the odds of 9:1. A further requirement is that these degrees of belief ought to follow and be understood by the principles of the probability calculus. This will include inductive confirmation. When belief is understood in terms of degrees, the notion of confirmation or evidence is made clear. When, in accordance with the probability axioms, the probability of a hypothesis, *H*, is increased with the introduction of a new statement, *E*, then *E* is evidence for *H*, or *E* confirms *H*.[67] This process is conditionalization; any change in a person's beliefs, which amounts to a decrease or increase in the belief's probability, is rationally bound by the probability axioms. Bayes' theorem has been the most widely used method to accomplish this:

$$P(H|E) = P(E|H) \times P(H) / P(E)$$

[66] As with most –isms, Bayesianism has many forms. I am not interested in the various forms, just in the essence of Bayesianism with which most, if not all, Bayesians agree. See, for example, Earman, J., *Bayes or Bust? A Critical Examination of Bayesian Confirmation Theory* (Cambridge: MIT Press, 1992).
[67] I list and discuss the probability axioms in chapter 5.

On the left side of the equality, we have the probability of hypothesis, *H*, conditional on evidence, *E*. This is the posterior probability of *H*; that is, this represents the degree of belief that ought to be held given the addition of some piece of evidence, *E* (along with other assumptions). This posterior probability is determined by the right side of the equality. The first part, P(*E*|*H*), represents the probability of *E* given the truth of *H*; this is also referred to as the likelihood of *H*. And the other two, P(*H*) and P(*E*), represent the prior probabilities of *H* and *E*, respectively. The prior probability of *H*, for example, would be the probability assigned to *H* prior to the introduction of evidence *E*. With the introduction of the concept of conditional probability, Bayes' theorem is derivable from the probability axioms themselves.

The principle of conditionalization states that the newly updated probability of a proposition (when the evidence has come in, so to speak) ought to equal the old conditional probability of the proposition given the evidence (when the evidence was not yet known). If we let P_{old} be the old prior probability assignment, let P_{new} be the new posterior probability assignment, and let *X* be some proposition and *E* be the total amount of evidence gained between the old assignment and the new one, then the principle of conditionalization may written as follows:

$$P_{new}(X) = P_{old}(X \mid E)$$

A justification for this principle was given by Paul Teller who attributes it to David Lewis.[68] Teller and Lewis argued that violating this principle will inevitably lead to the possibility of being Dutch booked. Assuming that a person's degrees of belief can be interpreted as fair betting prices for the truth of the belief, a Dutch book is a series of bets which are bought or sold at prices that guarantee a net loss. For most Bayesians, if a person is susceptible to being Dutch booked, then the person's beliefs are either inconsistent in themselves or they violate at least one of the probability axioms. This topic is discussed in further detail in chapter five; for the point at hand, the upshot of this discussion is that there is powerful motivation to obey the principle of conditionalization. Typically, Bayesians regard a violation of this principle to be a mark of irrationality.

One way to combine the essences of Bayesianism and IBE is for explanatory considerations to play a role in the determination of the posterior probability of hypotheses, without upsetting the integrity of the conditionalization process. This may be accomplished by accounting for the explanatory power of a hypothesis in the development of the prior probability of the hypothesis. By accounting for the explanatory considerations of a hypothesis in its prior probability, the equality expressed in the principle of conditionalization may be left intact and remain uncompromised.[69] This process allows the IBE user to recognize the explanatory power of a possible explanation, to allocate an increase in the probability of the possible explanation (because of its explanatory power), and to obey the rules and norms of Bayesianism. One additional upshot of probabilizing IBE is that the conclusion would no longer be just a

[68] See Teller, P., "Conditionalization and Observation," *Synthese*, 26 (1973), 218-258. Lewis' version can be seen in his "Why Conditionalize?," *Papers in Metaphysics and Epistemology* (Cambridge: CUP, 1999), 403-07. I explain the basic argument for this principle in chapter 5.

[69] Once again, I provide further details of how a probabilized version of IBE is tenable in chapter 5.

black-and-white matter of truth or falsity. The inferred explanation could be taken as a degree of belief, somewhere between 0 and 1 (though in most cases the degree of belief will be much closer to 1 than to 0).

How would probabilizing IBE negate the first premise of van Fraassen's argument? The first premise of van Fraassen's argument is essentially stating a necessary condition for the justification of an instance of IBE; namely, that the IBE user is required to have a good reason to believe that the lot of possible explanations under consideration is likely not a bad lot. If IBE is not construed probabilistically, then this condition does appear to be necessary. But when IBE is sufficiently probabilized, then this condition is not required in order for an instance of IBE to be justified.

To illustrate this, I start with the first case. Let us assume a non-probabilized version of IBE: the IBE user bluntly infers the belief that the best of the pool of possible explanations being considered is the true one. Van Fraassen admits that to believe a proposition is to consider it to be more likely to be true than not.[70] In terms of probability, I understand this to mean that when the IBE user infers the truth of the conclusion, and consequently believes it to be true, the IBE user assigns it a probability that is greater than .5. At this point, van Fraassen's objection can be described in the following way: in order for the IBE user to make this probability assignment (and hence to believe that the best explanation, i.e., the conclusion, is true), the IBE user needs to also believe that the truth is already within the pool of possible explanations that he or she is considering. In other words, the IBE user is surely required to assign a probability of at least .5 to the proposition that the pool of possible explanations being considered is a good lot if he or she is assigning a probability of at least .5 to the possible explanation

[70] Van Fraassen, *Laws and Symmetry*, 143.

deemed best—anything less than .5 could not yield (without violating a standard of consistency) a probability greater than .5 to any particular possible explanation within the pool. This means that if the subjective probability of using a good lot is less than .5 then the IBE user could not rationally conclude that the probability of one of the possible explanations within the pool is greater than .5. Thus, when IBE is construed non-probabilistically, van Fraassen's proposed necessary condition appears to apply—the IBE user appears to be required to provide a reason to believe that the lot of possible explanations under consideration is likely a good lot.

However, this proposed necessary condition does not apply when IBE is construed probabilistically. The essential outcome of combining IBE with Bayesianism is Bayesianism with a nod toward explanatory considerations within the process of conditionalization. If explanatory power increases the probability of a hypothesis being true (all things being equal) then once an IBE user recognizes that a hypothesis would be a good explanation, if it were true, the result (all things being equal) would be an increase in the probability of the hypothesis being true. This increase of probability could happen even if the subjective probability of using a good lot is less than or equal to .5. In fact, so long as the IBE user assigns a probability which is greater than 0 to the proposition that the pool of possible explanations being considered is a good lot, it is possible to increase the probability of a possible explanation as a result of its potential explanatory power. Therefore, assuming a probabilistic version of IBE, the proposed necessary condition is dramatically weakened: instead of requiring the IBE user to assign a probability of at least .5 to the proposition that the pool of possible explanations being considered is a good lot, the probabilistic IBE user is merely required to assign a probability that is

greater than 0 to the same proposition. Obviously, this will not be a problem; given that Bayesians will typically reserve the 0 probability assignment for logical contradictions, the probabilistic IBE user must simply avoid generating a pool of possible explanations that somehow constitutes, or entails, a logical contradiction.

7. Conclusion

In this chapter I rebutted van Fraassen's argument from the bad lot. I first carefully analyzed van Fraassen's wording of the argument and pointed out that it was ambiguous: claiming that the IBE user may be using the "best of a bad lot" could be construed in two different ways. Van Fraassen may be asking the IBE user to provide reasons to believe that the lot of possible explanations under consideration is most definitely a good lot, or he may be asking the IBE user to provide reasons to believe that the lot of possible explanations under consideration is likely a good lot. I argued that the latter interpretation is the most charitable. Thus, I interpreted van Fraassen as claiming both that an instance of IBE is justified only if the IBE user has a good reason to believe that the lot of possible explanations under consideration is likely a good lot, and that the IBE user will never have a good reason to hold this belief. I then argued against both of these claims. The second claim was shown to be false by appealing to the principle of privilege. This idea is roughly that the IBE user may have, at his or her disposal, relevant background knowledge and relevant approximately true background beliefs that will weed out unlikely or implausible possible explanations and thus significantly raise the probability of beginning with a good lot of explanations. The first claim was shown to be false because of the availability of a sufficiently probabilized version of IBE. This

version of IBE combines the essence of IBE—explanatory power is a guide to believing what is the case—with Bayesianism. A result of this marriage is that van Fraassen's proposed necessary condition for the justification of IBE is not a necessary condition at all. An increase in the probability of a possible explanation as a result of a recognition of the possible explanation's explanatory power may occur within the constraints of Bayesianism even when the probability of using a good lot is low. In fact this increase in probability may occur, mathematically, just as long as the probability of using a good lot is greater than zero—a requirement that is hardly problematic.

CHAPTER FOUR

The Argument from Indifference

1. Introduction

Van Fraassen proposed the argument from the bad lot as an objection to the claim that IBE is a justified rule of rational inductive inference.[71] He argued that the IBE user is not justified in making the inference that the best explanation of a pool of possible explanations is true *unless* the IBE user can provide a good reason for believing that the true explanation is already likely to be included in the pool. Moreover, van Fraassen claimed that the IBE user will ultimately fail to provide any good reason for believing that the true explanation is already likely to be included within the pool.

Recall that van Fraassen anticipated two responses to his argument from the bad lot: privilege and retrenchment.[72] To retrench is to contend that the original problem was set up incorrectly. The mistake is in construing IBE as a mode of reasoning whereby, from a pool of possible explanations, the explanation that would best explain the phenomena, if true, is inferred to be absolutely true. Instead, the retrencher suggests construing IBE more modestly. The explanatory features of a hypothesis ought to be a guide to arriving at the truth; but this guide ought to be fallible. To infer the absolute truth of an explanation on the basis of its explanatory features may not be epistemically appropriate in all cases (if ever). The retrenched IBE user is merely suggesting that, when updating our beliefs, we exploit the true claim that explanatory power makes hypotheses more likely to be true, all things being equal.

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[71] Van Fraassen, *Laws and Symmetry*, 142-43.

[72] Van Fraassen also entertains a third response, called "force majeure," which is not worth discussing. For this response, see van Fraassen, *Laws and Symmetry*, 144-45.

One way to accomplish this is to combine IBE with a form of epistemological probabilism, namely Bayesianism. With this, beliefs are not necessarily an all-or-nothing matter; rather, they are understood in terms of degrees, and these degrees of belief are represented in terms of probabilities. Not only are these degrees of belief bound by the principles of the probability calculus, the process, itself, of updating one's beliefs, known as conditionalization, is also bound by such principles.[73]

The explanatory considerations that help guide an inductive inference, or that figure into the conditionalization process, may either be rationally required, as part of the rule known as IBE, or not. Van Fraassen distinguishes between two forms of retrenchment that correspond to this option. Form 1 is the idea that a hypothesis' explanatory power increases the probability of the hypothesis being true, all things being equal, but no person is rationally required to adopt this as a rule when updating their beliefs. Form 2 is exactly the same idea, but with the added requirement (of rationality) that these features be taken into account when updating one's beliefs.

Van Fraassen argues against both of these forms. His argument against form 2 will be taken up in the next chapter. His argument against form 1 will be called the "Argument from Indifference" and is the topic of this chapter. With this objection, van Fraassen is once again intent on establishing that every instance of IBE is unjustified. I present this objection, however, as a two-premise argument, where the premises are intended to establish that the conclusion of every instance of IBE is highly likely to be false. Obviously if this claim is true, then no instance of IBE would be justified. I also contend that this argument fails. I argue that it fails because van Fraassen does not

[73] A thorough explication of various forms of Bayesianism is found in John Earman's *Bayes or Bust? A Critical Examination of Bayesian Confirmation Theory* (Cambridge: MIT Press, 1992).

provide an adequate reason to believe one of the premises, and moreover, there is good reason to believe that this premise is very likely to be false. Additionally, even if this premise were true, it would fail to establish the conclusion van Fraassen intended to establish; this is partly due to the availability and the tenability of a sufficiently probabilized version of IBE described in an earlier chapter (and in a later chapter).

2. The Problem

In *Laws and Symmetry*, van Fraassen begins his criticism of form 1 by flatly denying that the best explanation we have is ever likely to be true. His explanation for this denial immediately follows, and functions as a description of his argument from indifference[74]:

> I believe, and so do you, that there are many theories, perhaps never yet formulated but in accordance with all evidence so far, which explain at least as well as the best we have now. Since these theories can disagree in so many ways about statements that go beyond our evidence to date, it is clear that most of them by far must be false. I know nothing about our best explanation, relevant to its truth-value, except that it belongs to this class. So I must treat it as a random member of this class, most of which is false. Hence it must seem very improbable to me that it is true.[75]

The argument seems to be stated clearly enough, yet van Fraassen teamed up with other empiricists to explain it once more. This formulation, though quite similar to the initial one, is stated much more clearly:

> [S]ince, for every choice of a particular theory *T* as best explaining the evidence *e*, there will be (probably infinitely) many unborn hypotheses, inconsistent with *T* and with one another, which explain *e* at least as well, and since only one of these

[74] An earlier version of this argument, which is substantially similar, appears in van Fraassen's "Armstrong on Laws and Probabilities," *Australasian Journal of Philosophy*, 65 (1987), 243-260, on p. 259.
[75] Van Fraassen, *Laws and Symmetry*, 146.

> can be true, it is very improbable that the theory considered to be the best explanation is true.[76]

Here, however, he is explicit in proposing that the number of unborn possible explanations which explain the phenomena at least as well as the chosen best explanation is probably infinite.

The IBE user infers that the theory which would be a sufficiently good explanation of the available evidence *and* provide the best explanation of the available evidence, if it were true, is also the theory that is (likely or most likely to be) true.[77] In his argument van Fraassen seems to be taking advantage of a central feature of IBE: the fact that the IBE user must choose from among only those possible explanations that have been already formulated. The IBE user does not infer the truth of the explanation that is the best of *all possible explanations*. It would be impossible for the IBE user to consider each and every possible explanation. There will always be many possible explanations, possibly an infinite number of them, left out of consideration. Thus, the pool of possible explanations from which the IBE user must choose will always be limited to some degree. When the IBE user chooses a possible explanation and infers that it is the best, this is relative only to the pool of possible explanations under consideration—it is inferred to be the best of the ones in the pool.

According to van Fraassen, however, the fact that the pool of possible explanations is always limited plays an important role in the justification of IBE itself.

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[76] Ladyman, Douven, Horsten, and van Fraassen, "A Defence of van Fraassen's Critique of Abductive Inference: Reply to Psillos," 309.

[77] Or, similarly, the probabilistic IBE user will increase the probability of the hypothesis through conditionalization as a result of the proper recognition of its explanatory power. The increase may be large or small, depending on many factors, including whether or not the hypothesis is deemed the best of the pool of possible explanations under consideration.

He claims that of these possible explanations that are left out of consideration, many of them, possibly an infinite number of them, would explain the phenomena, if true, *just as well* as the inferred explanation. Moreover, the inferred explanation ought to be treated merely as just another random member of the entire class of possible explanations, presumably, because there would be no distinguishing characteristic that would set the inferred explanation apart from the unborn possible explanations, other than the fact that it is being considered—that is, other than the fact that it is in the pool of possible explanations being considered. And the fact that it is being considered is not a privileging characteristic with regards to the question of whether it is the true explanation. Thus, for van Fraassen, it ought to be treated the same.

It stands to reason, he argues, that if it is true that the inferred explanation is just a random member of this class of possible explanations that all would explain the phenomena equally well, if true, then, given the assumption that these possible explanations are inconsistent with one another and given the assumption that only one of them can be true, the probability of the chosen explanation actually being true, from the IBE user's perspective, would be very small indeed. In fact, the greater the number of competing possible explanations that would explain the phenomena equally as well as the chosen explanation, then presumably the greater the improbability of the chosen explanation being true.

Van Fraassen's argument may be identified as having two premises, which I shall name the multiple best explanations premise (MBE) and the random selection premise (RS):

The Multiple Best Explanations premise (MBE): For any inferred best explanation (i.e., for any conclusion of an IBE), there will be a very high (possibly infinite) number of possible explanations, contrary to the inferred best explanation, that would explain, if true, the phenomena just as well.

The Random Selection premise (RS): The inferred best explanation is a random member of this class of possible explanations that all would explain, if true, the phenomena just as well.

If these are the two premises to his argument, then the conclusion can be stated thusly:

Conclusion: Any inferred best explanation is highly likely to be false.

If van Fraassen is correct in this argument, then it seems that no instance of IBE is justified.[78] But in fact, amazingly, it is worse than this. Van Fraassen seems not only to be undermining the justification of IBE in this criticism, he seems to be presenting positive reasons for believing that the conclusion of every instance of IBE is in fact (very) *unlikely* to be true. And he is making this sweeping allegation without ever discussing any particular use of IBE. Fortunately, the argument fails.

3. Psillos' Response

As far as I am able to know, Psillos appears to be responsible for providing van Fraassen's objection, the argument from indifference, a name.[79] And the reason for the name may provide some understanding of the objection. The principle of indifference (also known as the principle of insufficient reason) is a principle of probability theory

[78] Arguably, van Fraassen is pushing another form of underdetermination. MBE, especially, for instance, resembles a statement of empirical equivalence—a claim often used to support underdetermination.
[79] See, for example: Psillos, S., "On van Fraassen's Critique of Abductive Reasoning," *The Philosophical Quarterly*, 46 (1996).

that dates back to at least 1871, and probably earlier.[80] Perhaps the earliest best formulation of it was given by John Keynes:

> The Principle of Indifference asserts that if there is no *known* reason for predicating of our subject one rather than another of several alternatives, then relatively to such knowledge the assertions of each of these alternatives have an *equal* probability. Thus, *equal* probabilities must be assigned to each of several arguments, if there is an absence of positive ground for assigning *unequal* ones.[81]

The statement of this principle can easily be understood in terms of van Fraassen's objection. Given the recognition of a great number of unborn possible explanations that are at least as good as the inferred explanation, given the absence of any non-arbitrary reason to assign a greater probability to the inferred explanation over the unborn ones, and given that each of these possible explanations, the ones being considered and the ones unborn, shall be assigned equal probabilities, one must assign a very low probability to the inferred explanation.

Psillos not only objects to van Fraassen's main premise, MBE, he also alleges that the argument from indifference, if it succeeds, would prove more than what van Fraassen intended—namely, it would count equally well as an objection against van Fraassen's own epistemology, constructive empiricism. Here, I will explain and discuss Psillos' criticism of MBE, but I will omit the latter criticism because of its relevance. Whether the argument from indifference applies to constructive empiricism or not, it does not necessarily help defend the IBE user. Either the argument from indifference is a legitimate objection to IBE or it is not. It makes no difference who the author of the argument is, where the argument comes from, or what personal epistemologies are held

[80] Hacking, *The Emergence of Probability*, 126.
[81] Keynes, *A Treatise on Probability*, 42.

by the one making the argument. Alleging that the argument from indifference proposes just the same sort of challenge to the constructive empiricist as it does to the IBE user, *qua* scientific realist, is not a positive defense of IBE. The best this sort of strategy may accomplish is to establish that perhaps IBE is no worse off than any other inductive inference type—but this was not Psillos' goal.

A possible explanation is one which would, if true, explain the phenomena in question. Possible explanations may be worthy of consideration, or they may not be. Some are good; some are bad. However, only a true explanation will ever do any actual explaining. I have already stated above that I am willing to grant that for any phenomena that is in need of an explanation there will be a very high number, likely an infinite number, of possible explanations. This claim is a necessary condition for van Fraassen's first premise, MBE. There cannot be a very high number, and certainly not an infinite number, of possible explanations that would, if true, explain the phenomena *equally as well as the inferred possible explanation* unless it is true that there will be a very high number, or an infinite number, of possible explanations for each phenomena that is in need of an explanation. MBE appears to be a special instance of this more general claim. I will refer to this more general claim as the multiple explanations premise (ME):

> *The Multiple Explanations premise (ME)*: For any phenomena in need of an explanation, there will be a very high (possible infinite) number of possible explanations that would, if true, explain the phenomena.

Psillos is not willing to grant MBE and furthermore, appears not to be willing to even grant ME. He rightly responds to the argument from indifference by claiming that van Fraassen not only needs to present a good reason to accept MBE, he needs to present

a good reason to accept ME. For the argument from indifference to succeed, MBE is essential—after all, it seems to do most of the work. But the truth of MBE relies on the truth of ME. And, for Psillos, ME is not self-evidently true. Since it is not self-evidently true, a good reason to accept it needs to be presented. Psillos writes that:

> van Fraassen grants that *T* [i.e., the inferred best explanation] has passed several tests and has qualified as the best explanation of *e*. Then he claims that *T* … is as probable as all other *unborn*/potential explanations of *e*. In order to assert this, one must first show that *there always are* other potentially explanatory hypotheses to be discovered, let alone that they explain the evidence at least as well. But how do we know this in advance?[82]

Psillos, even before discussing the prospects of MBE, is demanding of van Fraassen that he must *first* provide a good reason to believe ME. If there are no good reasons to accept ME, then *ipso facto*, there are no good reasons to accept MBE.

It is interesting that Psillos is asking van Fraassen to present reasons to believe that there are always many unborn possible explanations for a given phenomenon. The ability to create a high number, or even an infinite number, of possible explanations that would explain the phenomena at hand, if true, is often granted. Surely there are good reasons, and obvious reasons, to believe that there are indeed always a very high (possibly infinite) number of unborn possible explanations for a given phenomenon in need of an explanation. For example, if I am lying in bed at night and hear a strange noise in the other room, I would then be presented with a phenomenon in need of an explanation: what explains the noise? Given my background knowledge and my situation, there are only a few plausible possible explanations that I would actually consider. For example, my clumsy dog could have knocked over the coffee table, or the

[82] Psillos, *Scientific Realism: How Science Tracks Truth*, 223.

house could be settling, or a poorly affixed picture frame could have fallen to the floor, etc. But there also seems to be a very high number of possible explanations that, as a matter of fact, I would not even consider, and thus they would remain unborn. For example, an invisible gremlin could have created the noise in the other room, or two invisible gremlins, or three invisible gremlins, or even fifty invisible gremlins. Or maybe it was an invisible green gremlin, or an invisible blue gremlin, or one invisible green gremlin and one invisible blue gremlin. Each of these seems possible, but at the same time, none of these will even enter my mind as a legitimate contender of an explanation. It is easy to see how a very high (possibly infinite) number of possible explanations could be created, even if most of them, due to their utter implausibility, will not be considered.

Psillos, however, does concede that, for any inferred explanation, there will always be "trivial alternatives" that entail the exact same evidence.[83] His examples of such trivial alternatives include possible explanations that are notational variants of the inferred explanation and possible explanations that are generated by just "tacking things on" to the inferred explanation.[84] This way of putting things seems to suggest that Psillos accepts ME. But if Psillos accepts ME, then why does he demand van Fraassen to show that "*there always are* other potentially explanatory hypotheses to be discovered"? The reason for this apparent discrepancy is that Psillos seems intent on limiting the number of possible explanations to those that have some relatively high degree of initial plausibility: the non-trivial potentially scientifically interesting ones. He writes that

> [i]t is only reasonable, I think, to demand that any alternative to [the inferred best explanation] should be scientifically interesting in the sense that the scientific

[83] Psillos, *Scientific Realism: How Science Tracks Truth*, 223.
[84] Ibid.

> community has independent theoretical reasons to accept them as genuine empirically equivalent rivals…[85]

I believe this is an acceptable demand for someone who is interested, as Psillos is, in defending a version of scientific realism and who sees IBE as an integral part of this defense. I, however, do not necessarily want to limit the number of possible explanations to those that are potentially scientifically interesting. I am interested in defending IBE against van Fraassen's objections, and my defense of IBE should not necessarily discriminate between the scientific IBE user and the non-scientific IBE user, so to speak. Thus, perhaps unlike Psillos, I am willing to grant the truth of ME without making a distinction between possible explanations that are potentially scientifically interesting and possible explanations that are not. Like Psillos, however, I am not willing to simply grant the truth of MBE. This is a claim that requires an argument. Psillos later provides the details of what he expects van Fraassen to show:

> [the argument from indifference] intends to establish that *permanent suspension of judgement* is the right attitude towards a theory that provides the best explanation of the evidence on the grounds that there are unborn hypotheses that explain the evidence at least as well. This is, however, an assumption which cannot be simply taken for granted. Van Fraassen needs to *argue* for this. In particular, he needs to show that for any theory there is a non-trivial alternative such that the two theories are indefinitely indiscriminable by any evidence and the application of any method.[86]

The last sentence is clearly a demand for a reason to believe MBE. Notice, however, that Psillos is not directly objecting to the sheer number of possible explanations alleged in MBE—a very high, possibly infinite number of them. Rather, Psillos is simply asking van Fraassen for a reason to believe that for any inferred best explanation, there will exist

[85] Psillos, *Scientific Realism: How Science Tracks Truth*, 223.
[86] Ibid.

a non-trivial alternative possible explanation—just one will do!—that is empirically and explanatorily equivalent. In other words, van Fraassen needs to present a reason to believe that, for each use of IBE, there will never be a unique best explanation. And he has yet to provide such a reason.

4. Van Fraassen's Rebuttal

Interestingly, van Fraassen teamed with other empiricists and responded directly to Psillos' challenge. His response is three-fold. Because these three parts are related, I will explain each of them before I begin my reply. Firstly, he reformulates his objection without making any assumptions about alternative possible explanations to the inferred best explanation. He claims that this reformulation exposes an unjustified assumption made by every IBE user. He writes:

> [let us assume that] none of the unborn hypotheses offers a better explanation of the evidence than the best of those which scientists have come up with. Even this would not suffice for the conclusion that IBE is acceptable. For that conclusion would require (at least) one further premise, *viz*., that there is (almost) always a *unique* best explanation ... But what justification is there for this premise?[87]

In other words, even if the IBE user assumes correctly that there are no unborn possible explanations that are better than the chosen explanation (i.e., the possible explanation deemed to be the best one of the lot), the IBE user may infer that the chosen explanation is (likely to be) true *only if* there is almost always a unique best explanation whenever IBE is used. Once again, van Fraassen has alleged that the IBE user must make an

[87] Ladyman, Douven, Horsten, and van Fraassen, "A Defence of van Fraassen's Critique of Abductive Inference: Reply to Psillos," 309.

unjustified assumption. In this case the purported unjustified assumption is that, when using IBE, there is almost always a unique best explanation.[88]

Recall that Psillos complained that van Fraassen needed to provide a reason for thinking that, for each use of IBE, there are many unborn possible explanations that are just as good as the chosen explanation. Psillos interprets van Fraassen as claiming that, for each use of IBE, there *will actually be* many unborn possible explanations that would, if true, explain the phenomena just as well as the chosen explanation. For the second part of his response, van Fraassen denies this. He alleges that it is entirely irrelevant as to whether there *actually exist* many unborn possible explanations that are just as good as the chosen explanation or that it is *merely possible* that they exist. And if it is true that this distinction is irrelevant, then van Fraassen does not need to make it. He can make the weaker of these two claims, the latter one—that for each use of IBE, it is possible that there exist many unborn possible explanations that are just as good as the chosen explanation. He claims that:

> the possibility that there may be equally good rivals to *T* [the chosen explanation] already suffices to make an ampliative step from the evidence to *T* unwarranted.[89]

Finally, the third part of his overall response is a rejoinder to this claim. Van Fraassen states that particular scientific cases constitute evidence for thinking that the possibility of a chosen explanation having equally good rivals has actually occurred. If

[88] It is important to remember that the allegation of this assumption is conditional upon a further assumption, namely, that there are no unborn possible explanations better than the chosen one. Van Fraassen, of course, would not let the IBE user make this further assumption without argument.
[89] Ladyman, Douven, Horsten, and van Fraassen, "A Defence of van Fraassen's Critique of Abductive Inference: Reply to Psillos," 309. Once again, this reformulation of the argument from indifference closely resembles the problem of underdetermination. In fact, Kyle Stanford's recent argument is substantially similar (in *Exceeding Our Grasp: Science, History, and the Problem of Unconceived Alternatives* (New York: Oxford University Press, 2006)) in that he uses this claim as a way to establish a long history of underdetermination in order to support a new kind of pessimistic induction.

this is true, then van Fraassen can strengthen his previous claim—not only is it possible that there are unborn possible explanations that are just as good as the chosen explanation, but sometimes these possible explanations are actualized and considered. He claims that fundamental physics can provide "well known examples of empirically equivalent theories," and, without much detail, he cites Bohm's mechanics and elementary quantum physics as one example.[90]

5. Why the Argument from Indifference Fails

To begin with, van Fraassen appears to have shifted the burden. One question about this burden shift is whether or not it is legitimate. As the one providing the objection, van Fraassen alleged that for each use of IBE there will be many unborn possible explanations, possibly an infinite number of them, which would explain, if true, the phenomena just as well as the chosen explanation. This allegation constitutes the heart of the problem of indifference. The obvious retort, made by Psillos, is that van Fraassen needs to present reasons to believe that this allegation is true. No reason has been given. Instead, van Fraassen reformulates the problem. The allegation then becomes that the IBE user is making an unjustified assumption; namely, that there is almost always a unique best explanation whenever IBE is used. So long as this assumption remains unjustified, IBE itself remains unjustified. Van Fraassen has thus shifted the burden onto the IBE user by demanding that the IBE user defend this assumption.

[90] Ladyman, Douven, Horsten, and van Fraassen, "A Defence of van Fraassen's Critique of Abductive Inference: Reply to Psillos," 310.

It is important to remember that van Fraassen has only reformulated the same alleged problem; he is not raising a second problem. The bottom line is that van Fraassen is arguing that the best possible explanation we have is *never* likely to be true. This is, after all, how he begins his entire argument. Given the logical strength of this claim—it is a much stronger claim to deny flatly that something will happen than it is to admit that it may happen sometimes and may not happen other times—the burden might very well be on van Fraassen to defend it. The question of where the burden lies is often a tricky one. Nevertheless, if the IBE user indeed has the burden to defend this assumption, and this is not obviously true, the IBE user still has something to say.

For instance, the IBE user may suggest that indeed there is a unique best explanation for each phenomenon in need of an explanation—the true one! If there is an explanation for each phenomenon in need of an explanation, and if there is only one true explanation for the same phenomenon, then it follows that, by identifying the best possible explanation with the explanation that is true, there is only one best possible explanation for each phenomenon in need of an explanation. IBE users are, after all, in pursuit of the truth, if not all of the time, then at least the vast majority of the time. The question each IBE user is asking is surely: what is the true explanation of the phenomenon in question? If this is so, then no possible explanation will be better than the true one. This would make the best possible explanation, in each instance, unique.

It could be objected, however, that the IBE user may not recognize the true explanation even if it was considered. It may be the case that the IBE user, due to certain features of the situation itself perhaps, deems a false explanation to be better than the true one. In fact, with the benefit of hindsight, there may even be actual historical cases of

this happening in science. After all, the point of using IBE is that we do not yet have the truth prior to making the inference. So it is not enough to simply point out that the true explanation will always be the unique best explanation; the question ought to be considered from the perspective of the IBE user. What evidence will the IBE user have to believe that, in each use of IBE, the unique best explanation will be ascertained? Or, perhaps, why think that, when reflecting on the pool of possible explanations, the discriminatory powers had by the IBE user in conjunction with background theories will almost always yield a single explanation that is deemed best? If this is van Fraassen's demand, and it most likely is, then it still misses the mark.

At this point it appears that the IBE user must concede something; namely, that the criteria used in evaluating and comparing possible explanations will not always lead to a unique best one, from the IBE user's perspective. Van Fraassen knows this; hence, the inclusion of the words "almost always" in the demand. Is there any good reason to believe that it *ever* happens? That is, is there any good reason to believe that the comparative evaluation of possible explanations will ever yield a unique best one? The answer is yes; and it should be obvious. Just one example would prove it, and van Fraassen seems to grant this as well. So the question appears to be whether there is any good reason to believe that the criteria used to evaluate possible explanations will *almost always* yield a unique best explanation. However, before answering this question, we should ask a different question. We should first determine whether the IBE user is even required to answer it: must the IBE user present a good reason to believe that a unique best explanation will almost always be produced? The answer is no.

Recall that van Fraassen is purportedly objecting to the retrenched IBE user of the first form. So he must be alleging that the retrenched IBE user must defend this assumption in question.[91] The retrenched IBE user, in van Fraassen's own words, is someone who claims that "the special features which make for explanation among empirically unrefuted theories make them (more) likely to be true."[92] If this is given a probabilistic bent, then the retrenched IBE user (or the probabilistic IBE user) can be said to be someone who recognizes that explanatory power increases the probability of hypotheses, all things being equal, and who incorporates this fact in the updating of one's beliefs. This could be accomplished, as I have indicated earlier, by combining the merits of IBE with Bayesianism. The result would be the recognition that explanatory considerations ought to play a proper role in the determination of the posterior probabilities of hypotheses, with the integrity of the conditionalization process essentially intact. It follows from this that one can be a probabilistic IBE user and admit that sometimes the best possible explanation we have is still not likely to be true. In a case like this, the potential explanatory power of the hypothesis may still increase the probability of the hypothesis, yet the probability may still be low enough to make it unlikely to be true.[93] In the same vein, the probabilistic IBE user may even admit that sometimes there may appear to be more than one best explanation; that is, in some cases perhaps the evaluating criteria used to judge between the possible explanations have yielded two or more possible explanations that are equally good and that are inconsistent

[91] For the sake of this discussion, it makes no significant difference if we are referring to the retrenched IBE user of the first form or of the second form; they are substantially similar. The only difference being that, in the second form, the appropriate recognition of the connection between explanatory power and truth is a *rational requirement* when updating one's beliefs.

[92] Van Fraassen, *Laws and Symmetry*, 146.

[93] Any time I refer to an increase in the probability of a hypothesis through Bayesian conditionalization, in this way, I assume, of course, that the prior probability of the hypothesis is greater than zero. There is more on this requirement below.

with one another, and, moreover, that there are no other possible explanations under consideration that are better. Once again, if this were to happen, the potential explanatory power of the competing hypotheses may still increase the probability of each hypothesis. The extent of the increase of the probability may well depend, at least in part, on the number of hypotheses in competition. The result of the increases of the probabilities may still yield a probability too low to warrant acceptance. The probabilistic IBE user is surely allowed to suspend judgment in certain cases such as these; for he or she cannot be forced to accept a poor explanation that is nonetheless deemed best of the lot, and, likewise, cannot be forced to choose from among equally good possible explanations that are also deemed best of the lot. But when probabilistic IBE is used and the conditionalization process grants a hypothesis a high enough probability due to its potential explanatory power, among other things, the result might be acceptance or full belief in the hypothesis. When this happens the probabilistic IBE user presumably will have strong evidence that the pool of possible explanations was a good lot, and that there were no other possible competing explanations in the pool that were nearly as good.

Again, it might be objected that this is not good enough. Even if the probabilistic IBE users are not necessarily affected by the possibility of multiple competing best hypotheses, must they still defend the claim that this explanatory "tie" does not usually happen? After all, if an explanatory tie happens more often than not, then the full uses of IBE, i.e., the uses of IBE that result in a unique and highly probable conclusion, will diminish. If the probabilistic IBE user needs to reply to such a concern, and once again, this is not obviously true, then there are a couple of points that may be made. First of all,

these sorts of ties do not seem to happen very often. The criteria used to judge possible explanations are not systematically applicable, and the result seems to be a certain level of subjectivity in the judgments.[94] One upshot from this subjectivity, if there is one, might be the infrequency of explanatory ties. This is an empirical question that will be very difficult to answer. Van Fraassen appears, at first, to provide an example of an explanatory tie by citing the standoff between Bohm's mechanics and quantum theory, but he instead provides an example of what he claims to be empirical equivalence. Empirical equivalence, however, is not the same as explanatory equivalence. Two hypotheses may be empirically equivalent without being equally good explanations. Secondly, when the probabilistic IBE user does indeed draw a strong, highly probably conclusion, it would require strong evidence, and thus it may be presumed that there were no explanatory ties between competing hypotheses. Whether or not there should exist an explanatory tie between competing hypotheses ought to be judged on a case by case basis. The probabilistic IBE user should not be expected to declare that an explanatory tie will or will not occur *prior to* engaging in this kind of inductive inference. The details of the situation, including the relevant background knowledge and the possible explanatory power of the hypotheses, will determine whether or not an explanatory tie is likely to occur.

The second part of van Fraassen's response requires a similar defense. Recall that van Fraassen claims that the mere possibility that there may be equally good rival possible explanations to the chosen explanation is sufficient to make the conclusion of every IBE unjustified. This means that he is suggesting that because it is possible that

[94] The criteria to which I refer include the usual explanatory virtues such as simplicity, fruitfulness, unification, etc.

there are equally good rival possible explanations, it follows that no instance of IBE is justified. This inference is certainly questionable. First of all, and most importantly, he provides no reason to believe that the premise is true. What kind of possibility is he intending? Given the context, I take him to mean logical possibility. Are we supposed to just assume that, for each instance of IBE, it is logically possible that there are equally good rival hypotheses? Once again, if the IBE user is interested in scientifically interesting explanations—possible explanations that are taken seriously by the scientific community—then this claim is not obviously true. Quite clearly, this claim requires an argument. Secondly, notice that van Fraassen has backtracked from the claim that there are equally good rival possible explanations for each instance of IBE to the claim that it is possible that there are equally good rival possible explanations for each instance of IBE. There is no good reason to believe the former claim; certainly, van Fraassen provides none. And the latter claim, if it is true, may in fact be trivial. It resembles a restatement of the problem of induction. If the objection is that the inference is unjustified because it is possible that the conclusion is false, and the conclusion is possibly false because there might be another unborn conclusion that is equally good or better, then the objection overreaches. Once again, IBE is an inductive inference. It is trivial to point out that the conclusion is possibly false. It is perhaps by definition that every inductive inference is invalid—the conclusion is possibly false, given the truth of the premises. Pointing out that IBE is inductive is no objection to IBE.

In fact, not only does the premise appear to be trivial, if it is true, the conclusion does not even follow from it. If we grant that the objection amounts to more than the problem of induction, Van Fraassen is still mistaken in thinking that this premise is

sufficient to prove that each instance of IBE is unjustified. The mere possibility of equally good alternatives is not sufficient to prove that there will never be good reasons to believe that the chosen explanation is true, or highly probable. Nor is it sufficient to prove that there will never be good reasons to believe that the pool of possible explanations under consideration contains, or is likely to contain, the true explanation. In this way, this objection appears to be very similar to the earlier argument from the bad lot. This argument claimed that each instance of IBE is unjustified unless there are good reasons to believe that the pool of possible explanations under consideration is likely to contain the true explanation. Notice that if there are good reasons to believe that the pool of possible explanations under consideration is a good lot, then *ipso facto*, there are good reasons to believe that there are no equally good unborn rival possible explanations. Likewise, if there are good reasons to believe that the chosen explanation is true, or highly probable, then *ipso facto*, there are good reasons to believe that there are no equally good unborn rival possible explanations. Thus, the claim that, for each instance of IBE, it is possible that there are equally good rival possible explanations becomes irrelevant. The mere possibility of equally good rivals is simply not enough to prevent a justified instance of IBE. There will certainly be instances of IBE when there are plainly good reasons to believe that there are no equally good rival possible explanations to the chosen explanation—even though their existence would still be logically possible.[95]

Moreover, it ought to be pointed out that the probabilistic IBE user, in particular, will not be bothered by the logical possibility of equally good rival possible explanations. Recognizing the potential explanatory power in a hypothesis and properly integrating this

[95] There will be more on how this may be done in the next section. Peter Lipton suggests a specific way this may be done in his rebuttal of the argument from underconsideration.

information into the conditionalization process in order to ascertain a posterior probability can be done even with the possibility that there may be equally good rivals. Of course, as I have argued above, this possibility will be tempered (made more or less probable) with the help of relevant background knowledge and the potential explanatory considerations of the hypothesis; and consequently, will play a role in the determination of the posterior probability of the hypothesis. Thus, even with the acknowledgment of equally good possible rivals, the potential explanatory power of the hypothesis may still increase the probability of the hypothesis, even to a high degree. This increase will occur as a result of using Bayesian conditionalization. And an increase of this kind will only properly occur if the prior probability of the hypothesis is greater than 0. One interesting way to interpret van Fraassen's argument from indifference is with probability: the existence of a huge number of equally good rival hypotheses causes the prior probability of the chosen hypothesis to lower. The number of equally good rival hypotheses would have to be infinitely large to bring the prior probability of the chosen hypothesis to 0. Thus, van Fraassen may have a legitimate objection if he alleges that there will always be an infinite number of equally good rival hypotheses for each use of IBE. The problem, once again, is that there are no good reasons to believe that this is true. And moreover, as I have already argued, there will be instances of IBE when there will be good reasons to believe that this is indeed false.

On a last note, the probabilistic IBE user interested in defending a version of scientific realism seemingly must defend, however, the claim that there are many cases when the recognition of a possible explanation as the best one increases the probability of it so much that it turns out to be probable, and perhaps even highly probable. But this

defense has less to do with the mode of inference, I believe, than it does with the content of the inference; the content of the inference being the merits and demerits of scientific realism and its alternatives. If this is what the probabilistic IBE user must defend, then van Fraassen has missed the mark with his objection.

6. Lipton's Argument from Underconsideration

There are further points to be made against van Fraassen's objection. Specifically, Peter Lipton has discussed the objection and has offered an interesting way to respond. He combined van Fraassen's two main objections to IBE, the argument from the bad lot and the argument from indifference, into a more general skeptical argument that he has termed the "argument from underconsideration."[96] The two-premise argument can be put into the following form:

> *The Ranking Premise*: We, or the scientific community specifically, are able to rank competing possible explanations with respect to the likelihood of them being true, and this ability of ours is highly reliable.
>
> *The No-Privilege Premise*: We have no good reason to believe that the process by which we generate these competing possible explanations makes it likely that a true explanation will be among those generated.
>
> *Conclusion*: The best, or the highest ranked, possible explanation may be true, but we can never have any good reason to suppose it is.

It follows from the ranking premise that we, or perhaps the scientific community, have an extraordinary inductive ability to rank all of the possible explanations in order of their probability of being true. Obviously, if the true explanation is included in the pool it will very likely be ranked first. But this is an entirely comparative process—the ranking

[96] Lipton, *Inference to the Best Explanation*, 152.

premise does not grant that we would thereby know how probable the most probable possible explanation, or any other possible explanation in the pool, would be. The no-privilege premise entails that we have no good reason to believe that the true explanation will ever be included within the pool of possible explanations being considered. The conclusion is predictably rather pessimistic. In all of these cases, it is essentially that no instance of IBE is ever justified.

The specific relationship between the argument from underconsideration and the two arguments given by van Fraassen, the argument from the bad lot and the argument from indifference, is not obvious at first. But the argument from underconsideration seems to capture the more general skeptical attacks alleged by the latter two arguments. Thus the latter two arguments may be reformulated as specific instances of this more general worry that constitutes the argument from underconsideration. For instance, the argument from the bad lot grants that the IBE user may be good at ranking the possible explanations that are being considered in the pool, but still alleges that there is good reason to doubt that the best ranked of these possible explanations is true. This doubt is expressed by motivating the idea that the IBE user must make an unjustified assumption; namely that the pool of possible explanations is already likely to contain the true explanation. Similarly, the argument from indifference grants that the IBE user once again may be good at ranking the possible explanations that are being considered in the pool, but still alleges that there is good reason to doubt that the best ranked of these possible explanations is true. This doubt, however, springs from the alleged claim that, for each use of IBE, there will possibly be a very high number (maybe an infinite number) of equally good rival hypotheses. This is how both of these arguments may fit

under this more general skeptical worry that even if the IBE user is granted the reliable ability to rank possible explanations, there will still be good reason to doubt that the best ranked of these possible explanations is true, or even probably true.

Lipton argues that the two premises of the argument from underconsideration are in fact incompatible. He begins this process by emphasizing the important role background theories play in theory ranking and theory selection.[97] The background theories provide a strict guide in the ranking of theories. And if the ranking is to be accurate or reliable at all, as the ranking premise suggests, then, Lipton argues, the background theories must be true, or at least highly probable. It follows then that the ranking premise entails that the background theories are true, or at least highly probable. Lipton writes that:

> [i]f most of the background theories were not even approximately true, they would skew the ranking, leading in some cases to placing an improbable theory ahead of a probable competitor, and perhaps leading generally to true theories, when generated, being ranked below falsehoods. The ranking premise would be violated. So the ranking premise entails that the background is probably (approximately) true.[98]

When this idea is applied recursively, the background theories themselves were at one time generated and ranked, and moreover the best, or the highest ranked, of the current theories will form the background theories of tomorrow. This means that the highest ranked current theories have to be true, or probably true. But the only way the current theories can be true, or probably true, is if the true ones are probably among the ones generated. This means that there is good reason to believe that the true theories are likely

[97] Lipton cites Richard Boyd, in particular, as someone who has written extensively on the importance of background theories in theory choice and on their close relationship with scientific realism.
[98] Lipton, *Inference to the Best Explanation*, 157.

to be generated, and consequently, that the no-privilege premise is false. Therefore, if the ranking premise is true, then the no-privilege premise cannot be.

The key to this argument is the alleged entailment from the ranking premise to the truth of the background theories. If this entailment is false, then it would be possible to have highly reliable rankers who work with false background theories. At first, this possibility appears plausible. Scientists, or IBE users, who are highly reliable rankers of current theories, but who nevertheless are working with false background theories, may seem quite plausible. So does the ranking premise actually entail the truth of the background theories? Here is Lipton's argument for why it does:

> Consider now two isolated scientific communities that are equally reliable rankers, but who in the past generated quite different ranges of candidate theories and so come to have quite different backgrounds. One community was lucky enough to generate true theories, while the other was uninspired enough to generate only wildly erroneous ones. If present reliability depended only on prior ranking, we would have to suppose that these two communities are now equally reliable rankers of new theories, which is clearly incorrect.[99]

Lipton suggests that the background theories will determine, at least in part, the level of reliability had by the community of rankers. But, specifically, it is the content of the background theories that does the work. And the level of reliability will increase or decrease as the content of the background theories comes closer or comes further away from the truth. Thus, the reliability of the rankers will be very high only if the background theories are true, or probably true.

Lipton argues that the two communities would not now be equally reliable rankers of the current theories because it is very likely that each of them would end up with a different ranking. And they apparently cannot each be highly reliable rankers and yet

[99] Lipton, *Inference to the Best Explanation*, 158.

provide different rankings of the same theories. If they are equally reliable rankers and if only one ranking is the correct one, then the two communities would generate the same ranking. But a community with false background theories, just like a community with true background theories, will nevertheless rank the current theories according to how probable each one is *on condition that* their background theories are true. In other words, this ranking will be done by assuming the truth of the background theories, and the truth of this assumption will not necessarily be known by the community. The ranking by the community with false background theories will likely, but not necessarily, result in a different ranking of the theories. But there would still be some kind of skill being shown by the community of rankers—they would still be doing something right with regards to the ranking. Their ranking would not necessarily reflect the "actual" comparative probability of the theories with respect to the truth. But even the best, most accurate rankers who are working with false background theories would probably still end up with a different ranking than the "actual" one. The problem is allowing these rankers to be highly reliable. This notion of reliability seems to automatically suggest that the rankers will likely get the correct answer with respect to the truth, i.e., true background theories—and not merely get the correct answer with respect to their own background theories which may or may not be true. This is seen quite commonly as a necessary condition of what is meant by "reliable" in philosophical circles. A reliable method is a method that typically generates the true, or approximately true, answer.[100] It is not simply the best method that someone is able to adopt at the time; for the best method that

[100] One needs only to understand the epistemological position of reliabilism in order to understand how the term "reliable" is used. See for instance, Steup, M., *An Introduction to Contemporary Epistemology* (Upper Saddle River, NJ: Prentice Hall, 1996).

someone is able to adopt may very well be a method that typically generates a false answer. And this would be called an unreliable method.

In the argument from indifference (and in the argument from the bad lot), van Fraassen explicitly grants the IBE user the ability to correctly discriminate among the pool of possible explanations and identify the best one, if there is one. Lipton understands this ability as an ability to rank possible explanations *highly reliably*. This means, for Lipton, that the IBE users are able to correctly rank the possible explanations according to the truth—not according to their own background theories, which may or may not be true. But van Fraassen never uses the word "reliable." He could very well not grant the ranking premise to Lipton's reconstruction for precisely the reason that by granting reliability to an IBE user, the no-privilege premise is called into question. This is why Lipton appears to be begging the question against the arguer from underconsideration; and likewise, against the arguer from indifference. The argument from underconsideration could avoid this alleged problem of incompatible premises by altering the ranking premise to omit any notion of reliability and to replace it with terminology that clearly limits the IBE user's ranking ability to be conditional upon their particular background theories which may or may not be true.

Whether or not the premises of the argument from underconsideration are compatible, and whether or not the argument from underconsideration is a charitable reconstruction of van Fraassen's series of arguments, Lipton still has a crucial and helpful response to make. He notices that the argument relies on the apparent gap between a comparative ranking of theories and an absolute ranking of theories. The ranking premise only grants the highly reliable ability to rank theories comparatively. By itself

this provides no basis for making any absolute judgments of the actual probability of any of these theories. But, if somehow the scientific community were granted a magical (and unrealistic) power to generate or entertain every possible theory for some area of scientific inquiry, then they would know that the true theory would be among the possible theories under consideration. The ranking premise would then grant them the ability to know that the highest ranked of these theories will be true, or probably true. Thus, if this gap collapses, then the conclusion of the argument would be false.

Is there any plausible way to close this gap? One way in which Lipton considers to close the gap is by sheer exhaustion. It would be akin to granting the scientific community the very same magical power as mentioned above. Obviously, this is entirely implausible. Lipton, however, argues that sheer exhaustion is not necessary to close the gap between comparative and absolute judgment of theories. This could be done rather simply by considering a theory and its contradiction. He writes that:

> [i]t is enough that the scientists consider a theory and its negation, or the claim that a theory has a probability greater than one-half and the claim that it does not, or the claim that X is a cause of some phenomenon and the claim that it is not, or the claim that an entity or process with specified properties exists or it does not.[101]

Granting the power to rank to contradictory theories, in conjunction with the high reliability granted through the ranking premise, entails that the scientists would be able to determine which of the theories is true, or probably true. This would be sufficient to show that the argument from underconsideration fails.

But how does this work as a rebuttal to the argument from indifference, specifically? If granted the power to rank contradictories, the IBE user may then, in

[101] Lipton, *Inference to the Best Explanation*, 155-56.

certain cases or maybe in most cases, have good reason to believe that the pool of possible explanations being considered either contains the true explanation or is likely to contain the true explanation. This would happen when the contradiction of the best explanation (or even a negation of a disjunction of two or three of the better possible explanations, perhaps) is sufficiently improbable.

7. Conclusion

In this chapter I rebutted van Fraassen's argument from indifference. This argument, once again, was not easy to pin down. At first, van Fraassen appeared to claim that for each use of IBE there will be many unborn possible explanations, possibly an infinite number of them, which would explain, if true, the phenomena just as well as the chosen explanation. If this were true, then the probability of the chosen explanation would always be very low. I pointed out that van Fraassen failed to provide any reason to believe that his allegation is true. Van Fraassen then rephrased his allegation. He claimed that for each instance of IBE the IBE user must make an unjustified assumption that there is almost always a unique best explanation. Aside from pointing out that the *true* explanation may qualify as the unique best explanation, I interpreted this allegation to be that the comparative step of IBE will almost always result in an explanatory tie. I showed that this allegation is likely false, given the subjective character of the explanatory virtues. And I showed that this allegation need not worry the probabilistic IBE user; for when IBE is construed probabilistically, then even if two possible explanations are equally good, the IBE user may still increase the probability of them after considering their explanatory power.

Van Fraassen then rephrases his argument once again, claiming that the mere possibility that there may be equally good rival possible explanations to the chosen explanation is sufficient to make the conclusion of every IBE unjustified. This means that he is suggesting that because it is *possible* that there are equally good rival possible explanations, it follows that no instance of IBE is justified. After pointing out that van Fraassen again does not provide any reason to believe that this claim is true, I argued that this possibility van Fraassen identified is trivial, if it is true. Moreover, van Fraassen's conclusion does not follow from it. An instance of IBE may be justified even when it is true that it is logically possible that the chosen explanation has equally good rivals. I showed that, once again, the availability of a sufficiently probabilized version of IBE is sufficient to illustrate this.

Finally, I presented and examined Lipton's argument from underconsideration. This argument is meant to characterize at once both of van Fraassen's first two objections, the argument from the bad lot and the argument from indifference. I explain Lipton's argument for why the argument from underconsideration fails and show how Lipton might be begging the question against van Fraassen. The upshot of this discussion, aside from Lipton's novel way of characterizing van Fraassen's arguments, is Lipton's interesting suggestion that judging a possible explanation and its contradictory (or several possible explanations and their contradictories) may provide the IBE user with a way to evaluate all possible explanations—even ones that remain unborn.

CHAPTER FIVE

The Dutch Book Argument against Inference to the Best Explanation

1. Introduction

Van Fraassen's initial argument to undermine IBE's status as a justified rule of inductive inference is the argument from the bad lot. Briefly, this objection alleges that the IBE user makes an unjustified assumption regarding the probability that the pool of possible explanations under consideration is a good lot. Van Fraassen alleges that the IBE user must assume either that the lot of possible explanations under consideration is most definitely a good lot, or that it is more likely than not that the lot of possible explanations under consideration is a good lot. In either case, for van Fraassen, this assumption is unjustified, and consequently every use of IBE is unjustified.

In a previous chapter I have argued that this objection fails, and that it fails for at least two reasons. Firstly, it may very well be that in certain cases the IBE user is privileged in the sense that he or she has good reason to believe that the lot of possible explanations under consideration is a good lot. As background knowledge and approximately true background beliefs increase, so too does the probability of being privileged in this sense. Secondly, by construing IBE probabilistically, the IBE user may argue that the problem of the bad lot is set up incorrectly. In his argument van Fraassen construes IBE as an all-or-nothing inference. Given a finite lot of possible explanations, the IBE user deems that the best of these is absolutely true. But IBE does not need to be construed so narrowly. The IBE user recognizes that a possible explanation of some phenomena could be the best explanation of the lot and, simultaneously, recognize that it

is a very bad explanation. Thus, deeming a possible explanation the best of a lot of possible explanations is not always sufficient to infer that it is true. It may be that the best explanation of a lot of possible explanations is poor enough to warrant a suspension of judgement; or in exceptional cases, the best explanation of the lot of possible explanations may be so utterly poor that it warrants believing that it is probably false.

Van Fraassen anticipates the probabilistic construal of IBE, almost as if he realizes that a sufficiently probabilized version of IBE will easily sidestep his initial argument. Once van Fraassen gives his argument from the bad lot he entertains several responses to it: one of them is privilege, as I have already discussed, and one of them is retrenchment. This latter response takes two forms, form 1 and form 2, corresponding to two general ways one may sufficiently probabilize IBE. One way to construe IBE probabilistically is by claiming that beliefs come in degrees and by claiming that the explanatory power of a given hypothesis raises the probability of it, all things being equal. This may be done without admitting any additional constraints regarding the updating of one's beliefs given new evidence. This seems to be van Fraassen's retrenchment of form 1. Van Fraassen presented an argument against this position, named the argument from indifference, and it was the subject of the previous chapter.

Another way, and perhaps the most obvious way to construe IBE probabilistically, is by thoroughly combining it with Bayesianism. For Bayesians, beliefs are understood in terms of degrees, and the degrees of belief can be represented as probabilities. The process by which these degrees of belief are updated or amended, given the introduction of new information or evidence, is called conditionalization. This process and the degrees of belief themselves are bound by the principles of the

probability calculus. The IBE user may take advantage of this epistemology by allowing explanatory considerations to play a role in the determination of the posterior probability of hypotheses without tainting the conditionalization process and thus without violating the principles of the probability calculus. Doing this would presumably allow the IBE user to admit that in some instances of IBE the inferred possible explanation is likely or even highly likely to be true. This seems to be what van Fraassen is calling retrenchment of form 2. For van Fraassen, it is a rule of rationality (under this form but not necessarily under the first form) to take into account the explanatory features of a hypothesis when updating one's beliefs. He presents a different argument against form 2, a Dutch book argument, and this will be the subject of the chapter.[102] I first explain Bayesianism and how it uses Dutch book arguments; this will include an explanation of the Teller-Lewis argument. I then present van Fraassen's Dutch book argument against the probabilized version of IBE (i.e., retrenchment of form 2), and allege that it fails to establish that this version of IBE is incoherent, and that consequently anyone adopting this version of IBE is irrational, by arguing that one of the premises is false and that van Fraassen has failed to sufficiently establish the truth of another premise.

2. Bayesianism and Dutch Book Arguments

In order fully to understand van Fraassen's Bayesian version of IBE, and his subsequent argument against it, it is first necessary to understand many of the features of

[102] I am using the term "Dutch Book argument" to refer to an argument that purports to illustrate susceptibility to being Dutch booked while inferring negative consequences regarding rationality, inconsistency, or incoherence. Others, for instance, Jonathan Kvanvig , in his "A Critique of van Fraassen's Voluntaristic Epistemology," *Synthese* 98 (1994), 325-348, maintain that Dutch Book Arguments are specifically synchronic, while Dutch Book Strategies are diachronic. Using this terminology, van Fraassen's argument would be a Dutch Book Strategy.

Bayesianism itself. There are many forms of Bayesianism—too many to list here.[103] Fortunately, it is not necessary to list them all; it will be sufficient given the task at hand to simply explain the basics of Bayesianism, especially as they relate to van Fraassen's argument.

Aside from advocating a quantitative rather than a qualitative approach to confirmation, Bayesians tend to agree on two matters: (1) beliefs are a matter of degree, and (2) when new evidence surfaces, changes in one's degrees of beliefs ought to be governed by the rule of conditionalization. When beliefs are taken to be a matter of degree, and these degrees can be given a numerical value, the use of probability theory is a natural and reasonable way to check or determine the consistency of one's beliefs and the amount of confirmation had by a belief given new evidence. Thus, the probability axioms and theorems that comprise the probability calculus are interpreted epistemically so that the probability function measures a person's credence in a particular proposition.

At the center of Bayesianism, there are three axioms governing a person's epistemic probability assignments:

(A1) $P(X) \geq 0$ for all sentences, X, in language L,

(A2) $P(X) = 1$ for any sentence in L that is a tautology, and

(A3) $P(X \vee Y) = P(X) + P(Y)$ for all sentences, X and Y, in L, such that X and Y are mutually exclusive.

[103] A cursory glance through Earman's *Bayes or Bust?: A Critical Examination of Bayesian Confirmation Theory* (Cambridge: MIT Press, 1992), for instance, will confirm this statement. And according to I. J. Good, in a letter he had written to *The American Statistician*, 25 (1971), 62-3, there are at least 46656 ways to be a Bayesian, and remarkably, he provides the details of how he calculated this result.

The axioms may be intuitive enough, but nevertheless, each of them may be justified by a Dutch book argument.[104] Dutch book arguments make the common assumption that a person's degrees of belief match his or her betting prices: if (and only if) I assign probability *p* to some proposition X, then I would regard the proper value of a bet that pays *S* if X (and 0 if not X), where *S* is some positive amount, as *p* multiplied by *S*. For example, if I assign a .25 probability to the proposition that a random card chosen from a standard deck of cards will be a diamond, then the value of a bet that pays $1 if the card is a diamond (and nothing if it is not a diamond) will be, for me, 25 cents. A Dutch book is a series of bets which are bought or sold at prices that guarantee a net loss.[105] A Dutch book argument for the justification of one of the probability axioms will be an argument that shows how violating the probability axiom will enable a person to be susceptible to a Dutch book. Proofs of this type have been given for each probability axiom.[106] The result is the synchronic Dutch Book Theorem, which states that if a set of betting prices violate any one of the probability axioms, then there is a Dutch book consisting of bets that are fixed at those prices. But this theorem, by itself, does not guarantee a resistance to being Dutch booked, even when the probability axioms are followed; something more is required. A related theorem, the converse synchronic Dutch Book Theorem, states that if a set of betting prices satisfy the probability axioms, then no such Dutch book

[104] Axioms, being axioms, are usually assumed without argument. The justification here, however, is pragmatic.

[105] This assumes that a bettor will be indifferent to buying or selling a bet that is of the value determined by the bettor's degree of belief.

[106] For the initial proofs, see Frank Ramsey's "Truth and Probability," first published in *Foundations of Mathematics and Other Logical Essays* (London: Routledge and Kegan Paul, 1931), and also Bruno de Finetti's "La Prevision: ses lois logiques, se sources subjectives" (Annales de l'Institut Henri Poincare 7, 1937), translated in H. Kyburg and H. Smokler (eds.), *Studies in Subjective Probability* (Huntington, NY: Krieger, 1980). John Earman, in his *Bayes or Bust? A Critical Examination of Bayesian Confirmation Theory* (Cambridge: MIT Press, 1992), argues that the strategy for such proofs can even be traced back to Thomas Bayes himself.

consisting of bets that are fixed at those prices exists.[107] When these two theorems are combined there is significant motivation to conform one's degrees of beliefs to the probability calculus. In fact, nearly all Bayesians take the conformation of one's degrees of beliefs to the probability calculus as a requirement of rationality. Moreover, many understand the theorems themselves to demonstrate that a person would be irrational to violate the probability calculus, because they take Dutch book susceptibility to be a failure of rationality.[108] The irrationality, however, is one that seems to be internal to a person's degrees of belief. It is essentially a question of consistency, and through a Dutch book argument, the problem is exposed in a practical manner.[109]

Another requirement, the diachronic condition of conditionalization, is often understood to be a rational constraint on the way Bayesians update their degrees of beliefs upon receiving new information or evidence. This rational constraint is the process of conditionalization, which can be achieved by correctly applying Bayes' theorem. One version of Bayes' theorem asserts that:

$$P(H \mid E) = P(E \mid H) \times P(H) / P(E)$$

As I have explained previously, the left side of the equality is the probability of hypothesis, *H*, conditional on evidence, *E*. This is the posterior probability of *H*; that is, this represents the degree of belief that ought to be held given the addition of some piece

[107] For the proof of this theorem, see John Kemeny's "Fair Bets and Inductive Probabilities" in *The Journal of Symbolic Logic*, 20 (1955), 263-73, or R. Sherman Lehman's "On Confirmation and Rational Betting" in *The Journal of Symbolic Logic*, 20 (1955), 251-62.
[108] Hajek, in "Scotching Dutch Books?," *Philosophical Perspectives*, 19 (2005), 139-151, explains quite nicely how this inference is supposed to work.
[109] In the literature this requirement is called the synchronic condition of probabilistic coherence.

of evidence, *E* (along with other assumptions and bits of background knowledge). This posterior probability is determined by the right side of the equality. The first part, P(*E*|*H*), represents the probability of *E* given the truth of *H*; this is referred to as the likelihood of the evidence, *E*. And the other two, P(*H*) and P(*E*), represent the prior probabilities of *H* and *E*, respectively. The prior probability of *H*, for example, is the probability assigned to *H* prior to the introduction of evidence *E*. When the idea of conditional probability is made explicit, this theorem is derivable from the probability axioms.

The principle of conditionalization, then, states that the newly updated probability of a proposition (when the evidence has come in, so to speak) ought to equal the old conditional probability of the proposition given the evidence (when the evidence was not yet known). If we let P_{old} be the old prior probability assignment, let P_{new} be the new posterior probability assignment, and let *X* be some proposition and *E* be the total amount of evidence acquired between the old assignment and the new assignment, then the principle of conditionalization may written as follows:

$$P_{new}(X) = P_{old}(X \mid E)$$

A justification for this principle was given by Paul Teller who attributes it to David Lewis.[110] Teller and Lewis argued that violating this principle will inevitably lead to the possibility of being Dutch booked (I will refer to this as the Teller-Lewis argument). The result is the diachronic Dutch Book Theorem, which claims that the use

[110] See Teller, P., "Conditionalization and Observation," *Synthese*, 26 (1973), 218-258. Lewis' version can be seen in his "Why Conditionalize?," *Papers in Metaphysics and Epistemology* (Cambridge: Cambridge University Press, 1999), 403-07.

of any updating rule that is not conditionalization results in the susceptibility to being Dutch booked. Once again, this theorem, by itself, does not guarantee a resistance to being Dutch booked altogether, even when the principle of conditionalization is properly followed. It could be the case that we are all still susceptible to being Dutch booked even if we properly follow the principle of conditionalization. However, when this theorem is combined with the converse diachronic Dutch Book Theorem, which states that if one's updating rule follows the principal of conditionalization then one will not be susceptible to being Dutch booked, this gap is shored up; and there is a powerful motivation to adopt the rule of conditionalization when updating one's beliefs.[111]

3. The Teller-Lewis Argument

The Teller-Lewis argument is an important argument for many reasons, but for the purposes of this chapter, it is especially important because, as we will see, van Fraassen's argument against his probabilistic version of IBE is simply a special instance of the Teller-Lewis argument. For this reason, a recapitulation of this argument is necessary.

Suppose a person, let us call him Smith, does not follow the principle of conditionalization. In particular, suppose Smith uses a rule to update his beliefs that produces a new posterior probability which is less than the old prior conditional probability. Given the terminology above, this means that, for Smith, the following would be true if *E* turned out to be true:

[111] The justification of this converse theorem has been provided by Brian Skyrms in his "Dynamic Coherence and Probability Kinematics," *Philosophy of Science*, 54 (1987), 1-20.

$P_{new}(X) < P_{old}(X \mid E)$

If we let *a* equal $P_{old}(X \mid E)$ and let *b* equal $P_{new}(X)$ subtracted from $P_{old}(X \mid E)$, then a bookie may sell the following bets to Smith at the maximum price that he would be willing to pay (where the fair value of the bet is determined by the stake of the bet multiplied by Smith's personal probability assignment that governs the proposition that is at issue):

Bet (1) \$1 if *X* and *E*; 0 otherwise,

Bet (2) \$*a* if not *E*; 0 otherwise, and

Bet (3) \$*b* if *E*; 0 otherwise.

For Smith, the value of these three bets, and thus the price for which he would be willing to buy them, would be the sum of the following: $P_{old}(X \text{ and } E)$, *a* multiplied by $P_{old}(\text{not } E)$, and *b* multiplied by $P_{old}(E)$. If *E* turns out to be false, then Smith would lose bets (1) and (3), and win bet (2), thus receiving \$*a*. When this winning amount is compared to the overall price of the bets, however, it is clear that Smith has a net loss of \$*b* multiplied by $P_{old}(E)$.[112] If *E* turns out to be true, then Smith would lose bet (2) and win bet (3), thus receiving \$*b*. But bet (1) is still undetermined because the truth of *X* is still undetermined. At this point, the bookie offers to buy a fourth bet from Smith at the minimum price Smith would be willing to pay:

[112] This is because the overall price of the three bets amounts to $P_{old}(X \text{ and } E) + aP_{old}(\text{not } E) + bP_{old}(E)$, which by the probability calculus, equals $P_{old}(X \mid E) + bP_{old}(E)$. If *E* turns out to be false, then Smith just wins back $P_{old}(X \mid E)$; thus the net negative difference to Smith is $bP_{old}(E)$.

Bet (4) $1 if *X*; 0 otherwise.

The price deemed fair by Smith would be $P_{new}(X)$, which is equal to $P_{old}(X \mid E)$ minus *b*. Therefore Smith will receive $\$b$ (from winning bet (3)) and $P_{old}(X \mid E)$ minus *b* (from selling bet (4)); and this of course is just equal to $P_{old}(X \mid E)$. When this gain is compared to the overall price of the initial three bets, Smith's net loss is once again *b* multiplied by $P_{old}(E)$. Notice that Smith incurs a net loss of *b* multiplied by $P_{old}(E)$ no matter if *E* happens and no matter if *X* happens—whatever occurs regarding *E* and *X*, Smith loses. Thus, by Smith using a rule to update his beliefs that produces a new posterior probability which is less than the old conditional probability, he is susceptible to being Dutch booked. Moreover, if Smith uses a rule to update his beliefs that produces a new posterior probability which is greater than, rather than less than, the old prior conditional probability, then the same result occurs. The argument for this is substantially similar; the only difference being a reversal between the buying and selling of bets between Smith and the bookie. The upshot is that a person who fails to properly conditionalize is susceptible to being Dutch booked. Since, for many Bayesians, to be susceptible to being Dutch booked is a failure of rationality, this amounts to the claim that a person is rational only if he or she properly follows the principle of conditionalization. But some Bayesians, like van Fraassen for instance, contend that conditionalization is not the only way to rationally respond to new evidence or information.[113] Van Fraassen claims that adopting a rule for belief change (any rule) is not rationally required—it is merely

[113] Van Fraassen's views on Bayesianism, induction, and voluntarism can be found in his *Laws and Symmetry* (Oxford: Oxford University Press, 1989), chapters 6 and 7, and his "Empiricism in the Philosophy of Science," in P. Churchland and C. Hooker (eds.), *Images of Science* (Chicago: University of Chicago Press, 1985), 245-308.

rationally permissible.[114] Thus, adopting the rule of conditionalization is not rationally required *per se*. Adopting the rule of conditionalization is rationally required *if* one chooses to adopt a rule that governs belief change at all.

4. Van Fraassen's Dutch Book Argument

With his argument from the bad lot and his argument from indifference already given, van Fraassen now believes that he has steered the IBE user into a corner; and it is here that he will deliver, in his mind, the final blow to IBE. Van Fraassen's confidence in this purported ringing of the death knell is abundantly clear. After he details his Dutch book argument, he writes:

> What is the moral of this story? Certainly, in part, that we should not listen to anyone who preaches a probabilistic version of Inference to the Best Explanation, whatever the details. Any such rule, once adopted as a rule, makes us incoherent.[115]

From reading this, one would think that van Fraassen must have generated, and refuted, every possible probabilistic version of IBE. Obviously, he could not have done this, so his stated moral of the story might be a bit exaggerated. As we will see, van Fraassen appears to assume that his probabilistic version of IBE is general enough to account for all probabilistic versions of IBE. And as it will be argued in the next section, this assumption is plainly false. Nevertheless, the question remains whether he has successfully argued against the IBE user who is defending a probabilistic version of IBE

[114] For van Fraassen, rationality concerns permissibility rather than obligation; thus, in this context, rationality concerns what a person may believe rather than what a person is obliged to believe.
[115] Van Fraassen, *Laws and Symmetry*, 169.

that is of the type described above—as retrenchment of form 2. In this section I will thoroughly explain van Fraassen's argument; in the next section, I will argue that it fails.

Van Fraassen's argument features several characters but focuses on a willing gambler named Peter and a friend of Peter's who happens to be a Bayesian bookie. Prior to engaging in a series of bets with the bookie, Peter becomes influenced by a preacher of IBE who recommends to Peter that the explanatory power of a hypothesis ought to increase the probability of it, all things being equal. Peter is persuaded to adopt the preacher's rule. This means, specifically, that Peter decides to adopt the rule of increasing the probabilities of explanatory hypotheses on the basis of how well they would explain the data if they were true. The greater the explanatory power of a hypothesis, the greater the increase of its probability. Yet Peter himself is a Bayesian, and so he needs to reconcile the preacher's rule with his Bayesianism—specifically he needs to reconcile the preacher's rule with the conditionalization process. Van Fraassen's solution is to have Peter increase the probability of the explanatory hypotheses *after* the conditionalization process; this means that the posterior probabilities of the explanatory hypotheses are given post-conditionalization bonus points, where the amount of bonus points received by the explanatory hypotheses is directly correlated with the amount of explanatory power the hypotheses would provide were they to be true.

Peter and the bookie then engage in a series of bets regarding the outcome of a die—in particular, whether or not the die comes up *ace*. The die, however, may not be fair, and the particular bias of the die is not known (van Fraassen refers to the die as *alien*). There are ten ways the die may be biased with respect to the outcome of *ace*, ranging from a perfect bias (meaning that *ace* will always come up) to a bias of 0.1 of

coming up *ace*. The probabilities of each of the biases will be affected by new evidence and will change accordingly as the evidence is gathered; that is, the probabilities of the biases of the die will be increased or decreased accordingly as the die is tossed and the outcomes are observed. The process governing the way in which these probabilities will be changed is Bayesian conditionalization. But notice that Peter will stray from this. He will add bonus points to the hypotheses that would explain the data best if they were true. This means, for example, that if the die's initial tosses all turn up *ace*, then Peter will accordingly afford a hypothesis of high bias a greater probability, given that this hypothesis would best explain the string of *aces*, if it were true, than Bayesian conditionalization would suggest. It is this discrepancy that will be taken advantage of by the bookie. In terms of the Teller-Lewis argument, Peter would be one who is following the principle whereby the new posterior probability of the hypothesis is greater than its old prior conditional probability:

$$P_{new}(X) > P_{old}(X \mid E)$$

As the Teller-Lewis argument tells us, anyone who adopts and follows a rule that leads to such an inequality will be susceptible to being Dutch booked. And this is exactly what van Fraassen proceeds to illustrate with regards to Peter and his probabilized version of IBE.

The bookie proposes the following three bets regarding two propositions, *E* and *H*. *E* refers to the claim that the first four tosses of the die show *ace*, and *H* refers to the claim that the fifth toss of the die shows *ace*.

> Bet I pays $10,000 if *E* is true and *H* is false; nothing otherwise.
>
> Bet II pays $1300 if *E* is false; nothing otherwise.
>
> Bet III pays $300 if *E* is true; nothing otherwise.

The fair price of these bets depends on the initial probability of the events in question, and the initial probability of these events can be determined with the help of the probability calculus. For example, the initial probability that the die will come up ace for each of the first four tosses (*E*) will be equal to the average of the probabilities of this happening given each of the ten possible ways the die may be biased. Van Fraassen lists the following initial probabilities of each bet[116]:

> Initial probability that *E* is true equals the average of $(0.1)^4$, …, $(0.9)^4$; that is, 0.25333
>
> Initial probability that *E* is false is 0.74667;
>
> Initial probability that *E* is true and *H* false is the average of $(0.1)^4(0.9)$, …, $(0.9)^4(0.1)$, 0; that is 0.032505.

The fair price of each bet will equal the initial probability multiplied by the payoff. Van Fraassen claims that both Peter and the bookie will agree on the price of the bets—the price they both deem fair given their shared initial probabilities. He lists the following fair costs of each bet:

> Fair cost of Bet I equals $325.05

[116] Van Fraassen, B., *Laws and Symmetry*, 168. There is a small error here in the passage. In calculating the initial probability of *E* being true, van Fraassen has only appeared to average nine of the possible biases rather than all ten; yet the result, 0.25333, is the correct outcome of averaging all ten possible biases.

Fair cost of Bet II equals $970.67

Fair cost of Bet III equals $76.00

The resulting cost of all three bets equals $1371.72. At this price, Peter buys all three bets from the bookie. At this point one of two outcomes will occur: either the first four tosses of the die will come up *ace*, or they won't. In other words, either *E* will turn out to be true or not. Suppose that *E* turns out to be false. This means that Peter has lost Bets I and III, and has won Bet II. The payoff for Peter is $1300, but this amount is less than the original cost of the three bets ($1371.72) which means that Peter has lost a total of $71.72. Suppose, on the other hand, that *E* turns out to be true. This means that Peter has lost Bet II, but won Bet III. This time the payoff for Peter is $300, but the outcome of Bet I is still undetermined.

At this point, the bookie proposes to buy a bet from Peter, a fourth bet:

Bet IV pays $10,000 if *H* is false; nothing otherwise.

Van Fraassen points out correctly that Bet I has now become equivalent to Bet IV, and the bookie is simply offering to buy the bet back from Peter. But what, from Peter's perspective, is the fair cost of this new bet? Given the available evidence so far (the first four tosses coming up *ace*), the probability calculus, specifically Bayes' theorem, can be used to generate the initial probability of *H* turning out to be true, and this probability turns out to be 0.87. Peter, however, is not following Bayes' theorem strictly—he is adding bonus probability points to hypotheses that would provide good explanations of

the data if they were true. And Peter sees that a string of four *aces* in a row might be best explained by a severe bias in the die for *ace*. Thus Peter will add explanatory bonus points to the hypotheses which claim a more severe bias of the die toward coming up *aces*. So van Fraassen determines that Peter's initial probability would not be 0.87, as it perhaps should be, but rather 0.9—an explanatory bonus of 0.03. If Peter believes that the probability of *H* being true is 0.9, then he will presumably believe that the probability of *H* being false is 0.1. It follows from this that Peter would identify the fair cost of Bet IV as $1000, and he agrees to buy the bet from his bookie for this amount. At this point, the outcome of Bet IV is irrelevant for van Fraassen's purposes—Peter will endure a net loss regardless of the outcome. Either the bookie wins Bet IV or not. If the bookie wins, then Peter pays $10,000; if the bookie loses, then Peter pays nothing. In either case Peter will not get paid anything more. So Peter has now paid $1371.72 for the first three bets, received $1000 for selling the fourth bet, and has won only $300 from Bet III. He will either lose $10,000 or nothing at all from Bet IV. This means that Peter will, once again, have a net loss of (at least) $71.72. Thus, Peter adopted and followed a rule that led to his engaging in a series of bets which he deemed fair but which guaranteed him a net loss.

As expected, van Fraassen's probabilistic version of IBE is a combination of IBE and Bayesianism. At first glance, there seem to be multiple ways of combining these approaches; but with no argument, van Fraassen settles on one. His tactic, as just described, is to have the Bayesian respond to new evidence by applying the rule of conditionalization to generate updated degrees of beliefs, and then increase the updated degree of beliefs for hypotheses deemed explanatory by boosting their probability with

bonus points. (Likewise, those hypotheses deemed to be poorer possible explanations of the evidence would have their probabilities decreased.) The most notable part of this version is that the explanatory bonus points are added to the probability of the respective hypotheses once the conditionalization process has been completed.

Given the discussion of the Teller-Lewis argument in the last section, the structure of van Fraassen's argument should not be surprising. The result of van Fraassen's probabilistic version of IBE is that, ultimately, it is a violation of the principle of conditionalization. This principle suggests that, as a Bayesian, we ought to abide by the following equality: $P_{new}(X) = P_{old}(X \mid E)$. Van Fraassen's retrenched Bayesian will not be abiding by this equality; instead he or she will follow a rule that will sometimes produce a new posterior probability which is less than the old prior conditional probability, and will sometimes produce a new posterior probability which is greater than the old prior conditional probability. If the Teller-Lewis argument is successful, it establishes that anyone who fails to properly conditionalize will be susceptible to being Dutch booked. This is precisely what van Fraassen believes happens when someone adopts his probabilistic version of IBE: anyone following this probabilistic version of IBE will be susceptible to being Dutch booked. Van Fraassen then claims that because Dutch book susceptibility is a failure of rationality, it follows that anyone who abides by this probabilistic version of IBE is irrational. The essence of this compound argument may be made explicit in the following way:

(1) All probabilistic versions of IBE that follow retrenchment of form 2 will be such that explanatory considerations will be accommodated by allocating bonus points to the probabilities of hypotheses *after* the conditionalization process.

(2) The adoption of such a rule will cause one to be susceptible to being Dutch booked.

(3) Therefore, anyone who adopts as a rule a probabilistic version of IBE that follows retrenchment of form 2 will be susceptible to being Dutch booked.

(4) Anyone susceptible to being Dutch booked is irrational.

(5) Therefore, anyone who adopts as a rule a probabilistic version of IBE that follows retrenchment of form 2 is irrational.

If this argument is successful, then it is a blow to the IBE user who desires compatibility (or possibly even a marriage) with the Bayesian. Fortunately, it is not successful.

5. The Failure of Van Fraassen's Dutch Book Argument

Van Fraassen's argument, reconstructed above, fails to establish its overall conclusion, (5), for at least two reasons. Firstly, the argument fails to establish the first conclusion, (3), because van Fraassen fails to establish the truth of the second premise, (2). The only reason he presents for believing the second premise is his story of Peter and the bookie; but, as I will argue, Peter applies the probabilistic version of IBE incorrectly, rendering the story ineffective. We are left with no good reason to believe the truth of the second premise. Moreover, the first premise, (1), is false. The falsity of the first premise, and the failure to establish the second premise, are independent reasons for believing that the first conclusion has not been established. And if there are good reasons to believe that the first conclusion has not been established, then there are good reasons to believe that the overall conclusion, (5), has not been established.

6. Undermining the Story of Peter and the Bookie

Van Fraassen's second premise, (2), claims that the adoption of his probabilistic version of IBE as a rule will cause one to be susceptible to being Dutch booked. The only support that is provided for this premise is an example of someone (Peter) who apparently adopts this rule and ends up being Dutch booked. The key to the success of this example (for van Fraassen) is that Peter is following a rule that produces an inequality between the new posterior probability of a hypothesis and its old prior conditional probability. Using the Teller-Lewis argument, it is this inequality that becomes exploited; for the argument shows that anyone who follows a rule that produces such an inequality will be susceptible to being Dutch booked. In what follows, I will argue that this example fails to support the claim made in the second premise. Specifically, I will argue that van Fraassen's Peter is being inconsistent when he is applying this probabilistic version of IBE as a rule. He is applying the rule correctly at one moment, but at a previous moment, when the rule ought to be applied, Peter does not apply the rule at all. If this allegation is true, then van Fraassen has not provided a fair and accurate example of someone whose adoption and consistent application of such a rule will inevitably lead to being susceptible to being Dutch booked. His example simply would be of a person who is applying this rule inconsistently; and hence incorrectly. It would thus undermine any support this example may provide the second premise of van Fraassen's argument.

It is an important part of van Fraassen's example that Peter and his bookie agree to the prices that set for the bets that are being bought or sold; in particular, it is important

that Peter deems the prices fair, or at least acceptable. These prices are determined by the product of Peter's subjective probability assignments and the stakes (as dollar amounts, for this example) of the bets, and so it is assumed that when the prices are calculated in this way, they will appear to Peter to be fair.

Clearly, Peter finds himself in this predicament—being susceptible to being Dutch booked—because his assignment to the new posterior probability of a hypothesis is greater than the prior conditional probability to which he has already seemingly committed. Once the first four tosses of the die came up *aces*, *E*, his probability assignment for the claim that the fifth toss will come up *ace*, *H*, turned out to be greater than the probability assignment he would have given for the conditional claim that the fifth toss would come up *ace* given the first four tosses come up *ace*. The conditional probability assignment of *H* given *E* can be determined by the probability assignments of *E* and of both *H* and *E* together, given the following definition of conditional probability:

$$P(H \mid E) = P(H \,\&\, E) / P(E)$$

Peter and his bookie calculate, together, the probability of *E* as 0.25333. Given the information available in the example, the probability of *E* and *H* together (the numerator on the right hand side of the equality) can be deduced—this is just the probability of the first five tosses of the die coming up *ace*. In van Fraassen's story, he did not have Peter (or the bookie) explicitly calculate the probability of *E* and *H*. Rather they calculated the probability of *E* and not *H*; and they did this by averaging these occurrences given each of the ten possible biases. Thus, in the same manner, the probability of the first five

tosses of the die coming up *ace*, i.e. P(*H* & *E*), will be equal to the average of $(0.1)^5$, … , $(0.9)^5$, 1, which turns out to be 0.220825. When this number is divided by the probability of *E*, which is 0.25333, the result is 0.87169, which is the conditional probability of P(*H* | *E*). None of these calculations, so far, utilize the preacher's rule, so to speak. They are arrived at just by using the probability calculus. If Peter is just using the probability calculus (and not the preacher's rule), then his commitment to the initial probability of Bet III implies that he ought to assign the conditional probability of P(*H* | *E*) as 0.87169. However, once *E* turned out to be true, Peter does *not* assign the probability of *H* to be 0.87169. Rather he adds bonus points to this probability on the basis that it would provide a good explanation of the run of *aces*, if it were to be true. This is because he chooses to follow the preacher's rule. And so Peter assigns it a probability of 0.9. Thus, the apparent problem is that Peter seems to have committed himself, at least implicitly, to a conditional probability assignment, P(*H* | *E*), but then, at a later time when *E* has been discovered to be true, he commits himself to a different probability assigned to *H*. If he knew these probability assignments were going to be different—and this is surely something he could and would know, all of it is readily available—then why would he agree to the initial series of bets? In particular, why would he agree to Bet I?

Jonathan Kvanvig has argued this line of reasoning.[117] A far cry from van Fraassen's own moral of this story, Kvanvig offers a different lesson to be learned:

> Once these facts [the ones explained above] are noted, the lesson is clear: if you want to give a bonus for explanatory hypotheses and also want to avoid [Dutch book strategies], you had better not commit yourself to other values inconsistent with the extra weight you want to assign because of explanatory value. In other words, because Peter knows that he will give *H* a value of .9 after learning *E*, he

[117] Kvanvig, "A Critique of van Fraassen's Voluntaristic Epistemology," 325-48.

> had better not agree to calculations that imply anything but .9 as the value for the probability of *H* given *E*.

Kvanvig's lesson is correct. If Peter wished to avoid being susceptible to being Dutch booked, while still wanting to follow the preacher's rule, then he should not have agreed to the initial probability of Bet I, and consequently would not have agreed to the price of the bet offered by the bookie.

One point to notice is that this allegation is very similar to, but not identical to, Patrick Maher's complaint against all diachronic Dutch book arguments. Maher has claimed that when a person adopts a rule that governs belief change which results in a violation of the principle of conditionalization, the person is not necessarily susceptible to being Dutch booked.[118] This is because, according to Maher, the person could easily do the calculations ahead of time and see the Dutch book coming. If this is true, then the person could simply avoid engaging in any series of bets that result in a Dutch book by using a kind of "look before you leap" principle. If this principle is accurate, and the method of application sound, then it follows that the diachronic Dutch Book Theorem would fail to be a theorem at all.

While Kvanvig's lesson is correct, Maher's complaint is too quick.[119] If a person's probability assignments violate the principal of conditionalization, then the diachronic Dutch Book Theorem claims that the person is *susceptible* to being Dutch booked. It is the *susceptibility* to being Dutch booked that is the mark of irrationality—not that one is being Dutch booked or will be Dutch booked soon. This mark of

[118] Maher, "Diachronic Rationality," 120-41.
[119] Maher, himself, seemed to acknowledge in a later work that the diachronic Dutch Book Theorem is actually correct, in his *Betting on Theories* (Cambridge: Cambridge University Press, 1993). This appears to be at least partly due to Brian Skyrms' criticism, in his "A Mistake in Dynamic Coherence Arguments?," *Philosophy of Science*, 60 (1993), 320-28.

irrationality cannot be avoided by simply refraining to engage in betting behavior. Whether or not a person actually chooses to engage in a series of bets with someone is entirely irrelevant to the question of whether or not the person's probability assignments make them susceptible to being Dutch booked. The point is that if the person *were* to engage in a certain series of bets, using their own probability assignments, then, at prices deemed fair (or at least favorable), the person would be guaranteed to lose.

Maher's complaint should not be confused with Kvanvig's allegation. Maher, for example, would presumably advise Peter to refrain from buying at least the first two bets from the bookie; whereas Kvanvig would presumably advise Peter not to agree to the initial probability of Bet III to begin with (if Peter wishes to stick with the preacher's rule). These bits of advice are different because Maher and Kvanvig are diagnosing the problem differently.

I have no quarrel with the diachronic Dutch Book Theorem; I will not argue against it or against Dutch book arguments in general. While Kvanvig's diagnosis is true, I maintain that it does not go far enough. Before Peter buys the three bets, he calculates the initial probabilities of each of them. His calculation of the initial probability of Bet I—that *E* is true and *H* is false—turned out to be 0.032505. This calculation is derived entirely from the probability calculus. But if Peter is really following the preacher's rule, then presumably he ought to apply it in this situation. This means that his assignment for the initial probability of Bet I ought to be lower than 0.032505.[120] If *E* turns out to be true (and there is an initial run of four *aces*), then, for Peter, there will be a high

[120] Just how much lower would Peter's calculation of the probability assignment be? It is unclear. As expected, van Fraassen provides no systematic quantitative rules for applying the explanatory bonus points. Nevertheless, based on his "explanatory adjustment" of Peter's initial probability of Bet IV—going from 0.87 to 0.90—we can easily make an educated guess.

probability of a bias in the die toward ace. A bias toward *ace* would explain this run of *aces*. Thus, for Peter, the probability that the fifth toss of the die will turn up *ace* will be higher than 0.87169, due to the explanatory bonus points. This implies that the probability that the fifth toss of the die will not turn up *ace* will be lower than his initial calculation of 0.032505. If Peter were following the preacher's rule consistently, he would have used it in the calculation of the initial probability of Bet I—and not just in the later calculation of the probability of not *H*, given that *E* turned out to be true. But he did not apply the rule both times as he should have.

It follows from this that van Fraassen's story is ineffective. The story is meant to be the support needed for premise (2), but, because Peter applies the preacher's rule inconsistently (and thus incorrectly), the story can be no reason for thinking that premise (2) is true.

Unfortunately, for van Fraassen, the situation is worse than this. If Peter had applied the preacher's rule consistently, then, given the same betting scheme, he would no longer be guaranteed a loss. As explained above, by applying the rule consistently, his initial probability of Bet I would have been different—it would be lower. This also means that the price for this bet would have been different—it would also be lower. Even with this small, yet significant, adjustment the same betting scheme would not result in a guaranteed loss for Peter. For example, if Peter calculates the initial probability of Bet I to be 0.022505—just 0.01 lower than the previous calculation—then the price of Bet I would be lowered by $100 to $225.05. The total price of the three bets would then equal $1271.72 rather than $1371.71. If *E* turns out to be false, then Peter wins Bet II and is paid $1300—a net gain of $28.28. If *E* turns out to be true, then Peter

wins Bet III and is paid $300. If the bookie then buys back Bet I at the price of $1000, then, regardless of whether *H* turns out to be true or false, Peter once again has a net gain of $28.28. And if the bookie does not buy back Bet I, then Peter is not guaranteed an overall loss. When the preacher's rule is applied consistently, the example fails to establish premise (2). This is not to say that, with this rule applied consistently, Peter is thus forever immune to being Dutch booked. However, if Peter can apply the rule consistently and still adhere to the principle of conditionalization, then, according to the converse diachronic Dutch Book Theorem, he would not be susceptible to being Dutch booked. Van Fraassen has failed to show us that anyone who adopts the preacher's rule while, at the same time, still adhering to the principle of conditionalization would be open to a Dutch book. And this is what he needs to show us in order to support premise (2).

7. Undermining the First Premise

The first premise in van Fraassen's argument states that all probabilistic versions of IBE that follow retrenchment of form 2 will be such that explanatory considerations will be accommodated by allocating bonus points to the probabilities of hypotheses *after* the conditionalization process. This is van Fraassen's probabilized version of IBE. Given that van Fraassen has claimed that no probabilized version of IBE is coherent—no matter what the details of the version are!—the truth of this premise is utterly crucial to his argument. This premise is a straightforward universal generalization; thus, if there can be a probabilized version of IBE that follows retrenchment of form 2 but does not accommodate explanatory considerations by allocating bonus points to the probabilities of hypotheses after the conditionalization process, then the premise is false. The

question, then, is whether there can be a probabilized version of IBE that follows retrenchment of form 2 but is different from the one that van Fraassen is suggesting. And the answer, as I will argue, is that, quite plausibly, there is a different probabilized version of IBE that easily counts as retrenchment of form 2. Consequently, there is a good reason to believe that van Fraassen's idiosyncratic way of combining IBE and Bayesianism is not the only way to combine IBE and Bayesianism. And, thus, there is a good reason to believe that his first premise is false.

Given the machinery of Bayesianism, there appear to be a limited number of ways a Bayesian could possibly accommodate IBE. The heart of Bayesianism is the process of conditionalization. If IBE is to be combined with Bayesianism, then presumably it will have to be done without disrupting this process. So where is IBE going to fit? In other words, how can a Bayesian accommodate, in a proper way, the explanatory power of hypotheses? Overall, there appear to be three options: explanatory considerations can be accommodated either (1) before the conditionalization process, (2) during, or as a part of, the conditionalization process, or (3) after the conditionalization process. Van Fraassen's probabilized version of IBE consists in the third option.

The prospects for the second option look dim. Not only is it difficult to see how explanatory considerations could be a part of the conditionalization process without compromising the process itself, it is also difficult to see what benefits this option would present—especially if one of the other two options appear to be more promising. And, in fact, the prospects for the first option look much more promising. In order to properly conditionalize, a Bayesian needs to be aware of several probabilities—the likelihood of the evidence, the prior probability of the hypothesis, and the prior probability of the

evidence. Rather than interfering in the conditionalization process, perhaps explanatory considerations can play a role in forming some of these probabilities. In particular, the two probabilities that would be most affected by the explanatory power of a hypothesis would be the likelihood of the evidence and the prior probability of the hypothesis. If explanatory considerations play a role in forming these probabilities, which are essential to the conditionalization process, then the outcome of the conditionalization process, namely the posterior probability of the hypothesis, will have been affected by the explanatory power of the hypothesis itself, in a way that would seem to be very pleasing to the IBE user.

To see how this might work, consider two competing hypotheses, H_1 and H_2, which are possible explanations of some evidence, E. If H_1 is deemed a better possible explanation of E than H_2, then, all things being equal, the IBE user would assign a higher probability to H_1 than to H_2. This preference would be reflected in the assignment of a higher conditional probability of E given H_1 than E given H_2, thus the inequality: $P(H_1|E) > P(H_2|E)$. In fact, as is common in Bayesianism, if the relevant background knowledge of the IBE user is made explicit and added to the equation as K, then the inequality may be expressed as: $P(H_1|E \,\&K) > P(H_2|E\& K)$. If E is already known to be true, then it may be subsumed under K, resulting in straightforward prior probabilities of the two hypotheses, H_1 and H_2. Notice that this result is exactly what the IBE user is hoping for—greater explanatory power produces a higher probability assignment.

If the prior conditional probability of H_1 given E is greater than the prior conditional probability of H_2 given E (where E is yet to be incorporated), due to H_1's explanatory power, then, once the conditionalization process is completed (that is, once

the probability assignments have been updated to include *E*), the result will retain this inequality: the new posterior probability of H_1 will be greater than the new posterior probability of H_2. And this is in alignment with the principle of conditionalization and, once again, it is in alignment with the IBE user's claims regarding the connection between explanatory power and truth.

Bayesians may often point out that today's prior probability assignments are yesterday's posterior probability assignments. Obviously, the conditionalization process lends itself to this kind of thinking. Applying the principle of conditionalization when accommodating new evidence means identifying the new posterior probability of a hypothesis with its old prior conditional probability. Thus, another way explanatory considerations may factor into the prior probability of a hypothesis is by figuring into this transition. This may be accomplished by incorporating explanatory considerations in the probability assignment of the likelihood of the evidence. Likelihoods are not easily calculated; apart from the situation of a hypothesis entailing the evidence claim, it is difficult to judge the probability assignment of the evidence given the hypothesis. The explanatory features of the hypothesis as it pertains to this evidence, however, may be the best criteria when judging this probability assignment. Certainly, appraising how well the hypothesis, if it were true, would explain the evidence would seemingly provide insight—perhaps the best insight—into how probable the evidence is, given the hypothesis. The IBE user (or any Bayesian) might very well take advantage of the connection between possible explanations and the probability of the possible data that they would explain if they were true. In particular, this correlation suggests that as the

quality or goodness of the possible explanation increases, then so does the probability of the possible data that the possible explanation would explain if it were true.[121]

The resulting probabilized version of IBE is the result of taking the first option—of accommodating explanatory considerations before the conditionalization process takes place. Not only does this option appear more promising, it has also enjoyed greater support. Gilbert Harman may have been the first to suggest that explanatory considerations play a role in determining prior probabilities.[122] He wrote that:

> Perhaps reasoning is concerned with subjective probability, but it is important to see that one fixes such probability by appeal to explanatory plausibility rather than vice versa. … [O]ne arrives at an estimate of subjective probability by considering the plausibility of various explanations. … [In forming probability assignments] perhaps we should speak of a *subjective plausibility function*, which could be derived from judgments of explanatory plausibility. Subjective probability would depend on one's beliefs and one's subjective plausibility.[123]

Much later, he adds to this thought by writing:

> Suppose one has two hypotheses, *h* and *h'*, both of which accommodate the evidence *e*, but one takes *h* to be the better explanation because it is simpler. Suppose that this difference inclines one to infer *h* rather than *h'* or any of the infinitely many other competing hypotheses that could account for *e*. This does not mean that such explanatory considerations play a role in addition to conditionalization. Rather, the fact that one is inclined to infer *h* rather than *h'* shows that
>
> $$cond\text{-}prob(h, e) >> .5 >> cond\text{-}prob(h', e)$$
>
> This implies that
>
> $$prob(h\&e) >> prob(h'\&e)$$

[121] Peter Lipton has suggested this line of reasoning. See his *Inference to the Best Explanation*, 114.
[122] Harman, "Pragmatism and Reasons for Belief," 123-47.
[123] Harman, "Induction. A Discussion of the Relevance of the Theory of Knowledge to the Theory of Induction," 93-4.

in one's prior probability distribution.[124]

Obviously, Harman is claiming that explanation considerations will influence the formation of both the prior probability of a hypothesis and the conditional probability of the hypothesis.

More recently, Samir Okasha has pushed for this option as well, except Okasha adds that explanatory considerations also will influence the formation of the likelihood of the evidence.[125] He writes:

> The correct way of representing IBE, I suggest, views the goodness of explanation of a hypothesis *vis-à-vis* a piece of data as reflected in the prior probability of the hypothesis P(H), and the probability of the data given the hypothesis P(e/H). The better the explanation, the higher is one or both of these probabilities. Relative to this account, favouring a hypothesis on the grounds that it provides a better explanation of one's data than other hypotheses, and indeed making it a rule to do so, is perfectly consistent with Bayesian principles.[126]

Interestingly, Okasha argues that this approach is better than van Fraassen's approach because it does a better job of capturing the phenomenology of IBE. Van Fraassen's approach results in a two-stage process: responding to the evidence through conditionalization, and then apportioning explanatory bonus points to the appropriate hypotheses. Okasha points out that the proper account of IBE should reveal only a one-stage process: using explanatory considerations as a way of responding to the evidence. And Okasha's approach certainly comes much closer to capturing this account.

[124] Harman, "Pragmatism and Reasons for Belief," 139.
[125] Okasha, "Van Fraassen's Critique of Inference to the Best Explanation," 691-710.
[126] Ibid., 703.

Finally, Peter Lipton has suggested that IBE and Bayesianism are "broadly compatible" in much the same way.[127] He lists and explains four different ways in which explanatory considerations "might help to lubricate the Bayesian mechanism."[128] In addition to arguing that explanatory considerations play a role in determining the prior probability of a hypothesis and the likelihood of the evidence, Lipton argues that explanatory considerations help to determine the relevant evidence. Regarding this latter point Lipton writes that:

> Bayes's theorem describes the transition from prior to posterior, in the face of specified evidence. It does not, however, say *which* evidence one ought to conditionalize on. …So it seems that a Bayesian view of inference needs some account of how the evidential input into the conditionalizing process is selected, and this seems yet another area where the explanationist may contribute. To give just one example of how this might work, consider how we sometimes discover supporting evidence for a hypothesis by seeing what it would explain. My suggestion is that we sometimes come to see that a datum is epistemically relevant to a hypothesis precisely by seeing that the hypothesis would explain it.[129]

Relatedly (and fourthly), Lipton also points out that explanatory considerations aid in the discovery of hypotheses. No part of the Bayesian machinery helps to generate or discover a hypothesis; and yet, the conditionalization process cannot take place without one. Figuring out what would explain the available evidence is certainly helpful in the act of generating hypotheses.

All of this discussion suggests that, quite reasonably, there is an alternative probabilized version of IBE to van Fraassen's. The only question remaining is whether or not this alternative satisfies the conditions set forth by van Fraassen's premise; namely,

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[127] Lipton, *Inference to the Best Explanation*, 106.
[128] See Lipton, "Is Explanation A Guide to Inference?," in G. Hon and S. Rakover (eds.), *Explanation: Theoretical Approaches and Applications* (Dordrecht: Kluwer, 2001), 93-120.
[129] Lipton, *Inference to the Best Explanation*, 116.

can this alternative rightly be considered retrenchment of form 2? The answer is yes. In order for a probabilized version of IBE to fall under this heading, it must rationally require (as a rule) the proper use of explanatory considerations as part of the belief-updating process. On the face of it, there appears to be no reason why this probabilized version of IBE cannot be consistently and coherently adopted as a rule—the violation of which resulting in irrationality. To argue otherwise, one would need either to show that the adoption of this rule would lead to Dutch book susceptibility, or to show that the rule itself is somehow otherwise incoherent. From van Fraassen's side, no such arguments have been forthcoming. My goal in this section was to offer a plausible and reasonable alternative to van Fraassen's probabilized version of IBE; and I have done so. The existence of this alternative is sufficient to show that van Fraassen's first premise is false.

8. Conclusion

In this chapter I rebutted van Fraassen's Dutch book argument against a probabilized version of IBE. I first showed how this argument is a special instance of the Teller-Lewis argument, which establishes that violating the principle of conditionalization will inevitably lead to the possibility of being Dutch booked. I characterized van Fraassen's argument as suggesting that every probabilistic version of IBE must accommodate explanatory considerations after the conditionalization process by providing bonus points to the probabilities of the hypotheses. Van Fraassen alleges that this process results in a violation of the principle of conditionalization, and hence results in the possibility of being Dutch booked. He illustrates this point with an example involving Peter and his bookie. By essentially equating Dutch book susceptibility with

irrationality, van Fraassen concludes that anyone who adopts a probabilistic version of IBE is irrational. I alleged that this conclusion is not established by arguing that van Fraassen's example of Peter and his bookie included a misapplication of van Fraassen's own probabilized version of IBE. Additionally, I argued that van Fraassen is mistaken to claim that every probabilistic version of IBE must accommodate explanatory considerations with the use of post-conditionalization bonus points.

CHAPTER SIX

Conclusion

In this dissertation I presented and rebutted a series of arguments given by Bas van Fraassen which were meant to establish that IBE is not a justified mode of inductive inference. Even if I am correct and van Fraassen's arguments fail, IBE may still be an unjustified or an unreliable mode of inference. So far I have not been concerned with any argument that purports to show that IBE is in fact justified or reliable; but I would like to end this dissertation with a very brief discussion of the prospects of demonstrating the justification or reliability of IBE. So, to this topic, I turn.

If IBE can be shown to be a reliable method for getting at the truth, then the debate between scientific realism and scientific antirealism would be seriously affected. Philosophers have attempted such arguments, but so far no consensus has been reached regarding their effectiveness. Each side of the debate seems to find defects in each of the opposing arguments—the most common defect allegedly being the fallacy of begging the question or circularity. For this reason Andre Kukla has already declared that the entire debate is likely to be irreconcilable.[130] I am not so pessimistic (yet).

The most prominent argument in support of the reliability of IBE is a variant of the No-Miracle argument. This argument suggests that mature and successful scientific theories ought to be judged to be true or approximately true because this would be the best explanation of their success. In addition, since IBE is an essential part of the

[130] See for example Kukla (1998), 162-63. Also in a review of Stanford's book, *Exceeding our Grasp: Science, History, and the Problem of Unconceived Alternatives*, Kukla provides a humorous version of a pessimistic induction that suggests this view: "all past arguments in support of either realism or anti-realism have been found to be defective; therefore I predict that present and future arguments for realism or anti-realism will also turn out to be defective." For the full review, see Kukla (2010).

scientific methodology that generates and supports such theories, IBE itself ought to be judged to be a reliable method for getting at the truth.[131] The obvious objection to this line of reasoning is that it is circular, for the argument clearly seems to be using IBE to justify IBE. Consequently there has been much discussion regarding the precise nature and meaning of the fallacy of circularity, and how it may or may not apply to this argument. For example, Psillos tries to establish that although the argument is circular, it is not *viciously* circular because it is rule-circular rather than premise-circular.[132] And Lipton claims that the argument may beg the question, but only against those who are already not willing to accept that IBE is truth-tropic.[133] I believe there is some merit in pursing these or similar lines of reasoning. Due to the fact that the goal is to justify a form of *inductive* inference, it also appears as if the defender of IBE may be forced to accept some level of circularity in their arguments. The support for IBE must either be *a priori* or *a posteriori* in nature. It is unclear how IBE could be convincingly supported with *a priori* reasons. In fact, I am not even sure what these reasons would look like. Perhaps the *a priori* justification of IBE would involve the idea of it being intuitively grasped or understood, or perhaps it would be entailed by a set of axiom-like principles that are themselves known to be true *a priori*. In either case, the prospects for this kind of defense are dim to say the least. That IBE is justified, if it is, is certainly not a necessary truth, or a logical or conceptual truth; if it is justified, then surely this is a contingent fact discoverable through our interactions with the world.

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[131] I have discussed this line of argument already in chapter two, attributing it to Richard Boyd. It should be known that other philosophers have seemingly endorsed it as well, including Psillos (1999) and Lipton (2004).
[132] Psillos, *Scientific Realism: How Science Tracks Truth*, 81-90.
[133] Lipton, *Inference to the Best Explanation*, 184-92.

Therefore, if a justification of IBE is to be given, the reasons must be *a posteriori*. But this immediately suggests that the support for IBE's justification must be inductive. A deductively valid argument intending to establish the justification of IBE can reasonably be given—in fact quite easily—but surely the best reasons in support of one of the premises will ultimately require the use of induction. Arguing for IBE's justification with an inductive argument is dangerously similar to attempting to resolve the problem of induction with an inductive argument—to justify induction with the use of induction. The only difference, it seems, is that in the latter case the attempt is to justify induction as a whole but in the former case the attempt is to justify a particular inductive type of argument. Nevertheless, arguments of this type will always hint at circularity, if they are just not outright circular. The problem will be to show that this circularity is not vicious, or to show that, in some other way, this circularity is just not problematic. Of course, we go on using induction even if we accept that we are unable to provide any non-circular rational justification for it. Naturally, the justification of induction is independent of our ability or inability to demonstrate it. The same is true for IBE, but this hardly helps the cause. Whatever the details, it seems that the strategy of inductively supporting the justification IBE will involve inferring that IBE is a reliable method for getting at the truth by emphasizing the past and current successes of IBE. Perhaps this emphasis might also include an argument for the epistemic status of certain explanatory virtues like simplicity and fruitfulness. An argument for the claim that certain explanatory virtues are evidential will have to be an empirical argument similar to the following line of reasoning: our experiences of the world inform us that true explanations tended to have certain qualities like being simple or being fruitful, and so we should

expect that future true explanations will also tend to have these qualities. But this will be a difficult argument to make.

Aside from downplaying the apparent circularity of the justification for IBE, from arguing that the circularity is not vicious, or from establishing that certain explanatory virtues are indeed evidential, another promising approach involves an externalist defense of the justification of IBE. Psillos, for example, seems to think highly of this strategy.[134] Within epistemology there are different accounts of justification, and one account is externalism. According to this view, the factors that justify a belief are not required to be accessible to the believer upon reflection. One particular externalist position is reliabilism. Generally, reliabilism is the view that the reliability of a belief-forming process is both sufficient and necessary for a belief which is formed through this process to be justified.[135] If IBE is a reliable belief-forming process, then, given this general form of reliabilism, the outcome of an instance of IBE will be justified. And furthermore, given the externalist nature of reliabilism, whether or not IBE is a reliable belief-forming process is an empirical question requiring an empirical investigation. Moreover, if IBE is a reliable belief-forming process, then the users of IBE are justified in their conclusions even if they have no reason or evidence to suppose that IBE is reliable at all. Thus, given this strategy, all that would be left to do, besides adopting externalism and reliabilism, is to provide a reliabilist defense of IBE. Samuel Ruhmkorff has also taken up this strategy and has already outlined the general scheme for a reliabilist defense of a rule of inference.[136]

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[134] See, for example, Psillos, *Scientific Realism: How Science Tracks Truth*, 84-90.
[135] There are, of course, more sophisticated versions of reliabilism. See for example Steup (1996, chapter 8).
[136] See Ruhmkorff (2005).

The key to all of these strategies is to somehow derive, without begging the question in a problematic way, IBE's justification or reliability from past experience, where the most important parts of this past experience are the actual successful instances of IBE. Unfortunately, none of these strategies will lead to an easy resolution to the problem. As I have already stated in the introduction, presumptively, it appears that IBE is generally a reliable way for getting at the truth. But if there are good reasons to believe otherwise, we should take notice. Van Fraassen has claimed to provide us with good reasons. By dispensing with van Fraassen's arguments against the reliability of IBE, however, I have restored this presumptive attitude.

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