Historical Inductions: New Cherries, Same Old Cherry-Picking

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Abstract. In this paper, I argue that arguments from the history of science against scientific realism, like the arguments advanced by Kyle Stanford and Peter Vickers, are fallacious. The so-called "Old Induction," like Vickers', and the so-called "New Induction," like Stanford's, are both guilty of confirmation bias, specifically, of cherry-picking evidence that allegedly challenges scientific realism, while ignoring evidence to the contrary. I also show that the historical episodes Stanford adduces in support of his New Induction are indeterminate between a pessimistic interpretation and an optimistic interpretation. For these reasons, these arguments are fallacious, and thus do not pose a serious challenge to scientific realism.

1. Introduction

Scientific Realism is usually taken to include one or more of the following theses (Psillos 2006, 135):

> *The Metaphysical Thesis*: The world has a definite and mind-independent structure.
>
> *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.
>
> *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. (See also the introduction to Psillos 1999 and Chakravartty 2007 for a useful classification of realist and antirealist views about science.)

Arguments from the history of science are typically advanced by antirealists against the epistemic thesis of scientific realism. Antirealists point out that there are scientific theories that were predictively successful, i.e., they made novel predictions that were borne out by observations or experimentation, but that are now considered to be strictly false. As Lipton puts it:

> The history of science is a graveyard of theories that were empirically successful for a time, but are now known to be false, and of theoretical entities—the crystalline spheres, phlogiston, caloric, the ether and their ilk—that we now know do not exist. Science does not have a good track record for truth, and this provides the basis for a simple empirical generalization. Put crudely, all past theories have turned out to be false, therefore it is probable that all present and future theories will be false as well. That is the pessimistic induction (Lipton 2005, 1265).

The historical premise of this pessimistic argument is often supported by a list that Laudan complied in his (1981). Here is Laudan's list:

- Crystalline spheres of ancient and medieval astronomy;
- Humoral theory of medicine;
- Effluvial theory of static electricity;
- 'Catastrophic' geology, with its commitment to a universal (Noachian) deluge;
- Phlogiston theory of chemistry;
- Caloric theory of heat;
- Vibratory theory of heat;
- Vital force theories of physiology;
- Electromagnetic aether;
- Optical aether;
- Theory of circular inertia;
- Theories of spontaneous generation (Laudan 1981, 33).

More recently, Vickers (2013) sought to "confront" scientific realism by expanding and improving upon Laudan's list. Vickers (2013, 190) claims that "the new realist position [i.e., the *divide et impera* strategy (see, e.g., Kitcher 2001, 18 and Psillos 1999, 106)] needs to be thoroughly tested against relevant episodes from the history of science." For Vickers,

> This means presenting cases from the history of science in which novel predictive success was achieved by a scientific theory later rejected as (very) false and then asking whether one or another variation on the divide et impera strategy succeeds for such cases (Vickers 2013, 190).

Vickers proceeds to list twenty such cases, three of which he discusses at great length.

Another argument from the history of science against scientific realism was advanced by Stanford (2006). According to Stanford (2006, 20), "the history of scientific inquiry itself offers a straightforward 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, 19) calls his argument "the *new induction over the history of science*" (emphasis in original) to distinguish it from the old pessimistic induction (cf. Enfield 2008).

In what follows, I argue that both the Old Induction and the New Induction are guilty of the same charge, namely, confirmation bias, or more specifically, cherry-picking a few cases from the history of science that are taken to pose a challenge to scientific realism. Since scientific realism is supposed to be a scientific explanation that is supported by a defeasible Inference to the Best Explanation (IBE), however, this strategy of arguing against scientific realism is ineffective.

Here is how I plan to proceed. In Section 2, I show that the Old Induction, even the one based on Vickers' (2013) new and improved list, is fallacious. In Section 3, I show that Stanford's (2006) New Induction is fallacious as well. I also show that the historical episodes Stanford adduces in support of his New Induction are indeterminate between a pessimistic interpretation and an optimistic interpretation. I conclude that the Old Induction and the New Induction are fallacious arguments, and thus they fail to pose a serious challenge to scientific realism.

2. The Old Induction

As I understand it, Vickers' list is supposed to expand and improve upon Laudan's list. Here is Vickers' list:

1. Caloric
2. Phlogiston
3. Fresnel's theory of light and the luminiferous ether
4. Rankine's vortex theory of thermodynamics
5. Kekulé's theory of the Benzene molecule
6. Dirac and the positron
7. Teleomechanism and gill slits
8. Reduction division in the formation of sex cells
9. The Titius-Bode law
10. Kepler's predictions concerning the rotation of the sun
11. Kirchhoff's theory of diffraction
12. Bohr's prediction of the spectral lines of ionized helium
13. Sommerfeld's prediction of the hydrogen fine structure
14. Velikovsky and Venus
15. Steady state cosmology
16. The achromatic telescope

17. The momentum of light

18. S-matrix theory

19. Variation of electron mass with velocity

20. Taking the thermodynamic limit (Vickers 2013, 191-194)

Vickers claims that *divide et impera* realism (see, e.g., Kitcher 2001, 18 and Psillos 1999, 106) is "challenged by" his new list (Vickers 2013, 209) and that the "20 cases presented leave the realist having to say something" (Vickers 2013, 194).

Unfortunately, Vickers does not say exactly how these cases are supposed to "confront" or "challenge" scientific realism (of the *divide et impera* variety) and why "the realist has to say at least something about them" (Vickers 2013, 194). There are basically two ways to interpret Vickers' list as a challenge to scientific realism:

> *Inductive generalization*: Like Laudan's list, Vickers' list is supposed to be a sample from which it can be inferred by inductive generalization that most predictively successful theories are (and will be) rejected as strictly false.
>
> *Deductive argument from counterexamples*: Vickers' list is a list of counterexamples that refute the realist's claim that novel predictive success is a reliable indicator of approximate truth.

I will now consider these two interpretations of Vickers' argument against *divide et impera* realism.

2a. Vickers' argument as an inductive generalization

If Vickers' list does indeed support the inductive inference to the conclusion that most predictively successful theories are rejected as strictly false, then it would follow that novel predictive success is not a reliable mark of approximate truth, even as far as the working posits of successful theories are concerned. That is, the inductive generalization from Vickers' list would run as follows:

> 100% of the predictively successful theories on Vickers' list are strictly false.
>
> Therefore, probably,
>
> Most predictively successful theories are strictly false.

If this inductive generalization were cogent, then it would show that our predictively successful theories, even our current ones, will probably be rejected as strictly false eventually, since most predictively successful theories are strictly false.

As Park (2011) and I (2013a) have argued, since Laudan's list is not a random sample of scientific theories, which means that it cannot be a representative sample of scientific theories, any inductive argument that is based on Laudan's list commits the fallacy of biased statistics. (For other recent criticisms of the pessimistic induction, see Doppelt 2007, Frost-Arnold 2011, and Park 2014.) That is, an inductive argument from a sample that is based on Laudan's list is a bad inductive generalization because it commits the "fallacy of biased statistics" (Park 2011, 83) and because the "theories on Laudan's list were not randomly selected, but rather were cherry-picked in order to argue against a thesis of scientific realism" (Mizrahi 2013a, 3220), given that "Cherry picking is wrong on all statistical approaches" (Dienes 2011, 280). (See also Mayo 1996.)

So, if Vickers' argument, like Laudan's pessimistic induction, is meant to be an inductive generalization from a sample, then it, too, faces a serious problem. Like Laudan's list, Vickers' list is not a random sample of successful but false theories, which means that it, too, cannot be a representative sample of scientific theories. Like Laudan's list, Vickers' list is too small a sample to support an inductive generalization about successful theories. Moreover, the successful theories on Vickers' list were not randomly selected but rather were cherry-picked in an attempt to challenge scientific realism. A sample is random just in case "every member of the population you are drawing conclusions about has the same chance of making its way into the sample" (Godfrey-Smith 2011, 39). As far as Vickers' list is concerned, this condition of random sampling is violated, since the theories on Vickers' list were cherry-picked, and thus it is not the case that all successful theories had the same chance of making it into Vickers' list. Vickers' list, then, fails to "confront" or challenge scientific realism (specifically, *divide et impera* realism) because any inductive inference from Vickers' list to the effect that most successful theories are strictly false, and thus that *divide et impera* realism is probably false, would be fallacious, given that Vickers' list is a small and biased sample. In fact, when a proper random sample of successful theories is collected, the results might actually support an optimistic, rather than a pessimistic, induction (Mizrahi 2013a, 3222).

Arguing against scientific realism, particularly the epistemic thesis of scientific realism, by cherry-picking a few cases from the history of science is an ineffective strategy. To see why, recall that the scientific realist's main argument is the No-Miracles Argument (NMA). The NMA is an Inference to the Best Explanation (IBE). As such, its premises, if true, only lend probable, not conclusive, support to its conclusion. In other words, the inference is defeasible; scientific realism (of the *divide et impera* variety) is said to be the best explanation for novel predictive success, and hence probable. Indeed, scientific realists have been clear about this from the get go. As Putnam writes:

> That terms in mature scientific theory *typically* refer […], 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 (Putnam 1975, 73; emphasis added). (Cf. Worrall 2011, 13.)

Of course, trying to undermine scientific explanations that are based on defeasible inferences by cherry-picking a few cases is bound to fail unless we have good reasons to believe that the cherry-picked cases are representative of the general population (cf. Mizrahi 2013a, 3224). As far as Vickers' list is concerned, however, we have no reasons to think that the theories on his list are representative of the general population of successful theories. Since Vickers' list is not a random sample, we have no reason to think that it is a representative sample. Since we have no reason to think that Vickers' list is a representative sample, for all we know, it may consist entirely of outliers. Just as the claim that smoking increases one's chances of getting lung cancer cannot be "confronted" or challenged by selecting a few outliers (i.e., people who smoke but do not get sick with lung cancer), the scientific realist's claim that approximate truth is the best explanation for novel predictive success cannot be "confronted" or challenged by selecting a few outliers (i.e., successful but false theories). To seriously challenge scientific realism, what is needed is random sampling, not cherry-picking.

It is worth noting that to say that a sample is not random because the members of the sample have been cherry-picked is *not* to say that the cherry-picking was intentional or deliberate. The "tendency to cherry-pick evidence or argumentative points that favor the prior belief" (Henley 2014, 220) is a kind of confirmation bias, which "can occur unconsciously" (Coon and Mitterer 2013, 21). In other words, whether the arguer intended it or not, a sample that was selected non-randomly cannot be representative of the target population as a whole. For a sample to be representative of the general population, it must be the case that "every member of the population you are drawing conclusions about has the same chance of making its way into the sample" (Godfrey-Smith 2011, 39).

2b. Vickers' argument as a deductive argument from counterexamples

It might be objected that Vickers' argument is not meant to be an inductive argument at all. Instead, it is supposed to be a deductive argument from counterexamples (Lyons 2002). More explicitly, the objection goes, Vickers' argument is supposed to run as follows:

V1. If the approximate truth of the essential parts of successful theories explains their novel predictive success, then there would be no successful theories that are strictly false.

V2. There are successful theories that are strictly false (such as those on Vickers' list).

V3. Therefore, it is not the case that the approximate truth of the essential parts of successful theories explains their novel predictive success.

Although this argument appears to have a valid form (i.e., *modus tollens*), I think that this argument is fallacious for pretty much the same reasons that Laudan's pessimistic induction is fallacious when construed as a *reductio* of the epistemic thesis of scientific realism (Mizrahi 2013a, 3211-3215). To see why, note that Vickers himself characterizes *divide et impera* realism as follows:

> [DIR] When a theory achieves *novel predictive* success, then, *probably* (although not always), that means that the *parts* of the theory responsible for that success are a*t least approximately* true (Vickers 2012, 17; emphasis in original).

Accordingly, Vickers acknowledges that DIR is a statistical, not a universal, generalization. That is, the form of the realist's claim is not 'All *F*s are *G*s' but rather 'Most *F*s are *G*s', or more accurately, '*F*ness is a reliable predictor of *G*ness'. A claim of this form, however, cannot be refuted, or even challenged, by a few selected counterexamples. To see why, suppose, for example, that high scores on standardized tests are reliable predictors of college success. In that case, a few selected high school graduates who scored high on standardized tests but failed in college do not refute, or even challenge, the claim that high scores in standardized tests are reliable predictors of college success, since such a claim is based on statistical evidence, and the selected graduates who failed in college may simply be outliers. To allow a select few, albeit vivid, counterexamples to overshadow statistical evidence is a mistake in reasoning known as "misleading vividness" (Salmon 2013, 151).

Moreover, confirmation holism, which is generally accepted among philosophers of science, means that, strictly speaking, scientific hypotheses cannot be *refuted* by counterexamples. As Okasha explains:

> According to the doctrine of confirmation holism, also known as the 'Quine-Duhem' thesis, the empirical content of a scientific theory cannot be parcelled out individually among the constituent components of the theory. Thus when a theory makes an empirical prediction which turns out to be false, it will not be automatically obvious where to lay the blame, i.e., which component of the theory to reject. Logic tells us there is an error somewhere in the set of statements which implies the false prediction, but does not tell us where. So there will be various ways of modifying our theory to inactivate the false implication (Okasha 2002, 306).

Given confirmation holism, then, premise V1 would be strictly false, since DIR says that there would be *some* successful but false scientific theories. More importantly, to derive the prediction (i.e., the consequent of premise V1) from the hypothesis (i.e., the antecedent of premise V1), one would have to assume that successful theories that are considered false *by current lights* are indeed strictly false (cf. Saatsi 2005, 1092). But this auxiliary assumption appears to be in tension with the pessimist's claim that most successful theories are strictly false. For, in order to claim that successful theories are strictly false *by current lights*, one must assume that the current lights are (at least approximately) right. But the current lights, of course, are current theories (see Devitt 2011, 288 and Mizrahi 2013a, 3214-3215). So, at the very least, one would have to assume that abandonment of a scientific theory *entails* that it is strictly false. But this assumption is also problematic because there were successful theories that were abandoned for reasons other than being considered false (see my 2013a, 3213).

Whatever one assumes as an auxiliary assumption in order to derive the consequent of premise V1 from the antecedent of premise 1, i.e., from DIR, even if we grant that premise V2 is true, all that can be *validly* inferred is a disjunction, i.e., either DIR is false or the auxiliary assumption is false, not a "refutation" of DIR (i.e., 'DIR is false'). That is:

V1*. If DIR *and A* (where *A* is an auxiliary assumption such that abandonment of a theory is a sure sign of strict falsity), then there would be no successful theories that are strictly false.

V2. There are successful theories that are strictly false (such as those on Vickers' list).

V3*. Therefore, either it is not the case that DIR or it is not the case that *A*.

Accordingly, the negation of DIR cannot be *validly* inferred from premises V1* and V2. Indeed, we have good reasons to think that the auxiliary assumption is false (Mizrahi 2013a, 3213). For these reasons, construed as a *modus tollens* argument from counterexamples against DIR, Vickers' argument fails to "confront" or pose a challenge to DIR.

3. The New Induction

Stanford's (2006, 19) New Induction is an "*induction over the history of science*" (emphasis in original). (For criticisms of Stanford's problem of unconceived alternatives and the New Induction, see Magnus 2006 and 2010, Chakravartty 2008, Godfrey-Smith 2008, Devitt 2011, and Ruhmkorff 2011.) Unlike the Old Induction, however, Stanford's New Induction is about us (more specifically, scientists), not about theories *per se*. That is, according to Stanford:

> *we* have repeatedly found *ourselves* encouraged or even forced under the impetus provided by recalcitrant phenomena, unexpected anomalies, and other theoretical pressures to discover new theories that had remained previously unconceived despite being well confirmed by the evidence available to *us* (Stanford 2006, 19; emphasis added).

The historical episodes that support this claim, according to Stanford, are the following theoretical transitions:

1. elemental to early corpuscularian chemistry to Stahl's phlogiston theory to Lavoisier's oxygen chemistry to Daltonian atomic and contemporary chemistry
2. various versions of preformationism to epigenetic theories of embryology
3. the caloric theory of heat to later and ultimately contemporary thermodynamic theories
4. effluvial theories of electricity and magnetism to theories of the electromagnetic ether and contemporary electromagnetism
5. humoral imbalance to miasmatic to contagion and ultimately germ theories of disease
6. eighteenth century corpuscular theories of light to nineteenth century wave theories to the contemporary quantum mechanical conception
7. Darwin's pangenesis theory of inheritance to Weismann's germ-plasm theory to Mendelain and the contemporary molecular genetics
8. Cuvier's theory of functionally integrated and necessarily static biological species and from Lamarck's autogenesis to Darwin's evolutionary theory (Stanford 2006, 19-20)

Stanford (2006, 20) claims that "even this fairly short list suffices to illustrate that the pattern is characteristic of theoretical science across a wide variety of fields and historical circumstances." But does Stanford's list really provide sufficient evidence for this general claim?

Since Stanford (2006, 19) himself says that his argument is a "*new induction over the history of science*" (emphasis in original), I think that the most charitable way to interpret Stanford's New Induction against the epistemic thesis of scientific realism is as follows:

Stanford's list is supposed to be a sample from which it can be inferred by inductive generalization that most scientific theories have unconceived alternatives. (On Stanford's own response to the problem of unconceived alternatives, namely, "epistemic instrumentalism," see Fine 2008.) More explicitly:

> 100% of the successful theories on Stanford's list had unconceived alternatives.
>
> Therefore, probably,
>
> Most successful theories have unconceived alternatives.

According to Stanford (2006, 20), then, "the history of scientific inquiry itself offers a straightforward 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" (emphasis added). If Stanford's New Induction were cogent, then it would challenge the realist's claim that approximate truth is the best explanation for novel predictive success, since if there are indeed several alternatives that are equally well-confirmed by the evidence, and *not all of them can be approximately true,* then we should not believe that our current successful theories are approximately true (cf. Magnus 2010, 807).

As Stanford (2006, 20) himself admits, this sample is too small. Is it representative of scientific theories in general? I will argue that Stanford's sample is not a representative sample for the following reasons. First, Stanford's list of historical episodes is not a random sample. Second, the historical record on which Stanford's New Induction is based does not uniquely support a pessimistic conclusion. I will now flesh out these criticisms of Stanford's New Induction in more detail.

3a. Stanford's New Induction is fallacious

In light of the aforementioned considerations concerning the Old Induction, it should now be clear why Stanford's list does not support an inductive generalization from a sample. Like Laudan's list and Vickers' list, the historical episodes on Stanford's list were not randomly selected, but rather were cherry-picked to make a case against scientific realism. As Godfrey-Smith (2011, 40) points out, as far as inductive generalizations from samples are concerned, "the power of randomness is what gives us a 'bridge' from observed to unobserved." Since Stanford's list is not a random sample, there is no "bridge" between Stanford's sample of successful theories with unconceived alternatives and the general conclusion that most successful theories are such that they have unconceived alternatives.

Again, this is *not* to say that Stanford intentionally or deliberately selected historical episodes that he took to be positive evidence for his case against scientific realism. Rather, to say that a sample of data was cherry-picked is to say that the sample is biased, and thus fails to be representative of the general population. If the old pessimist claims statistical significance for successful but false theories without acknowledging the cases of successful but not false (because not rejected as such) theories, then she is picking "only the most ripe cherries from the tree" (Willink 2013, 184). Similarly, if the new pessimist claims statistical significance for successful theories that have unconceived alternatives without acknowledging the episodes of successful theories that have no unconceived alternatives that are equally well-confirmed by the evidence, then she is picking "only the most ripe cherries from the tree" (Willink 2013, 184). This is a problem not only because biased samples do not license inductive generalizations but also because a "cherry-picking" effect significantly increases the chance of a Type I error (false positives). Without random sampling, we cannot tell if Stanford's list is not simply a list of false positives (i.e., theories that are thought to have unconceived alternatives that are equally well-confirmed by the evidence but in fact do not). I will discuss some reasons to think that some of the items on Stanford's list are in fact false positives.

Before I do so, however, I should point out that an unconceived alternative, according to Stanford, is a *competing* theory that is "well confirmed by the body of actual evidence we have in hand" (Stanford 2006, 18). That is, a mere logical possibility is *not* an unconceived alternative because it is not a competing theory that is "*well confirmed by the body of actual evidence we have in hand*." For example, the hypothesis that the universe is accelerating because God is blowing wind on galaxies is *not* an unconceived alternative to the dark energy hypothesis because it is not confirmed (let alone *well* confirmed) by the body of actual evidence we have in hand. (Cf. Forber 2008, 137.)

I should also point out that, in light of the aforementioned discussion of Vickers' argument as a deductive argument from counterexamples, it should be clear why Stanford's New Induction would not work as a deductive argument from counterexamples. That is, if Stanford's New Induction is meant to be a deductive argument that runs as follows:

S1. If novel predictive success is a reliable mark of approximate truth (SR), then there would be no successful theories that have unconceived alternatives.
S2. There are successful theories that have unconceived alternatives.
S3. Therefore, it is not the case that SR.

Then, although this argument appears to have a valid form (i.e., *modus tollens*), it is fallacious for pretty much the same reasons that the construal of Vickers' argument as a deductive argument from counterexamples is fallacious. First, to give statistical significance to a select few, albeit vivid, counterexamples (in this case, successful theories with unconceived alternatives) while ignoring statistical evidence to the contrary (i.e., successful theories without unconceived alternatives) is a mistake in reasoning known as "misleading vividness" (Salmon 2013, 151). Second, given confirmation holism, premise S1 would be strictly false, since to say that novel predictive success is a *reliable mark* of approximate truth is to allow for *some* outliers (i.e., successful theories that are not approximately true). Since the episodes on Stanford's list were not randomly selected, they may simply be outliers. Third, to derive the prediction (i.e., the consequent of premise S1) from the hypothesis (i.e., the antecedent of premise S1), i.e., from SR, one would have to assume that unconceived alternatives cannot all be approximately true. That is, to undercut the supposed explanatory connection between novel predictive success and approximate truth, the new pessimist needs to show that some successful alternatives are strictly false. And so, if there are several successful alternatives, and not all of them can be approximately true, it follows that some successful alternatives must be false. If this is correct, then Stanford's argument, construed as a deductive argument from counterexamples, should run as follows:

- S1*. If SR and *A* (where *A* is an auxiliary assumption such that, of a set of successful alternatives, at least some must be false), then there would be no successful theories that have unconceived alternatives.
- S2. There are successful theories that have unconceived alternatives.
- S3*. Therefore, either it is not the case that SR or it is not the case that *A*.

As in the case of the deductive construal of Vickers' argument, the negation of SR cannot be *validly* inferred from premises S1* and S2. Instead, what follows deductively is a disjunction, i.e., 'not-SR or not-*A*', not a negation of SR. For these reasons, construed as a *modus tollens* argument from counterexamples against SR, Stanford's argument fails to pose a challenge to SR. In fact, there is a good reason to reject the auxiliary assumption. Recall that an unconceived alternative must be a *competing* theory (Stanford 2006, 18). But two theories may be competing theories and still be compatible in the sense that both could be true. For example, suppose that my car does no start. There are at least two potential explanations for this: (a) the car does not start because the battery is dead; (b) the car does not start because it is out of gas. These two explanations are competing hypotheses for the same fact, namely, that my car does not start, insofar as each, if true, would explain why the car does not start. But (a) and (b) are not incompatible, since both (a) and (b) could be true. If this is correct, then insofar as (a) and (b) are competing theories, it is not the case that at least one of them must be false. Even if we suppose for the sake of argument that (b) is an unconceived alternative to (a) that is equally well-confirmed by the available evidence, it is still the case that both could be true. If this is correct, then the auxiliary assumption *A* in S1* is false.

3b. Stanford's historical evidence is indeterminate

There are good reasons to think that Stanford's list is a list of false positives, i.e., theories that are thought to have unconceived alternatives that are equally well-confirmed by the available evidence but in fact do not. In other words, the historical record on which Stanford's New Induction is based does not uniquely support a pessimistic conclusion. Instead, Stanford's historical evidence is indeterminate between a pessimistic interpretation and an optimistic interpretation. To see why, take, for example, (7) on Stanford's list, which is the one that Stanford discusses at great length in his (2006). Now, consider the following historical facts about the development of theories of inheritance (Tobias et al 2011, 1):

1859 Charles Darwin publishes *On the Origin of Species*

1865 Gregor Mendel's experiments on plant hybridization presented to Brunn Natural History Society

1866 Mendel's report formally published

1868 Charles Darwin's "provisional hypothesis of pangenesis"

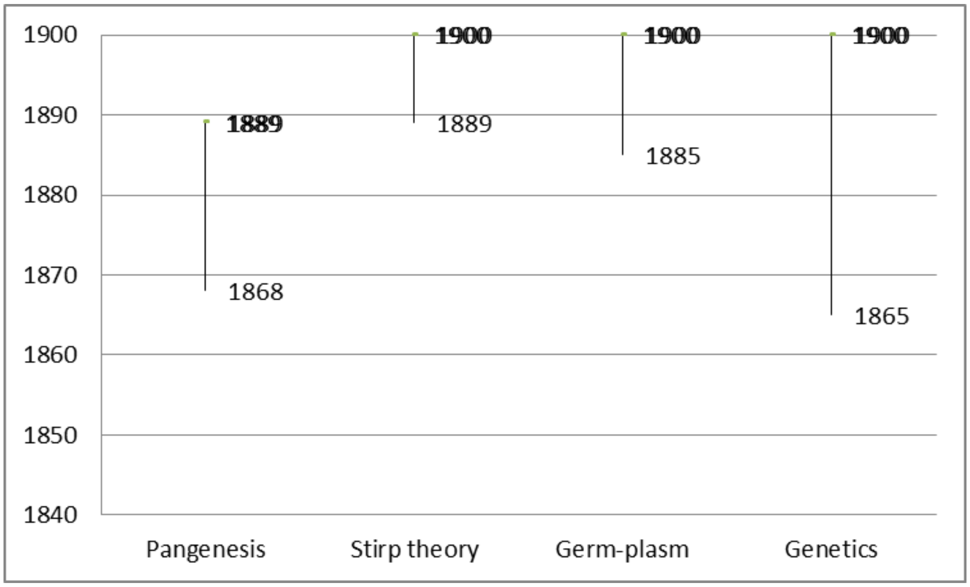
1885 "Continuity of the germ plasm" (August Weismann)

1889 Francis Galton's *Law of Ancestral Inheritance*

1900 Mendel's work rediscovered (de Vries, Correns, and Tschermak) (See also Harper (2008, Appendix II.)

Two patterns emerge from this timeline that are of particular interest for present purposes. First, Darwin's hypothesis of pangenesis, Galton's "stirp" theory of inheritance, and Weismann's germ-plasm theory were near contemporaries, as illustrated in Figure 1.

Figure 1. A timeline of theories of inheritance



Second, far from being an unconceived alternative to Darwin's hypothesis of pangenesis, Galton's "stirp" theory of inheritance, or Weismann's germ-plasm theory, the basic principles of genetics (albeit in rudimentary form) were conceived by Mendel in 1865, roughly around the same time (1869-1871) that Galton was experimentally testing Darwin's hypothesis of pangenesis (Bulmer 2003, 116-118). In fact, of these theories of inheritance, it looks like genetics has been around the longest, if we count from "the moment of conception," i.e., from Mendel's work.

Some might object to that by insisting that Mendel's work remained unknown until 1900 when it was rediscovered. But this objection will not do for the following reasons. First, even if Mendel's work remained unknown until 1900, Stanford's New Induction says that there are *unconceived* alternatives. In this case, however, Mendel did conceive of an alternative—even if he did not realize that—to contemporary hypotheses about inheritance. Second, Mendel's work was rediscovered in 1900. From then on, alternative hypotheses about inheritance were left behind and genetics became the dominant theoretical approach to inheritance. This is why each line corresponding to a theory of inheritance (except Darwin's hypothesis of pangenesis) in Figure 1 ends in 1900.

It might seem as if Stanford has a reply to this line of criticism (see also Forber 2008, 139). As Stanford writes:

> what ultimately matters of course is not whether individual scientists are able to exhaustively consider the space of well-confirmed alternative theoretical possibilities, but whether scientific *communities* are able to do so. As a general matter, the failure of a given individual scientist to conceive of or consider particular theoretical alternatives serves us simply as *evidence* that the relevant alternatives were not conceived of or widely considered in the community at large (Stanford 2006, 129; emphasis in original).

Note that this is another inductive argument. That is, from the fact that an individual scientist has failed to conceive of a theoretical alternative it doesn't *necessarily* follow that the scientific community has failed to do so. In other words, the failure of one scientist is not *conclusive* evidence that the scientific community has failed to conceive of alternatives, for there may be other scientists who have conceived of alternatives, even if one scientist has failed to do so. Hence, the fact that an individual scientist has failed to conceive of a theoretical alternative is supposed to be *statistical* evidence that the scientific community has failed to do so. In other words, the inference is supposed to run like this:

(i) Individual *I* is a member of community *C* who has failed to do *X*.

Therefore, probably,

(ii) All members of *C* have failed to do *X*.

It should be clear that (i) does not make (ii) significantly more likely to be true, unless community *C* consists of only one or two members. Scientific communities, however, are usually larger than one or two members. As far as the inheritance episode is concerned, for instance, some key members of the scientific community, in addition to Darwin and Galton, include George Romanes (1848-1894), Hugo de Vries (1848-1935), Wilhelm Johannsen (1857-1927), William Bateson (1861-1926), and Thomas H. Morgan (1866-1945). (See Bowler 1989, 46-64.)

For another example of the same patterns as those gleaned from the inheritance case, take (3) on Stanford's list. Now, consider the following historical facts about the development of theories of electricity and magnetism (adapted from Baigrie 2007, 137-143):

1600 William Gilbert's *De Magnete, Magneticisique Corporibus, et de Magno Magnete Tellure*: the term 'electric' coined by Gilbert, classification of electric and non-electric substances, and description of the Earth as a magnetic entity

1644 René Descartes' mechanical explanation of magnetism involves an interaction between effluvia, threads and ducts

1646 Sir Thomas Browne's *Pseudodoxia Epidemica, or, Enquiries into Very many received Tenets, and commonly presumed truths*: definition of electricity as "a power to attract strawes or light bodies, and convert the needle freely placed"

1745 Jean-Antoine Nollet proposes that electrical matter continuously flows between two charged objects

1751 Benjamin Franklin's letters to a colleague are published as *Experiments and Observations on Electricity*: work on positive and negative charges, the use of pointed conductors, improvements to the Leyden jar, and a plan for his kite experiment

1759 Franz Aepinus' *Tentamen Theoriae Electricitatis et Magnetismi*: treatment of electricity and magnetism in terms of mathematics

1802 Gian Domenico Romagnosi discovers a link between electricity and magnetism when he observes that a voltaic pile deflects a magnetic needle

1820 Hans Christian Ørsted notices that the magnetic needle of a compass aligns itself perpendicularly to a current-carrying wire

1846 Michael Faraday suggests that light could be an electromagnetic phenomenon

1847 Hermann von Helmholtz reads his paper *On the Conservation of Force* to the Physical Society of Berlin, providing an account of the principle of the conservation of energy that governs electrostatic, magnetic, chemical, and other forms of energy

1855 James Clerk Maxwell's *On Faraday's Lines of Force*: relating Faraday's conception of lines of force to the flow of a liquid and using analytical mathematics to derive equations for electric and magnetic phenomena

1864 James Clerk Maxwell's electromagnetism equations appear in *On a Dynamical Theory of the Electromagnetic Field*

1867 Ludwig Lorenz shows that Maxwell's equations can be derived from his scalar and vector potentials

1873 James Clerk Maxwell's *Treatise on Electricity and Magnetism*

1881 Hermann von Helmholtz argues that electricity is divided into elementary particles similar to atoms

1887 Heinrich Hertz builds an apparatus for generating and detecting the electromagnetic waves predicted by the work of James Clerk Maxwell and discovers the photoelectric effect

1892 Hendrik Lorentz expands and modifies James Clerk Maxwell's theory of electromagnetism to develop his own electron theory

1897 J.J. Thomson proposes that cathode rays consist of a stream of negatively charged particles much smaller than an atom

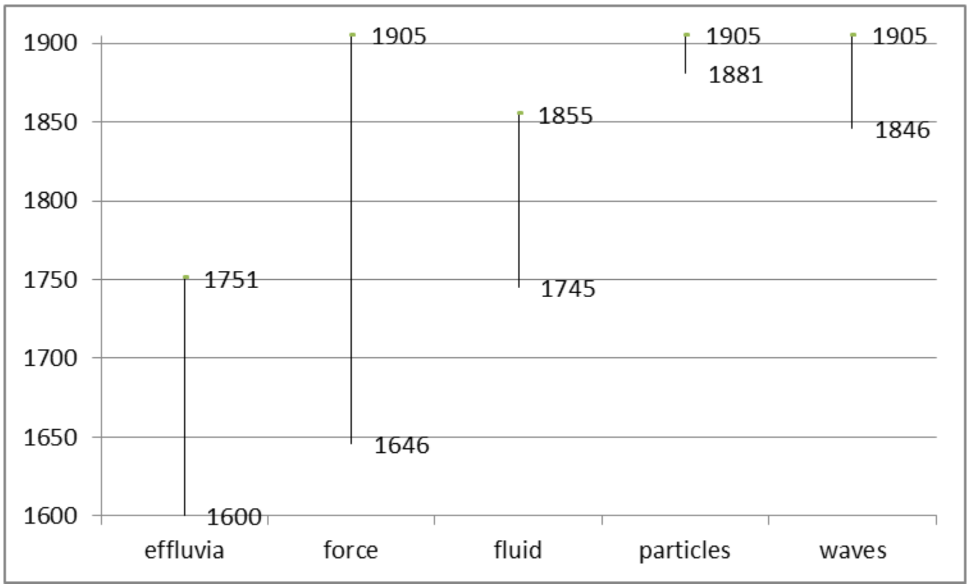
1900 Max Planck introduces his radiation law, the fundamental physical constant that bears his name, and his concept of energy quanta

1904 Hendrik Lorentz develops a set of equations known as the Lorentz transformations in his attempt to explain the results of the Michelson-Morley experiment

1905 Albert Einstein formulates his special theory of relativity, suggesting that electricity and magnetism are two aspects of a single phenomenon

As in the case of theories of inheritance, two patterns emerge from this timeline that are of particular interest for present purposes. First, the various conceptions of electricity as effluvia, single fluid, force, particles, and waves were near contemporaries, as illustrated in Figure 2.

Figure 2. A timeline of theories of electricity and magnetism



Second, far from being an unconceived alternative to these early conceptions of electricity, conceptions of electricity as a power or force go back as early as 1646, and early attempts to treat both electricity and magnetism mathematically go back to 1759. In fact, of all these conceptions of electricity and magnetism, it looks like conceptions of electricity and magnetism as powers or forces have been around the longest, if we count from "the moment of conception," i.e., from Sir Thomas Browne's work. (On "physical lines of force," "magnetic force," and "fields of force" in Faraday, Maxwell, and Hertz, see Cohen 2001, 301-312.)

To this some might object by claiming that the various theoretical conceptions of electricity enjoyed non-overlapping periods of *popularity*. That may be true but the historical episodes presented by Stanford do not support this claim. As we have seen, the fact that one or two scientists failed to conceive of an alternative does not make it significantly more likely that the scientific community as a whole failed to conceive of an alternative. Likewise, the fact that an alternative was neglected by a few does not make it significantly more likely that it was neglected by the scientific community as a whole. As Figure 2 shows, at any point in time there were at least two theoretical conceptions of electricity and magnetism available. To tell which theoretical conception was the *popular* one, we need more than one or two data points. For present purposes, however, there is no need to settle this in order to evaluate Stanford's argument. For, according to Stanford, to count as a conceived alternative, a theory need not be *popular*. It only needs to be a *competing* theory that is "well confirmed by the body of actual evidence we have in hand" (Stanford 2006, 18).

Similar patterns, I submit, can be gleaned from the other historical episodes on Stanford's list. For instance, concerning theories of organic species—(8) on Stanford's list—the "general idea of the possibility of species change is an old concept" (Sloan 2014). It can be traced as far back as Empedocles (ca. 495-435 BCE). In *On Nature*, Empedocles offers a theory of the generation of organisms, according to which they come into being by processes of separation and coalescence that are influenced by Strife and Love. And in Book Five of *On the Nature of Things* (*De rerum natura*), "Lucretius sets out a speculative account of the gradual origin of living beings from an initial atomic chaos through an undirected process that sorts out the best adapted forms and eliminates those not suited to their conditions" (Sloan 2014). (See also Johnson and Wilson 2007.) So, far from being an unconceived alternative to Cuvier's hypothesis that "animals were formed on a series of four distinct and autonomous body plans (*embranchements*) that may display some unity of type within the *embranchements*" (Sloan 2014) and Lamarck's theory of species transformism, the concept of evolution can be traced back to the Greek Atomists. In fact, if we count from "the moment of conception," then of all these zoogonies, it looks like the concept of evolution has been around the longest. Moreover, as in the cases of theories of inheritance and electricity, only one theory has survived as the dominant theory.

It might seem as if earlier zoogonies share some broad or general features with later ones but they are profoundly dissimilar in details. And so, if T_1 and T_2 are theories that are profoundly dissimilar in details, then T_1 and T_2 cannot be competing theories. If T_1 and T_2 are not competing theories, they cannot be alternatives. For, according to Stanford, an alternative is a *competing* theory that is "well confirmed by the body of actual evidence we have in hand" (Stanford 2006, 18). Consequently, the Greek Atomists' conception of evolution does not count as an alternative to other zoogonies, for it was profoundly different in details from these other theories. As Stanford (2006, 53) writes, "it will not be enough to undermine the new induction that earlier theorists managed to conceive of later alternatives in this abstract and general way."

For this defense of Stanford's argument to succeed, however, the new pessimist would have to specify criteria of similarities and dissimilarities between theories. After all, 'abstract and general way' is a vague expression. What are the *concrete* and *specific* ways in which later alternatives must be conceived of by earlier theorists? Of course, earlier theorists should not be able to conceive of later alternatives in *precise* and *exact* ways; otherwise, earlier theorists would be conceiving of *precisely* those later alternatives. Specifying the concrete and specific ways in which later alternatives must be conceived of by earlier theorists is no easy task, since the similarities and dissimilarities have to be *relevant* to whether the theories should properly count as competing or alternative theories. For example, here is a similarity between the General Theory of Relativity and the Special Theory of Relativity: both were proposed by Albert Einstein. But this similarity does not make them competing theories. Likewise, here is a dissimilarity between the "stirp" theory of inheritance and the germ plasm theory: the former was proposed by a knight, whereas the latter was proposed by a German biologist. But this dissimilarity does not mean that they are not competing theories or genuine alternatives. (Stanford regards them as such.)

Now, scientific realists will probably look at the aforementioned patterns and draw a realist conclusion to the effect that the best theory has prevailed. That is, the alternative theories were not as explanatorily and predictively successful as the prevailing theory, and thus they were abandoned. For realists, this is a reason for optimism. Antirealists, on the other hand, will probably look at these patterns and draw a pessimistic conclusion to the effect that scientists are not getting better at theory choice. For antirealists, the fact that alternative theories were abandoned in favor of one dominant theory is not a reason for optimism. After all, there *could* be as of yet unconceived alternatives even to the dominant theory. A feature of the scientific realism debate has been that "Pessimists look at the historical record of science and see failure, whereas realists look at the same historical record and see success" (Mizrahi 2013a, 3214).

The problem with this antirealist response to the aforementioned patterns is that it turns the problem of unconceived alternatives from a *probability* claim, which is supposed to be backed up by the historical record, to a mere *possibility* claim, and thus the New Induction becomes a *conceivability* argument about what is merely possible rather than an *inductive* argument about what is probable. However, the problem of unconceived alternatives is not supposed to be a logical problem of failing to conceive of logically possible alternatives to then-well-confirmed theories. Rather, it is supposed to be a real problem of failing to conceive of empirically viable alternatives to well-confirmed theories, which turned out to be equally confirmed by the evidence (cf. Votsis 2007). And the New Induction is supposed to show that this pattern will probably continue, i.e., that current scientists fail to conceive of alternatives to their current well-confirmed theories. As we have seen, however, in the case of theories of inheritance, electricity, and species origins, alternatives were very much conceived of around the same time, and all were abandoned except for the one that emerged as the dominant theory. If antirealists were to insist in response that there *could* be unconceived alternatives even to the dominant theory, then they would be making a mere possibility claim, not a probability claim that is supposed to be supported by the historical record.

For present purposes, however, there is no need to settle the question about which conclusion should be drawn from the aforementioned patterns (i.e., that several alternative theories were available around the same time until one prevailed as the dominant theory): an optimistic one or a pessimistic one. For the mere fact that one can draw both pessimistic and optimistic conclusions from the historical record shows that Stanford's historical evidence is indeterminate between a pessimistic interpretation and an optimistic interpretation, and thus that the New Induction is a fallacious argument against scientific realism. More explicitly, it shows that, contrary to what Stanford (2006, 47) claims, "this single series of historical episodes may [*not*] go a considerable distance toward showing that we are in possession of a quite general challenge to scientific realism," since the same series of historical episodes can be interpreted as supporting, rather than challenging, scientific realism.

To be clear, I am not suggesting that we should draw a realist or optimistic conclusion from Stanford's historical episodes. I am also not suggesting that there are no serious problems with the NMA. (See, e.g., Frost-Arnold 2010, Newman 2010, and Dicken 2013.) Nor do I have any interest in defending the NMA. In fact, I think that scientific realism cannot be defended on abductive grounds (see my 2012 and my 2013b, where I defend a realist position that does not rely on the NMA or IBE). Even if an argument (like the NMA) is bad, however, there are good and bad ways to object to it. Since scientific realism is supposed to be a scientific explanation that is based on a defeasible inference to the best explanation, trying to undermine such an argument by cherry-picking a few cases from the history of science is ineffective unless we have reasons to believe that the cherry-picked cases are representative (cf. Mizrahi 2013a, 3224). As far as Stanford's list is concerned, however, we have no reasons to think that it is representative of the general population (of scientific theories). Since Stanford's list is a small and biased sample, we have no reason to think that it is a representative sample. Since we have no reason to think that Stanford's list is a representative sample, for all we know, it may consist entirely of outliers. Again, a claim of the form '*F*ness is a reliable predictor of *G*ness' (e.g., smoking increases one's chances of getting sick with lung cancer) cannot be "confronted" or challenged by pointing to a few outliers (e.g., a few people who smoke but do not get sick with lung cancer), the scientific realist's claim that approximate truth is the best explanation for novel predictive success cannot be "confronted" or challenged by pointing to a few outliers (i.e., successful but false theories). Indeed, as we have seen, the very historical record to which Stanford appeals in his New Induction can be seen as supporting, not challenging, scientific realism.

Finally, some may complain that to ask pessimists to meet the standard of random sampling is to ask too much because they cannot meet this standard. In reply, I would like to make the following points. Random sampling is generally accepted as a standard of good inductive argumentation. As Govier (2013, 259) puts it, "the clue to reliable inductive generalizations is finding a sample that is representative of the population." So, if it is indeed the case that pessimists cannot meet this standard of reliable inductive argumentation, then they should not argue by induction. If the only arguments one can make are bad ones, then one should not make any arguments at all. Fortunately for pessimists, I think that this standard of good inductive argumentation can actually be met. Discussing the methodology in detail is beyond the scope of this paper, which is already quite long, so I will simply refer the reader to the work of authors who have followed a much more rigorous methodology of inductive argumentation than cherry-picking. The first example is the work of Fahrbach (2011 and forthcoming) where statistical evidence is presented for what Fahrbach calls "the exponential growth of science" (cf. Wray 2013). The second example is my own work (2013a) in which I present random samples of scientific theories and laws that were not abandoned and are not considered strictly false by current practitioners in the relevant fields.

4. Conclusion

In this paper, I have argued that Old Inductions over the history of science, like the one recently advanced by Vickers (2013), and New Inductions over the history of science, like the one advanced by Stanford (2006), are fallacious arguments. Such arguments from the history of science fail to pose a serious challenge to scientific realism (particularly the epistemic thesis of scientific realism or the *divide et impera* strategy) because they are based on unrepresentative (i.e., small and biased) samples. In addition, I have shown that the historical episodes Stanford cites in support of his New Induction are indeterminate between a pessimistic interpretation and an optimistic interpretation. The general lesson, I submit, is that more systematic studies of the history of science—as opposed to cherry-picking case histories—are needed in order to determine whether or not the historical record warrants a realist stance.

Acknowledgments

I am very grateful to three referees of *International Studies in the Philosophy of Science* for helpful comments on an earlier draft. Special thanks are due to the editor, James McAllister.

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