**How to be a Historically Motivated Anti-Realist:**

**The Problem of Misleading Evidence**

Greg Frost-Arnold

Hobart and William Smith Colleges

300 Pulteney Street

Geneva, NY 14456

US

gfrost-arnold@hws.edu

Abstract: The Pessimistic Induction over the history of science argues that because most past theories judged empirically successful in their time are not even approximately true, most present ones probably are not approximately true either. But *why* did past scientists accept those incorrect theories? Kyle Stanford’s ‘Problem of Unconceived Alternatives’ is one answer to that question: scientists are bad at exhausting the space of plausible hypotheses to explain the available evidence. Here, I offer another answer: the ‘Problem of Misleading Evidence.’ I argue that this proposal enjoys some advantages over Stanford’s, though both are probably needed to explain the historical record.

**Acknowledgements:** Many people have helped me articulate, develop, and defend the ideas presented here. Audiences at the 2018 PSA, the University of Pittsburgh’s Center for Philosophy of Science in February 2017, and the 2015 CLMPS meeting were generous with their constructive criticism. I benefited especially from detailed discussions with Liam Bright, Anjan Chakravartty, Haixin Dang, Peter Lewis, P. D. Magnus, Aaron Novick, Sam Ruhmskorff, and Kyle Stanford.

**1. Introduction**

Many past scientific theories that were widely accepted in their time turned out to be, by current lights, importantly incorrect. Suppose that, after looking over this ‘graveyard’ of theories—the geocentric theory of astronomy, the caloric theory of heat, the miasmatic theory of disease, and so on (Laudan 1981, 33; Stanford 2006, 19-20; Vickers 2013, 191-194; Tulodziecki 2017)—you conclude that the history of science gives us sufficient reason to believe that our current scientific theories are, for the most part, not even approximately true. After all, those mistaken past theories were considered empirically successful in their time, so the fact that we consider our current theories empirically successful is not sufficient reason to consider our current theories by and large approximately true. ‘Empirical success’ here can include more than mere consistency of a theory’s predictions with existing observational results: making novel predictions, avoiding *ad hoc* maneuvers, enjoying consilient lines of evidence, or clearing some other bar (short of entailing approximate truth). I call anyone who adopts a line of reasoning like the above a ‘historically motivated anti-realist.’

Multiple specific arguments could fall under the general characterization above. So the next question to ask is: what is the best particular version of historically motivated anti-realism? This paper compares three species of historically motivated anti-realism. I call the first the ‘Old-Fashioned Pessimistic Induction’ (OFPI). This is the familiar inductive generalization discussed for decades, often associated with Laudan (1981). Second is Kyle Stanford’s New Induction (NI), grounded in what he calls ‘The Problem of Unconceived Alternatives’ (PUA), which he claims has advantages over the OFPI. Finally, I will defend another version of historically motivated anti-realism, whose distinguishing feature is what I call ‘The Problem of Misleading Evidence.’ I argue that this final species of historically motivated anti-realism enjoys the same advantages Stanford claims for his NI over the OFPI, plus certain further advantages that the NI lacks. That said, there is still a place for Stanford’s PUA in the view I ultimately defend—but it will play a more limited role than in Stanford’s picture.

**2. A central objection to the old-fashioned PI, and how Stanford’s PUA answers this objection**

I use the phrase ‘historically motivated anti-realism,’ instead of the more familiar ‘pessimistic induction,’ because I need a label that covers all three of (i) the OFPI, (ii) Stanford’s New Induction, and (iii) my proposal. What I call the *Old-Fashioned Pessimistic Induction* can be formulated as the following enumerative induction:

**(OFPI)**[[1]](#footnote-1):

*Premise*: Most past fundamental scientific theories—even the ones empirically successful in their time—turned out to be not even approximately true.

*Conclusion*: Therefore, most current fundamental scientific theories—even the ones empirically successful in their time—will turn out to be not even approximately true.

Critics of the OFPI have objected to both the premise and the inference. This paper focuses on the inference step, which is the step Stanford intends his PUA to underwrite.

*2.1. The leading objection to the traditional pessimistic inductive inference*

The leading realist objection to the inference in the OFPI comes in several varieties, but the central idea is straightforward. In any past-to-present enumerative induction (such as the OFPI), the more evidence we have to think the past and present are relevantly dissimilar, the weaker the argument is. And we have reason to believe past scientific theories are *not* relevantly similar to present ones. As Magnus and Callender put it, “the realist may insist that the population of past theories was a different kettle of fish than the population of present theories” (2004, 326). Different versions of this objection derive from different reasons for thinking that present theories are importantly dissimilar to past ones. Leading reasons include:

(a) we today have more data or evidence than our predecessors did, so current theories are supported by more data than previous ones were (Roush 2009, 34; Fahrbach (2011, §6; *forthcoming*); Lipton 2000, 204).

(b) We today have more advanced instrumentation and other technologies that were unavailable to our forebears (Lipton 2000, 204; Devitt 2010, 96-98).

(c) We today have eliminated more incorrect hypotheses than previous scientists (Ruhmkorff 2013, 412), and we selected better ones (Lipton 2000, 202). Thus, as the pool of competitors shrinks, the probability that any remaining theory is approximately true increases.

The defender of the OFPI, however, has a reasonable reply. Scientists in 1850, looking back at science in 1720, could have correctly made all of these same points (a)-(c): the 1850 scientists had more data, better instruments, and had eliminated more hypotheses than their predecessor scientists in 1720.

So there are apparently important differences between past and present theories, but there are also important similarities between them—thus it is unclear whether the OFPI is doomed or not. Stanford recognizes this. He claims this situation generates a “stalemate” between the OFPI’s critics and its defenders (2006, 11). There are many past-to-present inferences where the present and the past are somewhat different (even in ways relevant to the truth of the conclusion). Sometimes, these inferences are still fairly strong—but other times they are weak.

How do we ascertain whether a particular enumerative induction is strong, when the past sample is not perfectly representative of the present population? As Stanford says, “enumerative induction… can lead us astray…, particularly when circumstances change in some way that is relevant to the continuity of the regularity or the mechanism that grounds it” (2006, 10). So to know whether a particular enumerative induction is strong, we need to know what is responsible for (i.e. what ‘grounds’) the regularity present in the historical sample. And then we need to ascertain, as best we can, whether that grounding factor is likely present in the current population. The more likely it is that this grounding connection is present in the current population, the stronger the enumerative induction is, ceteris paribus.

Thus, in order to justify the OFPI’s projection from past theories to present ones, and give us reason to think that past fundamental theories are sufficiently relevantly similar to current ones, the historically motivated anti-realist should show that the reason why past fundamental theories turned out fundamentally incorrect is probably still operative in current theories. And in order to do that, the historically motivated anti-realist needs an account of why past fundamental theories turned out incorrect.[[2]](#footnote-2)

*2.2. Stanford’s reply to the objection*

And this is precisely what Stanford’s PUA aims to accomplish. As Magnus puts it, the PUA “explains why science falls short when it falls short” (2010, 804). That is, the PUA provides a reason for, or a ‘mechanism that grounds,’ past scientific failures. On Stanford’s account, the reason past scientific theories are incorrect is typically because they are the results of eliminative inferences made by inquirers who failed to exhaust the space of scientifically plausible hypotheses.

**(PUA)** Scientists are unable (or at least unlikely) to conceive of all respectable theoretical explanations for the available data.

And Stanford’s New Induction (NI) argues that this mechanism is shared with current theories: current theories are also the products of eliminative inferences made by inquirers who are often unlikely to exhaust the space of scientifically respectable hypotheses that could potentially explain the available data.

**(NI)**

*Premise*: Past scientists often did not exhaust the space of scientifically respectable hypotheses that explain the evidence available to them.

*Conclusion*: Current scientists often do not exhaust the space of scientifically respectable hypotheses that explain the currently available evidence.[[3]](#footnote-3)

Stanford argues that this trait is shared across past and present science, since current scientists cannot be much better at exhausting the space of alternatives than scientists of the last few centuries: our psychology cannot have changed radically that quickly. Thus, since the PUA can break the stalemate with realism, but the OFPI alone cannot, historically motivated critics of realism should accept Stanford’s anti-realism instead of the OFPI.

For present purposes, I grant that Stanford’s view is superior to the OFPI in isolation, because the PUA provides the needed supplement to ground the projective inference that the OFPI lacks. But that does not show that Stanford’s position is the *best* version of historically motivated anti-realism. There could be alternative explanations of ‘why science falls short when it falls short,’ which still break the stalemate and ground the induction from past scientific activities to present ones just as well as the PUA does—and perhaps better. The following section describes and defends one such alternative explanation: the Problem of Misleading Evidence.

**3. The problem of misleading evidence: Another way to ground the inference from past theories to present ones**

*3.1. What is the Problem of Misleading Evidence?*

In a 19th Century discussion of Linneaus’s taxonomy, systematist George Johnson anticipates the Problem of Misleading Evidence, though with restricted scope:

Were we to analyze the genera [of Zoophytes and Lithophytes in the 12th edition of Linnaeus’s *Systema Naturae*] we should find, in almost every one of them, species which properly belong to a different class of animals, or whose characters are at variance with those assigned to the genus: but *many of these misplacements were the almost necessary consequences of the then state of knowledge* relative to the beings in question. (1838, 58; my emphasis)

Johnson says Linneaus’s taxonomy is mistaken; yet given the evidence available to the scientific community at the time Linneaus proposed his taxonomy, Linneaus offered the best-supported taxonomy possible. The Problem of Misleading Evidence (PME) generalizes this basic idea beyond systematics to fundamental science more generally:

**(PME)** The total body of evidence used by scientists at a particular time was often *unrepresentative* or otherwise *misleading*.

What does ‘misleading’ mean here? Here are two examples. If someone has been very effectively framed for a crime they did not commit, then the exhibits the prosecution enters into evidence, such as the defendant’s fingerprints at the crime scene, count as misleading evidence. Similarly, a coin-tossing trial of twenty flips, which resulted in twenty heads in a row, would count as misleading evidence, if the coin is actually fair. In these cases, *E* is *misleading evidence* for *H*: evidence *E* (incrementally) confirms hypothesis *H*, but *H* is not (even approximately) true.[[4]](#footnote-4) The PME provides an another explanation of why science falls short, when it falls short, distinct from the PUA: it is not a cognitive limitation on scientists’ part—a failure of imagination—but rather a problem with the scientists’ evidential *environment*, which (if scientists follow the evidence where it leads) systematically leads them away from the truth.

Here are examples of the PME in action. In the *Almagest* (I.5), Ptolemy considers the possibility that the earth is moving from place to place, including the possibility that it is orbiting the sun once a year. Why does Ptolemy reject this hypothesis? The ancient Greeks knew that the Earth is very far from the Sun. If the Earth were revolving around the Sun, then we should observe stellar parallax. But no stellar parallax was detected. So either the Earth is not going around the Sun, or twice the distance from the Earth to the Sun is as nothing with respect to the size of the cosmos. Ancient astronomers reasonably concluded that the former is more likely than the latter. The evidence available to ancient astronomers, namely the absence of stellar parallax, was misleading: it confirmed a geostatic model of the universe over a geokinetic model, even though the geostatic model is incorrect.

Here is a second historical example where the PME explains why past scientists failed to accept the (by current lights) correct theory. One famous episode in the spontaneous generation debates was the controversy between Lazzaro Spallanzani on the one hand, and John Needham and Comte de Buffon on the other, between 1765 and 1780. In experiments first published in 1748, Needham sealed containers of gravy, boiled them, and then examined the resulting liquid under his microscope. He and Buffon saw what they thought were small living creatures (Strick 2000, 6). Since the gravy had been boiled, presumably any microorganisms that had previously been in the gravy were killed, and since the container was sealed, presumably no new microorganisms could enter the gravy. So this appeared to be a case of spontaneous generation of life from non-living materials. Spallanzani attempted to replicate the experiment, but observed no microorganisms in his boiled gravy, so he rejected this purported demonstration of spontaneous generation, claiming that the microorganisms Needham observed must have gotten in despite Needham’s attempt to seal the container.

From the modern point of view, Spallanzani was correct. But what explains this difference in experimental outcomes? Spallanzani claimed (and later scholars repeated) that Needham and Buffon’s microscopes were inferior to his own. This fits nicely with the realist claim mentioned in 2.1 above: past scientists made their mistakes in part because of their inferior instruments; later scientists are more likely to find the (approximate) truth because of their advanced instrumentation. For example, if Ptolemy had 20th Century technology, he could have detected stellar parallax. Interestingly, this is *not* what happened in the Needham-Spallanzani controversy. Phillip Sloan has shown that Needham and Buffon had a *better* microscope than Spallanzani (1992). Specifically, Needham’s microscope could detect bacteria in Brownian motion, whereas Spallanzani’s could not. So Needham and Buffon could see certain objects that showed signs of being alive, which Spallanzani could not. And another contemporaneous scientist, von Gleichen-Russworm, attempted to replicate Spallanzani’s results with a better microscope than Spallanzani’s, but could not, and thus sided with Needham and Buffon (Sloan 1992, 433). So from the current point of view, having more evidence, as a result of having better instruments, actually led Needham and Buffon (and von Gleichen-Russworm) *away* from what later became the successor theory. That is, the data generated by the superior microscopes was misleading.

Parallel to the OFPI and the NI, we can formulate a Misleading Evidence Induction:

**(MEI)**

*Premise*: Past scientists often faced misleading total evidence sets.

*Conclusion*: Current scientists often face misleading total evidence sets.

*3.2. The case for the PME*

The most straightforward consideration in favor of the PME is that, for many historical episodes, it makes the right ‘predictions’: the reason a particular past theory was accepted is that the evidence available at that earlier time was misleading. For example, Ptolemaic astronomers believed the Sun revolved around the Earth because the evidence available to them made it look that way. Much of the evidence that would support the geo-kinetic hypothesis, e.g. actual stellar parallax (not detected until 1838) and the phases of Venus (not detected until 1610), was unavailable to Ptolemy and his successors for centuries. Needham and Buffon believed their observations supported spontaneous generation, because their microscope was powerful enough to detect bacteria in Brownian motion, and particles in Brownian motion appear to be alive. Note that in both cases, the successor hypotheses were conceived at the earlier time—so the PUA cannot explain them.

The PME can explain many other examples. Scientists who accepted the caloric theory of heat in the early 19th Century had substantial evidence against the kinetic theory of heat. This theory was certainly conceived, since it was preferred by the earlier mechanical philosophers, and had been articulated as roughly the current version of the kinetic theory of gases by Daniel Bernoulli in 1738. Stephen Brush outlines six reasons physicists from the middle-to-late 18th Century did not accept it; perhaps the strongest was “the fact that heat … can be transmitted across empty space (as from the sun to the earth) without any accompanying motion of molecules” (1976, 20-21). Another historical episode that appears to fit the PME pattern is Alfred Wegener’s theory of continental drift, proposed in 1912, but whose central claims were not accepted until half a century later. The historical evidential situation is complicated, but amongst historians of geology “there has been a general agreement that it was rational to reject Wegener’s theory of drift when it first appeared in the 1912–1915” (Šešelja and Weber 2012, 148). In other words, the total evidence available to earth scientists in the early 20th Century did not favor Wegener’s hypothesis.

Consider three further examples that historical anti-realists would presumably like to appeal to in making their case against realism, which the PME explains in a natural and straightforward way. Physicists in 1800 accepted the Galilean transformations over the Lorentz transformations, because those physicists had no evidence that the relative velocity of a moving body to its observer would alter the length of that moving body in the observer’s frame. And there was no evidence that the length of moving bodies depends on the speed of light. Similarly, there was no evidence in 1800 that, at sufficiently small scales, the equations of classical mechanics would break down and quantum mechanics would provide more accurate predictions. Finally, before Onnes discovered mercury’s superconducting state in 1911, scientists inferred that a body’s heat capacity is proportional to its temperature, and its electrical resistivity is proportional to temperature cubed. Onnes observed that below 4.2K, both quantities fall quickly to nearly zero. There was no evidence, prior to that discovery, that mercury would behave radically differently below a certain temperature. In each of these three cases, historical scientists lacked a sufficiently representative sample: for example, they only had observations of objects moving much slower than the speed of light, or of systems too large for quantum effects to be detectable, or of temperatures too high for superconductivity to be observed. In short, the historical record contains (rational) scientists often basing their inferences on (what from our current vantage point is) unrepresentative or misleading evidence.

*3.3. Objection: The PUA explains the historical record just as well as the PME*

Now, a defender of the PUA has a plausible objection: the PUA can explain these last three historical episodes too. No one in 1800 had conceived of Special Relativity or Quantum Mechanics. No one in 1900 had conceived that the electrical resistance of certain materials would drop rapidly to nearly zero below a certain critical temperature. So these examples do *not* favor the PME over the PUA.

The PME-proponent has two replies. In my view, these replies show why the PUA is insufficient to account for the patterns of acceptance and rejection we see in historical scientists, and why the PME is necessary. First, some of the earlier above historical examples involve *conceived* alternatives. Specifically, Ptolemy had conceived of the heliocentric theory of the cosmos, caloric theorists had conceived of the kinetic theory of heat, Needham and Buffon had conceived of the hypothesis that spontaneous generation does not happen, and Wegener’s opponents had conceived of the claim that the earth’s current land masses were once contiguous and subsequently moved apart.

The PUA-defender might retort that each of these are not detailed enough to count as genuinely conceiving the successor: for example, the mechanical philosophers’ slogan ‘Heat is the motion of constituent parts’ is not sufficiently specific to count as genuinely conceiving the kinetic theory as it was accepted in the 19th Century (Stanford 2006, 53). However, Daniel Bernoulli had in fact proposed more-or-less the modern kinetic theory of gases in 1738, a full century before it was eventually accepted by the scientific community (Brush 1976, 20). And other examples of successor theories were conceived in more-or-less full detail and nonetheless rejected at an earlier time. For example, shortly after Dalton claimed that water is HO, Avogadro proposed that it was H2O. However, “Avogardo’s ideas were rejected by Dalton and most other chemists as ad hoc, speculative, and implausible, and not generally accepted until half a century later” (Chang 2012, xviii). In cases such as these, where the eventual successor theory was conceived and rejected at an earlier time, the PUA cannot explain why the superseded theory was accepted at that earlier time, but the PME can. So one reason to accept the PME is that it can account for a wider variety of historical cases that historically-motivated anti-realists want to use as part of their brief against realism. An anti-realist who restricts herself to the PUA cannot use these ‘greatest hits’ from the anti-realist catalogue.

The PME-defender has a second reply. I grant that the PUA can give *an* explanation of why scientists in 1800 did not accept Special Relativity, Quantum Mechanics, and the existence of superconducting materials: nobody had proposed the Lorentz transformations, the Schrödinger equation plus the projection postulate, or the existence of critical superconducting temperatures back then. However, I believe the PME provides a *better* explanation for each of these three than the PUA does. Why? Imagine a physicist in 1800 who proposed replacing the Galilean transformations with the Lorentz transformations. Or who says that, below certain scales, classical equations of motion fail to hold, and should be replaced with the Schrödinger equation. In other words, imagine that the Problem of Unconceived Alternatives is removed from these historical cases: imagine the successor theory is conceived.

If the real reason scientists in 1800 accepted classical mechanics and not Quantum Mechanics was that scientists in 1800 had failed to conceive of Quantum Mechanics (as the PUA-proponent predicts), then in this alternative imagined history, the scientists in 1800 should accept Quantum Mechanics. But it is extremely unlikely that the scientific community of 1800 would accept Quantum Mechanics (or special relativity, or superconductivity), because these new theories introduce extra complications that are unsupported by any evidence available in 1800. Such theories would involve unmotivated reductions in theoretical parsimony. And presumably, if the PUA were the (primary) mechanism driving past scientists’ failure to accept later successor theories, then ‘removing’ the PUA would lead scientists to accept the successor theory. But in at least these three counterfactual scenarios, this seems very unlikely. So for several famous historical cases, the PUA does not appear to be the correct explanation for the scientists’ acceptance.

**4. Conclusion: the relationship between the PUA and the PME**

The PME and the PUA both aim to explain why past scientists accepted the theories they did, instead of those theories’ successors. Section 3 argued that the PME enjoys two kinds of explanatory advantages over the PUA: first, the PME can explain historical cases of *conceived* alternatives, whereas the PUA cannot, and second, in certain cases that the PUA can apparently explain, the PME provides a better explanation (e.g. why scientists in 1800 did not accept relativistic and quantum physics). However, none of what was said above entails that the PUA should be eliminated entirely. The fact that PUA is not the best explanation for many portions of the ‘graveyard’ of bygone theories does not mean that it is not the best explanation for *any* portion of the historical record.

Are there cases the PUA explains at least as well as the PME—or better? In such cases, as soon as the new, previously unconceived theory is publicly disseminated, the scientific community would consider it roughly at least as well-confirmed as its long-standing predecessor. For in such a situation, the historical scientists really were in an underdetermination scenario, but did not become aware of that fact until the previously unconceived alternative was explicitly articulated. And cases like this do appear in the history of science. Examples include Einstein’s proposal of the general theory of relativity at the end of 1915, and Friedrich Küchenmeister’s proposal of intermediate hosts to explain the tapeworm lifecycle (Farley 1977, 60-70). In both cases, the new theory handled then-available data well, and a ‘crucial experiment’ (*pace* Duhem) to break the underdetermination situation was proposed and subsequently carried out (the 1919 eclipse expeditions and feeding experiments, respectively). So it appears the PUA can play an explanatory role in these episodes: scientists accepted the Newtonian theory of gravitation instead of general relativity *in 1910* because the general theory of relativity had not yet been conceived. If Einstein had published it a few years before 1915, the physics community would presumably have considered it at least roughly ‘tied’ with Newtonian gravitation theory at that slightly earlier point, too. The fact that the PUA explains some episodes is compatible with the claim that PME frequently drives historical scientists’ acceptances and rejections of theories.

So the PME and the PUA each have a role to play in explaining past scientists’ patterns of acceptance and rejection. In short, if the evidence available at a particular time greatly favors the predecessor over the successor, then the PME better explains why scientists do not accept the successor; if the evidence available at that particular time does *not* favor the predecessor, then the PUA would better explain scientists’ continued adherence to the predecessor.

Furthermore, there are also cases when neither the PME nor the PUA provides the best explanation for why scientists did not accept successor theories at an earlier time. For example, there can be situations where scientists conceived of the successor alternative, and the available evidence favors that successor over the predecessor, yet the scientific community nonetheless continues to accept the predecessor. Cognitive biases, including racist and sexist biases, could generate exactly such a case: suppose two hypotheses *A* and *B* are both conceived, and the evidence, if weighed correctly, favors *A*, and furthermore *A* is approximately true. Yet the scientific community accepts *B*, because their bias causes them to weigh the available evidence incorrectly.

I believe a full understanding of why past scientists accepted what they did, when they did, will require drawing on the PUA, the PME, and cognitive biases—among others. Here, I have only argued that the PME can explain many ‘classic’ episodes that the PUA cannot, and that even when the PUA can offer an explanation, the PME’s is often better.

**References**

Brush, Stephen (1976). *The Kind of Motion We Call Heat*. New York: Elsevier Science Publishers.

Chang, Hasok (2012). *Is Water H2O? Evidence, Realism and Pluralism*. New York: Springer.

Devitt, Michael (2010). *Putting Metaphysics First: Essays on Metaphysics and Epistemology*. Oxford: Oxford University Press.

Fahrbach, Ludwig (2011). “How the Growth of Science Ends Theory Change,” *Synthese* **180**: 139-155.

Fahrbach, Ludwig (forthcoming). “Scientific Revolutions and the Explosion of Scientific Evidence,” *Synthese*. DOI 10.1007/s11229-016-1193-y

Fallis, Don, and Peter J. Lewis (2014). “Misleading Evidence,” available at SSRN: https://ssrn.com/abstract=2462771.

Fallis, Don and Peter J. Lewis (2016). “The Brier Rule is not a Good Measure of Epistemic Utility,” *Australian Journal of Philosophy* **94**: 576-590.

Farley, John (1977). *The Spontaneous Generation Controversy from Descartes to Oparin*. Baltimore: Johns Hopkins University Press.

Johnson, George (1838). *A History of the British Zoophytes*. Edinburgh: W. H. Lizars.

Laudan, Larry (1981). “A Confutation of Convergent Realism,” *Philosophy of Science* **48**: 19-49.

Lipton, Peter (2000). “Tracking Track Records,” *Proceedings of the Aristotelian Society* **74**: 179-205.

Magnus, P. D. (2010). “Inductions, Red Herrings, and the Best Explanation for the Mixed Record of Science,” *British Journal for the Philosophy of Science* **61**: 803-819.

Magnus, P. D. and Craig Callender (2004). “Realist Ennui and the Base Rate Fallacy,” *Philosophy of Science* **71**: 320-338.

Mizrahi, Moti (*forthcoming*). “The History of Science as a Graveyard of Theories: A Philosophers’ Myth?” *International Studies in Philosophy of Science*.

Roush, Sherrilyn (2009). “Optimism about the Pessimistic Induction,” in *New Waves in Philosophy of Science*, P. D. Magnus and Jacob Busch (eds). New York: Palgrave.

Ruhmkorff, Samuel (2013). “Global and Local Pessimistic Meta-Inductions,” *International Studies in the Philosophy of Science* **27**: 409-428.

Šešelja, Dunja and Erik Weber (2012). “Rationality and Irrationality in the History of Continental Drift: Was the Hypothesis of Continental Drift Worthy of Pursuit?” *Studies in History and Philosophy of Science* **43**: 147-159.

Sloan, Phillip R. (1992). “Organic Molecules Revisited,” in *Buffon ’88*. Paris: J. Vrin, 415-438.

Stanford, P. Kyle (2006). *Exceeding Our Grasp*. New York: Oxford.

Strick, James (2000). *Sparks of Life*. Cambridge: Harvard University Press.

Tulodziecki, Dana (2017). “Against Selective Realism(s),” *Philosophy of Science* **84**: 996-1007.

Vickers, Peter (2013). “A Confrontation of Convergent Realism,” *Philosophy of Science* **80**: 189-211.

Worrall, John (2000). “Relying on Meta-Induction?” *Proceedings of the Aristotelian Society* **74**: 207-235.

1. The OFPI can also be formulated as a reductio; nothing here depends on this choice. [↑](#footnote-ref-1)
2. Mizrahi (2013, 3219) and Worrall (2000, 233-4) both criticize the OFPI for, in effect, failing to provide evidence that the reason why past empirically successful theories failed will also hold for present theories. [↑](#footnote-ref-2)
3. The NI generates skepticism about the approximate truth of current theories as follows, according to Stanford. Many scientific theories are arrived at via an elimination-of-alternatives inference (disjunctive syllogism). The conclusion of the NI gives us reason to doubt the premise stating that one of the alternative hypotheses is true, because it is not exhaustive. [↑](#footnote-ref-3)
4. Don Fallis and Peter Lewis (2014) show that this simple and intuitive gloss of misleadingness both enjoys wide acceptance in the philosophical literature, but faces problems in certain cases (cf. their 2016). I do not have a definition of ‘misleading evidence.’ But all that is necessary for present purposes is that the historical examples described below are actually cases of misleading evidence. [↑](#footnote-ref-4)