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SCIENTIFIC REALISM VS. SCIENTIFIC ANTIREALISM

By

Seungbae Park

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A Dissertation Submitted to the Faculty of the

DEPARTMENT OF PHILOSOPHY

**In Partial Fulfillment of the Requirements
For the Degree of**

DOCTOR OF PHILOSOPHY

In the Graduate College

THE UNIVERSITY OF ARIZONA

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As members of the Final Examination Committee, we certify that we have read the dissertation prepared by Seungbae Park entitled Scientific Realism Vs. Scientific Antirealism

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ACKNOWLEDGEMENT

I am indebted to many people in producing this dissertation. Of course, my foremost debt goes to my advisor, Richard Healey. The numerous courses I took from him are the essential basis for this dissertation. His meticulous comments on the several drafts were of great help in interpreting various philosophical positions and in anticipating possible objections from antirealists. I could not have done anything without his advice and encouragement. It was more than a great pleasure to write a dissertation under his supervision.

Shaughan Lavine's detailed comments were of great help not only in substantive matters but also in stylistic matters. His experience as a journal editor was very helpful. His sense of humor was a reminder that doing philosophy can be fun. I remember that I also had a lot of fun TA-ing for his undergraduate philosophy of science course. Philosophical discussions with him were an arena for learning how to attack or defend a position. I wish that I had more time to learn from him, and that he taught more philosophy of science courses.

I am also grateful to Jenann Ismael, an excellent devil's advocate. I earlier gave a presentation on the coherentist defense of scientific realism during her seminar. My idea was at an inchoate stage, and she criticized it from all directions. I thought that scientific realism was dead. Through her comments I came to know more about standard antirealist objections to scientific realism in the literature.

I also thank Larry Lustig, a philosophy professor at the University of Maryland in Seoul. For me, he was a window to philosophy classes in English. I first met him when I was a KATUSA, a Korean soldier assigned to the US army in South Korea. I had the privilege to audit his introduction to philosophy course in which he taught Plato, Aristotle, and other philosophers. Subsequently, I took some of his philosophy courses. His courses were a springboard to my graduate study in the US.

Both non-realist philosophers and realist philosophers in the literature influenced me a lot. The non-realists include van Fraassen, Laudan, Fine, Ladyman, Kukla, and Stanford. The realists include Psillos, Kitcher, and Leplin. Their contributions to the debate are important for this dissertation. I take Laudan's position to be interesting. He is a reminder to me that philosophy is not a simple business. In my view, Psillos, Ladyman, Kukla, and Stanford are rising stars in this field.

Finally, I thank my parents, Nogeun Park and Insoon Kook, and the rest of my family members, for their support on my education. I would be doing something else, had it not been for their sacrifice and dedication that they gave to me.

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ABSTRACT

According to Boyd/Putnam, scientific realism is the view that successful theories are typically approximately true and that their key terms typically refer. The no-miracle argument for the view holds that approximate truth and reference provide the best explanation of the success of science. I try to defend scientific realism from the following six lines of antirealist objections:

First, constructive empiricists argue that inference to the best explanation is a problematic rule of inference. I try to show that their critiques of inference to the best explanation backfire on van Fraassen's positive philosophical theories, such as the contextual theory of explanation and constructive empiricism. Second, pessimistic inducers argue that successful current theories will follow the fate of successful past theories which turned out to be completely false. I reply that realists can get around the historical objection, once they take the realist attitude only toward successful theories that cohere with each other. Third, antirealists from van Fraassen (1980) to Stanford (2000) have been proposing antirealist explanations of the success of science, thereby challenging the realist claim that the realist proposal is the best. I criticize eight antirealist proposals that I found in the literature with a view to proving that the realist proposal is still the best of the proposals I know of. Fourth, antirealists reject realism based on their views on the nature of scientific explanation. I critically evaluate four antirealist objections coming from that route. Fifth, antirealists might object that the key realist predicate, 'approximate truth,' is obscure. I reply that the predicate is viable, because

there are clear cases of approximately true descriptions, and because Hilpinen/Lewis's theoretical account of approximate truth can handle those clear cases. Sixth, constructive empiricists claim that constructive empiricism is better than scientific realism because it explains science without extra epistemic risk. I attempt to prove, contrary to what the constructive empiricists believe, that empirical adequacy is harder to come by than approximate truth in the light of the pessimistic induction and the realist responses to it.

Conclusion: Semantic, economic, empirical, and pragmatic considerations as a whole favor scientific realism over scientific antirealism. when realists believe that our best theories, successful theories that cohere with each other, are approximately true, and antirealists believe that they are approximately empirically adequate. Scientific realism is overall better than scientific antirealism.

INTRODUCTION

1. Scientific Realism and the No-Miracle Argument for It

What kinds of things exist in the world? What does the world look like? Do we have reliable means to know about the world? These are some of the questions philosophers have asked throughout the history of philosophy. Different world views emerge, depending on what the answers are to these questions. This philosophical tradition continues in the debate between scientific realists and antirealists. The scientific realists, believing that scientific means are reliable even when applied to unobservables, trust some of what science says about unobservables. Thus, theoretical beliefs are included in their world view. The scientific antirealists, on the other hand, doubt that scientific means are reliable when applied to unobservables, and trust only some of what science says about observables. Thus, only observational beliefs are included in their world view. But what exactly does scientific realism (realism, henceforth) assert?

According to Boyd/Putnam, realism is the view, roughly, that successful scientific theories are typically approximately true and that their key terms typically refer. What does it mean to say that a theory is successful? As far as I know, Laudan provides the most precise definition of success:

...we would say that a theory is successful if it makes substantially correct predictions, if it leads to efficacious interventions in the natural order, if it passes a battery of standard tests. (1981, p. 23)

Accordingly, I shall assume that a theory is 'successful' so long as it has worked well, i.e., so long as it has functioned in a variety of explanatory contexts, has led to confirmed predictions and has been of broad explanatory scope. (ibid., p. 23)

In short, a theory is successful if and only if it has high explanatory, predictive, and manipulative powers. Keep in mind that to say that a theory is successful involves the claim that it has passed a battery of rigorous empirical tests.

What does it mean to say that a theory is approximately true? The concept of approximate truth is to be understood in contrast with the concept of probable truth. To say that a theory is probably true is to say that we do not have enough evidence to be certain that the theory is true, and the theory may perfectly match up with the world. The intuitive idea of approximate truth is that a theory is approximately true just in case what it describes is quite close to the actual world, and we may or may not have enough evidence to believe so. Suppose, for example, that John is in fact 178.3cm tall. Then, it is approximately true that he is 178cm tall. Strictly speaking, the statement is false. But it is not wildly off the mark like the statement that he is 120cm tall. In any case, I reply to some of the standard antirealist objections to the concept of approximate truth in Chapter 5.

Now, why should we accept Boyd/Putnam's view that successful theories are typically approximately true and that their key terms typically refer? The best argument for the view is said to be the no-miracle argument, which Putnam states as follows:

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.
(Putnam, 1975)

By 'mature,' Putnam seems to have in mind Kuhn's notion of mature science. In a mature science, scientists share metaphysical assumptions, instruments, and standards to evaluate scientific achievements. Scientists do not question those fundamentals, but engage themselves in puzzle-solving activities: applying theoretical assumptions to various parts of the world, articulating a paradigm, developing mathematical techniques, and improving instruments. If a scientist fails to solve a puzzle, the blame is directed not on the paradigm but on the scientist. In short, scientists do normal science under a stable paradigm.

In the no-miracle argument, Putnam is making an inference to the best explanation. The explanandum is the success of some scientific theories. He considers two rival hypotheses: the *miracle* hypothesis and the *no-miracle* hypothesis. The miracle hypothesis holds that 1. key terms of the successful theories do not refer, 2. the theories are not even approximately true, and 3. a miracle happened. The no-miracle hypothesis holds that 1. key terms of the successful theories typically refer, 2. the theories are typically approximately true, and 3. a miracle did not occur. Putnam is inferring from the phenomenon of success to the no-miracle hypothesis rather than to the miracle hypothesis, because he thinks that a miracle is an event that is not likely to occur, and that an explanation invoking a miracle is not reasonable. We are now ready to reconstruct the no-miracle argument in a standard form:

Premise 1: The success of some scientific theories needs to be explained.

Premise 2: The miracle hypothesis holds that key terms of successful theories do not refer, that the theories are not even approximately true, and that a miracle happened.

Premise 3: The no-miracle hypothesis asserts that key terms of successful theories typically refer, that the theories are typically approximately true, and that a miracle did not occur.

Premise 4: A miracle is an event that is not likely to occur.

Conclusion: The no-miracle hypothesis provides the best explanation of the success.

Notice that the word 'mature' does not figure in the reconstructed version, while it does in Putnam's original writing. I drop the word out of the picture because it invites unnecessary criticisms. Laudan points out that it is hard to test whether a branch of science has crossed the threshold of maturity, and that some immature theories in the history of science were successful (1981, p. 34). So the reconstructed version might not be a faithful representation of what Putnam had in mind, but it avoids Laudan's criticisms while saving the realist intuition that successful theories are highly likely to be approximately true and that their key terms are highly likely to refer.

Having stated realism and the best argument for it, we are now ready to ask the question: Should we accept the argument? Many people think that we should not. Fine, for instance, claims that "the realist programme has degenerated by now to the point where it is quite beyond salvage" (1986, p. 149). In fact, the no-miracle argument

received a barrage of criticisms from all directions: philosophy of science, history of science, sociology of science, and psychology of science. It is beyond the scope of this dissertation to address all those objections. Inevitably, I focus only on some of them. But it becomes clear in the end, I hope, that if realism is beyond salvage, antirealism is even more so.

2. Plan of the Dissertation

In Chapter 1, I discuss the constructive empiricists' claim that inference to the best explanation is a problematic rule of inference. Their critiques of inference to the best explanation, if correct, have a significant impact on the scientific explanations of phenomena and on the realist explanation of the success of science. Their critiques, as we will see soon, are built upon their insight that for any scientific theory, (it is possible that) there are indefinitely many hitherto unconceived rival theories which are all in accordance with the evidence so far but make incompatible claims that go beyond the evidence. I explore the disastrous implications their critiques have on van Fraassen's positive philosophical theories, such as the contextual theory of explanation and constructive empiricism. At the end of the chapter I make some attempts to diffuse their critiques.

In Chapter 2, I confront two historical challenges to realism. The first historical challenge, which is known as pessimistic induction or disastrous induction, is the strong claim that successful current scientific theories will follow the fate of successful past scientific theories which turned out to be completely false. I provide reasons to think that

the history of science does not force us to accept the pessimistic view about current scientific theories. The second historical challenge is the weak claim that there are historical counterexamples that undermine the realist inference from success to approximate truth. To counter this weak claim, I modify Boyd/Putnam's version of realism. The modified version asserts that successful theories that cohere with each other are approximately true and that their key terms refer. A story will be told of what it means for two scientific theories to cohere with each other. I compare my approach with Kitcher's approach to pessimistic induction, and argue that it points to the direction in which the problem plaguing Kitcher's approach could be solved.

In Chapter 3, I discuss the antirealist charge that the success of science can be explained without an appeal to approximate truth and reference. Antirealists from van Fraassen (1980) to Stanford (2000) have been proposing alternative antirealist explanations of the success of science in order to undercut the realist claim that the best explanation of the success of science is that successful theories are typically approximately true and that their key terms typically refer. As far as I know, there are eight such proposals in the literature. Given that the conclusion of the no-miracle argument states that the no-miracle hypothesis yields the best explanation of the success of science, the burden falls on the realists to prove that the realist explanation is better than all those antirealist explanations. At the end of the chapter I construct a pessimistic induction against the antirealist program, thereby drawing some epistemological morals against the strong historical challenge discussed in Chapter 2. I also argue that the

coherence of successful theories can be explained by approximate truth but not by approximate empirical adequacy alone.

In Chapter 4, I reply to the antirealist objections to realism that are based on the antirealist views on the nature of scientific explanation. Specifically, I critically evaluate following objections: 1. It is viciously circular to justify realism with inference to the best explanation. 2. Context-dependence of explanation is a good ground for doubting the truth of explanation. 3. The principle of economy enjoins us to believe that a theory with a high explanatory power is at best empirically adequate. 4. We can explain phenomena without believing that a theory we invoke is true. Discussions on this topic naturally lead us to the realist view of the relation between scientific explanation and philosophical explanation, and to the realist views on the epistemic and social aspects of scientific explanation.

In Chapter 5, I discuss a semantic challenge to realism. Antirealists might claim that the concept of approximate truth is so problematic that it cannot do any explanatory work in the realist explanation of the success of science. I argue that the concept of approximate truth is viable as long as there are clear cases and counter-cases of approximately true descriptions, that Hilpinen/Lewis's theoretical account of approximate truth can handle those clear cases, and that the antirealists are also in need of the concept of approximation. Finally, I address the issue of exactly how approximate truth and success, the explanans and the explanandum of the no-miracle argument, respectively, are related to each other.

In Chapter 6, I argue against the claim that constructive empiricism is better than realism because it can explain science without an extra epistemic risk. I compare the realist position that successful theories are approximately true with the antirealist position that they are empirically adequate. I attempt to prove, contrary to what some antirealists believe, that the antirealists take more epistemic risk than the realists. I critically assess van Fraassen's claim that the realists take more chance of being wrong than the antirealists. Finally, I argue that empirical adequacy is harder to come by than approximate truth in the light of the pessimistic induction and the realist responses to it.

CHAPTER 1

INFERENCE TO THE BEST EXPLANATION

1. Introduction

Inference to the best explanation (IBE, henceforth) is one of the main rules of inference scientists use to prove a theory and to disprove rival theories. In IBE, one infers from observational evidence to the truth of the theory which explains the evidence best.

Constructive empiricists believe that “the rule of IBE is unacceptable in general” (Ladyman et al., 1997, p. 312). If they are right, the scientific explanations of phenomena and the realist explanation of the success of science are not to be trusted, being the products of IBE. They reject IBE for various reasons. In this chapter, I discuss only what Psillos (1996b) calls the argument from a bad lot and the argument from indifference.¹ I aim to accomplish two things: First, I show that the argument from a bad lot and the argument from indifference, if they are good, wreak havoc not only with realism but also with van Fraassen’s various positive philosophical theories, such as the contextual theory of explanation and constructive empiricism. The backfires will put constructive empiricists in an uncomfortable dilemma. Second, I argue in the end that the two arguments are problematic.

2. The Argument from a Bad Lot and the Argument from Indifference

The argument from a bad lot holds that from the fact that a theory explains the evidence best, it does not follow that it is true. The reason is that we “watch no contest of the

theories we have so painfully struggled to formulate, with those no one has proposed” (van Fraassen, 1989, p. 143). It might be that all the rival theories scientists have considered so far belong to a bad lot, and that the best theory out of the bad lot is false. In other words, truth may lie not in the range of theories scientists have considered but in the range of rival theories scientists have not yet entertained. So to believe that the best explanation is true requires an additional premise: Truth is likely to lie in the range of conceived rival theories:

So our selection may well be the best of a bad lot... 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.
(van Fraassen, 1989, p. 143)

Put differently, it has to be proved first that the best of the conceived rival theories is likely to be better than all the rival theories not considered:

So to conclude that the best of these [conceived rival theories] 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.
(Ladyman et al., 1997, p. 306)

Keep in mind that the premise of the argument from a bad lot is that the best of the rival theories actually conceived may be the best of a bad lot, and the conclusion is that an additional premise is required that the truth is likely to lie in the range of conceived rival theories.

Ladyman et al. jointly emphasize that van Fraassen does not have to prove that truth is likely to lie in the range of hitherto unconceived rival theories to make the argument work against realism:

So whereas Psillos challenges van Fraassen to show that it is more likely that the truth is outside of the range, van Fraassen need only ask the proponent of IBE for reasons for believing that the truth is inside it. (Ladyman et al., 1997, p. 306)

All van Fraassen needs to do to refute realism is to ask realists to justify their claim that truth is likely to lie in the range of hitherto conceived rival theories, i.e., the best of those rival theories is likely to be better than all the unconceived rival theories. The argument from a bad lot, as far as I can tell, amounts to a request for the realists to fight ghosts: rival theories not yet conceived.

Similarly, the argument from indifference also amounts to a request to fight the ghosts. Van Fraassen and Ladyman et al. run two different versions of the argument from indifference. The different versions have to be kept apart because different realist responses are required as will be shown at the end of this chapter. Van Fraassen's version holds that the best of the actually entertained rival theories is probably false because there actually are infinitely many unconceived rival theories as good as the best of the conceived rival theories. The rival theories are all in accordance with the evidence so far but make incompatible claims that go beyond the evidence. So only one of them is true. It follows that the best of the conceived rival theories is probably false:

Is the best explanation we have, likely to be true? Here is my argument to the contrary. 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... Hence it seems very improbable to me that it is true. (van Fraassen, 1989, p. 146)

The version by Ladyman et al., on the other hand, holds that our belief in the truth of the best of actual rival theories is unwarranted because it is possible that there are infinitely many equally good unconceived rival theories to the best of the conceived theories:

... the possibility that there may be equally good rival theories to *T* already suffices to make an ampliative step from the evidence to *T* unwarranted. (Ladyman et al., 1997, p. 309)

Notice that Ladyman et al. do not claim that there actually are such rival theories, nor do they claim that the best of the actual rival theories is probably false.

The constructive empiricists' critiques of IBE can be summed up as a challenge to the realists to fight ghosts: hitherto unconceived rival scientific theories. The critiques are built upon the idea that for any scientific theory (it is possible that) there are indefinitely many hitherto unconceived rival theories which are all in accordance with the evidence so far but make incompatible claims that go beyond the evidence. In the next section, I show that the argument from a bad lot and the argument from indifference backfire on antirealists' positive philosophical theories, such as the contextual theory of explanation and constructive empiricism. The antirealists would also have to fight ghosts.

3. Backfire on the Contextual Theory of Explanation

3.1. IBE and the Contextual Theory of Explanation

Van Fraassen uses IBE to argue for the contextual theory of explanation. He claims that a “successful” and “correct” theory of explanation must explain the phenomena of rejections and asymmetries:

To be successful, a theory of explanation must accommodate, and account for, both rejections and asymmetries. I shall now examine some attempts to come to terms with these, and gather from them the clues to the correct account. (1980, p. 112)

He critically examines one by one the theories of explanation proposed by Hempel, Salmon, and Friedman. He accuses the three rival theories of, among other things, not being capable of handling the phenomena of rejections and asymmetries (ibid., p. 111). Let me briefly explain the problem of rejections and the problem of asymmetries.

The problem of rejections holds that “there are cases, clearly in a theory’s domain, where the request for explanation is nevertheless rejected” (ibid., p. 111). The famous example illustrating the problem of rejection involves paresis. Paresis is an illness one contracts only when latent syphilis is left untreated, and the probability that untreated syphilis develops into paresis is extremely low. Suppose that John had untreated syphilis and his brothers did not. Current medical science accepts the request to explain why John, rather than his brothers, contracted paresis. But it, having no answer, rejects the request to explain why he as opposed to other syphilitics got paresis. A “successful” and “correct” theory of explanation must account for why such a request is rejected.

The problem of asymmetry, on the other hand, holds that it is legitimate to explain one event in terms of a second event, but not vice versa, although an entailment relation holds in either direction. The famous example illustrating the problem of asymmetry involves

the flagpole. It is legitimate to explain the length of the shadow in terms of the length of the flagpole along with auxiliary assumptions and statements of initial conditions, but not vice versa, although the length of the flagpole is derivable from the length of the shadow along with the auxiliary assumptions and the statements of initial conditions.

After accusing the theories of Hempel, Salmon, and Friedman of not being capable of handling the phenomena of rejections and asymmetries, van Fraassen formulates his own theory of explanation. His theory holds, roughly, that an explanation is an answer to a why-question, and that the appropriateness of the answer depends on context: interests and contrast-class. The theory was suggested to van Fraassen, among other things, by Norwood Hanson's accident example (ibid., p. 125) and Bengt Hansson's apple example (ibid., p. 127). Let me briefly explain the two examples. The accident example involves the question: Why did the driver die in the traffic accident yesterday? A medical doctor, interested in the medical condition of the driver, would cite massive blood loss as the cause of death. A mechanic, interested in the mechanical condition of the car, would pick out a defect in the brake system as the cause of death. In some contexts, the medical explanation is appropriate, and in others, the mechanical explanation is appropriate. From this example, van Fraassen concludes that the interests of the discussants determine which of the alternative explanatory hypotheses is to be chosen. The second example, which provided a crucial clue to van Fraassen, involves the question: Why did Adam eat the apple? If the question were understood as asking why it was Adam, as opposed to Tom or John, who ate the apple, it would be appropriate to say that Adam ate the apple because, say, Eve gave the apple to Adam rather than to Tom or John. In this case, Tom

and John constitute the contrast-class. But if the question were construed as asking why it was the apple, as opposed to the orange or the grape, that Adam ate, it would be appropriate to say that Adam ate the apple rather than the orange or the grape because, say, he prefers apples to oranges and grapes. In this case, the orange or the grape constitute the contrast-class. From this example, van Fraassen concludes that the contrast-class is a factor that determines which of the alternative answers to a why-question is to be chosen.

After formulating the contextual theory of explanation, van Fraassen applies it to the phenomena of rejections and asymmetries. Recall that current medical science accepts the request to explain why John, rather than his brothers who did not have untreated syphilis, contracted paresis. But it, having no answer, rejects a request to explain why he as opposed to other syphilitics got paresis. Why is it that in the first case the request for an explanation is accepted and in the second case rejected? According to the contextual theory of explanation, the request is accepted in the first case because the contrast-class is John's brothers who did not have latent untreated syphilis. The request is rejected in the second case because the contrast-class is the people who had syphilis and current medical science has no answer to the why-question. In short, depending on what the contrast-class is, the same question, "Why does John have paresis?," is accepted or rejected (*ibid.*, p. 128).

After successfully explaining the phenomenon of rejections, van Fraassen predicts that his theory of explanation should also explain the phenomenon of asymmetries. He takes the prediction to be a "crucial test":

In addition, it should then also be possible to account for specific asymmetries in terms of the interests of questioner and audience that determine this relevance. These considerations provide a crucial test for the account of explanation which I propose. (ibid., pp. 130-131)

Recall that it is illegitimate to explain the length of a flagpole in terms of the length of its shadow along with the auxiliary assumptions and the statement of initial conditions, although the length of the flagpole is derivable from the length of the shadow and the auxiliary assumptions and the statement of initial conditions. What van Fraassen predicts in the quoted passage is that if his theory of explanation is true, it should be legitimate to explain the length of the flagpole in terms of the length of the shadow *in some contexts*. And he makes up a context where it makes sense to explain the length of a tower in terms of the length of its shadow (ibid, pp. 132-134). Briefly, the story goes as follows: A man killed a woman, whom he had loved to the point of madness, for having an affair with another man. He built a tower on the spot where he killed her and made the height of the tower such that the shadow of the tower reaches the terrace, where he first proclaimed his love, at every sunset.

Let me summarize how van Fraassen argues for his theory of explanation. First, he criticizes Hempel's, Salmon's, and Friedman's theories for not being capable of explaining the phenomena of rejections and asymmetries. Second, he formulates his own theory from the accident example and the apple example. Third, he tries to show that his theory explains the phenomenon of rejections. Fourth, he predicts that his theory should account for the phenomenon of asymmetries, and tries to show that the prediction is true. Finally, he claims that his theory of explanation is "successful" and "correct" (ibid., p.

112). I take it that van Fraassen's word "successful" means that the theory is capable of handling examples that are found in the phenomena of explanation like rejections and asymmetries, and that "correct" means that the theory is true. In sum, he claims that his theory of explanation is true on the ground that it has a higher explanatory power than the rival theories. It looks like he is using IBE, although he does not claim explicitly that his theory is the best.

Given that van Fraassen uses IBE to argue for the contextual theory of explanation, the argument from a bad lot and the argument from indifference backfire on the contextual theory of explanation.² I will not spell out how exactly the two argument could take the contextual theory of explanation down. But the conclusion is that the contextual theory of explanation is probably false, unacceptable, unwarranted, and not rationally compelling. Van Fraassen also has to fight ghosts: unconceived rival philosophical theories!

3.2. Implication of the Collapse

Let me explore one implication of the collapse of the contextual theory of explanation for realism-antirealism debate. Realists and antirealists alike seem to agree in the literature that the contextual theory of explanation is true, or at least seems rationally compelling. They also believe that the contextual theory of explanation, which claims that the appropriateness of an explanation depends on context, undercuts realism. For instance, Leplin, believing that the contextual theory of explanation is an obstacle on the way to

realism, claims that it is conceivable that the general theory of relativity explains gravity independently of context:

Surviving such possibilities is a straightforward sense in which general relativity explains gravity. This sense does not seem to be contextual or interest-relative. (Leplin, 1997, p. 11)

Ladyman replies that Leplin merely states an opinion without an argument:

...he appeals to the general relativistic explanation of gravity to refute a context-relative model of explanation, but just asserts what the proponent of the latter will deny, namely that general relativity really does explain gravity in any context independent sense (p. 11). (Ladyman, 1999, p. 185)

Notice that both Leplin and Ladyman presuppose that the context-dependence of explanation undermines realism. Both of them are reminded, however, that the contextual theory of explanation is probably false, unacceptable, unwarranted, and not rationally compelling. Why fuss about a theory of such nature? If they were rational cognitive agents, they would be entitled to deny that the appropriateness of an explanation depends on context.

3.3. Objections and Replies

3.3.1. Science vs. Philosophy

Constructive empiricists might retort that the argument from a bad lot and the argument from indifference work effectively against scientific theories, but not against philosophical theories, because scientific theories are connected with observations, but philosophical theories are not. A scientific theory meets the tribunal of *observations*

while a philosophical theory meets the tribunal of *examples*. So the two arguments do not backfire on the contextual theory of explanation.

The problem with this possible move is that the realists and the antirealists alike agree that scientists and philosophers use the same kind of rule of inference, IBE, and that a philosophical use of IBE is in the same boat as a scientific use of IBE. Van Fraassen, for instance, explicitly claims that both scientists and philosophers use IBE to arrive at their theories:

The inference from the phenomena that puzzle us, to their best explanation, appears to have our instinctive assent. We see putative examples of it, in science and philosophy no less than in ordinary life and in literature. (1989, p. 131)

Other philosophers are with van Fraassen on this account. Okasha and Glymour, for instance, note that IBE is prevalent in both science and philosophy:

According to its proponents, IBE is a paradigmatic, perhaps even the paradigmatic, form of non-demonstrative inference, widely used in science, everyday life, and in philosophy itself. Glymour describes IBE as a pattern of argument that 'is not bounded by time or subject matter...' (Glymour, 1984, p. 173). (Okasha, 2000, pp. 691-692)

Laudan, himself, and the realists he cites in the following passage, also agree that there is no methodological difference between science and philosophy because both philosophical and scientific theories are subject to empirical tests:

A growing number of philosophers (including Boyd, Newton-Smith, Shimony, Putnam, Friedman and Niiniluoto) have argued that the theses of epistemic realism are open to empirical test. The suggestion that epistemological doctrines have much the same empirical status as the sciences is a welcome one. (Laudan, 1981, p. 19)

Laudan and Fine make a further claim: The philosophical use of IBE is in the same boat as the scientific use of IBE. So, if the scientific use of IBE is problematic, so is the philosophical use of IBE. Laudan claims against Putnam's no-miracle argument that "this is a monumental case of begging the question" (1981, p. 45). Fine also claims that Putnam's no-miracle argument is "a paradigm case of begging the question" (1991, p.82). The problem of circularity would not even arise, if the philosophical use of IBE and the scientific use of IBE were in different boats. In short, if the above philosophers are right in asserting that there is no methodological difference between philosophy and science, the constructive empiricists' critiques of IBE inevitably backfire on the contextual theory of explanation.

Moreover, to successfully avoid the backfire requires that van Fraassen engage himself in the details of the argument from a bad lot and the argument from indifference as directed against the contextual theory of explanation. He needs to pinpoint exactly what is wrong with the following argument: (It is possible that) there are infinitely many unconceived rival theories of explanation. Such rival theories are all in accordance with the phenomena of explanations but incompatible with each other and with the contextual theory of explanation. So van Fraassen needs to support the additional premise that the true theory of explanation is likely to lie in the range of hitherto conceived rival theories. Also, our belief in the truth of the contextual theory of explanation is probably false and unwarranted.

3.3.2. Empirically Adequate?

Constructive empiricists might argue that the target of their critiques of IBE is the inference from success to the truth of the best explanation, and that the inference from success to the empirical adequacy of the best explanation is left untouched. Their critiques, they might continue, do not pose a threat to the claim that the contextual theory of explanation is empirically adequate.

But what does ‘empirical adequacy’ mean? What is the nature of the relation between success and empirical adequacy? Van Fraassen defines empirical adequacy as follows:

...a theory is empirically adequate exactly if what it says about the observable things and events in this world, is true – exactly if it ‘saves the phenomena.’
(1980, p. 12)

Notice that *all* the observational consequences of an empirically adequate theory are true, i.e., an empirically adequate theory saves all of its phenomena, past, present, and future.

On the other hand, to say that a theory is successful is to say that it has passed severe empirical tests, which entails that *some* observational consequences are true. So the inference from success to empirical adequacy is an inference from “some” to “all.”

Now, it seems to me that the constructive empiricists’ critiques of IBE also block any inference from “some” to “all.” Consider the following instance of such an inference. We conclude that all crows are black from the premise that all the crows we have observed so far are black. Now, it is possible that there are infinite number of generalizations that are compatible with the premise that some crows are black but incompatible with the

conclusion that all crows are black. Let me offer just two examples of such rival generalizations:

Conceived Rival 1: All crows are black except some blue crows we will observe in the future.

Conceived Rival 2: All crows are black except some pink crows that existed before the beginning of the human history.

For any finite set of data, (it is possible that) there are an infinite number of generalizations that are compatible with the set of finite data but incompatible with each other. So any inference from “some” to “all” is unwarranted, and any claim to “all” is probably false. Let me call “unobserved anomalies” the pink crows and blue crows that figure in Conceived Rival 1 and Conceived Rival 2 above. Unobserved anomalies are counterexamples that we have not yet actually observed. They block the antirealist inference from success to empirical adequacy, if unconceived rival theories block the realist inference from success to truth. They are the kind of ghosts the antirealists would have to fight, if unconceived rival theories are the kind of ghosts the realists would have to fight.

Unobserved anomalies also block inference from success to approximate empirical adequacy. But what does ‘approximate empirical adequacy’ mean? An approximately empirically adequate theory is more than merely successful but less than empirically adequate in a definite sense. The set of its true observational consequences is larger than

that of a merely successful theory, but smaller than that of an empirically adequate theory. So I define approximate empirical adequacy as follows: A theory is approximately empirically adequate just in case *most* of its observational consequences are true. So the inference from success to approximate empirical adequacy is an inference from “some” to “most.” The important point for my purpose here is that it is possible that *most* crows are blue, even if all the crows we have observed so far are black. So unobserved anomalies block the inference from success even to approximate empirical adequacy.

The concept of unobserved anomalies suggests that an antirealist alternative to constructive empiricism can be easily constructed. The central tenets of constructive empiricism are the following two claims:

- (1) Science aims to give us theories which are empirically adequate.
- (2) Acceptance of a theory involves as belief only that it is empirically adequate.

Now, consider the following antirealist alternative to constructive empiricism:

- 1* Science aims to give us theories which are *approximately* empirically adequate.
- 2* Acceptance of a theory involves as belief only that it is *approximately* empirically adequate.

Furthermore, it is possible that there are an infinite number of unconceived instrumental concepts that fall in between success and approximate empirical adequacy. And all those

concepts can be put in the place of 'approximately empirically adequate' which figures in the formulation of the antirealist alternative to constructive empiricism, thereby producing an infinite number of alternatives to constructive empiricism. Let me call the antirealist alternatives "mutated constructive empiricisms." Notice that mutated constructive empiricisms take less epistemic risks than constructive empiricism.

From the fact that mutated constructive empiricism takes less epistemic risk than does constructive empiricism, mutated constructive empiricists could make critical points against van Fraassen as van Fraassen does against the realists. For instance, van Fraassen is widely known to have claimed against the realists that "it is not an epistemological principle that one might as well hang for a sheep as for a lamb" (1980, p. 72). The idea is that some risk is involved in the inference from success to empirical adequacy. But it does not follow from this that one has to stick one's neck out all the way up to truth. The mutated constructive empiricists could also claim against van Fraassen that it is not an epistemological principle that one might as well hang for empirical adequacy as for approximate empirical adequacy. Van Fraassen is also widely known to have claimed against the realists that constructive empiricism is better than realism because "it makes better sense of science, and of scientific activity, than realism does and does so without inflationary metaphysics" (1980, p. 73). The mutated constructive empiricists also make the same point against van Fraassen, viz., mutated constructive empiricisms are better than constructive empiricism because it explains science without inflationary epistemic risk.

It might be argued that van Fraassen proposes constructive empiricism not for the sake of defending it but solely for the sake of refuting realism, and that he achieved the aim by simply proposing an alternative to realism. The mutated constructive empiricists, however, can say the same thing against constructive empiricism, i.e., that they are proposing the mutated versions solely for the sake of refuting constructive empiricism. Secondly, it does not matter whether van Fraassen's aim was to defend constructive empiricism or to refute realism. What is important is that mutated constructive empiricisms undercut constructive empiricism, if constructive empiricism undercuts realism.

4. Dilemma for Constructive Empiricists

If unconceived rival scientific theories undermine scientific theories, unconceived rival philosophical theories undermine van Fraassen's positive philosophical theories, and unobserved anomalies undermine the inference from success to (approximate) empirical adequacy. We are left with an extreme cognitive famine. Interestingly, the constructive empiricists resist skepticism:

Skepticism is an ugly threat; a philosophical position which leads to skepticism reduces itself to absurdity. (Ladyman et al., 1997, p. 317)

It is not clear, though, how they could avoid the absurdity, when they take the argument from a bad lot and the argument from indifference seriously and vigorously wield them against realism. Here is a dilemma for constructive empiricists: If they adhere to the two

arguments, everything goes down, and skepticism is in the offing. But if they give up the two arguments to avoid skepticism, they could no longer wield them against realism.

5. Critiques of the Critiques of IBE

So far I assumed for the sake of argument that the argument from a bad lot and the argument from indifference are good. From now on I will criticize them. Constructive empiricists might think that my criticisms are utter failures. If so, the backfires on their positive theories stand, and they need to tell us how they could avoid skepticism.

5.1. Criticism against the Argument from a Bad Lot

Let me summarize the argument from a bad lot in a standard form so that we can carefully evaluate the premise and the relation between the premise and the conclusion:

Premise: The best of conceived rival theories may be the best of a bad lot.

Conclusion: An additional premise is required that the truth is likely to lie in the range of conceived rival theories.

Obviously, the premise is true, and the realists are happy to accept it. But notice that no reason is provided to seriously worry about the possibility that the best of the conceived rival theories is the best of a bad lot. So we do not know whether the possibility is no different in kind from the possibility that we are all pink crows dreaming that we are human beings.³ Now, does the conclusion follow from the premise? The conclusion goes

beyond what the premise says. So the argument is not deductively valid. Then, the only question we need to press is this: Does the conclusion probably follow from the premise? For now, I do not see why we should think that the conclusion probably follows from the premise. Nowhere does van Fraassen provide reason to think that the conclusion is likely to be true in the light of the premise. Given that no such reason is provided, it is not clear that the realists should prove first that the truth is likely to lie in the range of the conceived rival theories when they believe that the best of the conceived rival theories is true.

The antirealists might retort that the conclusion is likely to be true, once we supply an additional premise to the argument that the truth frequently fell in the range of the unconceived rival theories in the history of science. That is, our best past theories like the Ptolemaic theory have routinely been in the bad lot. The historical fact compels us to conclude inductively that our best current theories are also in the bad lot. So our belief in the truth of current theories is unwarranted, unless we support the additional premise first that the truth is likely to lie in the range of conceived rival theories.⁴ This line of the defense of the argument from a bad lot seems to be productive and interesting. But the problem the defense raises is so serious that it deserves a separate chapter. I discuss the historical challenges to realism in the following chapters.

5.2. Criticisms against the Argument from Indifference

5.2.1. Criticism against van Fraassen's Argument from Indifference

Recall that van Fraassen's version of the argument from indifference holds that the best of conceived rival theories is probably false because there actually are an infinite number of equally good unconceived rival theories to the best of the conceived rival theories.

Psillos and Okasha reply that van Fraassen needs to offer an argument rather than merely state an assertion that there actually are such rival theories:

In order to assert this, one must show that *there always are* other potentially explanatory hypotheses to be discovered, let alone that they explain the evidence at least as well. (Psillos, 1999, p. 223)

Is it really true that for every theory, in every area of science, there exist many other, perhaps unformulated theories which explain the relevant phenomena just as well as the theory in question? (Okasha, 2000, p. 698)

Psillos and Okasha are certainly right to ask for support of the unsupported assertion. I must add that in the absence of an argument the principle of economy dictates that we not believe that there actually are unconceived rival theories as good as the best of the conceived rival theories.

Suppose for the sake of argument that there actually are infinitely many unconceived rival theories. Even so, Psillos argues, van Fraassen's argument from indifference is problematic because we do not know that the unconceived rival theories are equiprobable or equiexplanatory with the best of conceived rival theories:

Relatedly, even if we granted that there always are unborn/potential explanations of *e*, what is to say that as explanations of the evidence they are as good as the one offered by *T*? (Psillos, 1999, p. 223)

Psillos's criticism, however, is problematic. Van Fraassen can recast the argument from indifference while leaving the essential point untouched. The new version claims that the best of conceived rival theories is probably false because the unconceived rival theories jointly take up a significant portion of the probability space, say, 95%. The new version is neutral as to whether or not the unconceived rival theories are equiprobable with the best of the conceived rival theories. Suppose, for instance, that there are 95 unconceived rival theories and the probability of each is 1%, and that the probability of the best of the conceived rival theories is 5%. Or suppose that the probability of one of the unconceived rival theories is 95%. In both cases, the unconceived rival theories are not equiprobable with the best of the conceived rival theories. Yet, the best of the conceived rival theories is probably false. The point is that van Fraassen's conclusion that the best of conceived rival theories is probably false does not have to be saddled with the assumption that the unconceived rival theories are equiprobable with the best of conceived rival theories. It could well rest on the assumption that unconceived rival theories jointly take up a significant portion of the probability space (Richard Healey, personal communication).

This possible move, however, does not work. Psillos's original question can also be recast while the essential point is left untouched. What is to say that the unconceived rival theories actually take up a significant portion of the probability space? Consider that the unconceived rival theories may jointly take up an insignificant portion of the probability space, say, 0% or 20%, even if the number of the unconceived rival theories is infinite. It might be the case that the probability of each unconceived rival theory is zero. Or it might be the case that the probability of each unconceived rival theory is zero, except one

whose probability is 20%. From the mere fact that the unconceived rival theories are infinitely many, it does not follow that they take up a significant portion of the probability space. So it does not follow that the best of the conceived rival theories is probably false. In other words, the conclusion that the best of the conceived rival theories is probably false requires a further premise, which is (1), not (2):

- (1) The unconceived rival theories *actually* take up a significant portion of the probability space.
- (2) The unconceived rival theories *possibly* take up a significant portion of the probability space.

(1) proves that the best of the conceived rival theories is probably false. But it is hard to prove that (1) is true. (2) is obviously true. But it does not prove that the best of the conceived rival theories is probably false.

5.2.2. Criticism against the Argument from Indifference by Ladyman et al.

Recall that Ladyman et al.'s version of the argument from indifference holds that our belief in the truth of the best of the conceived rival theories is *unwarranted* because it is *possible* that there are infinitely many unconceived rival theories as good as the best of the conceived rival theories. Suppose that we have *good inductive* reason, although it is not clear how we could have such reason, for thinking that there are no such rival theories. Would Ladyman et al. regard our belief in the truth of the best of the conceived

rival theories as warranted? The answer is no, because even if we have such reason it is still *possible* that there are infinitely many unconceived rival theories as good as the best of the conceived rival theories. So Ladyman et al.'s version of the argument from indifference amounts to a request for the realists to *deductively* prove that there are no unconceived rival theories as good as the best of the conceived rival theories. The request, however, is absurd. The best we can hope for, when it comes to matters of fact, is an inductive proof. Along with this line of reasoning, contemporary epistemologists across the three different schools of thought on epistemic justification reject that a justified belief requires a deductive proof. That is, foundationalism, coherentism, and reliabilism all agree that a justified belief is fallible. i.e., it is possible that a warranted belief is false.⁵

It is clear that Ladyman et al. do not have in mind any of the three theories of epistemic justification when they claim that our belief in the truth of the best of the conceived rival theories is unwarranted. Hence, they need to tell us what theory of epistemic justification they have in mind. Suppose that they come up with a new theory of epistemic justification. In that case, the realists could run the argument from indifference against it. That is, the constructive empiricists' theory of warrant is unwarranted because it is possible that there are infinitely many unconceived rival theories of warrant as good as their theory of warrant. In short, Ladyman et al.'s version of argument from indifference is self-defeating.⁶

6. Conclusion

The constructive empiricists' critiques of IBE are requests for realists to fight ghosts: hitherto unconceived rival scientific theories. The ghosts are chased away. They may come back. But when they do, they will be with their friends specializing in antirealists.

End Note

¹ See Okasha (2000) for a critique of van Fraassen's Dutch Book argument against IBE.

² Interested readers might run the two arguments against van Fraassen's evolutionary explanation of the success of science (1980, pp. 39-40), and the English model of rationality he favors over the Prussian model of rationality (1989, pp.171-172).

³ Shaughan Lavine (personal communication) suggests that the absence of a satisfactory grand theory to unify the *general theory of relativity and quantum mechanics* is a symptom that the two theories are in a bad lot. It must be admitted that they are in a bad lot in the sense that they are not completely true. But the realist intuition remains intact that they are successful and approximately true, i.e., the lot they are in is not seriously bad.

⁴ Stanford (forthcoming) raises this sort of objection against realism. He argues that our best past theories have been routinely replaced by the then unconceived rival theories in the history of science. So our best current theories will be replaced in the future by hitherto unconceived rival theories. For instance, the Ptolemaic theory was replaced by the Copernican theory which was unconceived when the scientific community subscribed to the Ptolemaic theory. So the Copernican theory will be replaced in the future by a hitherto unconceived rival theory.

⁵ Let me rehearse briefly what the three schools would have to say about epistemic justification with a view to showing that a justified belief requires an inductive proof. Foundationalism distinguishes between basic beliefs and nonbasic beliefs. Different foundationalists give different accounts of the nature of the basic beliefs and of the nature of the inferential relation between basic beliefs and nonbasic beliefs (Steup, 1996, Chapter 5). But contemporary foundationalists all agree that a nonbasic belief is justified just in case it inductively follows from basic beliefs, i.e., a nonbasic belief receives its justification from basic beliefs via induction. Like contemporary foundationalists, coherentists do not require that a justified belief be deductively proved to be true. Coherentism maintains that a belief is justified just in case it coheres with other beliefs. For BonJour (1985, p. 96), a belief coheres with another belief just in case an inference from the belief to the other belief is to some degree truth-preserving. Reliabilists do not require a deductive proof either. They assert that a belief is justified just in case it is produced by a reliable cognitive process, and the reliability is cashed out in terms of high frequency of true beliefs. So none of the three schools of thought on epistemic justification requires a deductive proof for a belief to be justified.

⁶ The argument I am running against the skepticism by Ladyman et al. parallels to the one Long (1992) runs against Cartesian skepticism. Long claims that Cartesian skepticism is self-defeating. The gist of his argument goes as follows: When conscious mental events occur, they may occur to an agent or they may just occur without an owner. They can be mere happenings independent of any conscious mind. So from the fact that the conscious mental events occur, it does not necessarily follow that there exists an agent who has them. Now, Cartesian skepticism presupposes that there exists an agent who possesses them. After all, if there is no such agent, the evil demon can deceive nobody. But no justification is provided for the presupposition that conscious mental states occur to a mind.

CHAPTER 2

HISTORICAL CHALLENGES TO REALISM

1. Introduction

Historical objection to realism, so far as I can tell, comes in two forms. I call the first form “the strong historical challenge” and the second form “the weak historical challenge.” The strong historical challenge, which is known as pessimistic induction or disastrous induction, claims that successful current theories, as successful past theories did in the past, will turn out to be completely false, or that they will be replaced by new theories in the future. The target of the challenge is the current theories. The weak historical challenge, on the other hand, claims that there are historical counterexamples that undercut the realist claim that successful theories are approximately true. The target of the challenge is the realist inference from success to approximate truth, and it is silent about the status of the current theories.

The literature on this issue, so far as I know, does not distinguish between the strong and the weak historical challenges. But they need to be kept apart because different realist responses are in order. In order to counter the strong historical challenge, I present various historical reasons to think that the pessimistic force that arises from past theories can be successfully diffused. In order to meet the weak historical challenge, I reformulate realism, providing criteria to pick out only our best theories. The new version holds that successful theories that cohere with each other are approximately true and that their key terms refer. Finally, I compare my approach with Kitcher’s approach to pessimistic

induction, and argue that my approach points to the direction in which the problem plaguing Kitcher's approach could be solved.

2. Strong Historical Challenge

2.1. Basic Claim

The strong historical challenge, which a pessimistic inducer might advance against successful current theories, is summarized in the following main argument and the subsidiary argument:

The Main Argument

Premise: Successful *past* theories are not even approximately true and their central terms do not refer in the light of present theories.

Conclusion: Successful *current* theories are not even approximately true and their central terms do not refer in the light of *future* theories.

The Subsidiary Argument

Premise 1: The central terms of successful *past* theories do not refer in the light of the present theories.

Premise 2: Theories whose central terms do not refer cannot possibly be even approximately true.

Conclusion: Successful *past* theories are not even approximately true in the light of present theories.

Two things are worth noting. First, the first premise of the subsidiary argument does most of the work in the pessimistic induction. It is the source of the pessimistic conclusion against successful current theories. Put differently, if the first premise collapses, the force of the pessimistic induction evaporates. Second, the pessimistic inducer could claim that it inductively follows from the conclusion of the main argument above that even successful *future* theories are not even approximately true in the light of *remote future* theories. That is, we will continually judge that successful theories are not even approximately true till the end of science.¹ It is not just that we will continually judge that successful theories are completely false. Our judgments are based on theories that are fated to be judged to be completely false later, and we are doomed to make such judgments *indefinitely*. As we move on in the development of science, the data for the pessimistic induction accumulate, hence the pessimistic force gets bigger and bigger. Our future generations will be more inductively compelled to judge that successful theories are completely false than we are. There is no hope at all that someday human beings will finally reach the destination where true theories and approximately true theories will emerge. This is an extremely gloomy picture of our ability to know about how things really are.

2.2. Historical Replies

2.2.1. Some or All?

I earlier claimed that the first premise of the subsidiary argument does most of the work in the pessimistic induction. How would the pessimistic inducer justify this provocative historical claim? He would come up with a list of past theories whose central terms presumably do not refer in the light of current theories. In fact, Laudan, an antirealist philosopher who is widely believed to have run the pessimistic induction against successful current theories, provides a list of successful past theories, and claims that the list can be extended “*ad nauseam*.” Let me quote the list because it gives us a hint at how to diffuse the pessimistic force:

Laudan’s List

- . the crystalline spheres of ancient and medieval astronomy;
- . the humoral theory of medicine;
- . the effluvial theory of static electricity;
- . “catastrophist” geology, with its commitment to a universal (Noachian) deluge;
- . the phlogiston theory of chemistry;
- . the caloric theory of heat
- . the vibratory theory of heat;
- . the vital force theories of physiology;
- . the electromagnetic ether;
- . the optical ether

- . the theory of circular inertia; and
- . theories of spontaneous generation. (Laudan, 1981, p. 33) ²

Notice that all the theories on the list were developed before the 20th century. Now, let's ask this question: Does the list show that the central terms of *all* the successful past theories do not refer in the light of the current theories? They show at best, in my view, that the central terms of *some* successful past theories do not refer in the light of present theories. The difference between "some" and "all" is important. If only *some* successful past theories are not even approximately true in the light of current theories, then the realists could diffuse the disastrous implication the pessimistic induction is purported to have on successful current theories by constructing what I call an optimistic induction. Suppose that the pessimistic inducer constructs a pessimistic induction with one hundred examples of successful past theories which are not even approximately true in the light of current theories. In response, the realists could construct an optimistic induction with the equal number of successful past theories which are approximately true in the light of current theories. The pessimistic force and the optimistic force cancel out each other. Thus, the pessimistic inference against the successful current theories is completely blocked. We are now entitled to doubt that the successful current theories are likely to be not even approximately true. Notice that such a response of constructing an optimistic induction is not available to the realists, if it is true that *all* the successful past theories are not even approximately true in the light of current theories.

Now, it is not a hard task for the realists to construct the optimistic induction that would defeat the pessimistic induction. For any replaced theory on the list, there is a corresponding replacing theory whose central terms refer in the light of current theories. For example, for the Ptolemaic theory, there is the Copernican theory, for the phlogiston theory, the oxygen theory, for the caloric theory, the kinetic theory, and so on. The key terms of those replacing theories refer in the light of current theories. The replacing theories are clearly *past* theories, although their variations might be considered as current theories. The ground for regarding them as past theories is that there were periods when the replacing theories were competing against the replaced theories. So if the replaced theories are considered as past theories, so should be the replacing theories. If the caloric theory is past, for instance, so is the kinetic theory.

I must make one more point about the history of science. In his 1981 paper, Laudan claims that the central terms of *most* successful theories do not refer in the light of current theories:

I daresay that for every highly successful theory in the past of science that we now believe to be a genuinely referring theory, one could find half a dozen once successful theories that we now regard as substantially non-referring. (1981., p. 35)

But in his 1983 paper, he takes a substantially mitigated attitude toward the history of science in his critical reply to Hardin and Rosenberg (1982). Instead of claiming that *most* successful past theories are not even approximate true, he claims that *many* successful past theories are not even approximately true:

Yet we now believe that many of those earlier theories profoundly

mischaracterize the way the world really is. (1984b, p. 157)

If this new historical claim is true, we are not inductively compelled to believe that successful current theories are not even approximately true.

2.2.2. Theoretical Terms of Successful Past Theories

We have reasons to think that the central terms of most successful past theories refer in the light of present theories. Consider the fact that the major theoretical terms of successful theories of the 20th century refer in the light of present theories. Those terms include all the theoretical terms in the periodic table, 'X-ray,' 'electron,' 'proton,' 'neutron,' 'neutrino,' 'Big Bang,' 'black hole,' and 'DNA.' The list of theoretical terms of the 20th century which are still considered to refer can be extended *ad nauseam*, given that there are tons of theoretical terms in any introductory science text published since the 1990s. So the key terms of most successful theories of the 20th century refer in the light of current theories. If key terms of most successful theories of *the 20th century* refer, then the central terms of most of the successful *past* theories refer, even if we grant for the sake of argument that key terms of *all* the successful theories before the 20th century do not refer. The reason is that the body of scientific knowledge exploded in the 20th century with far more human and technological resources. Some historians of science put the explosion of scientific knowledge in the 20th century as follows:

At the end of the 20th century, we've now immersed ourselves in what Newton called the vast sea of knowledge. We've felt and tasted the water. We've washed it over ourselves – and what we've learned would astound him and the other great and imaginative minds of generations past. (Brody et al., 1997, p. 339)

Now, suppose that we put together the small number of nonreferring theoretical terms of before the 20th century and the large number of referring theoretical terms from the 20th century. *Most* members of the resulting population would refer just like most theoretical terms of the 20th century refer. Here is an analogy to illustrate the point. Put together a cup of salty seawater and tons of freshwater. The resulting water would be fresh just like the original freshwater.

In response, the pessimistic inducer might argue that successful theories of the 20th century should be considered to be *current* theories. He might draw a line to the effect that the year 1900 demarcates the line between past science and current science. Based on the demarcation, the pessimistic inducer might construct a pessimistic induction that key terms of most successful theories before the year 1900 do not refer, hence key terms of most successful theories after the year 1900 do not refer as a matter of inductive logic. But I see a problem with this possible move by the pessimistic inducer. The proposed demarcating line between past science and current science is arbitrary. I do not see how the pessimistic inducer could transform the arbitrary line into a principled line. Even if he miraculously justifies the demarcating line, there still is a problem. If there is such a principled distinction between past science and current science, then from the fact that past science performed poorly, it does not follow that current science will do the same. The realists might use the pessimistic inducer's justification for the demarcating line to protect current theories from the bad record of the past theories. In any case, it would be extremely hard for the pessimistic inducer to draw a non-arbitrary line between past

science and current science without ending up protecting current theories, thereby helping realism.

The predicates ‘past theory’ and ‘present theory’ are such a troubling spot in the pessimistic induction that the pessimistic inducer might replace them with ‘once accepted but no longer accepted theory’ and ‘currently accepted theory’ respectively. ‘Current’ does not refer to an absolute point of time. It refers to the year in which we run the pessimistic induction. It might be 1920 or 1980. Now, as long as there are enough theories that were once accepted but no longer accepted at any point of time in the development of science, we can construct a strong pessimistic induction that all the currently accepted theories will turn out to be completely false as all once accepted but no longer accepted theories did. The problem with the suggestion is that for any point of time in the development of science there are theories that once were accepted and continue to be accepted. The special theory of relativity belongs to this category in the year 2001. It once was accepted and still is accepted. We can construct an optimistic induction from such theories that once accepted theories will continue to be accepted. In short, the optimistic induction still neutralizes the force of the pessimistic induction, even if the problematic predicates, ‘past theory’ and ‘present theory,’ are replaced with new predicates, ‘once accepted but no longer accepted theory’ and ‘currently accepted theory’ respectively.

What is Laudan’s definition of past theory? He does not have any. He just uses synonyms: “past theory” (1981, p. 26), “theory in the past of science” (ibid., p. 35), and “earlier theories” (1983, p. 157). But it is clear that he does not equate a past theory with

a theory that once was accepted but no longer is accepted. It is not the case that for Laudan only replaced theories are considered as past. After all, he (1981) claims that key terms of most successful past theories do not refer in the light of current theories, which allows that key terms of *some* successful past theories refer in the light of current theories. The successful past theories whose central terms refer in the light current theories might include the Copernican theory, the oxygen theory, and the kinetic theory. They were all developed before the year 1900.

The pessimistic induction contains problematic predicates. But the realists should not dismiss the pessimistic induction. After all, as Laudan (1981) notes, the realists have not rigorously defined any of the realist key predicates: ‘success,’ ‘approximate truth,’ and ‘mature science.’

2.2.3. Extra Success and Extra Neighbor Constraint

In the previous two sections, I have argued that Laudan’s infamous list shows at best that some successful past theories are not even approximately true and that their central terms do not refer, and that the key terms of most successful past theories refer in the light of current theories due to the fact that the key terms of most successful theories of the 20th century refer. In this section, I provide two reasons to think that successful theories of the 20th century and onward are not in the same boat with the successful theories from before the 20th century on Laudan’s list, and I draw an epistemological moral.

Theories of the 20th century and onward are different from the theories from before the 20th century in the following two respects. First, the theories of the 20th century and

onward are far more successful than the theories from before the 20th century. It could be shown that for every successful theory on Laudan's list, there is a corresponding theory we now hold to be approximately true, and that the theory we now hold to be true is far more successful than its counterpart on Laudan's list. For example, the theory of plate tectonics is far more successful than catastrophist geology, and the kinetic theory of heat than the caloric theory of heat, the oxygen theory than the phlogiston theory, and so on. Some historians put the difference between the success of science before the 20th century and the success of science of the 20th century science, taking medical science as an example, as follows:

We entered the 20th century riding horses. We will leave it riding spaceships. We entered the century dying of typhoid and smallpox, and will leave it having conquered those diseases. At the turn of the 19th century, organ transplants were unthinkable, while by the turn of this century many will have survived because another person's heart or other vital donated organs sustains them. In 1900 the human life span was 47 years. Today it is 75. (Brody et al., 1997, p. 337)

Second, the theories of the 20th century and onward have better "birth qualities" than the theories from before the 20th century. The better birth qualities are due to what I take to be an important revolution in science that occurred at the end of 19th century and the beginning of the 20th century. During this period, various fields of science and special sciences were linked. Biology, for instance, was linked with chemistry at the end of the 19th century when Pasteur finally persuaded the medical community that diseases could be understood in chemical terms (Ashall, 1993, p. 149). Chemistry was independent of physics up until the early 20th century when Rutherford and Bohr laid the basis for the

atomic understanding of chemical bonding (Brody et al., 1997, pp. 66-78). The historians of science put what happened to science in the 20th century as follows:

In this century, all the branches and fields of science coalesced into one overlapping, interwoven, intertwined, and interdependent complexity of understanding that is now hurling us into the future. The borderlines that used to clearly separate astronomy, physics, and biology have become fertile fields of common interest and pursuit, together with other fields and subdisciplines... In the 20th century, science became symbiotic and overlapping and scientists no longer desired to maintain their isolation within a defined field. They recognized the need for cross-fertilization – the need to follow developments in the other fields on which they depended and to share information. (Brody et al., 1997, p. 344)

As a result of the establishment of the various links, scientists of the 20th century and onward, trying to formulate new theories in their fields, have to take into account the theories and the expanded observational data of neighboring fields and sciences. In other words, a theory has to be formulated in such a way that it is consistent with well-confirmed theories and expanded experimental data of neighboring fields and sciences. I shall call this phenomenon the “neighbor constraint.”

The unification of science that occurred at the end of the 19th century and in the early 20th century seems to have been the driving force that facilitates the neighbor constraints. The science of the 20th century and onward gives us far more unified picture of the world than the science of before the 20th century. Up until Copernicus’s time, it was believed that there is a fundamental difference between the terrestrial and the celestial regions; up until Darwin’s time between human beings and non-human organisms; up until Faraday’s time between electricity and magnetism; up until Pasteur’s time between organisms and non-organisms; and so on. I believe that atomism, which was officially adopted in the

early 20th century, added fuel to the unification process. In a disunified science, there is less room for a neighbor constraint. For instance, when Pasteur first claimed that diseases could be understood in chemical terms at the end of the 19th century, medical scientists laughed at Pasteur because they believed that organisms are fundamentally different from non-organisms, and that chemists have nothing to say about human physiology. The metaphysical belief associated with the disunified picture of the world prevented chemistry from putting a neighbor constraint on biology.

The concept of neighbor constraint is different from the concept of simplicity. Simplicity is an intrinsic property of a theory, and neighbor constraint is not. T_1 formulated with more neighbor constraint than T_2 is not necessarily simpler than T_2 . Let me take current geology and catastrophist geology on Laudan's list as examples. Geologists today use the radiometric dating technique, the method to determine the ages of materials, when they formulate a theory about distant past events. The technique was developed at the end of the 19th century by physicists who were investigating the phenomena of radioactive decay. Catastrophists, on the other hand, did not take the dating technique into account. Catastrophist geology, which was developed before the 19th century, postulated the existence of Noachian deluge to explain marine fossils in mountain ranges. Now, current geology might be simpler than catastrophist geology. But if so, that needs to be argued for independently of the fact that it took the dating method into account while catastrophist geology did not.

What would be the reason to think that neighbor constraint is evidentially relevant? Why is it that a theory that took into account neighboring theories and observational

evidence is more likely to be true than its rival theory that did not? To answer this question requires that we pay attention to the two components of neighbor constraint: theoretical constraint and observational constraint. What constrains a theory-formulation is both neighboring theories and neighboring observational evidence. So what? Even van Fraassen would agree that observational constraint is evidentially relevant, given that he says that observational evidence “provides some reason for believing in the truth” of a theory (1985, p. 255). But he would doubt that theoretical constraint is evidentially relevant, given that he says that “nothing except evidence can give justification for belief” (ibid., p. 255). That is, for van Fraassen, only observational evidence is evidentially relevant. This assumption, which seems to be the starting point of the empiricist philosophy of science, is critically evaluated in Chapter 6 where I argue at length that a successful theory can provide evidential support for another successful theory.

Now, what can we conclude from the fact that the theories of the 20th century and onward are more successful and have better birth qualities than theories from before the 20th century? I do not conclude that the theories of the 20th century and onward with those properties are approximately true. After all, it is up for grabs whether extra success and extra neighbor constraint have enough evidential power to show that the successful theories of the 20th century and onward are approximately true. Then, what do I conclude from the extra success and the extra neighbor constraint? I conclude, firstly, that pessimistic induction is problematic. An additional premise is required for the pessimistic inducer to infer from (1) to (2):

- (1) Successful theories from before the 20th century are not even approximately true.
- (2) Successful theories of the 20th century and onward are not even approximately true.

The additional premise is that extra success and extra neighbor constraints do not provide any evidential support for the theories of the 20th century and onward. Unless the additional premise is proven, the inference from (1) to (2) is unwarranted. Remember, however, that the pessimistic inducer is fully committed to the reliability of induction. After all, if the reliability of induction is suspect, pessimistic induction cannot even get off the ground. But if induction is a reliable rule of inference, extra success and extra neighbor constraint should be taken to have provided some evidential support for the theories of the 20th century and onward. So the pessimistic inducer cannot even in principle prove that the additional premise is true. Hence, the pessimistic induction against current theories cannot get off the ground. I conclude, secondly, that the pessimistic induction is a fallacy of biased samples. The pessimistic inducer took samples only from science before the 20th century. Recall that Laudan's infamous list consists of twelve successful theories all of which were developed before the year 1900. The minimum requirement for fair samples is that they be randomly taken from the sciences of both before and after the year 1900. Laudan's samples do not meet the minimum requirement. Moreover, if the samples were fairly chosen, most of the samples would be theories of the 20th century. The reason is that scientific knowledge exploded in the 20th

century and the 20th century is past. If most samples are from the science of the 20th century, it is no longer clear that most key terms of the samples do not refer in the light of present theories. In short, extra success and extra neighbor constraint entitle us to reject the conclusion of the pessimistic induction and to be optimistic about the successful theories of the 20th century and onward.

3. Weak Historical Challenge

3.1. Coherence Approach

So far, I tried to diffuse the pessimistic force that arises from past theories. But a successful refutation of the strong historical challenge does not amount to a successful defense of realism, for the weak historical challenge – that there are historical counterexamples that undermine the realist inference from success to approximate truth – remains. In order to counter the weak historical challenge the realists would have to develop a stringent predicate that would pick out our best theories and leave out the others which are on Laudan's list. Given that the theories of the 20th century are more successful and have better birth qualities than earlier centuries, as we have seen in the previous section, realists might try to develop stringent concepts of success and neighbor constraint that would leave out the past theories on Laudan's list. For now, I am skeptical that realists could come up with such concepts. The reason is that the successes and the neighbor constraints of both past and current sciences are in a continuum. Realists could say at best that the kinetic theory, for instance, is more successful and of a better birth quality than the caloric theory.

So, instead of taking that route, I develop the concept of coherence since that property is supposedly possessed by our best theories and not by others on Laudan's list. The intuitive idea of coherence is that two scientific theories cohere with each other just in case an inference from one to the other is to some degree truth-preserving. Coherence is more than the mere absence of contradiction. Also, coherence admits of degree, i.e., some theories loosely cohere with each other, and others tightly cohere with each other. I cash out the notion of coherence of scientific theories in the following three ways, which are not intended to be exhaustive.

First, as Friedman (1981) notes, in science a theoretical structure is used to explain not only the phenomena it was designed to explain but also, in conjunction with other theoretical structures, other phenomena it was not designed to explain. For instance, the molecular hypothesis of gases explains not only gas laws but also other kinds of phenomena in conjunction with atomic theory:

...the hypothesis of molecular constitution, in conjunction with atomic theory, for example, helps us to explain chemical bonding, thermal and electrical conduction, nuclear energy, genetic inheritance, and literally hundreds of other phenomena.
(Friedman, 1981, p. 7)

Friedman stresses that the molecular hypothesis of gases picks up additional confirmation from the areas where it works together with atomic theory. Now, Friedman's observation on theoretical explanation points to the direction in which the concept of coherence can be cashed out. Two theories can be said to cohere with each other if they have their own observational evidence, and work together to explain new phenomena. Let me provide two more examples fitting this definition. The theory of evolution and the theory of plate

tectonics have their own independent observational evidence, and jointly explain new phenomena (Brody et al. pp. 252-253). For example, why do some turtles in Brazil travel to an island in the middle of the Atlantic Ocean, risking their lives? Why don't they just stay in Brazil? The answer involves the theory of evolution and the theory of plate tectonics. Millions of years ago Brazil was only a stone's throw from the island, and the turtles laid eggs on the island to protect them from the mainland predators. As the continent and the island gradually drifted apart, the turtles mentally and physically adapted to the gradual increase of the distance. Notice that the two theories jointly explain the new phenomena. It is not the case that the theory of evolution or the theory of plate tectonics alone explains why the turtles travel. They have to work together to explain the new phenomena. Also, the two theories have observational evidence other than the fact that the turtles travel. Here is another example. The theory of DNA and the theory of electromagnetism have their own observational evidence, and work together to explain new phenomena. For example, why do we develop skin cancer when exposed to sunshine for long time? According to a proffered theoretical explanation, ultraviolet radiation destroys DNA, and the destruction of DNA leads to the skin cancer. Notice again that the theory of DNA or the theory of electromagnetism alone could not yield such a theoretical explanation, and that the two theories have their own observational evidence other than the skin cancer. So the theory of DNA and the theory of electromagnetism cohere with each other.

Second, two theories cohere with each other, if they, having been independently confirmed, are used to point to the same assertion about unobservables. The kinetic

theory of heat, the electro-chemical theory of electrolysis, and the theory of electromagnetism, for instance, were used to calculate Avogadro's number, the number of molecules of an arbitrary gas contained in a given volume at a set temperature and pressure. They all point to the same number of molecules. The theories have their own independent observational evidence other than the experimental outcomes pointing to the number. So they cohere with each other.

Third, two theories cohere with each other, if they have their own observational evidence, and a component of one theory explains a component of the other theory. The theory of DNA asserts that all current living organisms on earth use the same genetic code (Sober, 1993, p. 41). Suppose that Darwin's original theory of evolution claimed that there was exactly one common ancestor for all current living organisms on earth. Then, the theory of evolution and the theory of DNA would perfectly fit this definition, because the theoretical statement that there was exactly one common ancestor would explain why all current living terrestrial organisms use the same genetic code.

Unfortunately, Darwin was not sure whether there was exactly one common ancestor or a few common ancestors. So, strictly speaking, no component of Darwin's theory of evolution explains a component of the theory of DNA. Yet, I am inclined to think that the theory of evolution and the theory of DNA approximately fit the definition, and they are more than merely consistent with each other. Of course, they cohere less tightly with each other than they would if Darwin's original theory of evolution had asserted that there was exactly one common ancestor.

Let me say one more thing about the concept of coherence. Two theories may cohere with each other either directly or indirectly. Suppose that they stand in any of the foregoing three relations. Then, they directly cohere with each other. Now suppose that T_1 directly coheres with T_2 , and that T_2 directly coheres with T_3 . Then, T_1 indirectly coheres with T_3 . They do not cohere with each other as tightly as they do with the intermediary theory, T_2 . Still, they are more than merely consistent with each other. Now suppose that T_1 and T_3 also directly cohere with each other by standing in any of the foregoing three relations. Then, T_1 , T_2 , and T_3 cohere with each other both directly and indirectly. This abstract scheme of relations can be illustrated with the following example:

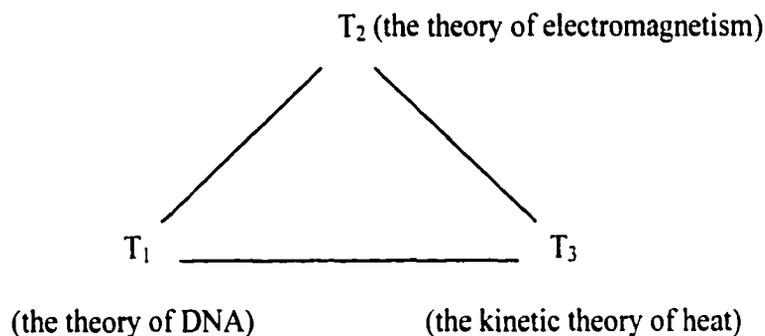


FIGURE 1

The theory of DNA directly coheres with the theory of electromagnetism because they jointly explain new phenomena like skin cancer. The theory of electromagnetism directly coheres with the kinetic theory of heat because they were used to calculate Avogadro's

number. The kinetic theory of heat directly coheres with the theory of DNA because they jointly explain new phenomena like muscle contractions.³ So the three theories cohere with each other both directly and indirectly.

What follows from the fact that some successful theories of the 20th century and onward cohere with each other? First of all, they are different in kind from the successful theories on Laudan's list. For example, one of the theories on the list is the humoral theory of medicine. This theory was developed in ancient Greece to explain diseases (Mason, 1962, p. 227). The theory postulated four distinct humors solely for the sake of explaining the medical phenomena. It is not clear if neighboring scientific theories constrained its formulation, let alone cohered with it. What is clear, though, is that its possible contemporary counterpart, the germ theory of disease, coheres with the theory of DNA, which in turn coheres with the theory of evolution, and which in turn coheres with the theory of plate tectonics.

Secondly, coherence is an impressive performance of a theory at the theoretical level, whereas success is an impressive performance of a theory at the observational level. It is possible that, for instance, two independently successful theories fail to explain new phenomena of their domain. Put differently, the conjunction of the two successful theories might not be empirically adequate, although they are individually empirically adequate. So, if success cries out for an explanation due to its impressive performance, so does coherence. It would be hard to come up with reason to think that coherence does not need to be explained while success does. We need to come to terms with the question: Why do some independently successful theories miraculously cohere with each other?

The realist answer is that the independently successful theories have approximately got a grip on how things really are, i.e., they are approximately true. This line of thinking suggests that realism could be strengthened by substituting 'success and coherence' for 'success' in Putnam's original no-miracle argument. The net result of the substitution is as follows:

The Modified Version of the No-miracle Argument

Premise 1: The success and the *coherence* of some scientific theories need to be explained.

Premise 2: The miracle hypothesis holds that key terms of successful theories that cohere with each do not refer, that the theories are not even approximately true, and that a miracle happened.

Premise 3: The no-miracle hypothesis asserts that key terms of successful theories that cohere with each other typically refer, that the theories are typically approximately true, and that a miracle did not occur.

Premise 4: A miracle is an event that is not likely to occur.

Conclusion: The no-miracle hypothesis provides the best explanation of the success and the coherence.

The pessimistic induction is a fallacy of *irrelevant* samples against the modified version of the no-miracle argument above. To refute the modified version, the pessimistic inducer

now has to come up with a new list, which would consist of successful theories that cohere with each other. The theories would have to be pulled out randomly from both before and after the year 1900. To take samples only from the science of before the 20th century is to commit the fallacy of *biased* samples because the science of the 20th century and onward is more successful and has more neighbor constraints than the science of before the 20th century. Next, the pessimistic inducer has to argue that the theories of the sample are not even approximately true and that their central terms do not refer in the light of present theories. The prospect for such a list, however, is bleak. As we go back into the past, the body of scientific knowledge becomes thinner and thinner, and the various fields of sciences and special sciences were largely independent of each other up until late 19th century and early 20th century, as we already noted in the previous section. So it is hard to find successful theories that cohere with each other from before the 20th century.

3.2. Difference between Coherence Approach and Kitcher's Approach

The historical challenges to realism elicited huge responses from realists, and Kitcher's response (1993, pp. 140-149) has been the most influential in the literature. Psillos (1996a, and 1999, Chapters 5 and 6) and Leplin (1997, pp. 141-152) largely follow Kitcher's lead on this matter. Kitcher's basic message is that successful past theories might be approximately true, given that their working assumptions are true, although their idle assumptions are false, in the light of current theories. A working assumption is a constituent of a theory that is severely tested by observations, that survives scientific

revolutions, and that scientists have confidence in the truth of. An idle assumption, on the other hand, is a component of a theory that is not severely tested by observations, that does not survive scientific revolutions, and that scientists have no confidence in. An example of a working assumption of the ether theory is the claim that light is a transverse wave, and an example of an idle assumption of the ether theory is the claim that ether is a medium of light transmission. Kitcher's historical case study includes the ether theory, Psillos's case study includes the caloric theory and the ether theory, and Leplin's case study includes Newton's theory of motion. So Kitcher's approach to the historical challenges can be characterized as an attempt to do justice to the theories on Laudan's list. The coherence approach I developed in this chapter radically differs from Kitcher's approach. It can be characterized as an attempt to do justice to theories of the 20th century and onward, for it focuses on how theories of the 20th century and onward differ from theories from before the 20th century.

Now, the problem with Kitcher's approach, antirealists argue, is that it is hard to distinguish between idle assumptions and working assumptions of *current* theories, although it is relatively easy to distinguish between idle assumptions and working assumptions of past theories in retrospect. Ladyman puts the problem as follows:

The trouble with this approach (that also besets Kitcher and Psillos) is how to make the distinction between eliminable and ineliminable components of a theory principled and not just an *ad hoc* strategy for coping with the refutation of realism by history. (Ladyman, 1999, p. 187)

Now, the coherence approach offers some principled criteria to separate working assumptions from idle assumptions. First, if two theoretical claims work together to

explain new phenomena, they are working assumptions. In this sense, it is legitimate to believe that species have evolved by natural selection and that continents move. The working assumptions are not just approximately true but completely true. Second, if two theoretical claims are explanatorily connected, they are working assumptions. Third, theoretical entities are sufficiently proved to be real if the postulated interaction between them explains new phenomena. In this sense, X-rays, molecules, and DNA could be legitimately claimed to be real.

4. Conclusion

For the last century, general philosophy of science has been done, by and large, under what I call the “individualistic tradition.” Philosophers of science tried to understand science by trying to understand a scientific theory without regard to its relation to other scientific theories. The topics discussed under this tradition include the followings: the pessimistic induction, the problem of underdetermination, the nature of scientific explanation, the theory-ladenness of observation, the structure of scientific theories, and simplicity. Attention is paid to a theory in itself and to the relation between the theory and observations rather than to the relation between the theory and other theories. This tradition is in contrast with what I call the “interactionistic tradition.”

Under the interactionistic tradition, philosophers of science would try to understand science by trying to understand how scientific theories interact with other scientific theories. What needs to be done under the interactionistic tradition is to develop rigorous concepts to reveal and describe the exact mechanisms of the interactions in a more fine-

grained manner than is done here in this chapter. The achievements in this research project will tell us what current science looks like and in what direction it will develop in the future. Besides, they will cast new light on the traditional topics discussed in the individualistic tradition. For instance, I speculate that a theory which coheres with other theories yields a better explanation of phenomena than a theory which is merely consistent with other theories.

More than a century has passed since various fields of science began to interact. An essential part of scientific training for undergraduate prospective scientists today is to be informed of the interactions between various fields of science (Trefil and Hazen, 1998). To be scientific is to be interactive. This is true of current science. But philosophers of science have paid more attention to the history of science than the interactions between current scientific theories. I agree with the historical tradition Kuhn set in the 1960s that the history of science is important to understand science. But I also believe that understanding the distinctive features of current science is important to understand science.

In accordance with the interactionistic spirit, I formulate the kind of realism I strive for as follows: Our best theories, successful theories that cohere with each other, are approximately true, and their key terms refer. Antirealists might try to come up with counterexamples from the history of science to refute this kind of realism, which is fair game. But be mindful that the explanation of success and coherence presented in this chapter might still be the best around, even if there are some counterexamples, unless a better antirealist explanation of success and coherence is proposed. The prospect for an

alternative antirealist explanation is bleak. I argue in the following chapter that the best semantic property for the antirealists, approximate empirical adequacy, cannot explain the coherence of successful theories.

End Note

¹ Leplin takes the pessimistic induction as claiming that "...we do continually come to judge that our theories are false..." (1997, p. 142)

² Does Laudan provide the list to advance the strong historical challenge or the weak historical challenge? In both publications (1981 and 1984c, Chapter 5) where he fully develops a historical challenge to realism, he never makes the ambitious claim that successful current theories are likely to be not even approximately true. He is not interested at all in proving that successful current theories are likely to be not even approximately true. Rather, he is interested in confuting the realist inference from success to approximate truth and reference by collecting counterexamples from the history of science. His conclusion in both publications, so far as I can tell, is a modest claim that there are historical counterexamples that prevent the realists from inferring approximate truth and reference from success. There is textual evidence supporting this interpretation. In the introduction of his 1981 paper, he summarizes the main thesis as follows:

Specifically, I shall show that epistemic realism, at least in certain of its extant forms, is neither supported by, nor has it made sense of, much of the available historical evidence.
(Laudan, 1981, p. 20)

After providing the infamous list, he claims that there is no necessary connection between success and approximate truth:

...there is no necessary connection between increasing the accuracy of our deep-structural characterizations of nature and improvements at the level of phenomenological explanations, predictions and manipulations. (ibid., p. 35)

In the concluding section of the paper, he claims that there are just too many counterexamples to the realist epistemology, i.e., realists can not explain the success of past science in term of approximate truth and reference:

Their epistemology is confronted by anomalies which seem beyond its resources to grapple with.
(ibid., pp. 47-48).

He mentions successful *current* theories only in one place throughout the two papers. But even there he does not make the ambitious claim that successful current theories are likely to be not even approximately true. Rather, he accuses the realists of "foreclosing the possibility that some future generation may come to the conclusion that some (or even most) of the central terms in our best theories are no more referential than was 'natural place,' 'phlogiston,' 'aether,' or 'caloric'" (ibid., p. 42). Notice that he suggests that it is *possible* that central terms of current theories might be nonreferring in the light of future theories. He is not claiming that it is *likely* that central terms of current theories are nonreferring in the light of future theories. In short, nowhere does Laudan run the pessimistic induction against successful current theories.

Realists and antirealists alike in the literature, however, do not interpret Laudan as I do. Kitcher, Leplin, and Ladyman attribute to Laudan the ambitious claim that successful current theories are likely to be not even approximately true and that their central terms are likely to be nonreferring in the light of future theories:

Laudan's use of the pessimistic induction from the history of science to discredit the claims to reference and truth of current science and to undermine a realist account of progress is the most sophisticated that I know. (Kitcher, 1993, p. 149)

He [Laudan] argues inductively that our current picture is unlikely, in retrospect, to have achieved referential success or approximate truth from the vantage point of future science, and that realism, according, has no application. (Leplin, 1997, p. 137)

The pessimistic meta-induction from the history of past theory change in science was originally proposed to confute the realist's claim that truth explains the success of science by showing that *our best present and future theories are most likely false* (Laudan [1981]). (Ladyman, 1999, p. 186)

I believe that it is wrong for the commentators to attribute the ambitious claim to Laudan. So, when I criticize the pessimistic induction, the strong historical challenge, in this chapter, I do not claim that I am criticizing Laudan's position. Rather, I claim that I am criticizing an imaginary pessimistic inducer's position.

³ See Yanagida (2001, p. 64) on how the two theories jointly explain muscle contractions.

CHAPTER 3

ANTIREALIST EXPLANATIONS OF THE SUCCESS OF SCIENCE

1. Introduction

Ever since Putnam (1975) claimed that the best explanation of the success of science is that successful scientific theories are typically approximately true and that their key terms typically refer, antirealists have been proposing various rival antirealist explanations of the success of science, thereby attempting to undermine the claim that the realist explanation is the best. As far as I know, there are eight of them in the literature. This chapter aims to critically examine one by one these antirealist proposals. Mostly, I raise empirical and conceptual objections. This project is important for two reasons: First, given that the no-miracle argument claims that the realist explanation is the best, realists have the burden to prove that the realist explanation is better than the eight antirealist explanations. Second, the discussions of the eight antirealist proposals will serve as the essential basis for defending realism from antirealist objections and raising difficulties against antirealism at the end of this chapter and in the remaining chapters.

2. Antirealist Proposals

2.1. Evolutionary History

2.1.1. Analysis of the Proposal

Van Fraassen is the first to propose a rival antirealist explanation of the success of science. His proposal does not invoke the realist semantic property: approximate truth.

Rather, it appeals to the Darwinian theory of evolution. The evolutionary explanation of the success of science, however, is not fully developed, and van Fraassen's relevant writing (1980, pp. 39-40) is somewhat elusive. We would have to do some interpretative work to evaluate the proposal. It is clear, though, that the evolutionary explanation is an application of the original evolutionary scheme of explanation to the non-biological phenomenon that some scientific theories are successful. Thus, we first need to get clear about the original evolutionary scheme of explanation.

In evolutionary biology, a biological property is explained in terms of two main ideas: the tree of life and natural selection. The tree of life specifies which species are descended from which others, and which species went extinct. It is a description of genealogical facts. Natural selection, on the other hand, is the main mechanism by which species have evolved. Natural selection occurs only when the following three conditions are met. First, variations must occur in a population of organisms. Suppose, for instance, that all mice run at the same speed. Then, natural selection on speed cannot occur. Some members must run faster than others in order for there to be a change in the composition of a population of mice in successive generations with respect to speed. Second, variations must contribute to fitness. Unless the ability to run fast makes some difference to survival and reproduction of a mouse, then there would not be a change in the composition of the population in successive generations. After all, if the ability to run fast conferred no advantage of increased ability to survive and reproduce, the fast-running mice would have an equal chance of survival and reproduction with the slow-running ones. Third, variations must be heritable. Even if the fast-running mice are fitter than the

slow-running ones, there would not be a change in the composition of the population in the successive generations, unless the ability to run fast is passed on to the next generation. In short, natural selection occurs only when there is heritable variation in fitness (Sober, 1993, p. 9).

We are now ready to unfold the evolutionary story of why, for example, current mice run fast from cats. The story goes as follows: In the past, mice did not run as fast from cats as mice do today. Variations occurred in the population of mice, i.e., some mice acquired the heritable ability to run fast from cats. (A genetic story might be given of how the requisite mutations occurred.) They were fitter than the slow-running ones, i.e., they had a better chance to survive and reproduce than the slow-running ones. The ability to run fast from cats was inherited by the following generations. (A genetic story also might be given of how the phenotype was passed on to the following generations.) Over many successive generations, natural selection favored the fast-running mice over the slow-running ones. As a result, all the slow-running mice died out, and only the fast running mice survived. That is why current mice run fast from cats. This is a paradigm example of how the theory of evolution explains a biological property of current organisms.

Now, van Fraassen extends the evolutionary scheme of explanation to the scientific phenomenon: Some scientific theories are successful. Before I unfold the evolutionary story, it should be noted that his conception of the success of science is not very different from that of realists. He takes the success to be the fact that some scientific theories regularly make true predictions:

Science, apparently, is required to explain its own success. There is this regularity in the world, that scientific predictions are regularly fulfilled; and this regularity, too, needs an explanation.” (van Fraassen, 1980, p. 39)

Notice that he does not include explanation and intervention as success of science. But the failure to include them does not seem to make much difference. In any case, we are now ready to unfold the evolutionary story of why some scientific theories are successful. He puts the story as follows:

...I claim that the success of current scientific theories is no miracle. It is not even surprising to the scientific (Darwinist) mind. For any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive – the ones which *in fact* latched on to actual regularities in nature. (1980, p. 40)

As it stands, however, van Fraassen’s writing does not tell us very much. We would have to work out the details of how the evolutionary story should go in this non-biological case. But before I unveil the full-fledged evolutionary story, we first need to be clear about what van Fraassen means when he claims that successful theories are the ones which latched on to regularities. What he means, I believe, is that the successful theories are the ones that are empirically adequate. A theory’s latching on to regularities is its being empirically adequate. So the evolutionary explanation of the success of science boils down to the evolutionary history of how empirically adequate theories have evolved by artificial selection.¹ The history is fleshed out as follows:

In the past, scientific theories were not empirically adequate, though current scientific theories are. Variations occurred in the population of scientific theories, i.e., some

scientific theories that scientists were considering were empirically adequate. To put it differently, scientists somehow hit on some empirically adequate theories. The empirically adequate theories were fitter than empirically inadequate ones, i.e., they had a better chance to survive and reproduce than the empirically inadequate ones. Survival here means that the theories were *accepted* by scientists, and reproduction here means that the theories got *spread* through the scientific community. The property of being empirically adequate was passed on to the next generations, i.e., the theories preserved the property of being empirically adequate as they moved from one scientist's mind to another scientist's mind. As they moved through many scientists' minds, natural selection favored the empirically adequate theories over the empirically inadequate ones. i.e., scientists threw away the empirically inadequate theories, and kept the empirically adequate ones. As a result, all the empirically inadequate theories died out, and only the empirically adequate theories survived. For example, the Ptolemaic theory and the caloric theory died out, and the Copernican theory and the kinetic theory survived. That is why some scientific theories are successful. In short, the explanation of the success of science does not have to consist in approximate truth. It may rather consist in the evolutionary survival of some empirically adequate theories. Thus, antirealists have an explanation of the success of science, and it does not appeal to the realist semantic property, approximate truth.

2.1.2. Critiques

The evolutionary story, as reconstructed above, presupposes that empirical adequacy is the sole property that contributes to the increased ability to survive and reproduce. But in real science there are other properties that scientists take into account when they decide to accept a theory, e.g., elegance, easy computability, fruitfulness, and so on. So, from the fact that a scientific theory is fitter than its competitors, it does not follow that it is empirically adequate. The same is true of the biological case. It might be strong hearts, strong lungs, or strong legs that are responsible for the ability to run fast. From the fact that fast-running mice are fitter than slow-running ones, it does not follow that the fast-running mice have, say, strong hearts. Analogously, from the fact that successful theories are fitter than unsuccessful ones, it does not follow that they are empirically adequate (Shaughan Lavine, personal communication).

Moreover, the pessimistic induction discussed in Chapter 2, if correct, poses a serious empirical problem to the evolutionary proposal. It is obvious that the pessimistic induction is a serious challenge to the realist quest for truth. But it is also a serious challenge to the antirealist quest for empirical adequacy. The reason is that successful past theories turned out to be false via running into empirical anomalies. The successful past theories turned out to be empirically inadequate. So successful current theories will turn out to be empirically inadequate as well. Then, it is simply wrong to say that the explanation of the success of science consists in the evolutionary survival of some empirically adequate theories because successful theories are empirically inadequate.²

Now, let me raise a different sort of problem with van Fraassen's evolutionary explanation of the success of science. Is van Fraassen in a position to appeal to the

Darwinian theory of evolution to explain something? He is a constructive empiricist, which implies, among other things, that he need not believe that successful scientific theories are true. Thus, he need not believe that the Darwinian theory of evolution is true, since it is a scientific theory. But if he need not believe that it is true, how can he use it to explain something? How can he claim that only successful theories survive? Suppose that van Fraassen utters the following two sentences:

(A) Only successful theories survive.

(B) I do not believe that only successful theories survive.

He has to utter (A) to explain the success of science, and has to utter (B) to be faithful to constructive empiricism. The speech act is odd. It looks like we are required to believe that a scientific theory is true when we use the theory to explain phenomena. The belief in the truth of an explanatory theory is built into the act of giving an explanation coherently. This point will be argued at length in the next chapter in a more precise manner.

It might be replied that van Fraassen does not have to believe that his evolutionary explanation is true in order to diffuse the realist argument from success. All he needs to do to refute the no-miracle argument is to exhibit an alternative explanation that does not appeal to the realist semantic property. The problem with the suggestion is that the refutation comes only at the expense of introducing another argument for realism. Suppose that the evolutionary explanation, the alternative, is true. Then, we have a true

scientific theory in hand, viz., the theory of evolution. If the theory of evolution can be legitimately believed to be true, other scientific theories that are in the same boat with the theory of evolution can also be legitimately believed to be true. The realists can happily give up Putnam's no-miracle argument and embrace the evolutionary explanation as a means to argue for realism.

Now, it might be argued that van Fraassen can believe the theory of evolution without giving up constructive empiricism. Suppose that the theory of evolution does not contain a theoretical term, and that truth collapses into empirical adequacy in this case. The problem with this possible move is that the theory of evolution is not free from theoretical terms. Contemporary evolutionary biology defines evolution in terms of changes in gene frequencies (Sober, 1993, p. 1), and 'gene' is a theoretical term. Moreover, even if we grant for the sake of argument that the theory of evolution is free from theoretical terms, and that truth collapses into empirical adequacy in this case, constructive empiricists are not in a position to believe that the theory of evolution is true. The reason is that Darwin uses inference to the best explanation (IBE, hereafter) all the time to arrive at the theory of evolution, and constructive empiricists believe that IBE is a deplorable rule of inference regardless of whether the hypothesis inferred is about observables or unobservables. Darwin claims in numerous places of *The Origin of Species* that his theory has a higher explanatory power than creationism. Let me quote just one passage:

And we can clearly understand these analogies, if species have once existed as varieties, and have thus originated: whereas, these analogies are utterly inexplicable if each species has been independently created.

(*The Portable Darwin*, 1993, p. 146)

The constructive empiricists flatly reject the reliability of IBE. IBE is unacceptable whether a hypothesis inferred is about observables or about unobservables:

Some readers have apparently thought that, in chapter 2, section 3 of *The Scientific Image*, I assert inference to the empirical adequacy of the best explanation to be a correct inductive principle. Not so...
(van Fraassen, 1985, p. 295, footnote)

Indeed, the argument's conclusion is that the rule of IBE is unacceptable in general. (Ladyman, Douven, Horsten, and van Fraassen, 1997, p. 312)

So it looks like the constructive empiricists are not in a position to believe that the theory of evolution is true even if it is free from theoretical terms.

Van Fraassen might argue that he can use the theory of evolution because there is independent reason to believe that the theory of evolution is true. That is, we can arrive at the theory of evolution by a method other than IBE. This possible move, however, is riddled with problems. Suppose that he succeeded in specifying such a method, and that he believes that the theory of evolution is true. Then, he is no longer a constructive empiricist but a realist. After all, he believes that a scientific theory, the theory of evolution, is true. To believe that the theory of evolution is true is to give up the antirealist program altogether. Furthermore, if the independent method entitles us to believe that the theory of evolution is true, it might also entitle us to believe that other scientific theories, such as the kinetic theory of gases, are true. The possible move does not work.

2.2. Robust Winnowing Method

2.2.1. Basic Claim

Laudan claims that science is successful because scientific theories “result from” robust scientific methods. A drug, for instance, works well because it is produced by rigorous testing methods, such as double-blind experiments in which even the researcher does not know whether a particular patient belongs to the control group or the testing group:

Science is successful, to the extent it is successful, because scientific theories result from a winnowing process which is arguably more robust and more discriminating than other techniques we have found for checking our empirical conjectures about the physical world. (1984a, p. 101)

To put it another way, a scientific theory is successful because scientists applied robust experimental methods to it. Laudan goes on to say that robust methods do not produce true theories but *reliable* theories:

Those procedures are not guaranteed to produce true theories; indeed, they generally do not produce true theories. But they do tend to produce theories which are more reliable than theories selected by the other belief-forming policies we are aware of. (ibid., pp. 101-102)

Laudan does not say exactly what reliable theories are. I take it, though, that reliable theories are the same as successful theories. So Laudan’s proposal comes down to the suggestion that science is successful because scientists use *robust methods* that select some theories and screen out others.

The significance of Laudan’s proposal is, I believe, that it brings our attention to one of the conditions for a scientific theory to be successful, viz., scientists employ stringent

experimental methods to test scientific theories. Unless scientists use robust methods in experiments, a scientific theory does not even have a chance to be successful. We would not say that a scientific theory that merely passed sloppy and loose tests is successful. So it is conceptually legitimate to invoke robust methods to explain the success of science.

2.2.2. Critiques

The proffered explanation, however, has no explanatory force. The reason is that the application of robust methods could be also cited to explain why some scientific theories are unsuccessful. Suppose that scientists use robust methods in an experiment and a theory fails the test. Why did the theory fail the test? It failed the test because scientists used robust methods that select some theories and screen out others, i.e., because the unsuccessful theory has resulted from robust winnowing processes. If it makes sense to say that some theories are successful because they result from robust winnowing processes, it should also make sense to say that some theories are unsuccessful because they result from robust winnowing processes. After all, unless scientists had used robust methods, the unsuccessful theories would not have failed the test. The application of robust methods is also one of the conditions for a scientific theory to be unsuccessful. So it is also conceptually legitimate to invoke robust methods to explain why some scientific theories are unsuccessful.

Now, an explanatory factor that can be applied to both an explanandum event and an event that is in contrast with the explanandum event does not give rise to explanatory force. Suppose that one explains why an apple falls down from a tree by citing the fact

that Spinoza planted the apple tree in the past. We would not be satisfied with such an explanation, although there is nothing conceptually wrong with it, because Spinoza's having planted the tree is equally responsible for an apple's hanging on a branch. No explanatory force arises from citing such a condition. Likewise, no explanatory force arises from citing the fact that scientists use robust methods. A good explanans is one that is presumed to be responsible for the occurrence of the explanandum event to the exclusion of the event that is in contrast with the explanandum event. When realists ask why some scientific theories are successful, they want to know what properties successful theories have that unsuccessful theories do not have. That is, they want to know the distinctive features of successful theories. Truth and empirical adequacy are presumed to be the distinctive features of successful theories. So an explanation invoking such semantic properties does have explanatory force.

Laudan might retort that the distinctive feature of successful theories is that scientists *accepted* them. Successful theories are the ones that scientists accepted through using robust experimental methods, and unsuccessful theories are the ones that scientists rejected through using robust experimental methods. The observation is fair enough. But it is conceptually problematic to explain why some scientific theories are successful in terms of the fact that scientists accepted them. It sounds odd that some scientific theories make true predictions because scientists accepted them. The flow of explanation should be the other way around, i.e., the explanans and the explanandum should switch places. The resulting story is that scientists accept some theories because they made true

predictions. After all, scientists come to accept a theory by first observing that the theories made true predictions.

2.3. Empirical Adequacy

Fine (1986, p. 154) and Ladyman (1999, p. 186) point to the possibility that epistemological instrumentalists could explain the success of science in terms of empirical adequacy. Some scientific theories are successful because they are empirically adequate. Recall that the inference from success to empirical adequacy is an inference from “some” to “all” (Section 3.4.2., Chapter 1). So to explain success in terms of empirical adequacy is like saying that some crows are black because all crows are, as Musgrave notes (1988, p. 242). Now, what could realists say in response?

Leplin takes the rival antirealist account seriously and tries to refute it. He argues that empirical adequacy is a poor explanatory property, because generalizations “do not explain their instances” (1997, p. 23), and because empirical adequacy itself is what needs to be explained:

However, as an explanation either of what is observed or of how a theory manages to predict what is observed, empirical adequacy is an attribute that itself cries out for explanation. (Leplin, 1997, p. 23)

Ladyman replies that Leplin makes a claim that “needs to be argued rather than merely stated...” (1999, p. 186). Kukla (1996, p. S303-S305) makes a similar point to Ladyman, arguing that it is hard to justify stopping not at empirical adequacy but at approximate

truth in the chain of demand for explanation. Ladyman and Kukla's point is fair enough. But should we accept the antirealist proposal?

Two comments are in order. First, the pessimistic induction, if correct, completely knocks the antirealist proposal down. Any antirealist explanation of the success of science invoking empirical adequacy runs into a serious empirical problem which the pessimistic induction poses. Second, while antirealists challenge realists to justify the realists' stop at approximate truth in the chain of demand for an explanation, they do not even attempt to justify their stop at empirical adequacy. As Stanford notes, no reason is offered to think that it is empirical adequacy at which "we should draw the line and end our demands for explanation" (2000, p. 268). The realists are no worse off than the antirealists on this account.

2.4. As-If-True

Fine (1986, p. 157) also suggests that epistemological instrumentalists could explain the success of science in terms of as-if-true. A scientific theory is successful because the world operates as if it were true, i.e., because the observable phenomena are as if it were true. The proposal is distinctively antirealistic because one does not have to believe that a theory invoked is true in order to give such an explanation. The as-if-true explanation is neutral as to whether an explanatory theory is true or false, because the world may operate at the observational level as if it were true when in fact it is false. Then, what is the as-if-true explanation committed to? Musgrave (1988) thinks that the empirical adequacy of the explanatory theory is involved in such an explanation. He argues that

there is only a verbal difference between the appeal to as-if-true and the appeal to empirical adequacy, and Stanford (2000, p. 268) agrees with Musgrave. I believe that Musgrave and Stanford are right in their interpretation of as-if-true explanation. After all, a true theory, if it has observational consequences at all, is empirically adequate. So I take it that there is no difference in substance between the suggestion that the theory is successful because the world operates as if the theory were true and the suggestion that the theory is successful because it is empirically adequate.

Now, what could realists say in response? Leplin argues that the explanans, as-if-true, cries out for an explanation and that truth would explain it:

Why does everything happen as if the theory is true? The theory's truth would explain this, so it would seem a legitimate query. (Leplin, 1997, p. 25)

Stanford agrees with Leplin, saying that the fact that the world is as if the theory were true “does indeed both cry out for some further explanation and invite the truth of the theory...” (2000, p. 269). Leplin’s response, however, is not a knockdown argument against the antirealist proposal. The pessimistic induction can do the job. If the pessimistic induction is correct, a successful theory is empirically inadequate. Then, it is simply wrong to say that the explanation of the success of a theory consists in the operation of observable phenomena as if the theory were true, because not all observable phenomena operate as if the theory were true, i.e., because the theory is empirically inadequate.

2.5. Instrumental Reliability

Fine (1991) suggests that antirealists could explain the success of science in terms of instrumental reliability. A scientific theory is successful because it is instrumentally reliable. He makes two important points about the concept of instrumental reliability.

First, instrumental reliability is nothing but the disposition of the theory to be successful:

This is an explanation of outcomes by reference to inputs that have the capacity (or “power”) to produce such outcomes. It is a typical dispositional explanation, just like Newton’s account of the phenomena of gravitation. (Fine, 1991, p. 83)

Second, a theory is instrumentally reliable just in case it is useful:

For according to instrumentalism what we want from our theories, posits, ideas etc. in all the various contexts of inquiry is instrumental reliability. That is, we want them to be useful in getting things to work for the practical and theoretical purposes for which we might put them to use. (ibid., p. 86)

Hence being reliable amounts to no more than being useful for getting things to work (or work out). (ibid., p. 95)

In a nutshell, a scientific theory is successful because it has the disposition to be successful, i.e., it is useful in experimental situations. I take the proposal as implying that the usefulness of the theory is what disposes the theory to be successful.

Fine makes two general comments on the antirealist proposal. First, when the antirealists explain success in terms of instrumental reliability, they are inferring from the observation of the success of a scientific theory to their philosophical theory that the scientific theory is instrumentally reliable. Fine calls the inference an “instrumentalist version of inference to the ‘best’ explanation” (ibid., p. 83). Second, the instrumental

explanation is better than the realist explanation, because instrumental reliability is what is minimally required to explain the output of the scientific method, viz., success:

If the output of the scientific method is instrumental success, why do we need to suppose that the input has features beyond what is useful for explaining the output? (*ibid.*, p. 82)

Put differently, the principle of economy favors instrumental reliability over approximate truth as an explanatory property for success.

The problem with Fine's proposal is that it is putting the cart before the horse to explain success in terms of usefulness. The explanans and the explanandum should exchange places. Suppose, for instance, that an astronomical theory regularly makes true predictions, hence it is successful. Suppose also that it helps us cope with future natural disasters, saving thousands of lives, hence it is useful. Now, how are the success and the usefulness related to each other? The success, I contend, is in part what makes the theory useful. The astronomical theory gets to be useful by means of being successful. After all, it helps us cope with future natural disasters by means of making true predictions. Suppose that a scientific theory regularly makes true predictions, but the predictions do not have such practical applications, hence it is not useful. Even so, the theory is successful. After all, it regularly makes true predictions. The point is that the usefulness of a theory does not contribute to its success at all. Rather, the success contributes to the usefulness. So the usefulness is to be explained in terms of the success. That is, a theory is useful in part because it is successful.

Fine might reply that a theory whose true predictions do not have practical applications is still useful because it satisfies scientists' intrinsic desires to make true predictions. It strikes me as odd, however, that a theory makes true predictions because it satisfies scientists' intellectual desires, i.e., because it is useful. After all, the scientists get satisfied by first observing that it makes true predictions. That is, a theory gets to be useful by first being successful. I conclude that there is a conceptual flaw with Fine's proposal that a theory is successful because it is useful.

2.6. Narrative Style of Explanation

Brown (1994) proposes an explanation of the success of science which he takes to be an alternative to Putnam's no-miracle argument. The style of the explanation, he argues, is narrative. On the narrative style, an explanandum is explained by a story in which the explanandum is embedded. Brown's example of narrative style of explanation is an evolutionary explanation of a biological property of current organisms, which I will not rehearse here because it is exactly like the previous story involving mice in Section 2.1. of this chapter. Brown goes on to say that on the narrative style of explanation, an explainer does not have to be confident of the truth of the narrative:

Is this meant by the evolutionist to be true? Not with any degree of confidence. It is only meant to be an evolutionary *possibility*, one of the many courses (within the Darwinian framework) that nature *might* have taken.
(1994, p. 21, italics in original)

Likewise, we can invoke *truth* to explain success, although "we cannot count on the (approximate) truth of the theories at all" (ibid., p25). So "the realist has an explanation

of the success of science: truth is the explanation and the style of the explanation is narrative” (ibid., p. 23). Notice that Brown takes the narrative style of explanation of the success of science to be realistic on the ground that truth is invoked.

It seems to me, however, that Brown’s explanation of the success of science is not realistic but antirealistic. What is crucial on Brown’s account is that the explainer does not have to be committed to the truth of the explanans statements. Then, how can we call his position realism? The fundamental difference between realism and antirealism is that realism is committed to theoretical truths, and antirealism only to observational truths (Psillos, 1999, p. xx). So I suspect that Brown inadvertently showed how antirealists, who are loath to be committed to theoretical truths, could explain the success of science without being committed to them. Since Brown’s proposal is distinctively antirealistic, critical realist responses are in order.

It is, to say the least, controversial that the evolutionary story is a purely speculative narrative that does not deserve any degree of our confidence. But this is a minor criticism because it might be that Brown just came up with a bad example to illustrate a good philosophical point that on the narrative style of explanation an explainer does not have to believe that explanans statements are true. My second critical response is that it is not clear that Brown’s explanation of the success of science is narrative. The explanation consists of just one sentence: “So, the realist has an explanation for the success of science: truth is the explanation and the style of the explanation is narrative” (ibid., p. 23). One sentence hardly constitutes a narrative. But again this is a minor criticism because he can come up with a better name for this kind of explanation.

Now, let me move on to what I take to be a substantive criticism. The suggestion that we can give an explanation of the success of science without being confident of the truth of explanans statements can be reduced to an absurdity. Consider the following example of a narrative explanation of the success of science. “On the first day of creation an evil demon created the earth, ..., and on the last day of creation the evil demon created human beings. He created human beings in such a way that once human beings grasp a mature theory, the theory gets to be successful.” A narrative is just given of how a theory gets to be successful. The realist and the antirealist alike would reject this kind of explanation. But it is a legitimate explanation of the success of science if Brown is right. After all, we do not have to be confident of the truth of the narrative. In a nutshell, if Brown is right, any narrative goes. Explanatory anarchism is in the offing.

2.7. Approximate Empirical Adequacy

Kukla (1996, p. S300) and Ladyman (1999, p. 186) point to the possibility that epistemological instrumentalists could invoke approximate empirical adequacy to explain the success of science. Recall that the inference from success to approximate empirical adequacy is an inference from “some” to “most” (Section 3.4.2., Chapter 1). So to explain success in terms of approximate empirical adequacy is like saying that some crows are black because most crows are.

My first critical point is that this kind of explanation is on the verge of being vacuous. It is not clear in what contexts this kind of explanation would be accepted. I concede, though, that this is hardly a knockdown argument against the instrumental proposal. So

let me turn to the pessimistic induction and see what it could do against the proposal.

Luckily, this one gets around the problem of the pessimistic induction. The fact that successful past theories ran into anomalies is compatible with the suggestion that they are approximately empirically adequate. Recall that not all observational consequences of an approximately empirically adequate theory are true. So there is no apparent empirical problem in explaining the success of past theories in terms of approximate empirical adequacy.

But a different sort of problem arises. If success cries out for an explanation, so does approximate empirical adequacy because they are close to each other in a continuum. If “some” cries for an explanation, so does “most.” It would be hard to come up with reason for thinking that “most” does not need to be explained while “some” does. Of course, the instrumentalists can say by fiat that approximate empirical adequacy is a brute fact of nature, not crying out for an explanation. But this move is arbitrary. Why is it that “some” cries out for an explanation whereas “most” does not? The antirealists need to justify the seemingly arbitrary stop in the chain of demand for an explanation. Now, suppose that the antirealists admit that approximate empirical adequacy cries out for explanation. Then, what would be the right explanatory property explaining approximate empirical adequacy? They cannot invoke empirical adequacy because empirical adequacy is hardly available in the light of the pessimistic induction. They cannot invoke approximate truth because that would amount to giving up antirealism. So the antirealists are in a dilemma. They have to stop at approximate empirical adequacy in the chain of demand for an explanation in spite of the fact that it cries out for an explanation, or they

have to invoke empirical adequacy to explain approximate empirical adequacy only to fall prey to the pessimistic induction.

Despite this problem, however, I take this antirealist proposal as the best of the eight antirealist explanations of the success of science. Unlike others, it does not have an empirical or a conceptual problem. It is distinctively antirealistic. Nor is it reduced to absurdity. It is semantically satisfactory too. It is reasonably clear what it means to say that a theory is approximately empirically adequate. In short, approximate empirical adequacy is the best semantic property for the antirealists.

2.8. Observational Similarity

The history of science suggests that some false theories were successful. What could explain the success of those false theories? Stanford's answer is that the explanation consists in the observational similarity between the false theories and true theories:

... the success of a given false theory in a particular domain is explained by the fact that its predictions are (sufficiently) close to those made by the true theoretical account of the relevant domain.
(Stanford, 2000, p. 275, italics in original.)

The Ptolemaic hypothesis, for instance, was successful because it was “able to generate sufficiently similar predictions to those of the Copernican hypothesis” (ibid., p. 274). To put it differently, a false theory is successful because its observational consequences are sufficiently similar to those of the true theory of the same domain. Stanford emphasizes that we do not have to know the content of the true theory to explain the success of the false theory. The explanation works, as long as there is a true theory that is

observationally similar to the false theory. We do not have to know about the content of, for instance, the Copernican hypothesis to explain the success of the Ptolemaic hypothesis:

Notice that the actual *content* of the Copernican hypothesis plays *no role whatsoever* in the explanation we get of the success of the Ptolemaic system: what matters is simply that there *is* some true theoretical account of the domain in question and that the predictions of the Ptolemaic system are sufficiently close to the predictions made by that true theoretical account.
(*ibid.*, p. 274, italics in original)

Stanford's proposal is distinctively antirealistic, because we do not have to believe that the theory whose success is explained is true, and because we do not have to know which of competing theories is true.

My first critical response to Stanford's proposal is that the proposal is silent about the success of true theories, and it is not clear how it can handle such cases. Why is, for example, the Copernican hypothesis successful? If we push Stanford's line of thinking, the story goes as follows: The Copernican hypothesis is successful because its observational consequences are sufficiently similar to those of itself. What is wrong with such an explanation? Anything can be explained this way. A tennis ball is spherical because it is sufficiently similar to itself with respect to shape. The tennis ball is flat again because it is sufficiently similar to itself with respect to shape. These explanations are all vacuous. The explanans is the very explanandum in disguise. The tennis ball's property of being similar to itself with respect to shape is the very property to be explained, *viz.*, the property of being spherical or the property of being flat. In short,

Stanford's scheme of explanation yields vacuous explanations in the cases of the success of true theories.

My second critical response concerns the concept of observational similarity. Does the observational similarity consist in the false theory's being successful, approximately empirically adequate, or empirically adequate? Stanford's following sentence gives us a hint as to how the sufficient observational similarity is to be understood:

...we do indeed explain the success of (revised) Ptolemaic system of epicycles by pointing out how closely its predictions approximate those of the true Copernican hypothesis. (ibid., p. 273)

In other words, a false theory is successful because it is approximately empirically *equivalent* to the true theory. Since a true theory, if it has empirical consequences at all, is necessarily empirically adequate, to say that a false theory is approximately empirically equivalent to a true theory is to say that it is approximately empirically *adequate*. So Stanford's explanation of the success of science can be summed up as the suggestion that a false theory is successful because it is approximately empirically adequate. The proposal is not different from that of Kukla (1996) which invokes approximate empirical adequacy, and I have already pointed out the problems with Kukla's proposal in the previous section.

Stanford might reply that his approach is different from that of Kukla because the fundamental explanatory property in his approach is not approximate empirical adequacy but observational similarity, and observational similarity is a primitive property. So a false theory is successful because it is approximately empirically adequate, and the

approximate empirical adequacy of a false theory is to be explained in terms of the observational similarity between the false theory and its corresponding true theory. It is not the case that the observational similarity is to be explained in terms of the approximate empirical adequacy.

There seems to be, however, something wrong with explaining the approximate empirical adequacy in terms of the observational similarity. The worry can be developed from the following everyday example: I am a son of my parents, and I am similar to my brother with respect to the way we are related to our parents. It sounds natural that I am similar to my brother with respect to the way we are related to a couple (our parents) because, among other things, I am their son. But it sounds odd that I am a son of the couple because I am similar to my brother with respect to the way we are related to them. The second suggestion sounds odd on my account because the relation between me and my parents is the ontological basis for the similarity between me and my brother with respect to the way we are related to our parents. The relation between me and my parents can exist, even if the similarity between me and my brother with respect to the way we are related to our parents does not exist. I am still a son of my parents even if I do not have a brother. But the similarity between me and my brother with respect to the way we are related to our parents cannot exist, unless the relation between me and my parents exists in the first place. I cannot be similar to my brother with respect to the way we are related to a couple (our parents) unless I am their son in the first place. So the similarity between me and my brother is to be explained in terms of the relation between me and our parents, not the other way around.

Now, a false theory is approximately empirically adequate, and it is similar to a true theory with respect to the way they are related to observables. It sounds natural that the false theory is similar to the true theory with respect to the way they are related to observables, because, among other things, it is approximately empirically adequate. But it sounds odd that the false theory is approximately empirically adequate because it is similar to the true theory with respect to the way they are related to observables. The second suggestion sounds odd on my account because the relation between the false theory and observables is the ontological basis for the similarity between the two theories with respect to the way they are related to observables. The relation between the false theory and observables can exist, even if the similarity between the two theories with respect to the way they are related to observables does not exist.³ The false theory is still approximately empirically adequate even if the true theory does not exist (although it is impossible that it does not exist). But the similarity between the false theory and the true theory with respect to the way they are related to observables cannot exist, unless the relation between the false theory and observables exists in the first place. The false theory cannot be similar to the truth theory with respect to the way they are related to observables, unless it is approximately empirically adequate in the first place. So the observational similarity between the two theories is to be explained in terms of the relation between the false theory and observables, not the other way around.

If Stanford takes observational similarity as more fundamental than approximate empirical adequacy, he cannot explain the *success* of the false theory in terms of observational similarity either. Success is a partial ontological basis for observational

similarity. So we should say that the false theory is observationally similar to the true theory in part because it is successful. It is wrong to say that the false theory is successful because it is observationally similar to the true theory. If, however, Stanford takes observational similarity as reducible to approximate empirical adequacy, he can explain the success of the false theory in terms of observational similarity. But then, his approach becomes no different from that of Kukla.

3. Pessimistic Induction against the Antirealist Program

So far, I criticized the eight antirealist explanations of the success of science that I found in the literature. All of them have problems. So what? We can construct a pessimistic induction against the antirealist program. Suppose that for the first time we came across Stanford's paper (2000) titled "An Antirealist Explanation of the Success of Science." Suppose also that we know that its predecessors, the seven antirealist explanations, are problematic. In a situation like this, we can draw the following two pessimistic conclusions:

First, we have a good inductive rationale for predicting that Stanford's theory will turn out to be problematic. The seven previous theories are enough to construct a disastrous induction against Stanford's theory. We do not have to read his paper through to make the prediction. That is, we do not even have to know what the theory says and what evidence Stanford adduces to justify his theory. All we need to know is the historical fact that the seven past philosophical theories of that domain are problematic. The fate of Stanford's theory can be justifiably assessed without regard to what the theory says and what

positive evidence Stanford adduces to justify his theory, however strong the positive evidence might be. All that counts is the historical second-order evidence.

Second, the historical fact that the eight antirealist accounts of the success of science have been proposed and turned out to be problematic is a good inductive rationale for thinking that there are more antirealist theories, hitherto unconceived, to come, and that they are all born problematic. The more antirealists propose antirealist theories to explain the success of science, the stronger inductive rationale we will have for thinking that the next antirealist theory is problematic, and the more hopeless the antirealist program becomes. The antirealists are doomed to propose problematic antirealist theories *indefinitely*. There is no hope at all that someday the antirealists will reach the final destination where an unproblematic antirealist theory will finally emerge.

Stanford might retort that in the case of scientific theories our evidence for approximate truth is success, and the pessimistic induction is an attack on the purported connection between success and approximate truth. But in the case of philosophical theories our evidence for truth is not success, and success does not play any role. So it is not clear what the pessimistic induction against Stanford's philosophical theory is an attack on. Thus, the pessimistic induction against antirealism misfires. The problem with this attempt to salvage the antirealist program is that the evidence for accepting Stanford's philosophical theory is also success. We would not accept his theory unless it *explains* the success of science. If the general connection between success and approximate truth is called into question, then Stanford's inference from the success of his philosophical theory to its truth is also called into question.

Stanford might still argue that only scientific theories are susceptible to a pessimistic induction because philosophical success is different from scientific success. Scientific success is connected with observational support from the world, but philosophical success is not. The reply to this possible move is that philosophical theories might be more susceptible to a pessimistic induction than scientific theories simply because scientific theories are connected with observations while philosophical theories are not. The tenuous connection of philosophical theories with observations might be detrimental rather than helpful to antirealism. So a successful avoidance of the disastrous induction on antirealism requires that the antirealists provide reason to think that the pessimistic induction against philosophical theories is less powerful than the pessimistic induction against scientific theories. I do not see how the antirealists could accomplish this enormous task. Moreover, the antirealists need to engage the details of the pessimistic induction, i.e., they need to pinpoint the problem with the following argument: All the eight antirealist theories explaining the success of science turned out to be problematic. Therefore, the next one to be conceived will turn out to be problematic as well.

Recall that the pessimistic induction against Stanford's positive philosophical theory holds that all we need to know in order to fairly assess Stanford's theory is whether previous philosophical theories of the same domain are problematic. In response, the antirealists might complain that it is an injustice to Stanford's theory to assess its fate without regard for its first-order positive evidence. That is, in order to fairly assess the fate of Stanford's theory, we need to know exactly what evidence Stanford adduces to justify his theory. The reasons are that Stanford's theory might be better than previous

theories, avoiding the pitfalls the previous theories suffered, and that the first-order positive evidence might defeat the second-order pessimistic force. The complaint is fair enough. Then, why not take the same attitude toward scientific theories? In order to fairly assess the fate of, say, the germ theory of diseases, we need to know exactly what evidence supports the theory. The reasons are that the germ theory of diseases might avoid the pitfalls of its predecessors: the humoral theory of diseases and the miasmatic theory of diseases, and the first-order positive evidence might defeat the second-order pessimistic force.

The moral is that the pessimistic induction discussed in Chapter 2 presupposes that what counts in assessing the fate of current scientific theories is only the second-order historical evidence that goes against them. Antirealists do not take into account that current theories might be better than their predecessors, and that the first-order positive evidence for them might defeat the second-order pessimistic force. Their attitude toward current scientific theories backfires on their positive philosophical theories. Their philosophical theories can be justifiably assessed regardless of whether they might overcome the shortcomings of their predecessors, and regardless of whether the positive evidence for them might defeat the second-order pessimistic force.

So far, I assumed that the second-order pessimistic force is real. From now on I challenge this assumption. Recall that the pessimistic induction against the antirealist program holds that the more the antirealists propose antirealist theories, the stronger inductive rationale we will have for thinking that the next antirealist theory is problematic, and the more hopeless the antirealist program becomes. In response, the

antirealists might argue that previous mistakes in a field of philosophy are not negative inductive evidence for an incoming philosophical theory. From the fact that previous antirealist explanations of the success of science are problematic, it does not even *inductively* follow that the next one will be also problematic. Through trials and errors, they might contend, philosophical theories get better and better in the field. There is a crucial difference, they might note, between the case where an apple keeps falling down from a tree and the case where the antirealists keep producing philosophical theories. The difference is that the apple does not learn from the previous experience not to fall down next time, i.e., the present event is independent of previous events. But the antirealists learn from previous experience and they take measures not to make the same mistakes, i.e., the previous event affects the present event. So the antirealists do not commit the gamblers' fallacy when they believe that the next one will be unproblematic, although they are not certain that it will be really so.

I accept this possible move to drive a wedge between previous mistakes and the incoming philosophical theory. But then, again why not take the same attitude toward science? Why take the mistakes in the history of science as negative evidence for an incoming scientific theory? Why not embrace the realist idea that through trials and errors scientific theories became better and better? Like the antirealists, scientists learn from previous experience and they take measures not to make the same mistakes, i.e., the previous event also affects the present event in the case of science. So the realists do not commit the gamblers' fallacy either when they believe that, unlike successful past theories, successful current theories are approximately true.

The pessimistic induction is a paradigm example of what I call “the losers’ fallacy.” The losers’ fallacy occurs when we infer from past experiences that the future will resemble the past. The inference is fallacious in cases in which past events affect future events. Losers, for instance, go to parties instead of studying hard for a mid-term exam. When asked why not prepare for the exam, they claim that the future will resemble the past, i.e., they will fail again as they did before. They just do not see that studying hard makes a difference to the outcome of the exam. The losers’ fallacy is in contrast with the gamblers’ fallacy. The gamblers’ fallacy occurs when we infer from past experiences that the future will be different from the past. The inference is fallacious in cases in which past events do not affect future events.

4. Empirical Superiority of Realism to Antirealism

I claimed in Section 2.7. of this chapter that approximate empirical adequacy is the best semantic property for antirealists. In the present section I attempt to show that unlike approximate truth, approximate empirical adequacy cannot yield an explanation of the coherence of successful theories. This would show that approximate truth explains more phenomena of science than approximate empirical adequacy.

Realists claim that successful theories cohere with each other because they are approximately true. This explanation involves the belief that approximate truth is one of the conditions under which successful theories cohere with each other, and the belief that the coherence of successful theories is evidence for believing that they are approximately true. Analogously, antirealists claim that successful theories cohere with each other

because they are approximately empirically adequate. The explanation involves the belief that approximate empirical adequacy is one of the conditions under which successful theories cohere with each other, and the belief that the coherence of successful theories is evidence for believing that they are approximately empirically adequate. Now, let's critically assess these two antirealist beliefs.

There seems to be nothing wrong with the first belief that approximate empirical adequacy is one of the conditions under which the successful theories cohere with each other. To refute this belief, the realists would have to come up with counterexamples: successful theories that cohere with each other, but nevertheless are not even approximately empirically adequate. I found it hard to come up with such theories. After all, the successful theories that cohere with each other, such as the theory of DNA and the theory of electromagnetism, do seem to be at least approximately empirically adequate. So let's move on to the second belief that the coherence of successful theories is evidence for believing that they are approximately empirically adequate.

The antirealists are inferring from the premise that successful theories cohere with each other to the conclusion that they are both approximately empirically adequate. What is wrong with the inference? The inference goes against the antirealist spirit. Coherence is a certain relation between theories containing theoretical terms, and approximate empirical adequacy is a relation of the observational consequences of a theory to the world. So to cash out the coherence of the theories inevitably involves theoretical terms, whereas to cash out the approximate empirical adequacy of the theories does not necessarily involve theoretical terms. So to infer from coherence to approximate

empirical adequacy is to take some theoretical statements as premises and observational statements as the conclusion. The inference implies that theoretical statements provide some evidential support for observational statements. This goes against the empiricist spirit. For constructive empiricists, theoretical statements are not trustworthy, hence, they cannot serve as premises for observational statements. The starting point of inference is always observational statements, and the ending point of inference is also observational statements. For this reason, approximate empirical adequacy cannot explain coherence.

There is more serious reason for thinking that approximate empirical adequacy cannot explain coherence (Shaughan Lavine, personal communication). The antirealist suggestion that approximate empirical adequacy yields the coherence of successful theories conflicts with what I call “radical underdetermination.” A theory is radically underdetermined just in case it is one of the rival theories that make *radically* different claims about unobservables.⁴ Suppose that we have two groups of rival theories from two neighboring domains. One group of rival theories, making radically different claims about unobservables, are competing with each other in Domain 1. The other group of rival theories, also making radically different claims about unobservables, are competing with each other in Domain 2. Then, it is just implausible that a theory from Domain 1 would cohere with a theory from Domain 2, even if they are empirically adequate. The empirical adequacy is not robust enough to make the two theories cohere with each other. After all, what a theory says about unobservables is only tenuously connected with what it says about observables in both cases. Only (approximate) truth has the required force in such cases. Here is an analogy to illuminate this abstract point. We cannot hold two

lizards, which do not get along with each other, together by tying their tails together.

They will just run away by cutting their tails off from their main bodies. The only way to hold them together is to tie their main bodies together.

The other side of the coin is this. Suppose that we observe that the two lizards are held together. Then, we can conclude that their main bodies are tied together. After all, it is only by virtue of their main bodies' being tied together that they can be held together. Tying their tails does not work. I claimed in the previous chapter that the theory of DNA, the theory of electromagnetism, and the kinetic theory of heat cohere with each other both directly and indirectly. We can conclude that they are approximately true. After all, it is only by virtue of being (approximately) true that they can cohere with each other. Suppose that the three theories are radically underdetermined, i.e., what a theory says about unobservables is only tenuously connected with what it says about observables in each case. Then, the empirical adequacy of the three theories alone cannot do the job of making them cohere with each other. Radical underdetermination backfires on antirealism.

Now, what antirealists might say is that the totality of current science is radically underdetermined.⁵ It is possible that there are unconceived rival sciences which make radically different claims about unobservables. So the fact that the three successful theories – the theory of DNA, the theory of electromagnetism, and the kinetic theory of heat – cohere with each other proves nothing. But it is reminded that the evicted ghosts come back with their friends to annoy the antirealists. More importantly, they come back with newly-born ghosts which are friendly to realists. It is possible that there are

unconceived rival sciences which cohere with the current science. Why do they cohere with each other? The antirealists cannot say that they cohere with each other because they are (approximately) empirically adequate for the foregoing reason. Calling in ghosts to refute realism does not work.

12. Conclusion

The literature on the realism-antirealism debate has focused so far almost exclusively on the status of realism. Antirealism, a competitor to realism, deserves our attention. What emerges from the discussions on the eight antirealist explanations of the success of science is that there are fundamental problems with the pessimistic induction discussed in Chapter 2, and that approximate empirical adequacy is the best semantic property for antirealists. Finally, a trouble with antirealism is that the best antirealist semantic property cannot explain an aspect of science, the coherence of successful theories. Ironically, radical underdetermination undercuts antirealism and helps realism.

End Note

¹ Richard Healey (personal communication) pointed out to me that the selection in the case of scientific theories is artificial rather than natural because scientists consciously select certain theories.

² Shaughan Lavine (personal communication) suggests that empirical adequacy comes in degrees, and what needs to be explained is that successive scientific theories get closer and closer to empirical adequacy. A possible realist explanation of the phenomena is that the successive theories get closer and closer to truth.

³ Jenann Ismael (personal communication) pointed out to me that (approximate) empirical adequacy is a relational property, pertaining to a theory and the world.

⁴ Radical underdetermination is in contrast with what I call “moderate underdetermination.” A theory is moderately underdetermined just in case it is one of the rival theories that make *slightly* different claims about unobservables. An example fitting this definition will be provided in Chapter 5. The significance of moderate underdetermination is that if one of the rival theories is true, the rest of them are approximately true. Of course, there is no guarantee that one of them is true. That is, the truth may lie outside the range of the rival theories. But there is no guarantee either that (even one of) the rival theories are empirically adequate. That is, the empirical adequacy may also lie outside the range of the rival theories. As Psillos (1997) points out, from the mere fact that rival theories produce similar predictions, it does not necessarily follow that they are empirically adequate. In fact, they may be far less than empirically adequate.

⁵ Kukla (1998, p. 65) brings up the underdetermination of the totality of science.

CHAPTER 4

SCIENTIFIC REALISM AND SCIENTIFIC EXPLANATION

1. Introduction

The realism-antirealism debate is closely related to different views on the nature of scientific explanation. One of the standard ways in which antirealists reject realism is to advance antirealist views on the nature of scientific explanation. In this chapter, I argue that antirealist objections coming from that route are not well-motivated. In doing so, I heavily exploit the various claims I already made in the previous chapters. What naturally becomes apparent in the discussion on this issue is the realist view on the relation between scientific explanation and philosophical explanation, and the realist views on the epistemic and social aspects of scientific explanation.

2. Problem of Circularity

Antirealists, both Laudanian and van Fraassenian alike, doubt that IBE is a reliable rule of inference. They do not believe that a scientific theory yielding an explanation of a wide variety of phenomena is likely to be true. The Laudanian antirealists reject IBE on the ground that the history of science is full of successful theories that turned out to be false. The van Fraassenian antirealists, on the other hand, reject IBE on the ground that for any scientific theory, (it is possible that) there are indefinitely many hitherto

unconceived rival theories which are all in accordance with the evidence so far but make incompatible claims that go beyond the evidence.

Now, Putnam (1975) uses IBE to justify realism. He claims that the best explanation of the success of science is that successful theories are typically approximately true and that their key terms typically refer. Not surprisingly, Laudan retorts that “this is a monumental case of begging the question” (1981, p. 45). The idea is that the antirealists are not likely to be impressed by Putnam’s second-order IBE, since they believe that IBE is a problematic rule of inference in the light of the history of science:

If non-realists are chary about first-order abductions to avowedly true conclusions, they are not likely to be impressed by second-order abductions...
(Laudan, 1981, p. 45)

Fine makes a similar point. He argues, having in mind the constructive empiricists’ attacks on the reliability of IBE, that it is viciously circular to use IBE to justify realism:

...the explanationist defense seems a paradigm case of begging the question, involving a circularity so small as to make its viciousness apparent.
(Fine, 1991, p. 82)

I welcome Laudan and Fine’s observation that if the scientists’ use of IBE is problematic, so is the realists’ use of IBE because the philosophical use of IBE is in the same boat with the scientific use of IBE. On this score, however, the realists are no worse off than the antirealists. The antirealists are also using IBE to explain the success of science! Laudan, for instance, claims that science is successful because scientific theories result from robust scientific methods. Fine goes further: The inference from success to instrumental

reliability is an “instrumentalist version of inference to the ‘best’ explanation” (1991, p. 83).

Fine might reply that inference to the instrumental reliability of the best explanation is not viciously circular whereas inference to the truth of the best explanation is because the constructive empiricists’ attacks on IBE are attacks not on the antirealist inference from success to instrumental reliability but on the realist inference from success to truth. The attacks prove, Fine might continue, that the realist inference is problematic while leaving the antirealist inference untouched. This possible move is not tenable in the light of the discussion in Chapter 1, where I argued that the argument from a bad lot and the argument from indifference can also be grounds for rejecting the antirealist inference from success to (approximate) empirical adequacy. If unconceived rival theories block the inference from success to truth, unobserved anomalies block the inference from success to (approximate) empirical adequacy.

Antirealists, not Fine, might argue that they do not employ IBE to explain the success of science because they do not claim that their explanations are the best. In fact, van Fraassen (1980), for instance, does not claim that his account is *better* than the realist one, let alone that his account is the *best*. This point is granted. But the charge of circularity stands. The antirealists use a rule of inference, which I call inference to an explanation (IE, from now on). In IE, a theory inferred may or may not yield the best explanation, and/or no claim is made that it yields the best explanation. Notice that IE is no more reliable than IBE, i.e., the beliefs produced by IE are no more likely to be true than those produced by IBE. Not claiming that an explanation is better than its available

rivals does not make the explanation more likely to be true. So, if IBE is under dispute, all the more so should IE be. If it is viciously circular to use IBE to justify realism, using IE to justify antirealism is even more so. In short, with respect to the problem of circularity, the realist explanation is no worse off than the eight antirealist explanations.

3. Problem of Context-Dependence of Explanation

Van Fraassen's contextual theory of explanation holds that appropriateness of an explanation depends on context, viz., on what the interests of the explainer and the explainee are and what the contrast-class is. Some antirealist philosophers seem to think that context-dependence of explanation is a good ground for doubting the truth of explanation. Goodness of explanation is incapable of showing the link between explanation and reality because goodness is relative to context. No matter how good an explanation might be, it does not follow that a theory used in the explanation is true. Hacking, for instance, does not accept the realist argument from the success of science on the ground that explanations are relative to human interests. We may feel good as a result of certain explanations. But the psychological satisfaction arising from the explanations does not have any evidential force:

Explanations are relative to human interests. I do not deny that explaining – 'feeling the key turn in the lock' as Peirce put it – does happen in our intellectual life. But that is largely a feature of the historical or psychological circumstances of a moment. ... Feeling the key turn in the lock makes you feel that you have an exciting new idea to work with. It is not a ground for the truth of the idea... (Hacking, 1983, p. 53)

He argues that some theoretical entities can be legitimately claimed to be real once we can manipulate them. But he talks as if the realist argument from explanatory success cannot do the job of proving that the theoretical entities are real:

Some people might have had to believe in electrons because the postulation of their existence could explain a wide variety of phenomena. Luckily we no longer have to pretend to infer from explanatory success (i.e., from what makes our minds feel good). (ibid., pp. 271-272)

Kukla's attitude toward explanation is similar to that of Hacking. He also argues that "the intelligibility of explanations is culturally relative," and he endorses Cushing's claim that the intelligibility of scientific theories is a matter of "psychological acclimation" (Kukla, 1996, p. S304). So from the fact that a theory yields an intelligible explanation it does not follow that it is true. Kukla offers two historical examples to support the claim that intelligibility does not have evidential force:

It used to be thought that teleological explanations give us understanding. Now we want to know the mechanism behind the appearance of teleology. More to the point, explanations that posit action at a distance have been considered intelligible and unintelligible in turn by successive generations. (ibid., p. S304)

A type of explanations was considered intelligible in a certain culture but considered unintelligible in others. So the fact that a type of explanation is intelligible shows nothing but that our minds have become adapted to that type of explanation. It does not show that there is a link between that type of explanation and aspects of reality.

It is not clear whether van Fraassen's contextual theory of explanation is what motivated Hacking and Kukla to embrace a pessimistic view about the evidential force of

explanation, although they claim that appropriateness of explanation depends on “historical circumstance” and “culture” respectively. But it is not important for my purpose here to determine exactly what influence van Fraassen had on Hacking and Kukla. Suppose, for the sake of argument, that Hacking is right that the psychological satisfaction that arises from some explanations is not a ground for their truth because psychological satisfaction is relative to historical circumstance. Suppose also that Kukla is right that intelligibility is not a ground for the truth of explanations because intelligibility is relative to culture. Now, let’s ask this question: Are the eight antirealist explanations in the previous chapter context-independent? Are they independent of human interests and cultures? Not at all! Antirealists may derive psychological satisfaction from their explanations, and their explanations may be intelligible to them.¹ But it does not follow that their explanations are true. The psychological satisfactions and intelligibility of their explanations are relative to human interests and cultures. In short, if the realist explanation of the success of science and the scientists’ scientific explanations of phenomena are to be dismissed for the reason that explanation depends on human interests and cultures, so are the antirealist explanations. The pessimistic view of the evidential force of explanation backfires on the antirealist explanations.

Now, is it true that psychological satisfaction and intelligibility do not have any evidential force for the truth of explanations? Recall that Kukla provides two historical examples where scientific explanations, once considered to be intelligible, turned out to be false. Kukla concludes that intelligibility of explanations shows nothing but psychological acclimation. This kind of inference has an absurd consequence. Suppose

that a young medical scientist came up with a new medical theory and the theory yields an explanation of puzzling phenomena. The young medical scientist introduces the theory to an eminent expert, perhaps a Nobel laureate, in the field, and also to a non-expert, perhaps a charlatan. It turns out that the explanation is intelligible and satisfactory to the expert but unintelligible and unsatisfactory to the non-expert. Do we seriously want to say that the intelligibility and the psychological satisfaction on the part of the expert prove nothing? To say yes is to treat the expert and the non-expert equally, which, I believe, antirealists would take to be unacceptable.

Now, what can realists say about Kukla's historical observation that we had fluctuating attitudes toward teleological explanation, mechanical explanation, local explanation, and nonlocal explanation? I believe that all that Kukla's historical examples prove is that our cognitive faculties are fallible, i.e., we sometimes make false judgments about reality. The examples do not establish the grandiose antirealist claim that intelligibility is incapable of revealing the link between explanation and reality. Consider that Aristotelians thought that phenomenal qualities like sound and taste are inherent in physical objects. Should we conclude from this that there is no link whatsoever between perception and reality? All that the historical fact shows is that perception is fallible. The antirealists should agree with me on this account. Otherwise, I do not see how they could go about proving that their explanations of the success of science are true.

4. Problems with the Principle of Economy

Van Fraassen claims the virtues of explanation provide at best “reasons for acceptance of the theory, but not for belief” (ibid., p. 294). In other words, the fact that a theory explains a wide variety of phenomena at best gives us reason to believe that it is empirically adequate. It does not give us reason to believe that it is true. The principle of economy is at the heart of the grounds for this claim. He puts the first ground as follows:

And the grounds of the argument were simple. To be more explanatory, the theory must be more informative... But to contain more information is, to put it crudely, to have more ways of being false and, hence, to be no more likely to be true and, hence, to be no more worthy of credence. (1985, p. 294)

The idea is that truth contains more information than empirical adequacy. One takes more epistemic risk by being committed to truth than to empirical adequacy. So empirical adequacy is more worthy of credence than truth. The second ground for settling for empirical adequacy as opposed to truth is that truth does not “brave a more severe test” than empirical adequacy, and “we can have evidence for the truth of a theory only via evidential support for its empirical adequacy” (ibid., p. 255). In short, a high explanatory power provides reason to settle for at best empirical adequacy, because empirical adequacy contains less information than truth, and because empirical adequacy is more severely tested than truth by observations. The belief in truth is “supererogatory” (ibid., p. 255).

Suppose for the sake of argument that van Fraassen is right that high explanatory power does not give us reason to believe that an explanatory theory is true. Then, we do not have to believe that the eight antirealist philosophical theories explaining the success of science are true, although they might have high explanatory powers. Recall that van

Fraassen's philosophical theory holds that only successful theories survive. The philosophical theory explains why some scientific theories are successful. But we do not have to believe that only successful theories survive. The fact that the philosophical theory explains the success of science well at best provides reason to believe that it is empirically adequate, whatever the empirical adequacy amounts to in this case. The belief in the truth of the philosophical theory is supererogatory.

Let me draw a moral from all this. It is granted that the principle of economy favors instrumentalism over realism. Instrumentalists take less chance of being wrong than realists. But suppose that instrumentalists advance positive theories of their own in a social world where every cognitive agent plays a fair game. In a situation like this, the instrumentalists face the following pragmatic problems: Instrumentalists' epistemic colleagues, whose theories have received instrumental treatments from the instrumentalists, will in return take instrumentalist attitudes toward instrumentalists' positive theories. They will refuse to believe that instrumentalists' positive theories are true. So what? We, as cognitive agents, are interested not only in being epistemically secure but also in disseminating to others what we take to be true. We can achieve the first goal via instrumentalism but only at the cost of distancing ourselves from the second goal.

Here is another pragmatic disadvantage for the instrumentalists. Suppose that the instrumentalists came up with positive theories of their own device, and that they feel confident that they are true. The problem in a situation like this is that they are not entitled to believe that their positive theories are true. It is anti-instrumental for the

instrumentalists to take realist attitudes toward their own theories. That is, they should also take instrumental epistemic attitudes toward their own theories in conformity with the instrumentalist spirit. This pragmatic disadvantage with instrumentalism looms large when we feel confident that the theories of our own device are true.

In short, when the instrumentalists put forward positive theories, nobody, including the authors themselves, believes that they are true. What follows is that instrumentalism is not an ideal epistemic policy for “theory-generators”: cognitive agents whose main cognitive activity consists in generating theories of their own rather than taking attitudes toward theories of their cognitive colleagues. But instrumentalism is an ideal epistemic policy for “attitude-takers”: cognitive agents whose main cognitive activity consists in taking attitudes toward theories of their cognitive colleagues rather than in generating theories of their own. Now, let me turn to a pragmatic disadvantage with instrumentalism that would make even the attitude-takers think that instrumentalism is not an attractive epistemic policy.

5. Belief and the Act of Giving an Explanation

Van Fraassen claims that an explainer can coherently explain something without believing that a theory invoked is true:

Thus acceptance involves not only belief but a certain commitment. ... It determines the terms in which we shall seek explanations. If the acceptance is at all strong, it is exhibited in the person's assumption of the role of explainer, in his willingness to answer questions *ex cathedra*. (van Fraassen, 1980, p. 12)

An explainer can give an explanation of something with a mere acceptance of a theory, viz., the belief that the theory is empirically adequate. The important point for my purpose here is that a van Fraassenian instrumentalist need not believe that a theory is true even when he uses the theory to explain phenomena.

For the sake of argument, suppose that van Fraassen is right that one can coherently explain something without believing that the explanatory theory is true. Now, recall that antirealists explain the success of science in terms of various philosophical theories. For instance, van Fraassen's theory holds that only successful theories survive. Now, the antirealists are skeptical, if van Fraassen is right, that their explanatory philosophical theories are true. They do not believe, for instance, that only successful theories survive. Then, why should we, the antirealists' audience, believe that their theories are true, when even the authors themselves do not believe that they are true? As explainees, we are entitled to reject the truth of the antirealist theories.

Now, is van Fraassen right that we can coherently explain something without believing that our explanatory theory is true? Contrary to van Fraassen, I believe that we cannot explain phenomena, using a theory, unless we believe that the explanatory theory is at least approximately true. That is, a belief in at least the approximate truth of the explanatory theory is built into the act of giving an explanation coherently.² I claimed in the previous chapter that there is something wrong with uttering the following two sentences:

(A) Only successful theories survive.

(B) I do not believe that only successful theories survive.

The speech act is odd, I believe, because the act of giving an explanation coherently requires that an explainer believe that the theory invoked is at least approximately true. This point can be illuminated from a general picture of what goes on between an explainee and an explainer in science. Let me start with the explainee's side. When the explainee asks the explainer to explain something, the explainee is interested in obtaining information on the matter. He is interested not in just any information but in at least approximately accurate information. The explainee knows that he has a chance to fulfill his epistemic goal, only if the explainer provides information the explainer takes to be at least approximately accurate. If the explainer puts forward information that even he himself does not take to be at least approximately accurate, then the explainee does not even have a chance to fulfill his goal. Now, let's move on to the explainer's side. When the explainer provides an explanation, he knows that the explainee has the epistemic goal of obtaining at least approximately accurate information, and that what is expected of him is to provide information that he takes to be at least approximately accurate. The explainer also knows that the explainee will no longer seek explanations from him once the explainee finds out later that he, the explainer, habitually provides information that he, the explainer, does not take to be at least approximately accurate. I believe that this is what goes on between the explainee and the explainer when the explainee asks for an explanation and the explainer provides an explanation in science. What is important for my purpose here is that to give an explanation involves recognizing the explainee's

epistemic goal, and helping the explainee to fulfill the goal by providing information the explainer takes to be at least approximately true. Some examples might be useful to illustrate that a belief in the approximate truth of an explanatory theory is built into a coherent act of giving an explanation.

Suppose, for example, that navigators are still using the two-sphere model to navigate around the world. They could use the model to predict the positions of celestial bodies. But they would not *explain* the positions of celestial bodies in terms of the model because they do not believe that the model is even approximately true. They would refuse to utter sentences like “Stars revolve around the celestial pole 15° an hour because they are firmly fixed on the giant celestial sphere which turns 15° an hour.” How could they utter sentences like that when they do not believe that the model is approximately true? Of course, the navigators use the model to *predict* the positions of the stars with respect to the horizon. But this proves only that the act of giving predictions is fundamentally different from the act of giving an explanation. The predictor is not required to believe that the theory he uses to predict is approximately true, whereas the explainer is required to believe that the theory he uses to explain is approximately true.

Here is another example. Suppose that Einstein gives an explanation of the photoelectric effect in terms of the properties of photons, and that after he finishes explaining he makes an additional claim that photons are real. Hacking notes that “To add ‘and photons are real,’ after Einstein has finished, is to add nothing to the understanding” (1983, p. 54). He takes this example to show that the reality of photons is not a part of the explanation, and that the explanation cannot be a ground for believing

that photons are real. Contrary to Hacking, I take the example to show that the *claim* that photons are approximately real is built into the act of giving an explanation coherently. Einstein already claimed that something like photons is real in the course of giving the explanation. The additional claim that photons are real is almost a repetition of what has already been said. It is not surprising that the additional claim adds nothing to the understanding.

Suppose that I am right that a belief in the approximate truth of an explanatory theory is built into a coherent act of giving an explanation. Then, instrumentalists are in a dilemma. They should either announce or not announce to their explainees, after finishing an explanation, that they do not believe that a scientific theory they invoked to explain phenomena is approximately true. If they announce it, they commit an odd speech act, leaving their explainees puzzled. If they do not announce it, they commit an act of epistemic irresponsibility in relation to the explainees. The explainees will no longer seek explanations from the instrumentalists, if they later find out that the instrumentalists do not believe that the explanatory theory is approximately true. The explainees know that they cannot fulfill their epistemic goal of obtaining approximately accurate information from the instrumentalists. In short, the instrumentalists commit either an odd speech act or an act of epistemic irresponsibility, when they use a scientific theory to explain phenomena. This problem prohibits instrumentalists from using a scientific theory to explain puzzling phenomena around us. This is the pragmatic disadvantage with instrumentalism that makes even the attitude-takers, the second kind of cognitive agents, think that instrumentalism is not an attractive epistemic policy.

How about philosophical theories? Are the antirealists also prohibited from using philosophical theories to explain the success of science? The antirealists might claim that philosophical theories are not in the same boat with scientific theories, and that they believe that their philosophical theories explaining the success of science are true. So they commit neither an odd speech act nor an act of epistemic irresponsibility. This may be the only way for the antirealists to get around the dilemma. If they take this route, however, they need to tell us why it is that their philosophical theories, having high explanatory powers, deserve their beliefs, while scientific theories, also having high explanatory powers, do not.

The antirealists might say, after accepting that their philosophical position is in trouble, that the realist position is also in trouble. They might point out that current physical theories typically involve idealizations, and that those theories are not even intended to be true. So the realists are not in a position to use them to explain puzzling phenomena either because they know that they are false. What could the realists say in response? The realists could reply that the physical theories, although not completely true, are approximately true, if the idealizations approximate the actual world. If the ideal gas law, for instance, is exactly true of the ideal gases, which approximate the actual gases, then the ideal gas law is approximately true (Psillos, 1999, p. 276). I believe that Cartwright would agree with Psillos that the physical theories involving idealizations can be considered as being approximately true. She talks as if models in physical theories approximate reality, when she claims that “Approximations and adjustments are required whenever theory treats reality” (1983, p. 13). Suppose that Psillos is right that successful

physical theories involving idealizations are approximately true. Then, the realists are not in much trouble. They could use the physical theories to explain phenomena, and add, after finishing the explanation, that something like that actually goes on in the world. In short, the realists can coherently invoke physical theories to explain phenomena because they believe that they are approximately true.

6. Conclusion

The antirealist objections to realism surrounding the nature of scientific explanation are not well-motivated. Being economical is good when I am economical toward others; it is bad when I and others are economical toward me. Moreover, scientific explanation is available only to believers.

End Note

¹ Interestingly, Kukla claims that empirical adequacy as an explanation of the success of science is unintelligible (ibid., p. S304). If it is unintelligible, however, he owes us an account of on what ground we can infer from success to empirical adequacy.

² Note that this claim is very different from the claim that a theory has to be at least approximately true to yield an explanation. I welcome the suggestion that a theory need not be even approximately true to yield an explanation.

CHAPTER 5

VINDICATION OF 'APPROXIMATE TRUTH'

1. Introduction

Realists explain the success of science in terms of approximate truth, and believe that successful scientific theories are approximately true. A lot of writers have tried to give satisfactory theoretical accounts of approximate truth in the literature. Psillos (1999, Chapter 11) reviews them, and ends up claiming that they have failed. This chapter aims to vindicate the predicate, 'approximate truth.' In the first half of the chapter, I argue that 'approximate truth' is viable as long as there are clear cases and counter-cases of approximately true descriptions, and that antirealists are also in need of the concept of approximation. Also, I explain how Hilpinen/Lewis's theoretical account of approximate truth could accommodate the clear cases of approximately true descriptions. In the second half of this chapter, I discuss how approximate truth and success, the explanans and the explanandum of the no-miracle argument are related to each other. Finally, I reply to Stanford's worry that approximate truth cannot explain the success of science.

2. 'Approximate Truth' is Viable.

Suppose that anthropologists enter a primitive society to study a tribe. They discover that the tribe has only two color terms: 'white' and 'black.' The members of the tribe call white what the anthropologists call white. But for them, all the rest of colors are black.

The anthropologists point to a rabbit, grass, the daytime sky, and night sky, and claim that they are gray, green, blue, and black respectively. The members of the tribe insist that they are all black. The anthropologists also discover that the members of the tribe agree with them that there is a perceptual difference between the colors of those objects. That is, the members of the tribe perceptually appreciate these different colors as the anthropologists do. In a situation like this, the dispute between the anthropologists and the members of the tribe is only over terminology. The anthropologists are fighting for 'gray,' 'green,' and 'blue,' and the members of the tribe for 'black.' It is just that the anthropologists and members of the tribe have different classification systems. There is no way the anthropologists can show that they are right, and that the members of the tribe are wrong. All they can do is to argue that their classification system is more useful than that of the members of the tribe, because it yields more fine-grained descriptions.

The situation is similar to the one surrounding the concept of approximate truth. The realists' conceptual scheme includes 'complete truth,' 'approximate truth,' and 'complete falsity' for evaluating a description. The antirealists' conceptual scheme contains only two predicates, 'true' and 'false,' for evaluating a description. It does not have 'approximate truth.' Now, consider the following list of statements:

(1)

Bill Clinton has ten daughters.

Bill Clinton is the husband of Hilary Clinton.

Bill Clinton was a president of the US.

Bill Clinton was a governor of Arkansas.

....

Notice that the first member of the list is radically false, but the rest of them are all true.

More true statements about Bill Clinton can be added to the list indefinitely. Now, realists classify the description as a whole as being approximately true. Antirealists classify it as being false on the ground that one of the components is false. In a situation like this, there is no fact of the matter as to which side is right or wrong. The dispute is merely over terminology. After all, the antirealists see, as the realists do, the difference between (1) and (2):

(2)

Bill Clinton has ten daughters.

Bill Clinton is a Korean.

Bill Clinton is a Harvard graduate.

Bill Clinton was a governor in Arizona.

Notice all the members of the second list are false. So the antirealists classify the description as a whole as being false. They classify both (1) and (2) as belonging to the same category of being false. But for the realists, (1) and (2) belong to different categories: (1) is approximately true and (2) completely false. In a situation like this, the realists cannot prove that they are right, and that the antirealists are wrong. All they can

do is to point out that their conceptual scheme is richer, yielding more fine-grained descriptions than that of the antirealists.

It is worth noting that (1) is not intended to be a representative example of approximately true description. There is another sort of approximately true description. Consider the following set of statements:

1*:

Bill Clinton is 180cm tall.

Bill Clinton is 80kg.

Bill Clinton's IQ is 120.

...

Suppose that Bill Clinton's height is exactly 180.3cm, his weight is exactly 79.8kg, and his IQ is exactly 121, and so on. Then, each member of the set is approximately true, although, strictly speaking, they are false. Since all members are individually approximately true, 1* as a whole is approximately true.

The antirealists might object that the concept of approximate truth is still problematic because a non-arbitrary line could not be drawn between approximate truth and complete falsity. In other words, it is not clear exactly how close a description should be to the complete truth in order for it to be classified as approximately true. The point is granted, and I have no solution to this problem. But so what? All the objection shows is that 'approximately true' is vague. The predicate is viable, as long as there are clear cases of

approximately true descriptions, clear cases of completely false descriptions, and clear cases of completely true descriptions. (1) and 1* are clear cases of approximately true description; (2) is a clear case of a completely false description.

The move I am making here about the concept of approximate truth is of exactly the same kind as the one van Fraassen makes about the concept of observables. The concept of observables plays an indispensable role in the formulation of constructive empiricism. When Maxwell charges that 'observable' is a problematic predicate because no non-arbitrary line could be drawn between observables and unobservables, van Fraassen replies that the vague predicate is "usable provided it has clear cases and clear counter-cases" (1980, p. 16). He goes further: If a vague predicate is not to be used, then every natural language is not to be used. I must add that the antirealists cannot even formulate some objections against realism without the vague predicate 'observables.' Those antirealist objections include pessimistic induction, the problem of underdetermination, and the theory-ladenness of observations. So both the realists and the antirealists have to live with vague predicates.

It might be objected that the realists should not use the predicate 'approximate truth' in the realism-antirealism debate because we do not have good evidence to believe that a current scientific theory is approximately true. The problem with this objection is that van Fraassen uses the predicate 'true' all the time when he argues against realism, although he does not believe that we have good evidence to believe that any of our current scientific theories is true. Laudan also uses the predicate 'approximately true' all the time when he launches a historical challenge to realism, although he is also skeptical that a

successful current scientific theory is approximately true. So, if the realists' use of 'approximate truth' is illegitimate on the ground that we do not have good evidence to believe that a current scientific theory is approximately true, so are the antirealists' uses of 'true' and 'approximately true,' and their objections against realism cannot even be formulated.

It might be further objected that (1) and 1* do not constitute a scientific example, and that the realists have not yet provided a real scientific example of an approximately true theory. Well, the realists might use the example van Fraassen uses to illustrate the problem of underdetermination. Suppose that Newton's theory of motion is exactly true, and that it consists of the three laws of motion, the law of gravity, and the hypothesis that the center of the gravity of the solar system is at rest with respect to absolute space. A rival theory, having the three laws of motion and the law of gravity as its components, claims that the center of gravity moves, say, 10 km/h with respect to absolute space. We can generate infinitely many rival theories by varying the absolute velocity of the center of the gravity. The rival theories are all observationally equivalent but incompatible with each other. Now, suppose that scientists were not confident of the absolute velocity of the center of the gravity of the solar system. Then, I contend that the rival theories are all approximately true because they share the important assumptions: the three laws of motion and the law of gravity. If the antirealists refuse to classify the rival theories as being approximately true, and if they insist that they are all false, the dispute is only terminological.

In Chapter 3 I distinguished between two forms of underdetermination: radical underdetermination and moderate underdetermination. A theory is radically underdetermined just in case it is one of the rival theories that make radically different claims about unobservables. A theory is moderately underdetermined just in case it is one of the rival theories that make slightly different claims about unobservables. Newton's theory of motion in the above case is moderately underdetermined.

3. Hilpinen/Lewis's Theoretical Account can Handle Clear Cases.

The foregoing clear cases of approximately true descriptions can be adequately accommodated by Hilpinen/Lewis's possible world account of approximate truth. Lewis summarizes the possible world approach as follows:

Risto Hilpinen has proposed that we might explain this closeness to the truth (or 'truthlikeness' or 'verisimilitude') in terms of closeness of possible worlds. A theory is close to the truth to the extent that our world resembles some world where that theory is exactly true. A true theory is closest to the truth, because our world is a world where the theory is true. (Lewis, 1986, p. 24)

What is attractive about the possible world approach is that it can handle both observational statements and theories. Suppose that John is exactly 178.3 cm tall, and that Newton's theory of motion is exactly true. Then, the possible world approach has the desired results that it is approximately true John is 178cm tall, and that a foregoing rival theory of motion is approximately true. On the possible world account, it is approximately true that John is 178cm tall because the possible world where John is exactly 178cm tall is close to the actual world where he is exactly 178.3cm tall. Also, the

rival theory of motion is approximately true because the possible world where it is exactly true is close to the actual world where the original theory is exactly true. Let me say one more thing about this example. Suppose that we have two rival theories of motion: T_1 claims that the absolute velocity is 10 km/h, and T_2 claims that it is 20 km/h. Our intuition says that T_1 is closer to the truth than T_2 is. The possible world approach agrees with our intuition. T_1 is closer to the truth than T_2 on the possible world account because the actual world is closer to the possible where T_1 is exactly true than to the possible world where T_2 is exactly true.

As is shown above, the concept of similarity plays a central role in the possible world approach. The distance between the actual world and the possible world is a function of the degree of similarity between the two worlds. But what is similarity? How are we to understand similarity? Lewis's answer is that similarity is a primitive property (*ibid.*, p. 176). The concept of similarity is not analyzable in terms of more fundamental concepts. I must add that there are other theorists who take the concept of similarity to be primitive. For instance, Giere, an advocate of the semantic view of scientific theories, claims that a scientific theory is families of models, and that the relationship between a model and a real system is similarity (1988, p. 80-81). All he says about the concept of similarity is that "human cognition and perception operate on the basis of some sort of similarity metric" (*ibid.*, p. 81).

Another objection to the possible world approach is that it is not clear exactly how close a possible world should be to the actual world for a theory to be approximately true. I am not sure how serious the objection is. Given that our intuition is obscure about the

boundary between approximate truth and complete falsity, a satisfactory theoretical account of approximate truth should also be fuzzy about the boundary. All that is required of a satisfactory theoretical account of approximate truth is to explain why clear cases of approximately true descriptions are approximately true. Of course, any theoretical account that cannot handle the clear cases of approximately true descriptions would be in trouble. But the possible world account can handle those cases.

The antirealists might not be satisfied with what I have said so far about approximate truth. But I must add that they are also in need of the concept of approximation for two reasons. First, they should settle for approximate empirical adequacy as opposed to empirical adequacy in the light of the pessimistic induction. If successful current theories are fated to run into anomalies, as the pessimistic induction suggests, then at best, they should believe that successful current theories are approximately empirically adequate. It is hopeless to give a formal definition of 'approximate empirical adequacy.' No non-arbitrary line can be drawn between approximate empirical adequacy and complete empirical inadequacy. In other words, it is not clear exactly what percentage of observational consequences have to be true in order for a theory to be considered to be approximately empirically adequate.

Second, a prediction statement can be said to be at best approximately true in some cases. Suppose that an experiment was carried out correctly, and yet the real experimental value fell slightly outside the range of expected values. Then, at best, the antirealists can believe that the observational consequence is approximately true. In response, they could take a moderate position about the concept of approximate truth. That is, the concept of

approximate truth can be legitimately applied to observational statements but not to theories. Their refusal to apply the concept to theories could be grounded on their epistemological worry that we do not have enough evidence to believe that they are approximately true. But this moderate position is not attractive to antirealists like the constructive empiricists. The constructive empiricists believe that theories are true or false, depending on whether or not they correspond to the world, although they do not believe that we have enough evidence to believe that they are true. Then, so far as I can tell, there is no good reason for them to take a different attitude toward approximate truth.

4. Model of Explanation for Realists and Antirealists

Realists explain the success of science in terms of approximate truth. Question: Exactly how is the explanans, approximate truth, related to the explanandum, success? Is an approximately true theory necessarily successful or probably successful? Laudan supposes that for the realists an approximately true theory is necessarily successful, and then accuses them of not having a definition of approximate truth according to which an approximately true theory is necessarily successful:

Whether there is some definition of approximate truth which does indeed entail that approximately true theories will be predictively successful (and yet still probably false) is not clear. (1981, p. 32)

Laudan criticizes Popper's definition for not entailing that an approximately true theory is successful:

Indeed, it is entirely conceivable that a theory might be approximately true in the

indicated sense and yet be such that *all* of its thus far tested consequences are false. (ibid., p. 31)

Laudan's point is granted that an approximately true theory is not necessarily successful. After all, an approximately true theory is not necessarily empirically adequate, i.e., it is possible that *some* of its observational consequences are false.

It is wrong, however, for Laudan to suppose that for realists an approximately true theory is necessarily successful. McMullin, Niiniluoto and Putnam, whom Laudan quotes in his paper (1981), are all claiming that approximate truth yields the best *explanation* of the success of science. The realists are not claiming at all that an approximately true theory is necessarily successful:

For just a small sampling of this view, consider the following: 'The claim of a realist ontology of science is that the only way of explaining why the models of science function so successfully... is that they approximate in some way the structure of the object' (McMullin 1970, pp. 63-64); 'the continued success [of confirmed theories] can be *explained* by the hypothesis that they are in fact close to the truth...' (Niiniluoto forthcoming, p. 21); the claim that "the laws of a theory belonging to a mature science are typically approximately *true*...[provides] an *explanation* of the behavior of scientists and the success of science' (Putnam 1978, pp. 20-21). (Laudan, 1981, footnote, p. 30)

Laudan seems to presuppose that the deductive-nomological model of explanation is the only model of explanation available to realists. But the presupposition is false. Realists could use the inductive-statistical model of explanation where an explanandum probably follows from the explanans. Consider the following example of an inductive-statistical model of explanation by Hempel (1965, p. 394):

Most patients with a streptococcal infection recover when treated with penicillin.

John, a patient with a streptococcal infection, is treated with penicillin.

∴ John will recover from the streptococcal infection.

In this case, it is entirely conceivable that John, treated with penicillin, does not recover from the infection. The treatment makes it likely that the John will recover from the infection. Yet, the treatment explains why John recovered, if he did. Likewise, it is entirely conceivable that all of the thus far tested consequences of an approximately true theory are false, i.e., that an approximately true theory is not successful. Yet, approximate truth could explain success, if the approximate truth of a theory makes it likely that the theory will be successful. The realists' model of explanation of the success of science is inductive-statistical in the sense that the explanandum *probably* follows from the explanans.

With respect to the problem Laudan raises against the concept of approximate truth, the realist explanation of the success of science is not worse off than Laudan's explanation of the success of science. According to Laudan (1984a), science is successful because successful scientific theories result from robust experimental methods. He does not have a formal definition of robust methods that would entail that theories selected by robust methods are successful. All he does to illuminate this concept is to offer a couple of examples of robust methods: controlled experiment and double-blind experiment. This pair of examples hardly constitutes a formal definition. Moreover, Laudan's explanation

of the success of science also has to be inductive-statistical. The reason is that, as he notes, robust methods do not always pick out successful theories:

The methods of science are not necessarily the best possible methods of inquiry (for how could we conceivably show that?), nor are the theories they pick out likely to be completely reliable. (1984a, p. 102)

Notice that he claims that the theories robust methods pick out are not completely reliable, i.e., the theories sometimes make false predictions. So it is entirely conceivable that the theories robust methods select are not successful.

The best antirealist explanation of the success of science, the one invoking approximate empirical adequacy, is no better off than the realist explanation and Laudan's explanation. Given that some observational consequences of an approximately empirically adequate theory are false, it is entirely conceivable that all of its thus far tested consequences are false, i.e., an approximately empirically adequate theory is not necessarily successful. The antirealists can say at best that an approximately empirically adequate theory is probably successful. So the antirealists' model of the explanation of the success of science has also to be inductive-statistical.

When I claim that an approximately true theory or an approximately empirically adequate theory is probably successful, I assume, for the sake of simplicity, that all the semantic and external conditions are met. The semantic conditions are the following two conditions: First, a theory has observational consequences. Unless it has them, it does not even have a chance to be successful. Second, the auxiliary assumptions and statements of initial conditions are true. The hypothetico-deductive model of confirmation tells us that

an observational consequence is not deducible from a main theory alone. Auxiliary assumptions and statements of initial conditions have to be supplied to the main theory to entail an observational consequence. So the observational consequences of even a true theory need not be true, unless the auxiliary assumptions and the statements of initial conditions are true. In addition to those two semantic conditions, external conditions have to be met in order for a theory to be successful. By ‘external conditions,’ I mean, for instance, that we have financial and technological means to ascertain that the observational consequences are true. I am assuming that all these conditions are met when I claim that an approximately true theory or an approximately empirically adequate theory is probably successful.

5. Approximate Truth does Not Yield a Vacuous Explanation.

Suppose that rival theories, one being true and the others being false, make similar predictions. Question: Why do the false theories produce similar predictions as the true one? What could possibly explain why they are as successful as the true one? Leplin’s answer is that the false theories share a structural similarity with the true one, i.e., they are approximately true:

... false, successful theories bear some connection or similarity to the true one beyond predictive success, which explains their predictive success. (1997, p. 14)

Leplin does not specify what the structural similarity consists in. But we could say that the structural similarity consists in shared laws and shared posits. Recall that Newton’s original theory shares the three laws and the law of gravity with the rival theories. I will

argue at some length in the next chapter that the theoretical terms of the past theories on Laudan's list *approximately* refer to posits of current theories.

Now, Stanford agrees with Leplin that there is a structural similarity between rival theories. But he rejects Leplin's claim that a structural similarity explains why false theories produce similar predictions to those of the true one. The ground for the rejection is "not that there is no such structural similarity, but rather that such structural similarity is so easy to come by as to be explanatorily vacuous" (2000, pp. 273-274). Such structural similarity is easy to come by because, for any two theories that make similar predictions, there is bound to be a structural similarity between them:

...between any two theories that make similar predictions over a domain of any significant extent, there is sure to be something we could fasten onto as a structural similarity or isomorphism between them. (Indeed, this can typically be achieved by working backwards from their systematic range of similar predictions: something we could characterize as a structural similarity or isomorphism is bound to show up.) (ibid., p. 274)

Note that Stanford's claim that a structural similarity is easy to come by does not require that we know which of the rival theories is true. That is, even if we do not know which of the rival theories is true, we know that there is a structural similarity between them.

Now, what are we to make of Stanford's claim that approximate truth yields a vacuous explanation because it is easy to come by? It seems to me that it is one thing that an explanation is vacuous; it is quite another that an explanans is easily attainable. From the fact that an explanans is easily attainable, it does not follow that an explanation is vacuous. Vacuousness arises only when an explainer repeats an explanandum usually in disguise. The realist explanatory property, approximate truth, is not identical at all with

success. Approximate truth concerns unobservable states of affairs, whereas success concerns only observable states of affairs. So the realist explanation is not vacuous at all, no matter how easy it is to obtain approximate truth. Now, suppose, for the sake of argument, Stanford is right that vacuousness occurs when an explanans is easily attainable. We can, then, turn this claim against Stanford's own explanation of the success of science according to which a false theory is successful because it is observationally similar to a true theory. The observational similarity is easily attainable because for any successful false theory, there is bound to be a true theory, which is observationally similar to the false theory. It follows that Stanford's explanation is vacuous if the realist explanation is. The charge backfires.

6. Conclusion

The key realist predicate, approximate truth, is somewhat problematic. But the realist ambition to explain success in terms of approximate truth remains intact. 'Approximate truth' is viable, because there are clear cases of approximately true descriptions, and because Hilpinen/Lewis's theoretical account of approximate truth can handle those clear cases. The discussion of Laudan's objection to the concept of approximate truth makes it clear that when realists and antirealist explain the success of science, both of their models of explanation should be inductive-statistical.

CHAPTER 6

APPROXIMATE TRUTH VS. EMPIRICAL ADEQUACY

1. Introduction

Suppose that the observable-unobservable distinction is tenable. Suppose also that realists believe that successful theories are approximately true, and antirealists believe that they are empirically adequate. Whose belief is more likely to be false? Ladyman et al. think that the realists take more epistemic risk than the antirealists:

Certainly the realist takes an extra epistemic risk by believing the background theories to be (approximately) true rather than only empirically adequate.
(Ladyman et al., 1997, p. 308)

I think otherwise. In this chapter I argue that, contrary to what Ladyman et al. believe, more epistemic risk is involved in being committed to empirical adequacy than to approximate truth. I critically evaluate van Fraassen's argument that realists take more epistemic risk than antirealists, and argue that empirical adequacy is harder to come by than approximate truth in the light of the pessimistic induction and the realist responses to it. But before I embark on that task, I clear up the issue of whether constructive empiricism is committed to the position that successful theories are empirically adequate.

2. Is Constructive Empiricism Committed to Empirical Adequacy?

It is not clear that constructive empiricism is committed to the position that successful scientific theories are empirically adequate. There are places in van Fraassen's

publications where he implies that he is willing to infer from success to empirical adequacy. But in other places he talks as if he remains neutral.

Van Fraassen claims that the high explanatory power of a theory provides reason to believe that the theory is empirically adequate, although it does not provide reason to believe that the theory is true, in the following places:

When we decide to choose among a range of hypotheses, or between proffered theories, we evaluate each for how well it explains the available evidence. I am not sure that this evaluation will always decide the matter, but it may be decisive, in which case we choose to accept that theory which is the best explanation. But, I add, the decision to accept is a decision to accept as empirically adequate. (1980, p. 71)

That a theory provides explanations for phenomena in its intended scope is indeed a virtue, and gives us reason to accept the theory; but being informational, it generally gives no added reason for belief. (1983, p. 169)

But acceptance of a theory does involve some belief, and when it comes to opinion about empirical matters of fact – what the actual, observable world is, was, and will be like – it is reasonable only to turn to science, insofar as it goes. (1985, p. 247)

...the virtues of explanation, insofar as they go beyond description, may indeed provide reasons for acceptance of the theory, but not for belief... (1985, p. 294)

It looks like van Fraassen is willing to believe that successful theories are empirically adequate. But in other places he indicates that he remains neutral. For instance, he explicitly denies that the best explanation is empirically adequate, and claims that IBE is unacceptable in general whether a hypothesis inferred is about observables or unobservables (See quotes in Section 2.2.2., Chapter 3). The two central tenets of constructive empiricism, strictly speaking, are also neutral as to whether successful theories are empirically adequate. The first central tenet states that science aims to give us

theories which are empirically adequate. To propose empirical adequacy as the aim of science is not necessarily to believe that any of the current theories are empirically adequate. The second central tenet states that acceptance of a theory involves as belief only that it is empirically adequate. The second tenet is a claim about acceptance, a psychological state, and it is silent about whether we should accept any of the current theories. In short, constructive empiricism remains neutral as to whether successful theories are empirically adequate.

To remain neutral, in my view, comes at the following costs: First, van Fraassen claims that constructive empiricism is better than realism because constructive empiricism explains science “without inflationary metaphysics” (1980, p. 73). It is clear here that he takes constructive empiricism to be a rival philosophical position to realism. But I do not see how the claim to be a rival position can be made coherent, if constructive empiricism remains neutral as to whether successful theories are empirically adequate. Second, Ladyman et al. claim that skepticism is absurd (1997, p. 317). But if the constructive empiricists resist skepticism, they need to tell us what they think we are justified to infer from success, and what they think we are rationally compelled to believe. Remaining neutral is not informative. Third, if the constructive empiricists remain neutral as to whether successful theories are empirically adequate, they cannot explain the success of science in terms of empirical adequacy. The reason is, as we noted in Chapter 4, to explain the success of science in terms of empirical adequacy requires one to utter the sentence, ‘Some theories are successful because they are empirically

adequate.' But to utter the sentence requires that one believe that the successful theories are empirically adequate.

What do commentators say about constructive empiricism? Fine thinks that constructive empiricism is committed to the position that successful theories are empirically adequate. He claims that according to constructive empiricism "the evidence that does warrant belief extends only to empirical adequacy" (Fine, 1986, p. 159).

Whether or not Fine is right about constructive empiricism, it does not matter for my purpose here. The main thesis of this chapter is that Ladyman et al. are wrong to claim that the realists take extra epistemic risk by believing that a theory is approximately true rather than only empirically adequate.

3. Van Fraassen's Argument and Its Problems

Why does van Fraassen think that realists take more risk than constructive empiricists? His argument can be extracted from the exchange between him and Musgrave. Against van Fraassen's famous slogan that it is not an epistemological principle that one may as well hang for a sheep as for a lamb, Musgrave replies that "the principle that one might as well hang for a sheep as for a lamb is a pretty sensible one" (1985, p. 199). That is, it is reasonable to believe that a successful theory is true rather than merely empirically adequate. Van Fraassen's response is twofold. He agrees with Musgrave that observational evidence "provides some reason for believing in the truth..." (1985, p. 255). But he argues that belief in truth is to be disdained. The reason is that "nothing except evidence can give justification for belief" (ibid., p. 255), and "we can have

evidence for the truth of a theory only via evidential support for its empirical adequacy” (ibid., p. 255). Given what van Fraassen says against Musgrave, it is plausible that he might run the following argument to show that the realists take more epistemic risk than the constructive empiricists:

Premise 1: Only observations provide support for belief in the truth of a theory.

Premise 2: Inference from success to truth has to go through empirical adequacy.

Conclusion: More epistemic risk is involved in being committed to truth than to empirical adequacy.

Fine takes the argument to be “simple and elegant,” and to show that we should “limit belief to empirical adequacy alone” (1986, p. 158). Fine is right that the argument is simple and elegant. But should we accept it?

What are we to make of the first premise that evidential support for a theory comes only from observations? Van Fraassen does not have an argument for the first premise, as far as I know. In fact, the first premise seems to be the starting point of the empiricist philosophy of science, which realists do not (have to) share. Hempel, for instance, claims that evidential support may come “from above”:

Support may also come “from above”; that is, from more inclusive hypotheses or theories that imply the given one and have independent evidential support. (Hempel, 1966, p. 38)

For instance, Balmer's formula received support from above when it was derived from Bohr's theory of the hydrogen atom. The theory of the hydrogen atom was supported by diverse evidence other than the spectroscopic measurements that lent inductive support to Balmer's formula. Hempel calls this kind of support "theoretical support" (ibid., p. 39).

Some scientists do not share the empiricist's starting point either. Theoretical support played a dominant role in finally persuading the scientific community of atomism in the early 20th century. What decisively convinced the scientists of the reality of atoms was the way Avogadro's number, the number of molecules in a mole of any substance, was calculated. Perrin lists thirteen distinct methods to establish the number, including the kinetic theory of gases, Brownian motion, radioactivity, and blackbody radiation. Those independent methods conclusively converted Ostwald, one of the most prominent and staunch opponents of atomism, to atomism (Salmon, 1984, pp. 213-219). The historical episode suggests not only that theoretical support is powerful in persuading opposing scientists but also, more importantly, that theories receive evidential support from the side, viz., from theories of neighboring fields. The independent theories involved in calculating Avogadro's number supported each other.

Antirealists might retort that theoretical support is in fact observational support in disguise, i.e., that all there is at the fundamental level is observational support. There are, however, three reasons to think that theoretical support is not reducible to observational support. The first reason can be found in the role of auxiliary assumptions in the context of confirmation in science. Suppose that a biologist investigates DNA with an electron microscope. Realists and Empiricists alike agree that the images on the microscope

support the theory of DNA. Now, the empiricists might claim that the theory of DNA receives support solely from the images. But the realists would claim that it receives support not only from the images but also from the theory of electrons. Why do the realists think so? The images are evidentially inert without the theory of electrons. They are taken as evidence for the theory of DNA only via the theory of electrons. Unless the biologist has the knowledge of how the microscope works, he would have no idea of what the images prove. The theory of electrons is what arms the images with evidential power for the theory of DNA. Hence, the theoretical support is not reducible to the observational support.

Second, Hung (1997, pp. 95-96) notes that a theory enhances the credibility of empirical generalizations. At least, scientists think so. Take, for instance, the empirical generalization that iron conducts heat. Previous experiences are the evidential support for the empirical generalization. But our confidence in the generalization increases when a theoretical account is given of why iron conducts heat. Here is an example from biology (Milius, 2000, pp. 20-21). Scientists have been collecting data on the size of marine iguanas in Galapagos Islands for eighteen years. The data suggested to them that the iguanas shrink and lengthen by as much as 20%. But at first they dismissed the apparent shrinkage as errors in their measurements. They came to believe that the iguana really shrink and lengthen only after they noticed that the shrinkage is correlated with ocean warmings. The explanation of the shrinkage was that the iguanas graze in marine algal beds, and the algal beds thin out when the ocean gets warm, leaving the iguana starving. These examples support the idea that the credibility of observational beliefs comes in part

from theories. Then, it is plausible that the credibility of a theory comes in part from other theories.

Third, in some cases, a theory “improves and corrects the empirical generalization that the theory is meant to explain” (Hung, 1997, p. 102). For instance, the kinetic theory of gases improved and corrected the equation of state of gases: $PV=kT$. Van der Waals derived what is now known as van der Waals’s equation from the kinetic theory of gases. The new equation made slightly different predictions from the equation of state of gases: $PV=kT$. Experiments later confirmed the new equation (ibid., p. 102). A dramatic example where a theory corrects observational beliefs can be found in the distinction between primary and secondary qualities. It is atomism that led Newton and Locke to believe that secondary qualities are not inherent in bodies. Empiricists are right that observational beliefs are capable of correcting theories. But the reverse is true as well. Theories are also capable of correcting observational beliefs. It is not the case that theories are always at the receiving end of corrections.

On the basis of the three reasons above, I conclude that theoretical support is something over and above observational support. If this is right, a theory, having independent observational evidence, is capable of supporting another theory. The idea of theoretical support undermines the first premise of van Fraassen’s argument that only observational evidence supports the belief in the truth of a theory. Now, let me turn to the second premise.

Recall that the second premise holds that inference from success to truth has to go through empirical adequacy. The realists infer more than the antirealists, when the

starting point of inference is observations. So the realists take more chance of being wrong than the antirealists. Note, however, that van Fraassen's critical point requires that the realists infer truth as opposed to approximate truth from success. But the realists have settled for approximate truth ever since Putnam (1975), and they can bypass empirical adequacy to arrive at approximate truth from success. An approximately true theory describes unobservables incorrectly to some degree. The mischaracterization might have some consequences at the observational level, making the theory empirically inadequate. So when the realists believe that successful theories are approximately true, it is not necessarily the case that they also believe that the successful theories are empirically adequate.

4. Empirical Adequacy is Harder to Come By than Approximate Truth.

In this section I attempt to prove that the realists take less epistemic risk than the antirealists when the realists believe that successful theories are approximately true and the antirealists believe that they are empirically adequate. Let me bring up the pessimistic induction. It is clear that the pessimistic induction, if correct, has a devastating consequence on the antirealist quest for empirical adequacy. If they want to believe that successful current theories are empirically adequate, they have to come to terms with the historical fact that successful past theories turned out to be empirically inadequate. Now, does the pessimistic induction also have lethal implications for the realist quest for approximate truth? The answer is no, if Kitcher and Psillos are right.

In Chapter 2, I claimed that Kitcher's critical response (1993, pp. 140-149) to the pessimistic induction has been the most influential in the literature, and that his basic message is that successful past theories might be approximately true, given that their working assumptions are true, although their idle assumptions are false, in the light of current theories. The antirealists' critical response to Kitcher's approach to the pessimistic induction in the literature is that it is hard to distinguish between idle assumptions and working assumptions of *current* theories, although it is relatively easy to distinguish between idle assumptions and working assumptions of past theories in retrospect. Stanford puts the problem as follows:

The problem with this strategy, of course, is that at the time of our commitment to a theory it is not usually possible to separate its operative elements from the extra baggage (assuming that this separation is coherent at all), as Maxwell's famous remark (paraphrased in Laudan 1981, 114) that 'the aether was better confirmed than any other theoretical entity in natural philosophy' reminds us. (Stanford, 2000, p. 278, footnote)

If Maxwell is right, the ether theorists were confident of the truth of the idle assumption of the ether theory. This goes against the realists' claim that scientists are not confident of the truth of idle assumptions.

There is a lot to be said against Stanford's response to Kitcher. First, the criticism, even if correct, is hardly a knockdown argument against Kitcher's suggestion that working assumptions are distinguishable from idle assumptions. All it shows is that the realists need to do more work to come up with a principled distinction between idle assumptions and working assumptions. Second, Stanford does not seem to be aware of what Kitcher has said about Laudan's report of Maxwell's remark. Kitcher claims that

“Maxwell was wrong” (1993, p. 149). The claim is based on his detailed case study on the ether theory, which I will not rehearse here. The upshot of the case study is that the working assumptions of the ether theory by Fresnel are “endorsed by contemporary physics” (ibid., p. 145), that “Fresnel typically makes no detailed claims about the nature of this medium [ether]” (ibid., p. 147), that the success of the ether theory provided support only for the working assumptions. Third, the most important of all, it does not matter whether or not we have a principled way to distinguish between working assumptions and idle assumptions. If the working assumptions are true, the successful past theories could be considered as being approximately true.¹ But we would not regard them as being empirically adequate because obviously they ran into empirical anomalies. So the antirealist belief that successful theories are empirically adequate is more likely to be false than the realist belief that they are approximately true.

Laudan would object that successful past theories cannot possibly be approximately true, even if their working assumptions are true, because their central terms do not refer. The phlogiston theory, for instance, is completely false because the central term ‘phlogiston’ does not refer. Kitcher’s response to the problem is to concede at the outset that ‘phlogiston,’ qua type, does not refer because there is no substance emitted in combustion. But he argues that some tokens of the term, ‘phlogiston,’ refer to oxygen, and that on some of those occasions phlogiston theorists made true assertions about the referent of ‘phlogiston.’ These two claims are based on his theory of reference of scientific terms and his historical case study on the phlogiston theory. Let me briefly explain.

According to Kitcher (*ibid.*, p. 75-80), the reference of a token of scientific language is fixed by three ways: descriptive mode, baptismal mode, and conformist mode.

Scientists use the descriptive mode of reference when they have an intention to pick out an object that satisfies a particular description. In this case, the reference is determined via the fact that the object fits the description. Scientists use the baptismal mode of reference when they have an intention to pick out a present object via a causal link between a linguistic item and the object. In this case, the linguistic item refers to the object irrespective of whether or not the object fits the linguistic description. Finally, scientists use the conformist mode of reference when they have an intention that their usage be parasitic on those of their fellows. In this case, the reference is determined by a long causal chain that connects their linguistic item with an initial usage, in which the reference is fixed either in the descriptive mode or in the baptismal mode of reference. What is crucial is that scientists use not just one mode of reference but the three modes of reference, i.e., “the linguistic community to which a scientist belongs allows a number of distinct ways of fixing the reference of tokens of terms.” (*ibid.*, p. 78).

Kitcher (*ibid.*, 95-105) applies his theory of reference of scientific language to a historical example. The terms, ‘phlogiston’ and ‘dephlogisticated air,’ fail to refer, if their references are fixed solely by the descriptive mode of reference because there is no substance emitted in combustion. But, on some occasions, phlogiston theorists use the baptismal mode of reference or the conformist mode of reference to pick out an object, and on some of those occasions, ‘dephlogisticated air’ refers to oxygen (*ibid.*, p. 100). He goes further: On those occasions some of their assertions about the referent of

'dephlogisticated air' are true (ibid., p. 102). For example, Priestley states that he feels light and easy when he breathes isolated dephlogisticated air, and Cavendish states that pure water is formed when dephlogisticated air (oxygen) chemically reacts with inflammable air (hydrogen). On these occasions, the phlogiston theorists "enunciate important new truths" (ibid., p. 102). In short, in some contexts 'dephlogisticated air' refers to oxygen, and phlogiston theorists made true assertions about oxygen.

The achievement of Kitcher's theory of reference is, I believe, to show that the past theoretical terms are not complete referential failures. But I am not sure how much it helps realism. It does not tell us which tokens of *current* theoretical terms refer. All we can conclude from Kitcher's theory of reference is that in some contexts the current theoretical terms refer but in others they may not. That is, no principled criteria are provided to separate referential tokens from nonreferential tokens of the current theoretical terms. So what? Kitcher's story does not tell us which of our current token statements containing theoretical terms, by virtue of being about real things, have the chance to be true.

Psillos's theory of reference (1999, Chapter 12) points to a direction in which this problem can be solved. Psillos argues that some purported central terms of the past theories on Laudan's list refer to the theoretical entities of their corresponding current theories. For instance, 'ether' refers to electromagnetic field. In order to defend this claim, he advances the causal-descriptive theory of reference, which is a hybrid of the causal theory of reference and the descriptive theory of reference. The gist of his theory is that a theoretical term refers to a theoretical entity in virtue of the causal relation between

“kind-constitutive properties” of the theoretical entity and “core causal descriptions” associated with the theoretical term. A kind-constitutive property, the first determiner of reference, is a property that makes an object, which possesses the property, belong to a kind. A kind-constitutive property of a theoretical entity is a property that causes observable phenomena. So, for instance, electrons and protons belong to different kinds because the kind-constitutive properties of electrons and the kind-constitutive properties of protons cause different kinds of observable phenomena. A core causal description, the second determiner of reference, is a description about a kind-constitutive property. It has its causal origin in the kind-constitutive property. What is crucial on Psillos’s account is that if core causal descriptions fail, then “the putative entity does not exist” (ibid., p. 298).

Now, Psillos applies his theory of reference to a historical example. The core causal descriptions of the ether theory are the descriptions about the properties of a putative entity which cause light phenomena. An example is that “the luminiferous ether was the repository of potential and kinetic energy during the light-propagation” (ibid., p. 296). The potential and kinetic energy of the putative entity caused light phenomena, which in turn led ether theorists to produce core causal descriptions. Psillos argues that ‘ether’ refers to electromagnetic field “because the core causal description associated with the term ‘electromagnetic field’ takes up the core causal description associated with the term ‘ether’” (ibid., p. 296).

Psillos’s theory of reference is an improvement on Kitcher’s theory of reference in that it yields a story of what assumptions of current theories, by virtue of being about real

things, have the chance to be true. Core causal descriptions of the current theories are about real things. But the problem with Psillos's theory of reference is that it has an uncomfortable consequence that some theoretical terms of past theories like 'caloric' do not refer.² The term is nonreferential on Psillos's account because the core causal description, 'the fluid that flows from a hot body to a cold body when the cold body gets hot,' fails. Then, we are back to Laudan's objection that the caloric theory of heat cannot possibly be approximately true because 'caloric' does not refer.

The way to cope with this objection is, I believe, to propose that the theoretical terms of past theories *approximately* refer to the posits of the corresponding current theories. The ground for the approximate reference is that the working assumptions of the past theories were carried over to the current theories, and that the overlapping assumptions have the same causal origins. The proposal implies that 'caloric' approximately refers to molecular motions, 'ether' approximately refers to electromagnetism, and so on.

I must stress, though, that Psillos is aware that the working assumptions of the past theories were carried over to the current theories. He is also aware that reference does not have to be all-or-nothing affair. After all, he claims at the outset that a term of a past theory "approximately refers" to a current posit (*ibid.*, p. 294). He just did not utilize these good resources in developing his full-blown theory of reference, and ended up presupposing that reference is all-or-nothing affair, thus running into the problem that some past theoretical terms do not refer.

5. Conclusion

A moderate position and an ambitious position are available for both realists and antirealists. The moderate realist position holds that successful theories are approximately true; the ambitious realist position, that they are true. The moderate antirealist position holds that successful theories are approximately empirically adequate; the ambitious antirealist position, that they are empirically adequate. This chapter makes it clear, I hope, that the ambitious antirealist position is epistemically less secure than the moderate realist position. Being restricted to observational beliefs does not guarantee better epistemic security. Antirealists can't claim that antirealism is better than realism because it explains science without an extra epistemic risk, once they take the ambitious position.

¹ From the historical fact that working assumptions of the caloric theory of heat were carried over to the kinetic theory of heat, and from the historical fact that the most eminent caloric theorists were not committed to the truth of the idle assumptions of the theory, Psillos concludes that “it makes perfect sense to talk of the approximate truth *this theory [the caloric theory of heat]...*” (1999, p. 113).

² But Psillos welcomes the consequence “not all abandoned theoretical terms refer” (ibid., p. 298). He takes ‘phlogiston’ to be nonreferential on the ground that the core causal descriptions of ‘phlogiston’ do not fit anything. Thus, he faces Laudan’s objection that the phlogiston theory is completely false, even if its working assumptions are true, because ‘phlogiston’ does not refer.

CONCLUSION OF THE DISSERTATION

Suppose that realists believe that our best theories, successful theories that cohere with each other, are approximately true, and that antirealists believe that they are approximately empirically adequate. Let me lay out explicitly the strengths and weaknesses of each position, and then decide which position is overall better.

From a semantic point of view, no position is better than the other. Both the realists and the antirealists have to live with vague predicates: 'approximate truth' and 'approximate empirical adequacy.' But there is a respect in which antirealism is better than realism.

Realists take more epistemic risk than antirealists. It is hard to be precise about the relation between approximate empirical adequacy and approximate truth because both concepts are vague. I assume here, though, that when realists believe that our best theories are approximately true, they also believe that they are approximately empirically adequate. After all, realists are interested to know about observable parts of the world as well as about unobservable parts of the world. So the realist belief is more likely to be false than the antirealist belief. This is, however, the only respect in which antirealism is better than realism.

Realism is empirically better than antirealism. There is an aspect of science, the coherence of successful theories, which approximate truth can explain but approximate empirical adequacy cannot. The empirical superiority of realism cancels out the epistemic superiority of antirealism, the only respect in which antirealism is better than realism. Let me list other respects in which realism is better than antirealism.

Explaining success in terms of approximate empirical adequacy is like saying that some crows are black because most crows are. The explanation is on the verge of being vacuous. The realist explanation, on the other hand, is like saying that some crows are black because their feathers share a certain atomic structure. With respect to the problem of vacuousness, the realist explanation is better than the antirealist explanation.

There is something that needs to be done about the antirealist explanation. Approximate empirical adequacy cries out for an explanation, if success does, because they are quite close to each other in a continuum. Approximate truth, on the other hand, is not as close to success as approximate empirical adequacy is. After all, approximate truth concerns unobservable parts of the world, and success, only observable parts of the world. With respect to the problem of an explanans crying out for an explanation, realism is better than antirealism.

Antirealists run the risk of distancing themselves from the epistemic goal of disseminating to others what they take to be true. Once they take instrumental epistemic attitude toward the positive theories of their cognitive colleagues, their colleagues will in turn take instrumental epistemic attitudes toward instrumentalists' positive theories. Realists do not run into such a pragmatic problem.

Antirealists are not entitled to believe that the positive theories of their own making are true because they are antirealists. It is against the antirealist spirit for them to take the realist attitude toward their own theories. Unlike the antirealists, the realists do not have such a problem. There is nothing wrong with them taking the realist attitude toward their own theories.

The problem of odd speech act or the problem of epistemic irresponsibility prevent antirealists from using a scientific theory to explain puzzling phenomena around us. Unlike antirealists, realists do not have such a problem. They are free to use a scientific theory to explain the puzzling phenomena around us. One of the main functions of a scientific theory is to yield an explanation of puzzling phenomena around us. The antirealists cannot enjoy the pragmatic benefit that arises from the main function of the scientific theory.

Conclusion: Scientific realism is overall better than scientific antirealism. Let's come back to the questions I raised in the introduction of this dissertation: What kinds of things exist in the world? What does the world look like? Should we include theoretical beliefs in our world view in addition to observational beliefs? Realists, believing that our best theories are approximately true, include theoretical beliefs in their world view. Antirealists, on the other hand, believing that they are only approximately empirically adequate, do not include theoretical beliefs in their world view. This dissertation shows, I hope, that we are better off, if we include theoretical beliefs in our world view.

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