The Pessimistic Induction: A Bad Argument Gone Too Far[1]

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Abstract: In this paper, I consider the pessimistic induction construed as a deductive argument (specifically, *reductio ad absurdum*) and as an inductive argument (specifically, inductive generalization). I argue that both formulations of the pessimistic induction are fallacious. I also consider another possible interpretation of the pessimistic induction, namely, as pointing to counterexamples to the scientific realist's thesis that success is a reliable mark of (approximate) truth. I argue that this interpretation of the pessimistic induction fails, too. If this is correct, then the pessimistic induction is an utter failure that should be abandoned by scientific anti-realists.

Keywords: anti-realism; inductive generalization; pessimistic induction; scientific realism

1. Introduction

In the context of the scientific realism/anti-realism debate, scientific realism is taken to include one or more of the following theses (Psillos 2006, p. 135):

> (MSR) *The Metaphysical Thesis*: The world has a definite and mind-independent structure.
>
> (SSR) *The Semantic Thesis*: Scientific theories are truth-conditioned descriptions of their intended domain. Hence, they are capable of being true or false. The theoretical terms featuring in theories have putative factual reference. So if scientific theories are true, the unobservable entities they posit populate the world.
>
> (ESR) *The Epistemic Thesis*: Mature and predictively successful scientific theories are well-confirmed and approximately true. So entities posited by them, or, at any rate entities very similar to those posited, inhabit the world (Cf. Psillos 1999 and Boyd 1989).

The main argument against (ESR), known as the "pessimistic induction," the "pessimistic meta-induction," or the "disastrous historical meta-induction," has different versions that differ in their details (see, e.g., Poincaré 1952, p. 160; Putnam 1978, p. 25; Laudan 1981). Generally speaking, the argument begins by recalling that many scientific theories were once successful (in terms of their explanatory and predictive power). However, the pessimist argues, most of these past theories are now considered strictly false. Therefore, the pessimist concludes, current successful theories will turn out to be false as well.

This anti-realist argument has generated an enormous literature (see, e.g., Psillos 1999, pp. 102-103) and it still plays a prominent role in the scientific realism debate (see, e.g., the recent issue of the *Journal for General Philosophy of Science*, 42, 2011). For instance, Dopplet (2011, p. 295) argues that any realist account must "overcome the pessimistic meta-induction." (See also Devitt 2011.) As Wray (2011) recently pointed out, however, there seems to be little

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agreement about the form the pessimistic induction is supposed to take. Some have construed this argument as a *reductio* of the view that the apparent success of the best scientific theories of mature sciences justifies belief in the approximate truth of these theories or in the existence of the unobservables they posit, i.e., as a *reductio* of (ESR). For example, Lewis (2001, p. 372) reconstructs the argument as follows:

(1) Assume that the success of a theory is a reliable test for its truth.

(2) Most current scientific theories are successful.

(3) So most current scientific theories are true.

(4) Then most past scientific theories are false, since they differ from current theories in significant ways.

(5) Many of these false past theories were successful.

(6) So the success of a theory is not a reliable test for its truth. (Cf. Psillos 1996; Saatsi 2005.)

Similarly, Lange (2002, p. 282) argues that the pessimistic induction "takes the form of a *reductio* of the view that the apparent success of some scientific theory justifies our believing in its accuracy regarding unobservables."

More recently, Wray (2011) has argued that to reconstruct the pessimistic induction as a *reductio* is a mistake. Wray (2011) says that "the argument purports to be an *inductive* argument" (original emphasis; see also Held 2011). He then elaborates as follows (Wray 2011):

> I believe that from a survey of the history of science, Laudan infers that most past successful theories have turned out to be false. This claim then figures as a premise in an argument whose conclusion is that *our current best theories are probably false* (original emphasis).

In this paper, I examine the pessimistic induction construed as a deductive argument (specifically, *reductio*) and as an inductive argument (specifically, inductive generalization). My overall argument has two stages. First, I will argue that the pessimistic induction is a fallacious argument as follows:

(P1) Either the pessimistic induction is a deductive argument or the pessimistic induction is an inductive argument.
(P2) Construed as a deductive argument (i.e., *reductio*), the pessimistic induction is an invalid argument.
(P3) Construed as an inductive argument (i.e., inductive generalization), the pessimistic induction is a weak argument.
(C) Therefore, the pessimistic induction is a fallacious argument.

Then I will argue that the pessimistic induction also fails when understood as pointing to counterexamples to the scientific realist's thesis that success is a mark of (approximate) truth, since the alleged counterexamples miss their intended target.

2. The pessimistic induction as a deductive argument

In this section, I will not rehearse the familiar objections against the *reductio* formulation of the pessimistic induction, which are well known to readers of this journal (see, e.g., Lewis 2001 and Lange 2002). Instead, I would like to examine more closely steps (3) and (4) of Lewis' *reductio* formulation of the pessimistic induction quoted above. These steps in the reasoning can be unpacked as follows:

(3`) Most current scientific theories are true.
(4`) Most past scientific theories differ from most current scientific theories in significant ways.
(4``) Therefore, most past scientific theories are false.

Clearly, from the fact that past and current theories differ in "significant ways," it does not necessarily follow that past and current theories must have different truth values. Past theories and current theories can differ in significant ways, and yet have the same truth value. For example, consider a past scientific theory, such as Georg Ernst Stahl's theory of combustion, according to which a "fire principle" called *phlogiston* is expelled from metals when they are heated (which accounts for the fact that some metals gained in weight when they were burned), and a current scientific theory, such as string theory. Surely, these two theories differ in significant ways. One was proposed by one physician, whereas the other is the result of the work of many physicists and mathematicians. One has only one formulation, whereas the other has many. One is supposed to be a theory about combustion, whereas the other is supposed to be a theory of everything (i.e., a theory of the fundamental forces and particles). One had some experimental backing at the time (e.g., the ash of a metal like lead—called a *calx*—weighed more than the original metal), whereas the other currently has none. Even though these two theories differ in significant ways, string theory could still turn out to be false, just as Stahl's combustion theory did.

Or consider a past scientific theory, such as William Harvey's theory of blood circulation, according to which the heart pumps blood through the arteries to the veins and back to the heart in a continuous circulation (which accounts for the fact that, when a corpse is dissected, the arteries are nearly empty, whereas the veins may still contain some blood), and a current scientific theory, such as string theory. Surely, these two theories differ in significant ways. One was proposed by one physician, whereas the other is the result of the work of many physicists and mathematicians. One has only one formulation, whereas the other has many. One is supposed to be a theory about blood circulation, whereas the other is supposed to be a theory of everything (i.e., a theory of the fundamental forces and particles). One had some experimental backing at the time (e.g., the relation of the heart's contractions to the pulsing of blood into the arteries), whereas the other currently has none. Even though these two theories differ in significant ways, string theory could still turn out to be true, just as Harvey's theory of blood circulation did.

The point, then, is this: from the fact that past and current theories differ in significant ways, it does not necessarily follow that they must have different truth values, regardless of whether past theories were true or false. So a deductive argument with (3`) and (4`) as premises and (4``) as its conclusion is an invalid argument. If this is correct, then the *reductio* formulation of the pessimistic induction, which is supposed to be a *reductio*, and hence a deductive argument, is an invalid argument.

Pessimists might respond to this charge by trying to clarify the ambiguous phrase 'differ in significant ways' in Lewis' *reductio* formulation of the pessimistic induction. They might claim that 'differ in significant ways' means "differ in truth value." With this clarification, then, the aforementioned argument can be reformulated as follows:

(3`) Most current scientific theories are true.
(4```) Most past scientific theories differ from most current scientific theories in their truth value (i.e., most past scientific theories are false).
(4``) Therefore, most past scientific theories are false.

However, this argument seems circular. It purports to establish that most past scientific theories are false, but it assumes as one of its premises, i.e., (4```), that most past theories are false, since 'differ in significant ways' here just means "having a different truth value." Of course, the question is precisely whether most past theories are false—a claim that scientific realists will undoubtedly deny—and so pessimists cannot assume that claim as a premise in their argument against scientific realism without begging the question. Pessimists must provide an independent reason for thinking that (4``) is true, rather than assume a paraphrase of (4``), i.e., (4```), as a premise in an argument that purports to establish that (4``) is true.

Another possible formulation of these crucial steps in Lewis' *reductio* formulation of the pessimistic induction quoted above is the following:

(3`) Most current scientific theories are true.
(4````) If most current theories are true, then most past scientific theories are false.
(4``) Therefore, most past scientific theories are false.

But what reasons do we have for accepting (4````)? One might think that, for any abandoned theory T, assuming our current theories to be approximately true is just to assume that T is false. In other words, in order to show that (4````) is true, pessimists could argue as follows:

(V1) Scientists abandon theory T_1 in favor of T_2 only if they think that T_1 is false and T_2 is true.
(V2) T_1 has been abandoned by scientists in favor of T_2.
(V3) Therefore, T_1 is considered false and T_2 is considered true by scientists.

If this argument were sound, it would show that, of two competing theories, the one that is currently accepted is considered true by scientists, and the one that was abandoned is considered false by scientists. In other words, this argument would lend support to (4````). Then, based on this argument, pessimists could argue that, since the "history of science is a graveyard of [abandoned] theories" (Lipton 2005, p. 1265), there is a good reason to think that most of our current theories will end up in that graveyard.

However, this argument depends on the assumption that abandonment of a theory *T* by the scientific community is an indication that *T* is considered false. But why should we assume that? As a general claim about scientific change, (V1) seems false, since it is open to counterexamples. For example, Newlands' law of octaves has been abandoned in favor of the periodic law, not because it was strictly false, but because it did not apply to elements of higher atomic weights. Both laws share the essentially correct insight that the properties of the elements are a function of their atomic weight (Caven 1921, p. 69).

To this pessimists might reply that examples such as Newlands' law are rare, and so (V1) is likely to be generally true. In other words, pessimists might argue as follows:

(V1*) Usually, when scientists abandon theory T_1 in favor of T_2 it is because they think that T_1 is false and T_2 is true.
(V2*) T_1 has been abandoned by scientists in favor of T_2.
(V3*) Therefore, (probably) T_1 is considered false and T_2 is considered true by scientists.

I do not think that Newlands' law is a rare example from the history of science of a law that was abandoned, not because it was considered strictly false, but because it was not comprehensive enough. But even if it is a rare example, it is enough to break the relation of entailment between abandonment of a theory and strict falsity as a reason for that abandonment. Pessimists need this relation between abandonment of a theory and strict falsity to be a relation of entailment for the *reductio* formulation of the pessimistic induction to be a valid argument. If pessimists want to abandon the *reductio* formulation and construe the pessimistic induction as an inductive argument instead, then it seems that we need a larger and more diverse sample of theories in order to examine the proportion of current accepted theories to past competitors that were abandoned. I will consider such a sample in Section 3.

Pessimists might also try to appeal to inference to the best explanation (IBE is not a deductive form of inference; Psillos 2007, pp. 442-443) to support the claim that most past scientific theories are false and argue as follows:

(I1) The history of science is a graveyard of abandoned theories.
(I2) The best explanation for (I1) is that abandoned theories are strictly false.
(I3) No other hypothesis can explain (I1) as well as strict falsity does.
(I4) Therefore, abandoned theories are strictly false.

Then, based on this argument, pessimists could argue that this historical trend will continue, and so our current theories will end up in the graveyard as well. (I recognize that some constructive empiricists might not accept this argument, since it is an inference to the best explanation, even if they endorse the pessimistic induction. See, e.g., van Fraassen 1980, p. 143; van Fraassen 1989, pp. 131-150; Muller 2008, p. 143).

It is worth noting here a rather peculiar feature of the scientific realism debate. Pessimists look at the historical record of science and see failure, whereas realists look at the same historical record and see success (particularly, predictive success). Putting aside the question of whether the track record of science is really as bad as pessimists claim, or as good as realists claim, the important question for present purposes is whether (I3) is true. Again, it seems that strict falsity is not always the best explanation for why a theory has been abandoned. For example, one might reasonably think that Wegener's theory of continental drift was replaced by the theory of plate tectonics, not because the former was strictly false, but rather because the latter provides a more comprehensive account of certain geophysical phenomena. In other words, there are many reasons why theories are abandoned or replaced by other theories, and strict falsity is not always the best explanation for such scientific change.

If this is correct, then, in order to show that (3`) and (4`) entail (4``), which must be the case if Lewis' *reductio* formulation of the pessimistic induction is to be a valid argument, pessimists actually have to show that strict falsity is the *only* explanation (Bird 2007b) for the abandonment of a theory. To show that strict falsity is the *only* explanation for the abandonment of a theory, pessimists have to eliminate all the alternative explanations for the abandonment of a theory, such as that the theory is not comprehensive enough, that the theory is not well-confirmed, that the theory is incomplete, that the theory has no forceful and influential proponents, and so on. These alternative explanations haven't been eliminated. In fact, in the case of Wegener's theory and Newlands' law, the comprehensiveness explanation seems more plausible than the strict falsity explanation.

Finally, pessimists might try to clarify the ambiguous phrase 'differ in significant ways' by saying that 'significant ways' means that current theory T_1 is incompatible with past theory T_2, where T_1 and T_2 are competing theories about the same domain in nature. For example, Stahl's theory of combustion is incompatible with Lavoisier's theory of combustion, since they postulate the existence of different theoretical entities, phlogiston and oxygen, respectively. So, if current theories are true, their competing past theories must be false.

There are several problems with this pessimistic move. First, it is not obvious that competing theories that postulate different theoretical entities must be (logically) incompatible. For example, suppose that Stahl's theory of combustion is true, and so there is phlogiston, which is the substance that is responsible for combustion. But it could still be the case that there is oxygen as well. In fact, it might actually be a useful addition to Stahl's theory of combustion, since one of its problems was to explain why, when some metals were calcined, the resulting calx was heavier than the original metal. Some tried to explain this by saying that, in some metals, phlogiston has negative weight. Instead, they could have said that phlogiston is lost but oxygen is gained, which would explain the heavier weight. Second, even if it is granted to pessimists that current theories are incompatible with their competing past theories, it does not follow that most past theories are false. For that conclusion to follow, pessimists have to assume that current theories are true. But this assumption would be inconsistent with the conclusion of the pessimistic induction, which purports to show that current theories are false (Cf. Devitt 2011, p. 288). Third, for this pessimistic move to work, pessimists have to assume that every current theory has an actual corresponding past theory that is in direct competition with it. I do not think that this assumption is true. Consider, for example, Ehrlich's side-chain theory. What is the past theory that is incompatible with Ehrlich's side-chain theory? Or consider Boyle's gas law. What is the past theory that is incompatible with Boyle's law? Or consider Guth's inflation theory. What is the past theory that is incompatible with it? The point, then, is that not every theory has a past competitor that is incompatible with it.

Pessimists might reply by saying that most current theories have past competitors, which is compatible with the claim that some current theories do not have past competitors. The question, then, becomes what is the proportion of current theories that are accepted theories to past competitors that were abandoned. To answer this question, it seems that we need a larger and more diverse sample of theories. I will examine such a sample in Section 3.

If the aforementioned considerations are correct, then it seems that the *reductio* formulation of the pessimistic induction is an invalid argument. Perhaps roughly similar considerations have led Saatsi (2005, p. 1092) to argue that the following statistical argument should be added as a supplement to Lewis' *reductio* formulation of the pessimistic induction:

(1*) Of all the successful theories, current and past, most are taken to be false by the current lights.[3]
(2*) The current theories are essentially no different from the past successful theories with respect to their "observable" properties. (Viz. properties potentially figuring in the realist's explanatory argument.)
(3*) Success of a current theory is not a reliable indicator of its truth (by the reduction argument above), and there is no other reliable indicator of truth for the current theories.
(4*) Therefore any current successful theory is probably false by statistical reasoning.

For present purposes, I would like to make two points about Saatsi's statistical argument. First, notice the tension between premise (4) in Lewis' *reductio* formulation of the pessimistic induction and premise (2*) of Saatsi's statistical argument. The former says that past theories differ from current theories in significant ways, whereas the latter says that current theories are essentially no different from past theories. This seems to be a problem if, as Saatsi (2005, p. 1092) argues, his statistical argument is supposed to supplement Lewis' *reductio* formulation of the pessimistic induction. Second, why think that the statistical premise (1*) is true? Presumably, it is based on a sample of theories. But what is that sample? Is it a representative sample of successful theories?

In the next section, I show why the pessimistic induction is a weak inductive argument. To do so, I discuss Godfrey-Smith's account of random sampling as a form of inductive inference that is in-principle justifiable. I then argue that the pessimistic induction fails to measure up to the standards of this kind of inductive inference, since it does not provide what Godfrey-Smith calls "a bridge from observed to unobserved." If this is correct, then the pessimistic induction is a weak inductive argument.

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[3] It seems that (1*) should read as follows: "Of all the past successful theories, most are taken to be false by the current lights." After all, it seems to be inconsistent to say that current successful theories are taken to be false by the current lights, for the current light are supposed to be current theories. Thanks to a perceptive reviewer for noticing this inconsistency.

3. The pessimistic induction as an inductive argument

To see why the pessimistic induction is a weak inductive argument, we need to understand the sort of inductive argument that the pessimistic induction is supposed to be. Godfrey-Smith (2011, p. 34) argues that "the familiar philosophical concept of 'induction' has conflated two kinds of inference, each of which is successfully exploited by science" (Cf. Okasha 2001). As he (2011, p. 34) writes:

> For each of the two inference patterns, an account can be given of its *in-principle reliability*. That account is a kind of philosophical justification. The package usually known as "induction" does not have that kind of justification, however. It combines elements from each method without combining parts that give rise to an in-principle reliable combination in its own right.

Godfrey-Smith is concerned with the pattern of argument used to answer questions of the form 'How Many *F*s are *G*?' This is a question about proportion or frequency, which could be expressed by asking 'What is the rate of *G* in the *F*s?' For example (2011, p. 33):

1. How many teenagers smoke?
2. How many ravens are black?
3. How many emeralds are green?
4. How many people in this room are third sons?
5. How many organisms have the amount of bases cytosine and guanine equal, and the amount of adenine and thymine equal, in their DNA?
6. How many electrons have charge of approximately -1.6×10^{-19} coulombs?

In induction, these questions are answered by making a generalization based on the relation between *F* and *G* in the observed cases. So the number of observed *F*s is supposed to have epistemic significance. The classic case of inductive inference is thus the one where all observed *F*s are *G*, which is then used to infer that all unobserved *F*s are also *G*.

One form of inductive inference that is justifiable in principle, according to Godfrey-Smith, is inference based on random sampling. Random sampling means that "every member of the population you are drawing conclusions about has the same chance of making its way into the sample." He (2011, p. 40) argues that, in these sampling cases, "the power of randomness is what gives us a 'bridge' from observed to unobserved." For example, if we want to know how many teenagers smoke, then we collect a random sample of teenagers, measure the rate of smoking in the sample, and then extrapolate to the teenager population. Why should we follow this method rather than any other? Because we have a statistical model that tells us why this method is reliable in principle. According to Godfrey-Smith (2011, p. 39):

> The model tells us how samples of different sizes will be distributed, in relation to the actual properties of the population being sampled. It tells us when, and the extent to which, the properties of a sample are reliable indicators of the properties of the underlying population.

In collections that can be randomly sampled, then, the “bridge” from the observed to the unobserved is random sampling (i.e., every individual in the collection has the same chance of ending up in the sample).

But what do we do when the collections we want to draw conclusions about cannot be randomly sampled? In that case, there is another “bridge” from the observed to the unobserved. As Godfrey-Smith (2011, p. 39) writes:

> If we want to make inferences about a population that cannot be sampled, we must ask: what *kind* of collection is this? Are these objects the products of a common origin? Do they have a common internal structure? What sort of causal relationship is there likely to be between properties we are projecting from and properties we are projecting to? There need not be “laws of nature” overtly on the scene here, but we are basing the inference on some kind of natural connection—some combination of laws, mechanisms, and etiologies.

Unlike in inferences from random samples, numbers are not epistemically significant in inferences based on causal structures and kinds. According to Godfrey-Smith (2011, p. 41):

> In the purest examples of this sort of investigation, *one* instance of an *F* would be enough, in principle, if you picked the right case and analyzed it well. Ronald Reagan is supposed to have said “once you’ve seen one redwood, you’ve seen them all.” When something like this is true, it is a powerful basis for inference.

In practice, we may wish to find more instances in order to find out how *F* and *G* are related. In principle, however, assuming we have picked the right case, we do not need more cases to find out that there is an *F*-*G* association.

So, Godfrey-Smith proposes two distinct “bridges” from the observed to the unobserved. The first is generalization from random samples. According to Godfrey-Smith (2011, p. 42), “This form of inference has the following features: sample size matters, randomness matters, and ‘law-likeness’ or ‘naturalness’ does not matter.” The second is the “seen one, seen them all” kind of inference or generalization based on causal structures and kinds. According to Godfrey-Smith (2011, p. 42), “In these cases sample size *per se* does not matter, randomness does not matter, but the status of the kinds matters enormously.” He (2011, p. 42) argues that “These two strategies of inference involve distinct ‘bridges’ between observed and unobserved cases: one goes via the power of random sampling, the other via reliable operation of causes and mechanisms.”

Now, it seems clear that the pessimistic induction is not a “seen one, seen them all” kind of inference or generalization based on causal structures and kinds, for even pessimists concede that past scientific theories “differ from current theories in significant ways” [See premise (4) in Lewis’ *reductio* formulation of the pessimistic induction quoted above; Lewis 2001, p. 373]. As Bird (2007a, p. 80) puts it:

> The falsity of earlier theories is the very reason for developing the new ones—with a view to avoiding that falsity. It would be folly to argue that because no man has run 100 m in under 9.5 seconds no man ever will. On the contrary, improvements in times spur on other competitors, encourage improvements in training techniques and so forth, that make a sub 9.5 second 100 m quite a high probability in the near future. The analogy is imperfect, but sufficiently close to cast doubt on Laudan's pessimistic inference. Later scientific theories are not invented independently of the successes and failures of their predecessors. New theories avoid the pitfalls of their falsified predecessors and seek to incorporate their successes. Even if the successor theory is false also, we cannot apply a simple enumerative induction. […] we cannot make any good inference from the premise 'the succession of theories T_1, . . .T_n are all false' to the conclusion 'later theories in this sequence will also be false' without additional information.

Likewise, Lipton (2000, p. 197) argues that we cannot infer 'future theories are likely to be false' from 'past theories turned out to be false' by enumerative induction because of the "Darwinian" evolution of theories. Whether or not these realist replies to the pessimistic induction succeed is not important for present purposes. For present purposes, the important point is that the sample of theories is not uniform in the way that a generalization based on causal structures and kinds requires.

In a way that is somewhat similar to the attempts to disambiguate 'significant ways' discussed in Section 2 above, pessimists might respond that the pessimistic induction can still be construed as a "seen one, see them all" inductive inference as follows:

(i) For most past successful theories, if theory T_1 postulates entity E_1, then there is a competing theory T_2 that postulates entity E_2.
(ii) Therefore, for most successful theories, if T_1 postulates E_1, then there is a competing theory T_2 that postulates E_2.
(iii) For most past successful theories, if T_1 is now considered false because it is believed that E_1 does not exist, then T_2 is now considered true because it is believed that E_2 does exist.
(iv) Therefore, for most successful theories, if T_1 is now considered false because it is believed that E_1 does not exist, then T_2 is now considered true because it is believed that E_2 does exist.

As I have argued in Section 2 above, it is not at all obvious that most current theories have actual past competing theories. But even if most current theories have actual past competitors, theories that postulate different entities do not have to be incompatible, such that if one is true, the other has to be false. That is why a sample of theories is not uniform enough to serve as a basis for an inductive generalization based on causal structures and kinds. A uniform—as opposed to diverse—sample might be a sample of, say, copper rods. From a sample of just a few copper rods that are tested for electrical conductivity, it is reasonable to conclude that all copper rods conduct electricity because, if you have seen one or two copper rods, you have seen them all (given their uniform atomic structure). Scientific theories, however, are not as uniform as copper rods. The point, then, is that any sample of theories is not going to be uniform in a way that is required for a "seen one, seen them all" inductive generalization.

Magnus and Callender (2004, p. 324) also reconstruct the pessimistic induction as an argument based on a sample, and add that "the sample includes past scientific inferences, whether inductive or otherwise" (footnote 4). If the pessimistic induction is not a generalization based on causal structures and kinds ("seen one, seen them all"), then perhaps it is an inductive generalization from random samples. The pessimistic induction can be construed as an inductive generalization as follows:

(PIG1) Most past successful theories are false.
(PIG2) Therefore, most successful theories are false.

As Godfrey-Smith points out, it is random sampling that allows us to project a property from the observed sample onto the general population. However, in the case of this pessimistic inductive generalization, the problem is that the sample of successful theories from the history of science is not random. To see why this is a problem, consider the form of this kind of inductive generalization:

X % of observed *F*s are *G*.
Therefore, *X* % of all *F*s are *G*.

In the pessimistic inductive generalization, the reference class (*F*) is successful theories. The attribute class (*G*) is being false, i.e., the property of being false is attributed to most past successful theories, and, by induction, to most successful theories.

Now, the pessimistic inductive generalization is a weak inductive argument because, as an inductive generalization, it fails to provide grounds for projecting the property of the observed members of the reference class to unobserved members of the reference class. And this is so because the pessimistic inductive generalization does not provide what Godfrey-Smith calls a "bridge" between observed and unobserved.

To put it another way, pessimists argue that there is "a good basis for inductively inferring that any presently accepted scientific theory is actually false" (Held 2011). This good basis is supposed to be Laudan's list of successful but false theories (1981, p. 33):

- the crystalline spheres of ancient and medieval astronomy;
- the humoral theory of medicine;
- the effluvial theory of static electricity;
- 'catastrophic' 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 aether;
- the optical aether;
- the theory of circular inertia;
- theories of spontaneous generation.

If the aforementioned considerations are correct, however, then this list does not provide a good basis for a pessimistic inductive generalization. Laudan's list does not provide a good basis for a pessimistic inductive generalization because Laudan's list is not a good sample. It is not a good sample because it is not a random sample, i.e., it is not a sample where every individual in the population has the same chance of ending up in the sample. The theories in this list were not randomly selected. Rather, they were selected precisely because they are considered to be successful but strictly false. In other words, Laudan's list is a biased sample, which makes the pessimistic inductive generalization a weak inductive argument. As Park (2011, p. 83) puts it:

> the pessimistic induction is a fallacy of biased statistics. The pessimistic inducer took samples only from science before the twentieth century. [...] The minimum requirement for fair samples is that they be randomly selected from the sciences of both before and after the year 1900. Laudan's samples do not meet this requirement.

I have argued that Laudan's list of successful but false scientific theories is a biased sample because not all theories had an equal chance of making it into the sample. In fact, there are only distant-past successful theories in Laudan's list, but no recent-past successful theories, and thus it cannot serve as a sample in an inductive generalization about most successful theories. Doing so would be analogous to making the following inductive generalization:

(A1) Most American politicians are rich.
(A2) Therefore, most politicians are rich.

(A2) cannot be inferred from a sample that contains only American politicians, for it is not representative of the general population of politicians, which includes European, African, and other politicians. Similarly, (PIG2) cannot be inferred from a sample that contains only distant-past theories, for it is not representative of the general population of successful theories, which includes recent-past successful theories as well.

If this is correct, then Laudan's list is a biased sample because it is unrepresentative of the general population of scientific theories. Laudan's list is unrepresentative because, as a sample, it is not diverse enough to support the conclusion that most successful theories are false. The theories in Laudan's list were not randomly selected, but rather were cherry-picked in order to argue against a thesis of scientific realism. If this is correct, then the pessimistic inductive generalization is a weak inductive argument.

To this pessimists might object that, if we simply do the required random sampling, then the pessimistic inductive generalization would be vindicated and shown to be a strong inductive generalization. So, to get a random of sample of scientific theories (i.e., a sample where theories have an equal chance of being selected for the sample), I used the following methodology:

- Using *Oxford Reference Online*, I searched for instances of the word 'theory' in the following titles: *A Dictionary of Biology*, *A Dictionary of Chemistry*, *A Dictionary of Physics*, and *The Oxford Companion to the History of Modern Science*.
  - I limited myself to these reference sources to make the task more manageable.

  - Since it is not clear how to individuate theories (e.g., is the Modern Evolutionary Synthesis a theory or is each of its theoretical claims, such as the claims about natural selection and genetic drift, a theory in its own right?), I limited myself to instances of the word 'theory'.
- After collecting 124 instances of 'theory' and assigning a number to each instance, I used a random number generator to select 40 instances out of the 124.
- I divided the sample of 40 theories into three categories: accepted theories (i.e., theories that are accepted by the scientific community), abandoned theories (i.e., theories that were abandoned by the scientific community), and debated theories (i.e., theories whose status as accepted or rejected is in question) (See Table 1).

Table 1. A random sample of 40 scientific theories divided into three categories.

	Accepted Theories	**Abandoned Theories**	**Debated Theories**
	Conformal field theory	Einstein theory of specific heat	Lattice gauge theory
	Density functional theory	Bohr theory	Supermembrane theory
	Lowry-Bronsted theory	Phlogiston theory	Unified-field theory
	Quantum theory of radiation	Projective relativity theory	Sliding filament theory
	BCS theory	Higgs field	Kaluza-Klein theory
	Atomic theory	Brans-Dicke theory	
	Perturbation theory		
	Free-electron theory		
	Molecular-orbital theory		
	Quantum electrodynamics		
	Clonal selection theory		
	RRKM theory		
	Endosymbiont theory		
	Trichromatic theory		
	Chemiosmotic theory		
	Frontier-orbital theory		
	Resonating valence bond theory		
	Ligand-field theory		
	Crystal-field theory		
	Acid growth theory		
	Electroweak theory / GWS model		
	Kinetic theory		
	Transport theory		
	Debye-Huckel-Onsager theory		
	Big-bang theory		
	VSEPR theory		
	Quantum chromodynamics		
	Band theory		
	McMillan-Mayer theory		
TOTAL	29	6	5

Based on this sample, pessimists could construct the following inductive generalization:

> 15% of sampled theories are abandoned theories (i.e., considered false).
> Therefore, 15% of all theories are abandoned theories (i.e., considered false).

Clearly, this inductive generalization hardly justifies the pessimistic claim that most successful theories are false. Even if we consider the debated theories as false, the percentages do not improve much in favor of pessimists:

> 27% of sampled theories are abandoned theories (i.e., considered false).
> Therefore, 27% of all theories are abandoned theories (i.e., considered false).

So, doing the required random sampling for a strong inductive generalization, it seems, does not vindicate the pessimist's claim that most successful theories (both distant-past and recent-past theories) are false.

Indeed, one might think that this sample actually justifies an optimistic induction (Cf. Nola 2008). For, based on this random sample of theories, as opposed to Laudan's biased sample, one could construct the following inductive generalization:

> 72% of sampled theories are accepted theories (i.e., considered true).
> Therefore, 72% of all theories are accepted theories (i.e., considered true).

Scientific realists might be quite satisfied with a ± 70% success rate for scientific theories.

For good measure, I ran another trial with instances of the word 'law' instead of 'theory'. Here are the results, which are consistent with the first trial:

Table 2. A random sample of 40 scientific laws divided into three categories.

	Accepted Laws	**Abandoned Laws**	**Debated Laws**
	Conservation of mass	Maxwell-Boltzmann law	Virial equation*
	Carnot principle	Newton's law of gravitation	Charles' law*
	Periodic law	Newton's laws of motion	Raoult's law*
	Partition law	Law of octaves	Joule's law*
	Hubble's law	Titius-Bode law	Avogadro's law*
	Hess law		Boyle's law*
	Law of conservation of energy		Pressure law*
	Mass-energy equation		
	Stefan-Boltzmann law		
	Planck's radiation law		
	Wien's displacement law		
	Law of chemical equilibrium		
	Law of constant proportions		
	Law of multiple proportions		
	Law of reciprocal proportions		
	Coulomb's law		
	Law of periodicity		
	Fermat's principle		
	2nd law of thermodynamics		
	Zeroth law of thermodynamics		
	Kepler's law		
	Graham's law		
	Hooke's law		
	Grottius-Draper law		
	Bragg's law		
	Balmer's law (Balmer series)		
	Moseley's law		
	Law of isomorphism		
TOTAL	28	5	7

(Laws marked with * on the "Debated Laws" column are laws pertaining to idealized entities, such as ideal gases and ideal solutions. Although some of these laws are accepted and used by scientists, anti-realists and pessimists would probably object to their inclusion in the "Accepted Laws" column, which is why I put them in the "Debated Laws" column.)

Based on this sample, pessimists could construct the following inductive generalization:

12.5% of sampled laws are abandoned laws (i.e., considered false).
Therefore, 12.5% of all laws are abandoned laws (i.e., considered false).

Clearly, this inductive generalization hardly justifies the pessimistic claim that most successful laws are false. Even if we consider the debated laws as false, the percentages do not improve much in favor of pessimists:

30% of sampled laws are abandoned laws (i.e., considered false).
Therefore, 30% of all laws are abandoned laws (i.e., considered false).

Again, doing the required random sampling for a strong inductive generalization, it seems, does not vindicate the pessimist's claim that most successful laws (both distant-past and recent-past laws) are false.

Indeed, one might think that this sample actually justifies an optimistic induction (Cf. Nola 2008). For, based on this random sample of laws, one could construct the following inductive generalization:

70% of sampled laws are accepted laws (i.e., considered true).
Therefore, 70% of all laws are accepted laws (i.e., considered true).

Again, scientific realists might be quite satisfied with a ± 70% success rate for scientific laws. To be clear, I do not endorse any optimistic inductive generalization. My present aim is simply to show that the pessimistic induction is a bad argument. But I do think that scientific realists would be better off relying on an optimistic inductive generalization, rather than the No-Miracles argument. For, as I have argued elsewhere, the No-Miracles argument fails to establish what it purports to establish (See Mizrahi 2012). Of course, realists would still need an additional argument that shows that 70% of all theories are approximately true (or that we are justified in believing that 70% of all theories are approximately true).

4. The pessimistic induction as pointing to counterexamples

If the aforementioned considerations are correct, then the pessimistic induction turns out to be an invalid deductive argument when construed as a *reductio* and a weak inductive argument when construed as an inductive generalization. At this point, pessimists might insist that the pessimistic induction is not meant to be an argument at all. Perhaps it is meant to point out counterexamples to the realist's claim that success is a mark of (approximate) truth. As Laudan (1981, pp. 47-48) writes: the realist's "epistemology is confronted by anomalies which seem beyond its resources to grapple with."

In order for counterexamples to succeed, however, their target claims must be understood as universal statements. For example, finding one bird that cannot fly would refute the claim that birds can fly only if this claim is understood as a universal generalization, i.e., "All birds can fly." However, finding one bird that cannot fly would not refute the claim that a keel is a reliable indicator of flying ability in birds. There are flightless birds, such as ostriches and kiwis, with a keel on their breastbone (though greatly reduced). But this is consistent with the claim that birds with a keel on their breastbone are more likely to be flying birds than flightless birds.

Similarly, to use Kitcher's (1993, p. 118) example, to say that single-stranded DNA is a counterexample to the double-helical model of DNA is to misunderstand the intended domain of the model. The model is not meant to be a universal statement about DNA. Now, it is not clear that the realist's claim that success is a mark of (approximate) truth is meant to be a universal statement. Success may be a reliable indicator of (approximate) truth, but this is compatible with some instances of successful theories that turn out not to be approximately true. In other words, that a theory is successful is a reason to believe that it is approximately true, but it is not a conclusive proof that the theory is approximately true. To illustrate:

(S1) If *T* is successful, then it is likely that *T* is approximately true (since success is a reliable indicator of approximate truth).
(S2) The big-bang theory is successful.
(S3) Therefore, it is likely that the big-bang theory is approximately true.

Conversely:

(S4) If *T* is unsuccessful, then it is unlikely that *T* is approximately true (since success is a reliable indicator of approximate truth).
(S5) The steady-state theory is unsuccessful.
(S6) Therefore, it is unlikely that the steady-state theory is approximately true.

For these reasons, to give a counterexample to (S1) is to misunderstand the realist's thesis. It would be as absurd as citing a few examples of 'A' students who have been absent more than fifty percent of the semester as a counterexample to the thesis that being absent on a regular basis is a reliable predictor of earning a failing grade. Similarly, it would be absurd to cite a few examples of successful but false theories as counterexamples to the thesis that success is a reliable predictor of (approximate) truth. As Putnam (1975, p. 73) writes: "That terms in mature scientific theory *typically* refer (this formulation is due to Richard Boyd), that the theories accepted in a mature science are *typically* approximately true […]—these statements are viewed by the scientific realist not as necessary truth but as part of the only scientific explanation of the success of science" (emphasis added).

5. Conclusion

In this paper, I have considered three interpretations of the pessimistic induction: as a *reductio*, as an inductive generalization, and as pointing to counterexamples against the scientific realist's thesis that success is a reliable indicator of (approximate) truth. If my argument is sound, then, contrary to what Saatsi (2005, p. 1098) claims, the pessimistic induction is not "a powerful force to be reckoned with." This is so because the pessimistic induction is a fallacious argument and the counterexamples to which it points miss their intended target.

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