A Pessimistic Induction against Scientific Antirealism

Abstract
There are nine antirealist explanations of the success of science in the literature. I raise difficulties against all of them except the latest one, and then construct a pessimistic induction that the latest one will turn out to be problematic because its eight forerunners turned out to be problematic. This pessimistic induction is on a par with the traditional pessimistic induction that successful present scientific theories will be revealed to be false because successful past scientific theories were revealed to be false.

Keywords
Pessimistic Induction, Scientific Antirealism, Success of Science

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1. Introduction

Science is successful in explaining and predicting phenomena. Why is science successful? Putnam's no-miracles argument (1975) says that science is successful because successful scientific theories are approximately true and their key terms refer. For example, the kinetic theory of heat, the germ theory of diseases, and the special theory of relativity are successful because they are approximately true and the theoretical entities they postulate exist. To say that a theory is successful entails that *some* of their observational consequences turned out to be true. It would be a miracle, Putnam says, if successful theories are not even approximately true, and if their key terms do not refer. Thus, approximate truth and reference generate the best explanation of the success of science. This explanation of the success of science is called the no-miracles argument. It is regarded as the best argument for scientific realism ('realism' from now on).

In this paper, realism is defined as the position that a successful scientific theory is (approximately) true, and antirealism is defined as the view that a successful scientific theory is empirically adequate. On these definitions, realists believe both what science says about observables and unobservables, whereas antirealists believe only what science says about observables. For example, realists believe that molecules are real, and that heat phenomena occur, whereas antirealists are skeptical that molecules are real, but they believe that heat phenomena occur.

The no-miracles argument received many criticisms from antirealists. One of them comes from the history of science that successful past theories, such as the caloric theory of heat, the humoral theory of medicine, and the ether theory, turned out to be false, so successful present theories, such as the kinetic theory of heat, the germ theory of diseases, and the special theory of relativity, will also turn out to be false as a matter of induction. I will call this historical objection the traditional pessimistic induction. The traditional pessimistic induction reflects the pessimistic inductions constructed by following eminent philosophers:

> The ephemeral nature of scientific theories takes by surprise the man of the world. Their brief period of prosperity ended, he sees them abandoned one after the other; he sees ruins piled upon ruins; he predicts that the theories in fashion today will in a short time succumb in their turn, and he concludes that they are absolutely in vain. (Poincaré, 1905/1952: 160)

> Most of the past theories of science are already suspected of being false; there is presumably every reason to anticipate that current theories of science will suffer a similar fate. (Laudan 1977: 126)

> ..the following meta-induction becomes overwhelmingly compelling: Just as no term used in the science of more than fifty (or whatever) years ago referred, so it will turn out that no term used now (except maybe observational terms, if there are such) refers. (Putnam, 1978: 25).

> Thus, the history of scientific inquiry itself offers a straightforward inductive rationale for thinking that there typically are alternatives to our best theories equally well-confirmed by the evidence, even when we are unable to conceive of them at the time. (Stanford, 2006: 20)

The slight differences among these pessimistic inductions do not matter for the purpose of this paper because their main insight is captured by my simple formulation that since past theories turned out to be false, so will present theories.

Throughout this paper I operate under the assumption that the traditional pessimistic induction is correct, exploring its devastating implications against antirealism. This paper is in stark contrast with other papers in the literature on the pessimistic induction. What is at stake in this paper is antirealism whereas in other papers it is realism. In the literature, antirealists run the pessimistic induction against realism. In response, realists criticize the pessimistic induction. Antirealists in turn defend the pessimistic induction and criticize the realist responses to it. This paper is intended to turn the dialectal terrain upside down, running the pessimistic induction against antirealism. It will become clear at the end of this paper that antirealists have every reason to fight the pessimistic induction just like realists.

Another line of objections to the no-miracles argument is predicated on the idea that there can be antirealist explanations not invoking approximate truth and reference. Those antirealist accounts undermine the realist contention that approximate truth and reference yield the best explanation of the success of science. I found nine such antirealist suggestions in the literature. In this paper, I will raise difficulties against all of them except the latest one (Lyons, 2003), and then construct a pessimistic induction against it. My pessimistic induction against it is on a par with the traditional pessimistic induction against successful current scientific theories.

2. Antirealist Explanations

2.1. Evolutionary Explanation

Van Fraassen (1980) argues that some *current* scientific theories are successful because successful theories survive and unsuccessful ones die out:

> ..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: 40)

The idea seems to be that scientists propose theories about the world. Some of them pass severe tests, explain a lot of phenomena, and accurately predict future phenomena. Others do

not. Scientists retain the former and discard the latter. Science is successful because scientists accept successful theories and reject unsuccessful ones.

How do realists react to van Fraassen's evolutionary explanation in the literature? Lipton (1991: 170ff.) and Psillos (1999: 96-97) argue that a full-fledged evolutionary explanation of the success of science would invoke approximate truth, just as a full-fledged evolutionary explanation of a phenotype invokes a genotype. Darwin's evolutionary theory was combined with molecular biology in the second half of the 20th century. As a result, genetic explanations can be given for why we have a certain observable property. Just as the evolutionary explanation of an observable property invokes genes, so should the evolutionary explanation of the success of science invoke approximate truth.

Van Fraassen would, however, stick to his original position, arguing that what is minimally required to explain the success of science is not approximate truth but the survival and deaths of scientific theories, and that the principle of economy dictates us to choose the survival and deaths over approximate truth. Therefore, Lipton and Psillos's response is not a knockdown argument against van Fraassen's evolutionary explanation.

My first response to van Fraassen's evolutionary explanation is to point out that the successful *past* theories, such as the caloric theory of heat and the humoral theory of medicine, are all counterexamples to it. They were successful, but they all died out. Why did they die out when they were successful? It is not clear what the evolutionary answer would be. It appears that the evolutionary explanation can be applicable at best to the success of present theories, but not to the success of past theories. In fact, van Fraassen's first sentence cited above suggests that he set out to explain only the success of *current* theories.

A more serious problem with the evolutionary explanation is that it appeals to the successful current scientific theory, viz., evolutionary theory. The traditional pessimistic induction implies that the evolutionary theory will be disclosed to be false like past theories. An explanation appealing to such a theory is *ipso facto* fated to be disclosed to be false. The evolutionary explanation appears to be true now, but in reality it is false, and it will turn out to be false.

2.2. Robust Method

Laudan claims that science is successful because scientific theories result from robust scientific methods:

> 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. (1984: 101)

Scientists use rigorous testing methods, such as double-blind experiments in which even researchers do not know whether a particular patient belongs to the control group or the testing group. If scientists do not use such robust methods, scientific theories would not be successful.

The proffered explanation, however, has no explanatory force. Imagine that we are puzzled over why an apple falls down as opposed to hanging on the branch of a tree. An explanation is proposed that an apple falls down from a tree because Spinoza planted the tree in the past. We would not be satisfied with such an explanation because Spinoza's act of planting the tree is equally responsible for an apple's hanging on the branch of the tree. Thus, an event that is responsible for both an explanandum and an event that is in contrast with the

explanandum does not give rise to explanatory force. Citing such an explanatory factor is not illuminating.

It is granted that the use of the robust method is a prior condition for a theory to be successful. But it is also a prior condition for a theory to be unsuccessful. Suppose that scientists use a robust method in an experiment, and that a theory fails the test. In such a case, the use of the robust method is a prior condition that contributed to the failure of the theory. After all, unless scientists had used the robust method, the theory would not have failed the test. Hence, using a robust method is equally responsible for a theory's being unsuccessful. Thus, citing such a factor is not illuminating to those who wonder why a theory is successful as opposed to being unsuccessful. When realists ask why some scientific theories are successful, they want to know what properties successful theories have in common that unsuccessful theories do not have.

Laudan might retort that successful theories are the ones that scientists accept, and that unsuccessful theories are the ones that scientists reject through using robust experimental methods, so some scientific theories are successful because scientists accept them. In my view, however, this explanation involves explaining an event in terms of another event posterior to it. After all, scientists accept a scientific theory by first observing that it makes true predictions and explains puzzling phenomena. It is wrong to explain an event in terms of another event posterior to it. Therefore, it is wrong to say that some scientific theories are successful because scientists accept them. We should rather say that scientists accept some theories because they are successful.

2.3. Empirical Adequacy

Musgrave and Ladyman suggest that antirealists could explain the success of a theory in terms of its empirical adequacy:

> Theory *T* is empirically adequate.
> Theory *T* yielded several novel predictions.
> Therefore, *T*'s novel predictions were true. (Musgrave, 1988: 242)

> So, for example, the antirealist may claim that the empirical adequacy of a theory explains its success and that is that. (Ladyman, 1999: 186)

Put differently, a theory is successful because it is empirically adequate. This suggestion is similar to the suggestion that "some crows are black because all crows are" (Musgrave, 1988: 242). After all, a theory is empirically adequate when *all* of its observational consequences are true, and to say that a theory is successful implies that *some* of its observational consequences are true.

Leplin objects that empirical adequacy is a poor explanatory property for success because generalizations "do not explain their instances" (1997: 23). It is a vacuous explanation that some crows are black because all crows are black. Leplin also claims that 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: 23)

Ladyman replies that Leplin's claim "needs to be argued rather than merely stated…" (1999: 186). In other words, antirealists might deny that empirical adequacy cries out for an

explanation, so Leplin needs to offer an argument to persuade antirealists of the need to explain empirical adequacy.

Kukla (1996: S303-S305) makes a similar critical point, arguing that it is hard to justify stopping not at empirical adequacy but at approximate truth in the chain of demand for explanation. Success can be explained by empirical adequacy, and empirical adequacy can be explained by approximate truth. For antirealists, the demand for an explanation should stop at empirical adequacy, whereas for realists, it should stop at approximate truth. Antirealists challenge realists to justify the realist position that the demand for an explanation should stop at approximate truth. Therefore, Leplin's response to the antirealist explanation is not a serious strike against it.

My response to the antirealist proposal is to conjure up the traditional pessimistic induction. As Park (2001) and Lange (2002) point out, if the pessimistic induction is correct, both successful past and present scientific theories are empirically inadequate:

> The successful past theories turned out to be empirically inadequate. So successful current theories will turn out to be empirically inadequate as well. (Park, 2001: 78)

> Most of these theories eventually turned out not to be empirically adequate. Therefore, we should believe that probably, most of the theories we currently accept are not empirically adequate either. (Lange, 2002: 282)

Successful past and present theories are all counterexamples to the suggestion that a theory is successful because it is empirically adequate. Consequently, empirical adequacy can explain neither the success of the past theories nor the success of the present theories.

2.4. As-If-True

Fine (1986) argues that an antirealist could explain the success of science in terms of the notion of as-if-true. The idea is that a scientific theory is successful because the world operates as if it were true, i.e., because the observable phenomena are as if the theory were true. The proposal is distinctively antirealistic because one need not believe that a theory invoked is true in order to explain its success. The as-if-true explanation is neutral as to whether the 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. What is the explanation committed to, if not to the truth of the theory? Musgrave thinks that it is committed to the empirical adequacy of the theory:

> For the explanation to go through it must assert *at least* that the world is *observationally* as if *T* were true. If it asserts no more than this, then it is just a fancy way of saying that *T* is observationally or empirically adequate. (Musgrave, 1988: 243)

In other words, there is only a verbal difference between the appeal to as-if-true and the appeal to empirical adequacy. Stanford concurs with Musgrave, saying that it is not clear that the appeal to as-if-true "is really any more than verbally distinct from the constructive empiricist's appeal to empirical adequacy" (2000: 268).

How do realists react to the antirealist proposal in the literature? 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: 25)

Stanford agrees with Leplin, saying that the operation of the world as if the theory were true "does indeed both cry out for some further explanation and invite the truth of the theory" (2000: 269).

Both Leplin and Stanford might be right that the truth of a theory explains why the world operates as if the theory were true. Antirealists, however, would insist that no reason is given for stopping at truth in the chain of demand for an explanation. They would stop at the as-if-true explanation, requesting that realists justify the need to explain the explanans of the as-if-true explanation. Therefore, Leplin and Stanford's response does not amount to a refutation of the as-if-true explanation.

My response to the as-if-true explanation is to advance the traditional pessimistic induction against it. The pessimistic induction asserts that successful past and present theories are all empirically inadequate. To say that a successful theory is empirically inadequate means that not all observable phenomena are as if it were true. Hence, it is 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" (Park, 2001: 87).

2.5. Usefulness

Fine (1991) argues that antirealists could explain the success of science in terms of instrumental reliability. A scientific theory is successful because it is instrumentally reliable. He claims that a theory is instrumentally reliable just in case it is useful:

> Hence being reliable amounts to no more than being useful for getting things to work (or work out). (Fine, 1991: 95)

In a nutshell, a scientific theory is successful because it is useful. Fine (1991: 82) also claims that the instrumental explanation is better than the realist explanation because instrumental reliability is what is minimally required to explain the success of science. To put differently, the principle of economy favors instrumental reliability over approximate truth as an explanatory property for success.

What do realists say about Fine's proposal in the literature? Psillos argues that approximate truth is what enables a theory to be instrumentally reliable:

> ..the property of being approximately true would ground the power of scientific theories to be instrumentally reliable. (1999: 93)

Since approximate truth grounds instrumental reliability, we can explain instrumental reliability in terms of approximate truth. In other words, we can say that a theory is instrumentally reliable because it is approximately true.

In my view, to explain success in terms of instrumental reliability or usefulness is to put the cart in front of the horse, i.e., the explanans and the explanandum should switch their places. Suppose, for instance, that an astronomical theory makes true predictions, thereby helping us to cope with future natural disasters. Obviously, the theory is useful. Notice that the theory gets to be useful by means of being successful. After all, it helps us to cope with future catastrophes by means of making true predictions. Thus, success is what grounds usefulness. It follows that usefulness should be explained in terms of success, not the other way around. In other words, instead of saying that a theory is successful because it is useful, we should rather say that a theory is useful because it is successful. Fine's proposal has a conceptual flaw.

2.6. Narrative Explanation

Brown (1994) proposes an explanation of the success of science which he takes to be an alternative to Putnam's realist explanation. The style of the explanation, he argues, is narrative. On the narrative style, an explanandum is explained by a story which depicts a series of past events leading to the explanandum. Brown's example of narrative explanation is an evolutionary explanation of a biological property of current organisms. In an evolutionary explanation, a biological property is explained by a story of what happened in the past. What is crucial on the narrative style of explanation is that 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. (Brown, 1994: 21)

We can explain a biological property in terms of an evolutionary narrative, even if we are not confident at all that the evolutionary narrative is true. Likewise, we can explain the success of theories in terms of their (approximate) truth, although "we cannot count on the (approximate) truth of the theories at all" (Brown, 1994: 25). So "the realist has an explanation of the success of science: truth is the explanation and the style of the explanation is narrative" (Brown, 1994: 23). Brown takes the narrative explanation of the success of science to be realistic on the grounds that truth is invoked.

It seems, however, that Brown's explanation of the success of science is not realistic but antirealistic. After all, what is crucial on Brown's account is that the explainer does not have to be committed to the truth of the explanans. If so, how can his account be realistic? The fundamental difference between realism and antirealism is that realism is committed to theoretical truths, and antirealism only to observational truths (Psillos, 1999: xx). It follows that Brown's proposal is distinctively antirealistic.

In my view, Brown's proposal involves Moore's Paradox. Moore (1993: 207-212) observes that a paradox arises when we utter a sentence of the form: p, but I do not believe that p. For example, Moore's Paradox arises when you say, "Only the fittest tend to survive, but I don't believe that only the fittest tend to survive." If you do not believe that only the fittest tend to survive, how can you *say*, "Only the fittest tend to survive"? When you say, "Only the fittest tend to survive," you are expressing your belief that only the fittest tend to survive. Thus, if you say afterwards, "But I don't believe that only the fittest tend to survive," you are contradicting yourself, i.e., you are negating what you just affirmed.

Moore's observation on the speech act goes hand in hand with the general practice of scholarly journals. Scholarly journals have stylistic guidelines for their prospective authors. One of them holds that authors should refrain from using the expression 'I believe.' Authors should say, for example, "Only the fittest tend to survive" instead of "I believe that only the fittest tend to survive." The rationale behind this stylistic guideline is that if authors state "Only the fittest tend to survive," readers assume that the authors believe that only the fittest tend to survive, so it is superfluous to add 'I believe.' To sum up, the utterance of p involves the belief that p.

Similarly, when we say "A theory is successful because it is true," we are expressing our belief that the successful theory is true. We are contradicting ourselves, if we say afterwards, "But I don't believe the successful theory is true." To generalize, when we explain an explanandum in terms of an explanans, we are expressing the belief that the explanans is true. If we do not believe that the explanans is true, we cannot use the explanans to explain the explanandum. Therefore, we ought to be committed to truth, if we invoke it to explain success, pace Brown.

2.7. Approximate Empirical Adequacy

Kukla (1996) introduces an interesting antirealist notion: approximate empirical adequacy. What does it mean to say that a theory is approximately empirically adequate? Kukla gives a provisional definition, and Park develops it as follows:

> I provisionally define "T is (approximately) empirically adequate" as "T is (approximately) empirically equivalent to a true theory." (Kukla, 1996: S300)

> I propose that a theory is approximately empirically adequate just in case most of its observational consequences are true. (Park, 2009: 117, footnote)

Antirealists could suggest that a scientific theory is successful because it is approximately empirically adequate. The suggestion is compatible with the traditional pessimistic induction. Thus, approximate empirical adequacy seems to be the right semantic property for antirealists.

In my view, however, the antirealist explanation is vacuous. To say that a theory is successful because it is approximately empirically adequate would be like saying that some crows are black because most crows are black. What is wrong with such explanations? Imagine that in the world there are five crows: c_1, c_2, c_3, c_4, and c_5. All of them are black except the last one, c_5, which is white. Consider the following exchange between a realist and an antirealist:

> Realist: Why are some crows black? Why are, say c_1 and c_2, black?
> Antirealist: They are black because most crows are black, i.e., because c_1, c_2, c_3, and c_4 are black.
> Realist: Why are c_3 and c_4 black?
> Antirealist: They are black because most crows are black, i.e., because c_1, c_2, c_3, and c_4 are black.

In a nutshell, the antirealist claims that c_1 and c_2 are black because c_3 and c_4 are black, and that c_3 and c_4 are black because c_1 and c_2 are black. Such explanations are circular, casting no light on why c_1 and c_2 are black, or why c_3 and c_4 are black.

Analogously, the problem of circularity arises when antirealists say that a theory is successful because it is approximately empirically adequate. Suppose that a theory has five observational consequences: o_1, o_2, o_3, o_4, and o_5. All the observational consequences except the last one, o_5, are true, so the theory is approximately empirically adequate. Imagine the following dialogue between a realist and an antirealist:

> Realist: Why is the theory successful? Why are, say o_1 and o_2, true?
> Antirealist: They are true because most of the observational consequences are true, i.e., because o_1, o_2, o_3, and o_4 are true.
> Realist: Why are o_3 and o_4 true?
> Antirealist: They are true because o_1, o_2, o_3, and o_4 are true.

In effect, the antirealist claims that o_1 and o_2 are true because o_3 and o_4 are true, and that o_3 and o_4 are black because o_1 and o_2 are true. Such explanations are circular. To sum up, it sounds acceptable that a theory is successful because it is approximately empirically adequate. On close examination, however, it is not.

2.8. Predictive Similarity

Stanford contends that a scientific theory is successful because its predictions are similar to those of its corresponding true theory. For example, the Ptolemaic theory was successful because its predictions were similar to those of the Copernican theory:

> ..*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: 275)

Stanford's proposal is antirealistic because we do not need to believe the content of a true theory in order to explain the success of its corresponding false theory. He emphasizes this point as follows:

> 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. (Stanford, 2000: 274)

Thus, an antirealist can explain the success of science without believing that any current scientific theory is true.

Park (2003) criticizes Stanford's proposal in detail. I can only summarize his main criticism here. Imagine that a tennis ball is in front of you. Why is the tennis ball spherical? One proposes that the tennis ball is spherical because it is similar to a baseball with respect to shape. You would think that the explanation is unacceptable. Why is it unacceptable? It is unacceptable because the tennis ball's property of being spherical is what grounds the similarity between the tennis ball and the baseball. The tennis ball is similar to the baseball with respect to shape because the tennis ball is spherical, not the other way around. Thus, if X grounds Y, Y should be explained in terms of X, not the other way around. We can apply this principle to Stanford's proposal.

We need to determine whether predictive similarity grounds success or success grounds predictive similarity. What makes a given theory predictively similar to its true theory is that the given theory is approximately empirically adequate, i.e., most of its observational consequences are true. If most of its observational consequences are false, it would not be predictively similar to the true theory at all. If all of its observational consequences are true, it would be predictively identical with, rather than similar to, the true theory. Thus, the truths of the most of the observational consequences are what ground the predictive similarity.

To say that a theory is successful entails that *some* of its observational consequences are true. It follows that success partially grounds predictive similarity, and hence to say that a theory is successful because it is predictively similar to its true theory is analogous to saying that a tennis ball is spherical because it is similar to a baseball with respect to shape. It is for this reason that Park concludes that it is conceptually problematic "to explain success in terms of observational similarity" (2003: 167-168).

2.9. Almost As-If-True

Let me move on to the latest antirealist explanation of success. Lyons (2003) contends that a theory is successful because its theoretical mechanisms would generate almost the same phenomena as actual mechanisms:

> The mechanisms postulated by the theory and its auxiliaries would, if actual, bring about all

> relevant phenomena thus far observed and some yet to be observed at time t; and these phenomena are brought about by actual mechanisms in the world. (Lyons, 2003: 900)

In simple terms, a theory is successful because the world operates *almost* as if the theory were true. Lyons's account does not say that all of the observational consequences of a theory are true. It rather says that some of them are false. Therefore, it is compatible with the pessimistic induction that past and present theories are all empirically inadequate. Should we accept Lyons's proposal?

3. Pessimistic Induction

It appears that Lyons's proposal does not have problems, but in reality it has, and they will be exposed in the future. Its eight forerunners were disclosed to be problematic, so it will also be unveiled to be problematic. We do not even have to know exactly what Lyons's explanation says. All we need to know is the historical fact that its antecedents were disclosed to be problematic. This pessimistic induction against Lyons's proposal mirrors the traditional pessimistic induction against current scientific theories that they appear to be true now, but in reality they are false, and they will turn out to be false in the future. We do not have to know about the content of a current scientific theory and the experimental evidence for it. All we need to know is the historical fact that its antecedents were revealed to be false.

Antirealists may argue that philosophical theories do not fall prey to a pessimistic induction while scientific theories do, bringing up differences between philosophical and scientific theories. The differences are that philosophical theories are about science whereas scientific theories are about the world, and hence philosophical theories are indirectly connected with observations, whereas scientific theories are directly connected with observations. It follows that philosophical theories are immune to a pessimistic induction, but scientific theories are not.

In my view, driving a wedge between philosophical and scientific theories does not necessarily help philosophical theories. After all, philosophical theories might be more vulnerable to a pessimistic induction than scientific theories simply because philosophical theories are indirectly connected with observations while scientific theories are directly connected with observations. After all, the history of science tells us that observational data are relatively stable while scientific theories come and go. Thus, the tenuous connection of philosophical theories with observations might be detrimental rather than helpful to philosophical theories.

Besides, we have reason for thinking that Lyons's proposal is more susceptible to a pessimistic induction than a current scientific theory, e.g., the kinetic theory of heat. Lyons's proposal has eight problematic predecessors whereas the kinetic theory of heat has only one false forerunner, viz., the caloric theory of heat. Eight instances constitute a more powerful inductive rationale than one instance. Antirealist philosophical theories of the success of science come and go more frequently than scientific theories of heat do, which shows that the philosophical theories are epistemically more unstable than the scientific theories.

Antirealists may reply that all the successful past scientific theories jointly constitute the inductive basis against the kinetic theory of heat, so the Ptolemaic theory, the humoral theory of medicine, the ether theory, and so on form the inductive basis against the kinetic theory of heat. In contrast, only the eight antirealist explanations constitute the inductive basis against Lyons's explanation. The sample size against the kinetic theory of heat is much larger than the sample size against Lyons's account. Therefore, the traditional pessimistic induction against the kinetic theory of heat is much stronger than my pessimistic induction against Lyons's proposal.

My reply is that if antirealists include in their sample all the extant scientific theories that are not related to heat phenomena with the view to refuting the kinetic theory of heat, I do not see why it is wrong to include in my sample all the extant philosophical theories, such as Plato's theory of Forms and Descartes's substance dualism, that are not related to the success of science with the view to refuting Lyons's explanation of the success of science. All those old philosophical theories jointly poison Lyons's account, just as all the old scientific theories jointly poison the kinetic theory of heat. Consequentially, Lyons's theory is epistemically no better off than the kinetic theory of heat.

Furthermore, both philosophical and scientific theories are the products of the same kind of rule of inference, viz., inference to the best explanation. Van Fraassen, for instance, explicitly claims that both scientists and philosophers use inference to the best explanation 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. (van Fraassen, 1989: 131)

In a similar vein, Laudan claims that there is no methodological difference between science and philosophy in that 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: 19)

Given that theories, philosophical or scientific, are products of the same method, viz., inference to the best explanation, it is wrong to say that a pessimistic induction refutes scientific theories, but not antirealists' philosophical theories.

Antirealists might run *reductio ad absurdum* against my pessimistic induction. If my pessimistic induction is correct, there would be no *need* to come up with a new antirealist explanation because it will turn out to be problematic. One may generalize that there would be no need to come up with a new idea in any area of human endeavor because the new idea, whatever it might be, will be disclosed to be problematic. This logical consequence of my pessimistic induction is absurd. Therefore, my pessimistic induction is absurd.

My response to the foregoing objection is to argue that the alleged absurd consequence of my pessimistic induction might not be absurd. Antirealists would someday arrive at an unproblematic antirealist explanation, if past antirealist explanations have been improving upon their predecessors, and if future antirealist explanations also improve upon their forerunners. It is not clear, however, that there has been any improvement from van Fraassen's explanation (1980) to Stanford's explanation (2000). After all, it is neither the case that the scope of the past antirealist explanations has been increasingly widening, nor the case that the problems with the past antirealist explanations have been increasingly less serious. In the absence of such improvements, we have an inductive rationale for thinking that a future antirealist explanation will be no better than its antecedents.

In contrast, scientific theories have been improving upon their forerunners. As Devitt (2011: 292) and Park (2011: 80) point out, present theories are more successful than past theories. For example, the germ theory of diseases has a broader scope than the humoral theory of medicine, and the kinetic theory of heat explains more phenomena than the caloric theory of heat. Two pieces of cold metal become hot, when rubbed at a high speed. This heat

phenomenon can be explained by the kinetic theory of heat, but not by the caloric theory of heat (Hung, 1997: 83). Therefore, there is a need for scientists to come up with new scientific theories.

4. Conclusion

After criticizing eight antirealist explanations of the success of science, I constructed a pessimistic induction that the latest antirealist explanation, Lyons's proposal, will turn out to be problematic because its eight antecedents turned out to be problematic. My pessimistic induction against Lyons's proposal is on the same boat as the traditional pessimistic induction against successful present scientific theories that they will be revealed to be false because successful past scientific theories were revealed to be false. The two inductions rise or fall together. There is no reason for thinking that philosophical theories are resistant to a pessimistic induction while scientific theories are vulnerable to it.

An interesting consequence follows from the equal status of the two pessimistic inductions. All the criticisms that the readers of this paper will raise against my pessimistic induction will equally apply to the traditional pessimistic induction. For example, antirealists may argue that unlike the past antirealist explanations, Lyons's explanation is unproblematic, and it will not turn out to be problematic. This antirealist position presupposes that the future will differ from the past, whereas my pessimistic induction presupposes that the future will resemble the past. On the new antirealist account, the presupposition of my pessimistic induction is false. My reply to this possible antirealist position is to point out that the traditional pessimistic induction also presupposes that the future will resemble the past, and that the presupposition of the traditional pessimistic induction is also false, so unlike successful past scientific theories, successful present scientific theories are true, and they will not be disclosed to be false. In any event, it falls outside the range of this paper to take issue with the traditional pessimistic induction. This paper has the modest aim of showing that the pessimistic induction has devastating consequences on antirealism.

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