Chapter 5
Key Arguments Against Scientific Realism

Abstract In this chapter, I present in canonical (or standard) form and then evaluate key arguments against scientific realism (or for antirealism about science). The first argument is known as the “pessimistic induction” or the “pessimistic meta-induction.” In its original formulation, attributed to Larry Laudan (Philos Sci 48(1):19–49, 1981), the argument is based on a list of theories that are supposed to be counterexamples to the realist thesis that empirical success is a mark of (approximate) truth. Other formulations of the argument have it as an inductive argument from a sample of theories that were discarded in the past to the conclusion that our present scientific theories will probably be discarded as well. The second argument is a positive argument for Bas van Fraassen’s (The scientific image. Oxford University Press, New York, 1980) antirealist position, namely, Constructive Empiricism. The third argument proceeds from the observation that scientists cannot claim to be in a privileged epistemic position. According to this argument, it is unlikely that our best scientific theories are (approximately) true because scientists are not especially skilled at developing theories that are likely true or approximately true. A contemporary proponent of this argument for antirealism is Brad Wray (Int Stud Philos Sci 22(3):317–326, 2008). The fourth is an argument for antirealism from what Kyle Stanford (Exceeding our grasp: science, history, and the problem of unconceived alternatives. Oxford University Press, New York, 2006) calls the “Problem of Unconceived Alternatives” (PUA). According to Stanford, the historical record of science reveals that past scientists typically failed to conceive of alternatives to their favorite, then-successful theories. This is supposed to make it more likely that present scientists fail to conceive of alternatives to their favorite, now-successful theories. For these reasons, Stanford argues, we should not believe our present scientific theories are (approximately) true. The fifth argument purports to show that many of the scientific theories we currently accept will be discarded sometime in the future, as the “pessimistic induction” does, but it proceeds from the premise that the research goals and interests of scientists change over time (Wray, Brad K., Resisting scientific realism. Cambridge University Press, Cambridge, 2018).

Keywords Approximate truth · Cherry-picking · Constructive empiricism · Epistemic privilege · Explanationist realism · Hasty generalization · Historical graveyard of science · Inference to the Best Explanation (IBE) · Predictive success · Problem of Unconceived Alternatives (PUA) · Random sampling · Representative sample · Research goals · Research interests
In this chapter, I present in canonical (or standard) form and then evaluate key arguments against scientific realism (or for antirealism about science). The first argument is known as the “pessimistic induction” or the “pessimistic meta-induction.” Even though “induction” is part of the name, there is no agreement among philosophers of science on whether to construe this argument as an inductive or a deductive argument. In its original formulation, attributed to Larry Laudan (1981), the argument is based on a list of theories that are supposed to be counterexamples to the realist thesis that empirical success is a mark of (approximate) truth. Laudan’s (1981, p. 33) list includes scientific theories from past science, such as the humoral theory of medicine, the phlogiston theory of chemistry, the vital force theory of physiology, and theories of spontaneous generation, among others. The list includes twelve scientific theories in total, although Laudan (1981, p. 33) claims that the list “could be extended ad nauseam.” Other formulations of the argument have it as an inductive argument from a sample of theories that were discarded in the past to the conclusion that our present scientific theories will probably be discarded as well. The second argument is a positive argument for Bas van Fraassen’s (1980) antirealist position, namely, Constructive Empiricism. The third argument proceeds from the observation that scientists cannot claim to be in a privileged epistemic position (Lipton 1993). According to this argument, it is unlikely that our best scientific theories are (approximately) true because it is not the case that scientists are especially skilled at developing theories that are likely true or approximately true. A contemporary proponent of this argument for antirealism is Brad Wray (2008) and (2018). The fourth is an argument for antirealism from what Kyle Stanford (2006) calls the “Problem of Unconceived Alternatives” (PUA). According to Stanford, the historical record of science reveals that past scientists typically failed to conceive of alternatives to their favorite, then-successful theories. This is supposed to make it more likely that present scientists fail to conceive of alternatives to their favorite, now-successful theories. For these reasons, Stanford argues, we should not believe our present scientific theories are (approximately) true. The fifth argument purports to show that many of the scientific theories we currently accept will be discarded sometime in the future, like the “pessimistic induction” does, but it proceeds from the premise that the research goals and interests of scientists change over time (Wray 2018).

5.1 The “Graveyard” Argument

If the Positive Argument for scientific realism, the so-called “no miracles” argument (see Chap. 4, Sect. 4.1), is the realist argument that has dominated the scientific realism/antirealism debate in contemporary philosophy of science, the so-called “pessimistic induction” is its antirealist counterpart. There is some debate among
philosophers of science concerning the structure of this antirealist argument. Some think that it is an inductive argument from a sample, others interpret it as a deductive argument, and still others understand it as an argument from counterexamples. What all these different interpretations of the argument share in common is an assumption that the history of science is a “graveyard of dead epistemic objects” (Chang 2011, p. 426), that is, discarded theories and abandoned theoretical posits. From this historical understanding of science as a “graveyard of dead epistemic objects” (Chang 2011, p. 426), antirealists then draw the conclusion that current (and future) scientific theories and theoretical posits will end up in that “graveyard” as well. That is why I refer to this argument as the “Graveyard” Argument. Still, antirealists get to the conclusion that our best current theories will end up in the “graveyard” of science in various ways. Some do it by inductive reasoning. An inductive version of the “Graveyard” Argument can be found in Brad Wray’s (2018, pp. 68–69) book-length defense of antirealism.

Wray, Brad K. 2018. Resisting scientific realism. Cambridge: Cambridge University Press.

A Pessimistic Induction is an inductive argument that draws a conclusion from the rejection of many successful scientific theories in the past. Sometimes a conclusion is drawn about the prospects of the theories that are currently accepted, and sometimes an inference is drawn about the prospects of future theories, those not yet developed or entertained by scientists.

As I understand it, the argument that Wray is making in this passage can be stated in canonical (or standard) form as follows:

(P) Many successful scientific theories were rejected in the past.

Therefore,

(C) Current (and future) successful scientific theories will be rejected.  

---

1 Arguably, this is partly because the original presentation of the argument that came to be known as the “pessimistic induction,” namely, Laudan (1981), lends itself to different interpretations. For a detailed discussion, see Mizrahi (2013a).

2 See, for example, James Ladyman’s (2011) for a discussion of the “pessimistic induction” as an argument from counterexamples against scientific realism. Ladyman (2011) argues that Structural Realism (see Chap. 3, Sect. 3.5) can account for one of those counterexamples, namely, the phlogiston theory.

3 Peter Lipton (2005, p. 1265) states the argument as follows: “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.” In Mizrahi (2016a), I provide empirical evidence against the “graveyard” picture of the history of science. Cf. Tulodziecki (2017).

4 Anjan Chakravartty (2008, p. 152) states the argument as follows: the pessimistic induction is “a two step worry. First, there is an assertion to the effect that the history of science contains an impressive graveyard of theories that were previously believed, but subsequently judged to be false.
Since this argument is not meant to be a deductive argument, the next question is not whether this argument is valid or invalid, but rather whether it is strong or weak. Does the premise (P) successfully provide probable support for the conclusion (C)? Well, it depends on exactly how many successful scientific theories were rejected and from which periods of past science. Suppose that the population of past successful scientific theories consists of one hundred theories. How many of them would have to be rejected to justify the inference to the conclusion that current (and future) successful scientific theories will be rejected as well? At least fifty? More than fifty? Recall that Laudan’s list of “previously successful but now dead” scientific theories contains twelve scientific theories. Throughout the history of science, however, hundreds of theories were proposed by scientists. Again, suppose that the population of past successful scientific theories consists of one hundred theories. This means that 12% of those theories, that is, twelve out of one hundred, are “previously successful but now dead” scientific theories. Is 12% of a sample enough to draw general conclusions about the general population, or make predictions about any given member of the general population, beyond the sample?

As we have seen in Chap. 1 (see Sect. 1.1), in inductive prediction, given that \(X\) percent of sampled things, \(Fs\), have a particular property, \(G\), we are entitled to conclude that, with a probability of \(X\) percent, a new \(F\) that has not been observed or surveyed yet will also have the property, \(G\), provided that \(X\) is greater than 50%, and there is no evidence that the new \(F\) is unlike previously observed \(Fs\) (Schurz 2019, p. 2). Accordingly, given that only 12% of past scientific theories “previously successful but now dead” scientific theories, as we are supposing for the sake of argument, we are justified in concluding, with a probability of 12%, that any given current scientific theory that is successful will end up dead, that is, will be abandoned by scientists in the future. But this means that any given current scientific theory that is successful has an 82% probability of being retained rather than abandoned. Therefore, for any given current scientific theory that is successful, it is much more likely that this theory will be retained rather than abandoned, given the evidence we have. For these reasons, some philosophers of science have argued that the sample of successful scientific theories that were rejected in the past is not large enough to warrant the inductive inference to (C). From a handful of examples of rejected theories, such as the phlogiston theory and the caloric theory of heat (Laudan 1981, p. 33), no strong inductive inferences can be drawn to the conclusion that current (and future) successful scientific theories will be rejected.\(^5\)

In addition to being too small to warrant an inductive inference to (C), or any inductive predictions about future scientific theories, the sample of past scientific theories on which the pessimistic induction is supposed to be based might also be unrepresentative of science as a whole. For, as we have seen, the examples typically cited in support of (P) tend to be from ancient science (for example, the humoral

\[^{5}\text{For more on this criticism of the pessimistic induction, see Park (2011), Fahrbach (2011), Mizrahi (2013a), and Mizrahi (2016a).}\)
5.1 The “Graveyard” Argument

theory of illness), medieval science (for example, the crystalline spheres), and early modern science (for example, the phlogiston theory) (Laudan 1981, p. 33). In that respect, these examples of rejected theories come from the distant past of science. But most current scientific theories are not from the distant past of science (see Chap. 4, Sect. 4.5 on the “exponential growth of science”). This means that the examples of discarded theories that are typically cited in support of (P) are not representative of science as a whole.

Finally, the examples of discarded theories of past science that are typically cited in support of (P) are not only too few and unrepresentative to warrant an induction about science as a whole but also cherry-picked instead of randomly selected. A strong inductive argument from a sample must be based on a randomly selected sample. If the sample is cherry-picked, rather than randomly selected, then it cannot be representative of the target population as a whole (see Chap. 2, Sect. 2.2). For example, if I survey 2000 Republican voters and find that 60% of them are satisfied with the job that President Donald Trump is doing, I am not entitled to conclude from this that 60% of American voters are satisfied with the job that President Donald Trump is doing. For my sample of voters consists of Republican voters only, whereas the general population of voters contains Democrats and Independent voters as well. Similarly, if I survey scientific theories from the distant past only (that is, from the ancient, medieval, and early modern periods only) and find that 60% of them are no longer accepted by scientists, I am not entitled to conclude from this that 60% of current (and future) scientific theories will be rejected as well. For my sample of scientific theories consists of pre-eighteenth century theories only, whereas the general population of scientific theories contains theories from the eighteenth, nineteenth, and twentieth centuries as well. In fact, if “at least 95% of all scientific work ever done has been done since 1915, and at least 80% of all scientific work ever done has been done since 1950,” as Ludwig Fahrbach (2011, p. 148) argues, then pre-twentieth century scientific theories cannot be representative of science as a whole (see Chap. 4, Sect. 4.5).

Accordingly, if the sample of past scientific theories used to support (P) is too small, non-randomly selected (that is, cherry-picked), and unrepresentative of scientific theories in general, then it cannot provide strong inductive support for the conclusion that current (and future) successful scientific theories will be rejected. Indeed, as we have seen in Chap. 2 (see Sect. 2.2), drawing general conclusions or making predictions from small, non-random, and unrepresentative samples is a mistake in reasoning called “hasty generalization.” According to Patrick Hurley (2006, p. 131), “Hasty generalization is a fallacy that affects inductive reasoning. […] The fallacy occurs when there is a reasonable likelihood that the sample is not representative of the group. Such a likelihood may arise if the sample is either too small or not randomly selected.” Since the sample on which the inductive version of the “Graveyard” Argument is based is too small and non-random, it is unlikely to be a
representative sample. For these reasons, the “Graveyard” Argument (or the so-called “pessimistic induction”) cannot be said to be a strong inductive argument.\(^6\)

Other antirealists get to the conclusion that, just like past scientific theories, current (and future) scientific theories will end up in the so-called “historical graveyard of science” (Frost-Arnold 2011, p. 1138) as well, not by inductive reasoning, but by deductive reasoning. A deductive version of the “Graveyard” Argument can be found in Timothy Lyons’ (2017, p. 3209) paper, where he argues that “the historical argument [that is, the “Graveyard” Argument] involves neither a meta-induction nor a conclusion that our scientific theories are (likely) false.” Lyons (2017, p. 3209) states his argument in canonical (or standard) form as follows:

1. If (a) the realist meta-hypothesis were true, then (b) none of the constituents genuinely deployed toward successes would be such that they cannot be approximately true.
2. However, (not-b) we do find constituents genuinely deployed toward success that cannot be approximately true.
3. Therefore, (not-a) the realist meta-hypothesis is false.

For Lyons, the premises of this argument purport to provide logically conclusive, as opposed to probable, support for the conclusion. In other words, he intends this argument to be a deductive argument (in particular, a *modus tollens*: If \(A\), then \(B\); therefore, not \(A\)). Since the premises of Lyons’ argument, namely, (1) and (2), successfully provide logically conclusive support for the conclusion, namely, (3), this argument can be said to be valid. The next question, then, is whether the premises are in fact true. Is Lyons’ argument, which purports to be a deductive version of the “Graveyard” Argument, sound?

For Lyons (2017, p. 3204), “the realist meta-hypothesis” is the claim that “successful scientific theories are (approximately) true.” If this realist meta-hypothesis were true, Lyons argues, then there would be no successful theories that are not approximately true. For, according to “the realist meta-hypothesis,” *all* “successful scientific theories are (approximately) true,” with no exception. If there are any exceptions, that is, any successful theories that are not approximately true, then “the realist meta-hypothesis” must be false, or so Lyons argues. Like other antirealists who believe that the history of science is a “graveyard of dead epistemic objects” (Chang 2011, p. 426), Lyons also believes that there are successful theories that are not approximately true. He thus concludes by deductive reasoning that “the realist meta-hypothesis,” that is, the claim that all “successful scientific theories are (approximately) true,” is false.

---

\(^6\)According to Sherri Roush, the pessimist has to show not only that past scientific theories have been failures but also that past scientific methods are failed methods of scientific inquiry. “But even if we grant their unreliability [that is, the unreliability of past methods of scientific inquiry],” Roush (2010, p. 55) argues, “nothing follows from this about whether we have a right to our confidence in our particular theories unless’’ the pessimist can also show “that their unreliability is a reason to think we are unreliable,” which has not been shown, “since the manifest difference in methods between us and our predecessors breaks the pessimist’s induction.”
Scientific realists, however, would not accept premise (1) of Lyons’ argument because they typically do not endorse what Lyons (2017, p. 3204) calls “the realist meta-hypothesis,” that is, the claim that all “successful scientific theories are (approximately) true.” Rather, for contemporary scientific realists, after the so-called “selectivist turn” (see Chap. 2), predictive success is a reliable indicator of approximate truth, not a sure sign that a scientific theory must be approximately true. Saying that predictive success is a reliable indicator of approximate truth is compatible with there being some exceptions, that is, a few scientific theories that are predictively successful but not approximately true. To put it another way, 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. Indeed, even before the so-called “selectivist turn,” scientific realists were careful in the way they formulated what Lyons calls “the realist meta-hypothesis.” For example, as he was advancing the Positive Argument for scientific realism (see Chap. 4, Sect. 4.1), Putnam (1975, p. 73) was careful to add 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). In other words, “successful scientific theories are (approximately) true” is not meant to be a necessary truth. If a theory is predictively successful, then that is a reason to believe that it is approximately true, but it is not a conclusive reason to believe that the theory is approximately true (Mizrahi 2013a, p. 3224).7

Conversely, then, a successful theory that is not approximately true does not constitute a conclusive reason to believe that predictive success and approximate truth are not reliably connected with each other such that predictive success is a reliable indicator of approximate truth. This is much like how an academically successful student who has a poor attendance record is not a conclusive proof that class attendance and academic success are not reliably connected with each other such that class attendance is a reliable indicator (or predictor) of academic success. This is because, on average, students with poor attendance records struggle to succeed academically. The fact that there are a few students who manage to succeed academically despite having a poor attendance record does not change the fact that class attendance is a reliable indicator (or predictor) of academic success. Accordingly, in much the same way that class attendance is a reliable indicator (or predictor) of academic success despite a few outliers (that is, some students who are academically successful despite having poor attendance records), realists would argue, predictive success is a reliable indicator (or predictor) of approximate truth despite a few outliers (that is, some scientific theories that are predictively successful but not even approximately true). For this reason, premise (1) of Lyons’ argument may be false, or at least unacceptable to scientific realists, which means that this deductive formulation of the “Graveyard” Argument, albeit valid, cannot be said to be sound.

7See also Park (2019, p. 607).
Finally, some antirealists prefer to understand the “Graveyard” Argument as an argument from counterexamples. After all, if the history of science really is a “graveyard of dead epistemic objects” (Chang 2011, p. 426), as antirealists claim it is, then it should be fairly easy to find examples of scientific theories that were predictively successful but were later abandoned. Those examples would then serve as counterexamples against the realist claim that predictive success is a reliable indicator of approximate truth. As Peter Vickers (2019, p. 572) puts it:

Putnam’s statement [namely, that scientific realism “is the only philosophy that doesn’t make the success of science a miracle” (Putnam 1975, p. 73; see Chapter 4, Section 4.1)] was immediately objectionable due to fairly straightforward historical ‘counterexamples’. Most famously, of course, Laudan explicitly targeted Putnam in his historical ‘confutation’. For a counterexample to Putnam’s success-to-truth inference all we need are examples where we have (significant) success, and yet what we infer certainly isn’t true. As Laudan argues (1981, pp. 24–26), Putnam is happy to accept that, for example, ‘aether’ is a non-referring term, and thus theories employing this concept are not even approximately true. Laudan then argues that nineteenth century aether theories were successful theories (pp. 26–27). And he goes on to give his famous list of other possible/probable counterexamples (p. 33) (emphasis added).

Understood as an argument from counterexamples, then, the “Graveyard” Argument takes the same form as Lyon’s deductive version of the “Graveyard” Argument, that is, modus tollens: If A, then B; not B; therefore, not A. This argument can be stated in canonical (or standard) form as follows:

1. If all successful scientific theories are approximately true, then any successful scientific theory, \( T \), must also be approximately true.
2. But a successful scientific theory, \( T \), is not approximately true.
3. Therefore, it is not the case that all successful scientific theories are approximately true.

Since the premises of this argument, namely, (1) and (2), successfully provide logically conclusive support for the conclusion, namely, (3), this argument can be said to be valid. The next question, then, is whether the premises are in fact true. Is this argument from counterexamples, which purports to be a deductive version of the “Graveyard” Argument, sound?

As we have seen, Larry Laudan’s (1981, p. 33) list of past scientific theories that were predictively successful but were later discarded includes the ancient humoral theory of illness, the crystalline spheres of medieval astronomy, and the phlogiston theory of early modern chemistry. Accordingly, one could take any of the “previously successful but now dead” scientific theories on Laudan’s (1981, p. 33) list and plug it in as a value for the \( T \) variable in the argument from counterexamples. This would make premise (2) of the argument from counterexamples true. But what about premise (1)? Are these and other “previously successful but now dead” theories of past science effective counterexamples against the realist thesis that predictive success is a reliable indicator of approximate truth?

Again, scientific realists would take issue with premise (1) of the argument from counterexamples, specifically with the claim that all successful scientific theories
are approximately true. Counterexamples are effective only if their intended targets are construed as universal statements or generalizations. For example, a bird that cannot fly would count as an effective counterexample against the universal generalization “All birds can fly.” On the other hand, a bird that cannot fly would not count as an effective counterexample against the claim that a keel is a reliable indicator of flying ability in birds. For the claim that birds with a keel on their breastbone are more likely to be flying birds than flightless birds is consistent with there being some flightless birds, such as ostriches and kiwis, with a keel on their breastbone (though greatly reduced). Likewise, a predictively successful but dead theory would count as an effective counterexample against the universal generalization “All successful theories are approximately true.” As we have already seen, however, that is not the scientific realist’s claim. The scientific realist’s claim is that predictive success is a reliable indicator of approximate truth. Therefore, a predictively successful but dead theory does not count as an effective counterexample against the claim that predictive success is a reliable indicator of approximate truth. For the claim that predictive success is a reliable indicator of approximate truth is consistent with there being some predictively successful but dead theories (Mizrahi 2013a, p. 3224).8 In that respect, to argue that the phlogiston theory (or any other predictively successful but dead scientific theory, for that matter) is a counterexample to scientific realism is like arguing that single-stranded DNA is a counterexample to the double-helical model of DNA.9 Both arguments fundamentally misunderstand the targets they aim to take down. The double-helical model of DNA is not supposed to be a universal statement about DNA and the realist thesis that predictive success is a reliable indicator of approximate truth is not supposed to be a universal generalization about scientific theories.10 For these reasons, if the “Graveyard” Argument is meant to be an argument from counterexamples against scientific realism, then it cannot be said to be a sound argument.

5.2 The Positive Argument for Constructive Empiricism

As we have seen in Chap. 3, Constructive Empiricism (see Sect. 3.3) is supposed to be an antirealist alternative to realist positions about science. Unlike scientific realists, who tend to think that we have good reasons to believe that our best (that is, explanatorily and predictively successful) scientific theories are approximately true, constructive empiricists think that we should accept our best scientific theories, that

8 Stathis Psillos (2018) puts this point as follows: “The relation between success and (approximate) truth, in this sense, is more like the relation between flying and being a bird: flying characterizes birds even if kiwis do not fly. If this is so, then there is need for more than one counter-example for the realist thesis to be undermined.”

9 The DNA example is borrowed from Kitcher (1993, p 118).

10 After all, scientific realists are always careful to talk about “our best scientific theories” rather than scientific theories in general. I will say more about this in Chap. 6, Sect. 6.4.
is, we should believe what those theories say about observables, but we should not *believe* what those theories say about unobservable entities, processes, and events. For constructive empiricists, to accept a scientific theory is to believe that it is empirically adequate, that is, to believe that what the theory says about observables is true, but to suspend judgment with respect to what the theory says about unobservables.

Like scientific realism, Constructive Empiricism has a Positive Argument of its own. (See Chap. 4, Sect. 4.1 for the Positive Argument for scientific realism.) This Positive Argument can be found in Bas van Fraassen’s seminal (1980, p. 73) book. van Fraassen, Bas C. 1980. *The scientific image*. New York: Oxford University Press.

*There is also a positive argument for constructive empiricism—it makes better sense of science, and of scientific activity, than realism does and does so without inflationary metaphysics.*

As I understand it, the argument that van Fraassen is making in this passage can be stated in canonical (or standard) form as follows:

(P1) Scientific activity involves positing unobservable entities, processes, and events in order to explain and predict observable phenomena.

(P2) The best explanation for scientific activity is Constructive Empiricism.

(P3) No hypothesis (for example, Explanationist Realism) explains scientific activity as well as Constructive Empiricism does.

Therefore,

(C) Constructive Empiricism is true.

Like the Positive Argument for scientific realism (see Chap. 4, Sect. 4.1), the Positive Argument for Constructive Empiricism is an instance of what philosophers of science call “Inference to the Best Explanation” (IBE). ¹¹ As James Ladyman (2002, p. 209) points out, IBE “is sometimes also known as ‘abduction’—following the terminology of Charles Peirce.” However, some philosophers have argued that IBE and Peirce’s abduction are different forms of inference (Douven 2017).¹² Be that as it may, for Peirce, abduction is a non-deductive form of inference. Likewise, IBE is typically construed as an ampliative, or non-deductive, form of argumentation that proceeds from a phenomenon that requires an explanation to the conclusion that the best explanation for that phenomenon is probably true.¹³ As Ladyman (2007, p. 341) describes it, “Inference to the best explanation (IBE) is a (putative) rule of inference according to which, where we have a range of competing

---

¹¹The phrase was coined by Gilbert Harman (1965).

¹²Daniel Campos (2011, p. 419) argues against the “tendency in the philosophy of science literature to link abduction to the inference to the best explanation (IBE), and in particular, to claim that Peircean abduction is a conceptual predecessor to IBE.”

¹³For example, Alan Baker (2010, pp. 37–38) defines IBE as “A method of reasoning, also known as abduction, in which the truth of an hypothesis is inferred on the grounds that it provides the best explanation of the relevant evidence. In general, inference to the best explanation (IBE) is an ampliative (i.e., non-deductive) method” (emphasis in original).
hypotheses all of which are empirically adequate to the phenomena in some domain, we should infer the truth of the hypothesis which gives us the best explanation of those phenomena.” The general form of IBE can be stated as follows:

1. Phenomenon $P$.
2. The best explanation for $P$ is $E$.
3. No other explanation explains $P$ as well as $E$ does.
4. Therefore, (probably) $E$.\(^\text{14}\)

In the case of the Positive Argument for Constructive Empiricism, the phenomenon that demands an explanation is the fact that practicing scientists posit the existence of unobservable entities, processes, and events (for example, genes, genetic mutation, and speciation) in order to explain and predict observable phenomena (for example, biodiversity, fossils, and extinction). This scientific activity is supposed to be explained by the hypothesis that practicing “Sci[entists] aim […] to give us theories that are empirically adequate” (van Fraassen 1980, p. 12). If Constructive Empiricism does indeed provide the best explanation for the fact that practicing scientists posit the existence of unobservables in order to explain and predict observable phenomena, then that would be a good reason to believe that Constructive Empiricism is probably correct. In other words, the premises of the Positive Argument for Constructive Empiricism, if true, would provide strong inductive support for its conclusion. Given that the Positive Argument for Constructive Empiricism is not meant to be a deductive argument, the next question is not whether it is valid or invalid, but rather whether it is strong or weak.

Scientific realists might protest against the Positive Argument for Constructive Empiricism by claiming that IBE is not a form of argumentation that is available to constructive empiricists. This is because constructive empiricists are generally critical of the use of IBE in science. As we have seen in Chap. 4 (Sect. 4.1), Bas van Fraassen (1980) takes issue with IBE. According to van Fraassen (1980, p. 143):

\begin{quote}
[IBE] is a rule that selects the best among the historically given hypotheses. We can watch no contest of the theories we have so painfully struggled to formulate, with those no one has proposed. So our selection may well be the best of a bad lot (emphasis added).
\end{quote}

According to constructive empiricists, then, from the fact that an explanation is the best one we could come up with, it does not follow that the explanation is probably true. For we might be working with a bad lot of explanations and the likely true explanation simply did not occur to us. If van Fraassen is right about this, then any inference to the best explanation would be a weak argument. For, even if no other explanation explains $P$ as well as $E$ does, $E$ would still not be more probable or likely to be true if $E$ is simply the best of a “bad lot” of explanations.\(^\text{15}\) If constructive empiricists are right about this, then one could apply the same reasoning to the Positive Argument for Constructive Empiricism. That is to say, when constructive

\(^{14}\)For more on the structure of IBE, see Psillos (2007).

\(^{15}\)We will revisit van Fraassen’s criticism against IBE as inference from a “bad lot” of explanations in Chap. 6, Sect. 6.4.
empiricists infer that Constructive Empiricism is better than realist positions about science on explanatory grounds, since Constructive Empiricism supposedly "makes better sense of science, and of scientific activity, than realism does and does so without inflationary metaphysics" (van Fraassen 1980, p. 73), they may simply be selecting the best hypothesis among several bad ones. If Constructive Empiricism is one hypothesis among several bad ones, then we have no good reasons to believe it, by the constructive empiricist's own lights. For this reason, even constructive empiricists cannot say that their own Positive Argument for Constructive Empiricism is a strong inductive argument, given their own objection to the use of IBE in science.16

For the sake of argument, however, let us grant that IBE is a legitimate form of non-deductive (or inductive) argumentation. The next question, then, is whether the premises of the Positive Argument for Constructive Empiricism are in fact true? Is the Positive Argument for Constructive Empiricism a cogent argument? Premise (P1) is not in question, of course, for it states the phenomenon to be explained. So, to determine the cogency of the Positive Argument for Constructive Empiricism we need to ask the following question: Does Constructive Empiricism provide the best explanation for the phenomenon of scientific activity (that is, scientists positing unobservable entities, processes, and events in order to explain and predict observable phenomena)? Or are there alternative explanations that explain this phenomenon just as well as Constructive Empiricism does?

Scientific realists would object to (P3) of the Positive Argument for Constructive Empiricism. They would argue that realist positions, such as Explanationist Realism, can explain scientific activity just as well as (if not better than) Constructive Empiricism can. On Explanationist Realism (see Chap. 3, Sect. 3.1), for example, practicing scientists posit unobservables to explain and predict observable phenomena not merely because they want to "save the phenomena," or get the observable facts right, but also because they want approximately true theories, or get the unobservable facts right, too. When scientific theories make predictions that are borne out by repeated experimentation and observation, then we can be quite confident that those theories are approximately true. For, if our best scientific theories were not even approximately true, they would not have been successful in the first place (see Chap. 4, Sect. 4.1). If this is correct, then (P3) of the Positive Argument for Constructive Empiricism cannot be said to be true as scientific realists would argue that there are alternative explanations for the phenomenon of scientific activity that can explain it just as well as (if not better than) Constructive Empiricism can. And if there are alternative explanations for the phenomenon of scientific activity that can explain it just as well as Constructive Empiricism can, then the Positive Argument for Constructive Empiricism, even if strong, cannot be said to be a cogent argument.

16 For more on this problem with the Positive Argument for Constructive Empiricism, see Mizrahi (2018).
5.3 The Underconsideration Argument

As we have seen in Sect. 5.1, some antirealists are pessimistic about the prospects of our current scientific theories. That is to say, they think that our current scientific theories will likely end up in the so-called “historical graveyard of science” (Frost-Arnold 2011, p. 1138). Other antirealists are also pessimistic about the epistemic credentials of scientists (as opposed to the epistemic prospects of scientific theories). That is to say, they argue that scientists are not in an epistemic position to discover scientific truths in the way that scientific realists claim that they are. This antirealist claim, namely, that scientists do not occupy an epistemically privileged position such that they can tell which of the theories they are testing are (approximately) true and which are not, is then used as a premise in an antirealist argument known as the “Underconsideration Argument.” A persistent defender of this antirealist argument is Brad Wray (2008, p. 317).

Wray, Brad, K. 2008. The argument from underconsideration as grounds for antirealism: a defence. *International Studies in the Philosophy of Science* 22 (3): 317–326.

When scientists evaluate theories they only ever consider a subset of the theories that can account for the available data, specifically, those theories that have been developed. As a result, the anti-realist argues, when a scientist judges one theory to be superior to competitor theories she is hardly warranted in drawing the conclusion that the superior theory is likely true with respect to what it says about unobservable entities and processes. Anti-realists claim that the inference to the likely truth of the superior theory presumes scientists are especially skilled at developing theories that are apt to be true. But the history of science seems to suggest otherwise: scientists are not so privileged.

As I understand it, the argument that Wray is making in this passage can be stated in canonical (or standard) form as follows:

(P1) Scientists are warranted in concluding that the theory that is judged to be superior to a few other theories from a subset of competing theories is likely true with respect to what it says about unobservables only if scientists are epistemically privileged (that is, scientists are skilled at developing theories that are apt to be likely true).

(P2) It is not the case that scientists are epistemically privileged.

Therefore,

(C) It is not the case that scientists are warranted in concluding that the theory that is judged to be superior to a few other theories from a subset of competing theories is likely true with respect to what it says about unobservables.17

Reconstructed in this way, the Underconsideration Argument is a deductive argument (in particular, a *modus tollens*: If A, then B, not B; therefore, not A). Since the premises of Wray’s Underconsideration Argument, namely, (P1) and (P2), successfully provide logically conclusive support for the conclusion, namely, (C), this

---

17 Cf. Brad Wray’s (2012, p. 377) reconstruction of the Underconsideration Argument. See also Wray (2018, p. 43).
argument can be said to be valid. The next question, then, is whether the premises are in fact true. Is the Underconsideration Argument sound?

The key premise of the Underconsideration Argument seems to be (P2). Before we can say whether (P2) is true or false, we need to understand what it means. What does it mean to say that scientists are (or are not) epistemically privileged? To be privileged is to have special rights or advantages. In that case, talk of scientists not being “epistemically privileged” could mean that scientists do not have or do not enjoy special epistemic rights or advantages. What sort of special epistemic rights or advantages can scientists claim to have or enjoy in the first place? Well, they can claim to have special training and, as a result, certain skills that allow them to investigate specific domains in nature. In that respect, trained scientists have special epistemic advantages over untrained laymen in terms of having the knowledge and skills to investigate nature by means of observation and experimentation. Scientists can also claim to enjoy special access to advanced technology, such as observation instruments and experimentation techniques, and to having the advantage of using these technologies in their investigations of nature. In that respect, trained scientists have special epistemic rights that untrained laymen lack in terms of being entitled to use advanced instruments of observation and experimentation, such as particle accelerators, electron microscopes, and DNA sequencers, in their investigations of the natural world. On this reading of “epistemically privileged,” then, why is it that scientists are not epistemically privileged? Given their unique training, knowledge, skills, and access to advanced technology, why should we think that scientists are not “skilled at developing theories that are apt to be true”? Presumably, because they never get better at what they do. That is to say, all the scientific training, knowledge, skills, and access to advanced technology in the world could never make scientists sufficiently skilled at developing theories that are approximately true. But why assume that? That is to say, (P2) of the Underconsideration Argument seems to presuppose that the aforementioned aspects of theory generation, such as scientific training, technical skills, advanced instrumentation, and the like, do not change over time and that scientists never get better at what they do. But this is an assumption that needs to be argued for rather than taken for granted.

In response, antirealists could appeal to the history of science, and argue, as Wray (2018, p. 43) in fact does, that “the history of science seems to suggest [that] [s]cientists [do] not have […] epistemic privilege.” Indeed, Wray (2012, p. 380) writes that the “no-privilege thesis [that is, (P2) of the Underconsideration Argument] asks us to acknowledge the similarities between current scientists and their predecessors.” He quotes Mary Hesse (1976, p. 266), who argues that the support for the no-privilege premise, that is, (P2) of the Underconsideration Argument, comes from an “induction from the history of science.” In that case, antirealists would be making an inductive argument, similar in structure to the “Graveyard” Argument (see Sect. 5.1), in support of (P2) of the Underconsideration Argument. This inductive argument can be stated in canonical (or standard) form as follows:

(P) Many past scientists were not epistemically privileged (that is, they were not skilled at developing theories that are apt to be likely true).

Therefore,
(C) Current (and future) scientists are not epistemically privileged.

If this inductive argument were cogent, it would provide probable support for (P2) of the Underconsideration Argument. For, if current (and future) scientists are just like past scientists, then they, too, are not epistemically privileged, just as past scientists were not epistemically privileged. But are current (and future) scientists really like past scientists? Or are there relevant differences between the two such that no strong inductive inferences can be drawn from what was true about past scientists to what is true about current scientists (and what will be true about future scientists)? That is to say, if current (and future) scientists are not quite like past scientists, then, even if past scientists were not epistemically privileged, the conclusion that current (and future) scientists are not epistemically privileged as well would not follow inductively from that because the sample of scientists in this inductive generalization from a sample would not be sufficiently uniform for projecting the property of “being epistemically unprivileged” from past scientists to current (and future) scientists. In other words, if current (and future) scientists differ from past scientists in relevant respects, then they may also differ in terms of being epistemically (un)privileged.

Now, let us consider some of the relevant respects in which current scientists are unlike past scientists (Mizrahi 2013b, p. 398):

- Current scientists learn from their predecessors’ successes and failures and seek to avoid their predecessors’ mistakes. For example, scientists have learned from the mistakes of Franz Joseph Gall and others, which had led to the development of phrenology. Nowadays, scientists would not make the mistake of supposing that there is a simple relationship between the relative size of different parts of a person’s brain and that person’s personality and character traits.
- Current scientists have access to methods and technologies that were not available to their predecessors. For example, Galileo and his contemporaries may have had telescopes, but they did not have radio telescopes, infrared telescopes, microwave anisotropy probes, and much more.
- Current scientists are able to collaborate with colleagues on increasingly large-scale, costly, and international projects. For example, it is difficult to see how large-scale, international scientific projects, such as CERN’s Large Hadron Collider, the Human Genome Project, the Event Horizon Telescope, and the Human Brain Project, could have been executed even just a few decades ago.

Of course, current scientists are also like past scientists in some respects. For example, both current and past scientists present their research to their academic peers, and both publish their results in academic books and professional journals. But these respects in which they are alike are not relevant to being epistemically privileged, that is, to developing theories that are likely to be approximately true, and hence to the inductive inference from the failures of past scientists to the claim that current (and future) scientists are likely to fail, too. This is so precisely because current (and future) scientists have an expanding track record of successes and failures to draw upon and learn from. In other words, just as “the manifest difference in
methods between us and our predecessors breaks the pessimist’s induction,” according to Sherri Roush (2010, p. 55; see Sect. 5.1), the aforementioned differences between current scientists and their predecessors break the inductive inference in support of (P2) of the Underconsideration Argument. Moreover, even if we grant that past scientists were not epistemically privileged (that is, they were not skilled at developing theories that are apt to be likely true), as Wray (2008, p. 317) argues, “nothing follows from this about whether we have a right to our confidence in our particular theories” (Roush 2010, p. 55) unless the antirealist can also show that the lack of epistemic privilege of past scientists is a reason to think that current scientists lack epistemic privilege as well, which has not been shown, since the manifest differences in scientific knowledge, methods, instruments, and technologies between current scientists and their predecessors break the inductive inference in support of (P2) of the Underconsideration Argument.

As we have seen in Sect. 5.1, for scientific realists, the problem with the “Graveyard” Argument is that it overemphasizes the similarities and underemphasizes the dissimilarities between current scientific theories and their predecessors. This is why, scientific realists argue, the failure of past theories does not warrant a strong inductive inference to the failure of current (and future) theories. Similarly, as far as scientific realists are concerned, the problem with the inductive argument in support for (P2) of the Underconsideration Argument is that it overemphasizes the similarities and underemphasizes the dissimilarities between current scientists and their predecessors. Current scientists learn from their predecessors and they seek to avoid their predecessors’ mistakes. Furthermore, current scientists have access to methods and technologies that were not available to their predecessors. For scientific realists, these aspects of scientific change make a difference insofar as the ability of scientists to develop theories is concerned. This is why, scientific realists would argue, the failure of past scientists does not warrant a strong inductive inference to the failure of current (and future) scientists. As Alexander Bird (2007a, p. 80) puts it:

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 (emphasis added).

Similarly, later scientists are not trained independently of the successes and failures of their predecessors. Current (and future) scientists avoid the pitfalls of their predecessors and seek to incorporate their successes. If this is correct, then the inductive argument that purports to provide support for (P2) of the Underconsideration Argument is a weak argument. Without strong inductive support for (P2), then, this premise of the Underconsideration Argument cannot be said to be true, and thus the Underconsideration Argument itself, although valid, cannot be said to be sound.
5.4 The Argument from Unconceived Alternatives

Much like the Underconsideration Argument (see Sect. 5.3), the Argument from Unconceived Alternatives, is an argument about *scientists* rather than scientific *theories*. The Underconsideration Argument proceeds from the premise that scientists are not epistemically privileged, whereas the Argument from Unconceived Alternatives proceeds from the premise that past scientists were not in a position to conceive of all the alternatives to the few theories they actually ended up testing. Subsequently, it turned out that those alternative theories, which were unconceived at the time, were equally well-confirmed by the evidence available at the time. From this, then, it is supposed to follow that current (and future) scientists are not in a position to conceive of all the alternatives to the few theories they actually end up testing, either. Therefore, the argument goes, it will probably turn out that those alternative theories, which are unconceived at the time, will be equally well-confirmed by the evidence available at the time, and thus that we have no good reasons to believe that our current best theories are approximately true.

This argument was made by Kyle Stanford on several occasions. According to Stanford (2006, p. 19), “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 calls this the “Problem of Unconceived Alternatives” (PUA). For him, the PUA provides the inductive basis for “the following New Induction over the History of Science” (Stanford 2001, p. S9).

Stanford, Kyle P. 2001. Refusing the devil’s bargain: what kind of underdetermination should we take seriously? *Philosophy of Science* 68 (S3): S1–S12.

As I understand it, the argument that Stanford is making in this passage can be stated in canonical (or standard) form as follows:

(P) Past scientists typically failed to conceive of alternatives to the few theories that they confirmed by the evidence available to them at the time.

Therefore,

(C) Current (and future) scientists fail to conceive of alternatives to the few theories that they confirm by the evidence available to them at the time.

If (P) provides strong inductive support for (C), then we should not believe that our current best theories are approximately true because there probably are many alternative theories, which are equally well-confirmed by the available evidence, but that we have not even conceived of yet (Magnus 2010, p. 807). In that respect, this New Induction over the History of Science, or Argument from Unconceived
Alternatives, is supposed to be an inductive argument. So the next question is whether this argument is strong or weak.\footnote{Darrell Rowbottom (2019) extends the Argument from Unconceived Alternatives to include aspects of science in addition to scientific theories, such as models, predictions, instruments, and more.}

As we have seen in Sects. 5.1 and 5.3, antirealist arguments that seek to draw conclusions from the history of science by induction tend to assume that past scientists and/or scientific theories are like current (and future) scientists and/or scientific theories, so what was true of past scientists and/or scientific theories is probably true of present scientists and/or scientific theories, and will probably be true of future scientists and/or scientific theories as well. But is this a warranted assumption. After all, scientists are human beings and, as any good social scientist knows, human beings are not only “much more complicated than anything else studied by the methods of natural science” (Rosenberg 1980, p. 93) but also differ greatly from one human society or group to another. Scientists are human beings, of course, and so they, too, are “much more complicated than moving bodies, chemical reagents, and ocean tides” (Rosenberg 1980, p. 10). For these reasons, no simple inductive inferences can be made from what is true about some scientists to what is true (or will be true) about other scientists. In other words, when we are dealing with human subjects, we cannot simply make what Peter Godfrey-Smith (2011, p. 41) calls “seen one, seen them all” inductive inferences. According to Godfrey-Smith (2011, p. 41), a “seen one, seen them all” inductive inference is when “one instance of an $F$ would be enough, in principle, if you picked the right case and analyzed it well.” This is because, as far as “seen one, seen them all” inductive inferences are concerned, “sample size per se does not matter, randomness does not matter, but the status of the kinds matters enormously” (Godfrey-Smith 2011, p. 42). In other words, if we have reasons to believe that our instance of an $F$ belongs to a certain kind of things—what philosophers of science sometimes call a “natural kind”—such that all $Fs$ are the same kind of thing, then the inductive inference from “this is an $F$ that is also a $G$” to “All $Fs$ are $Gs$” would be a strong inductive inference. For example, all copper rods have the same atomic structure, which is why if we have seen one copper rod, we have seen them all. Accordingly, from testing just a few copper rods for electrical conductivity, we can inductively infer that all copper rods conduct electricity. That is to say, the inductive inference from “these few copper rods conduct electricity” to “all copper rods conduct electricity” is a strong inductive inference because of the uniform atomic structure of copper rods. Copper rods are a natural kind, if you will. Scientists, however, are not like copper rods. Scientists do not have a uniform atomic structure. Scientists are not a natural kind. Consequently, when it comes to scientists, no strong “seen one, seen them all” inductive inferences can be made based on what holds for just a few scientists. For example, if we study a few past scientists and learn that they had failed to conceive of alternatives to the few theories that they confirmed by the evidence available to them at the time, no strong inductive conclusions about other scientists would
follow from this small sample. (Recall our discussion of the use of case studies as evidence from Chap. 2, Sect. 2.2.)

When we are dealing with complicated things, such as scientists, which do not belong to a natural kind, we need a different inductive bridge from evidence or premises to conclusions. This inductive bridge from premises to conclusion is generalization from random samples. As Godfrey-Smith (2011, p. 42) puts it, “This form of inference has the following features: sample size matters, randomness matters, and ‘law-likeness’ or ‘naturalness’ does not matter.” That is to say, when we are dealing with diverse individuals, we cannot generalize from one or two individuals by assuming that what is true for one or two is true for the others as well. This is because the “seen one, seen them all” inference does not work for things that do not belong to a uniform, natural kind. When we are dealing with diverse individuals, and we want to reason inductively about them, we need to make sure that we reason inductively from large and random samples, if we want our inductive inferences to be strong inferences. As far as the Argument from Unconceived Alternatives is concerned, however, the sample of scientists is small. Stanford (2006, pp. 19–20) provides only eight examples of allegedly unconceived alternatives.

Moreover, the sample of scientists was not randomly selected, but rather was cherry-picked in order to advance an argument against scientific realism. The lack of random sampling undermines the inductive inference from a sample of past scientists to what is true about current scientists (and what will be true about future scientists) because it biases the sample on which the inductive inference is based. When a sample is biased, the door is then open to all kinds of mistakes in inductive reasoning, such as confirmation bias and Type I errors. Confirmation bias occurs when one selects and gives more weight to any evidence that supports one’s previously held beliefs. Clearly, any inductive inference made from evidence carefully selected to support one’s previously held beliefs will likely be non-cogent. A Type I error occurs when one accepts a positive result when that positive result is in fact false. This is why Type I errors are also referred to as “false positives.” For example, like any other tests for viral infections, viral tests for Coronavirus Disease 2019 (COVID-19) can have false positive and false negative results. Suppose that a healthy person, who is not infected with the Severe Acute Respiratory Syndrome Coronavirus 2 (SARS-CoV-2), gets tested and the viral test returns a positive result, that is, a result that the person is infected with SARS-CoV-2. This is a false positive because the test result is positive even though the person is not infected. When we accept the false positive result of this test, we make a Type I error. On the other hand, suppose that a sick person, who is infected with SARS-CoV-2, gets tested and the viral test returns a negative result, that is, a result that the person is not infected with SARS-CoV-2. This is a false negative because the test result is negative even though the person is infected. When we accept the false negative result of this test, we make a Type II error.

Now, without random sampling, we cannot tell if Stanford’s sample of past scientists is not simply a biased sample. Perhaps, even unbeknownst to him, Stanford has selected and gave more weight to any evidence that supported his previously held beliefs. Furthermore, without random sampling, we cannot tell if Stanford’s
sample of past scientists is not simply a sample of false positives, that is, a sample of past scientists who are thought to have failed to conceive of alternative theories but in fact did not fail to do so. Perhaps, even unbeknownst to him, Stanford has made a Type I error. That is to say, he examined the history of science and “tested” past scientists for whether they succeeded or failed to conceive of alternative theories, and his tests returned false positive results, that is, past scientists who are thought to have failed to conceive of alternative theories but in fact did not fail to do so. Since Stanford’s sample of past scientists was not randomly selected, but rather was cherry-picked in order to advance an argument against scientific realism, it is likely that such errors were made. If the sample from which the conclusion of the Argument from Unconceived Alternatives is drawn is too small and not random, then no strong inductive generalizations can be made based on that sample. In other words, the Argument from Unconceived Alternatives is a weak inductive argument.

Indeed, as we have seen in Sect. 5.3, there are reasons to think that current scientists are in fact unlike past scientists in many relevant respects. Current scientists learn from their predecessors and they seek to avoid their predecessors’ mistakes. Furthermore, current scientists have access to knowledge, methods, instruments, and technologies that were not available to their predecessors. In other words, just as “the manifest difference in methods between us and our predecessors breaks the pessimist’s induction,” according to Sherri Roush (2010, p. 55; see Sect. 5.1), the manifest differences in scientific knowledge, methods, instruments, and technologies between current scientists and their predecessors break the inductive Argument from Unconceived Alternatives. Moreover, even if we grant that past scientists typically failed to conceive of alternatives to the few theories that they confirmed by the evidence available to them at the time, as Stanford (2001, p. S9) argues, “nothing follows from this about whether we have a right to our confidence in our particular theories” (Roush 2010, p. 55) unless antirealists can also show that the lack of theoretical imagination of past scientists is a reason to think that current scientists lack theoretical imagination as well, which has not been shown, since the manifest differences in scientific knowledge, methods, instruments, and technologies between current scientists and their predecessors break the inductive Argument from Unconceived Alternatives. If this is correct, then one might complain that the Argument from Unconceived Alternatives overemphasizes the similarities and underemphasizes the dissimilarities between current scientists and their predecessors. Given that there are relevant differences between past scientists and current scientists (see Sect. 5.3), the inductive inference from what was true of past scientists to what is true of present scientists, and what will be true of future scientists, is weak. In other words, the Argument from Unconceived Alternatives is a weak inductive argument.20

19 In Mizrahi (2015), I argue that some of the examples in Stanford’s sample are in fact false positives.

20 In Mizrahi (2015), I argue that the historical evidence Stanford cites in support of his New Induction is indeterminate between a pessimistic (antirealist) interpretation and an optimistic
There is another problem with the Argument from Unconceived Alternatives that makes it a problematic argument for antirealists to use as an argument against scientific realism. Recall that, according to Stanford (2001, p. S9), the “Problem of Unconceived Alternatives” (PUA) applies to “virtually every field.” That is to say, in every field of inquiry, inquirers often occupy an epistemic position in which they could conceive of only a few theories that are well-confirmed by the available data but could not conceive of alternative theories that subsequent history of inquiry would show to be equally well-confirmed by the available evidence. Since this is supposedly true of every field of inquiry, it is supposed to be true of philosophy as well, given that philosophy is a field of inquiry. In that case, philosophers, too, often occupy an epistemic position in which they could conceive of only a few theories that are well-confirmed by the available evidence but could not conceive of alternative theories that subsequent history of philosophical inquiry would show to be equally well-confirmed by the available evidence. This, in turn, means that Stanford’s New Induction applies not only to science but also to philosophy. Accordingly, one could make a Stanford-like New Induction over the History of Philosophy. Such an argument can be stated in canonical (or standard) form as follows:

(P) Past philosophers typically failed to conceive of alternatives to the few theories that they confirmed by the evidence available to them at the time.

Therefore,

(C) Current (and future) philosophers fail to conceive of alternatives to the few theories that they confirm by the evidence available to them at the time.

If this inductive argument were cogent, however, then it would mean that scientific antirealism is probably one of the few theories that philosophers could conceive of and that were well-confirmed by the available evidence, but that subsequent history of philosophical inquiry would show that there are alternatives to antirealism that philosophers could not conceive of and that are equally well-confirmed by the available evidence. In other words, if we should not believe that our current best scientific theories are approximately true because there probably are many alternative scientific theories, which are equally well-confirmed by the available evidence, but that scientists have not even conceived of yet (Magnus 2010, p. 807), then we should not believe that our current philosophical theories are approximately true because there probably are many alternative philosophical theories, which are equally well-confirmed by the available evidence, but that philosophers have not even conceived of yet (Mizrahi 2016b, pp. 60–63).

Accordingly, if antirealists would like to use Stanford’s New Induction as an argument against scientific realism, then they would have to concede that a Stanford-like New Induction applies to philosophical inquiry as well, given that the PUA applies to “virtually every field” (Stanford 2001, p. S9), including philosophy. Again, as Stanford (2006, p. 44) himself puts it:

(realist) interpretation. If the historical evidence is indeterminate between scientific realism and antirealism, then it cannot be used to argue in favor of one over the other (see Chap. 4, Sect. 4.5).
the problem of unconceived alternatives and the new induction suggest not that present theories are no more likely to be true than past theories have turned out to be, but instead that present theorists are no better able to exhaust the space of serious, well-confirmed possible theoretical explanations of the phenomena than past theories have turned out to be (emphasis added).

Of course, insofar as philosophers are theorists as well, the PUA applies to them because it applies to every theorist. That is to say, present philosophers, too, are no better able to exhaust the space of serious, well-confirmed possible theoretical explanations of the phenomena than past philosophers have turned out to be. Granting that a Stanford-like New Induction applies to philosophical inquiry, however, means that we should not believe that our current philosophical theories are (approximately) true because there probably are many alternative philosophical theories, which are equally well-confirmed by the available evidence, but that philosophers have not even conceived of yet (Mizrahi 2016b, pp. 60–63). From this, in turn, it follows that we should not believe scientific antirealism, since antirealism is probably one of those current philosophical theories with unconceived alternatives that are equally well-confirmed by the available evidence. If this is correct, then, by advancing Stanford’s New Induction as a cogent argument against realism about theories, antirealists would be undermining their own theory, namely, antirealism. For this reason, it seems that even antirealists cannot endorse the Argument from Unconceived Alternatives as a cogent argument against scientific realism, for, in doing so, they would be undermining their own antirealist position. So, even if it were a strong argument, the Argument from Unconceived Alternatives cannot be said to be a cogent argument against scientific realism.

5.5 The Argument from Changing Research Interests

As we have seen in Sect. 5.1, antirealists tend to subscribe to the view that the history of science is a “graveyard of dead epistemic objects” (Chang 2011, p. 426), that is, discarded theories and abandoned theoretical posits. From this historical understanding of science as a “graveyard of dead epistemic objects” (Chang 2011, p. 426), antirealists then draw the conclusion that current (and future) scientific theories and theoretical posits will end up in that “graveyard” as well (see Sect. 5.1 on the “ Graveyard” Argument). As many antirealists do, Brad Wray (2018) also thinks that our current scientific theories will be discarded in the future, but he develops an original argument for this conclusion that, for him, explains why 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.” In Mizrahi (2016a), I provide empirical evidence against this “graveyard” picture of the history of science. Cf. Tulodziecki (2017).

21 Cf. Sterpetti (2019). I reply to Sterpetti (2019) in Mizrahi (2019).
22 As Peter Lipton (2005, p. 1265) 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 is marked by discarded or failed theories. According to Wray (2018, p. 187), “the research interests that determine what sorts of issues a scientist investigates” change over time. For example, “At one point in the history of astronomy, astronomers were concerned with the question of whether or not planets were self-illuminating” (Wray 2018, p. 187). For current astronomers, Wray says, this question is no longer a concern. He takes this to be evidence against scientific realism or a reason “to expect that many of the theories we currently accept, despite their many impressive successes, will be discarded sometime in the future” (Wray 2019, p. 555). For Wray, this explains why the history of science is marked by failed theories, the so-called “historical graveyard of science” (Frost-Arnold 2011, p. 1138). He makes this argument in his book-length defense of antirealism (Wray 2018, p. 187).

As I understand it, the argument that Wray is making in this passage can be stated in canonical (or standard) form as follows:

(P1) If scientific theories are partial representations of the world, then scientists have to decide which features of the world to disregard in their theories and their decisions are determined by their research goals and interests.

(P2) Scientific theories are partial representations of the world.

Therefore,

(C1) Scientists have to decide which features of the world to disregard in their theories and their decisions are determined by their research goals and interests. [from (P1) & (P2)]

(P3) If scientists have to decide which features of the world to disregard in their theories, and their decisions are determined by their research goals and interests, then their theories will be replaced by new theories as they realize their research goals and their research interests change.

Therefore,

(C2) Scientific theories will be replaced by new theories as scientists realize their research goals and their research interests change. [from (C1) & (P3)]

This line of reasoning is valid. That is to say, at each step, the premises provide logically conclusive support for the conclusion that follows from those premises. The next question, then, is whether the premises are actually true. Is Wray’s Argument from Changing Research Interests sound?

A key premise of Wray’s Argument from Changing Research Interests is (P3). It assumes that the research goals and interests of scientists change over time. This
assumption is then used as a premise in support of the conclusion that scientific theories will change, that is, replaced by new theories, with the changing research goals and interests of scientists. Now, it may be the case that secondary, small-scale, or short-term research goals do change over time, as Wray claims, but perhaps there are primary, large-scale, or long-term research goals that do not change over time. For example, perhaps ancient astronomers aimed at answering “the question of whether or not planets were self-illuminating,” as Wray (2018, p. 187) says, whereas current astronomers have no such research goal or interest. But these may be secondary, small-scale, or short-term research goals and interests, for example, “Are planets self-illuminating?” or “Is there liquid water on the surface of Mars?” Perhaps ancient astronomers and current astronomers share primary, large-scale, or long-term research goals and interests, such as “What is the most accurate model of our solar system?” or “What is the best definition of ‘planet?’” Current astronomers aim to answer such questions, which date back to the origins of astronomy, as the following quote from David Weintraub’s book, Is Pluto a Planet? (2007, p. 220) illustrates:

Our quest to answer the question Is Pluto a planet? led us directly to a question about physics: What is a planet? Answering this second question, which was not simple or easy, has revealed that we live in a solar system that is quite different from the one we thought we lived in: The solar system has more than twenty planets! (emphasis in original)

Accordingly, if there are such primary, large-scale, or long-term research goals and interests that remain relatively fixed over time, then it does not necessarily follow that scientific theories will change with the changing research goals and interests of scientists from the fact that there are also secondary, small-scale, or short-term research goals and interests that do change over time. For the claim about changing research goals and interests fails to distinguish between the primary and the secondary research goals and interests of scientists.

In other words, (P3) of Wray’s Argument from Changing Research Interests is ambiguous. More specifically, the phrase “research goals and interests” is ambiguous between two interpretations: primary, large-scale, or long-term research goals and interests versus secondary, small-scale, or short-term research goals and interests. On the one hand, if the phrase “research goals and interests” means secondary, small-scale, or short-term research goals and interests, then change in such research goals and interests does not entail that scientific theories will be replaced by new theories as scientists realize their secondary research goals and their secondary research interests change because scientists might still have primary, large-scale, or long-term research goals and interests that remain relatively fixed over time. On the other hand, if the phrase “research goals and interests” means primary, large-scale, or long-term research goals and interests, then Wray does not provide sufficient evidence or reasons to believe that such primary research goals and interests do change over time.

In fact, as we have seen in Chap. 3, there are both realist and antirealist positions in the scientific realism/antirealism debate according to which scientists do have primary research goals and interests that are not supposed to change over time.
According to Constructive Empiricism (see Sect. 3.3), “Science aims to give us theories which are empirically adequate” (van Fraassen 1980, p. 12). For constructive empiricists, then, the primary research goal or aim of scientists is empirical adequacy. Empirical adequacy is a research goal or aim that is not supposed to change over time. If constructive empiricists are right about this, then (P3) of Wray’s Argument from Changing Research Interests is false. Even if scientists have to decide which features of the world to disregard in their theories, and their decisions are determined by their secondary research goals and interests, it does not necessarily follow that their scientific theories will be replaced by new theories as their secondary research goals and interests change, for empirical adequacy will remain their primary research goal or aim, even as they realize their secondary research goals.

As we have also seen in Chap. 3, constructive empiricists contrast their antirealist position with scientific realism by saying that, according to scientific realism, “Science aims to give us, in its theories, a literally true story of what the world is like” (van Fraassen 1980, p. 8), rather than empirically adequate theories. For scientific realists, then, the primary research goal or aim of scientists is approximate truth. Approximate truth is a research goal or aim that is not supposed to change over time. Again, if scientific realists are right about this, then (P3) of Wray’s Argument from Changing Research Interests is false. Even if scientists have to decide which features of the world to disregard in their theories, and their decisions are determined by their secondary research goals and interests, it does not necessarily follow that their theories will be replaced by new theories as their secondary research goals and interests change, for approximate truth will remain their primary research goal or aim, even as they realize their secondary research goals. For these reasons, (P3) of Wray’s Argument from Changing Research Interests may be false, which means that this argument, albeit valid, cannot be said to be sound.

5.6 Summary

In the contemporary scientific realism/antirealism debate, the antirealist argument that has attracted the most attention from scientific realists and antirealists alike is the so-called “pessimistic induction” or “pessimistic meta-induction.” According to this argument, the history of science is a graveyard of “once successful but now dead” scientific theories, and so our current theories will end up in the historical graveyard of science as well. Scientific realists object to this argument by pointing out that the few examples selected to support the “graveyard” premise of the argument are not representative of the general population of scientific theories because they are too few, and not randomly selected, but rather cherry-picked in an attempt to refute scientific realism. Moreover, selected counterexamples cannot refute scientific realism because a predictively successful but dead theory does not count as an effective counterexample against the claim that predictive success is a reliable indicator of approximate truth (see Sect. 5.1). Constructive empiricists make a positive argument for their position by arguing that Constructive Empiricism is the best
explanation for scientific activity. Scientific realists object by pointing out that constructive empiricists cannot help themselves to making Inferences to the Best Explanation (IBE) and that there are realist explanations that explain scientific activity just as well as Constructive Empiricism does (see Sect. 5.2). Two antirealist arguments that focus on scientists, rather than on scientific theories, are the Underconsideration Argument and the Argument from Unconceived Alternatives. According to the first, scientists are not epistemically privileged, which is why we should not believe that their best theories are approximately true (see Sect. 5.3). According to the second, scientists routinely fail to conceive of alternatives to their well-confirmed theories, even though those alternatives later end up being equally well-confirmed by the available evidence (see Sect. 5.4). Scientific realists object to these arguments by pointing out that there are relevant differences between past scientists and current scientists, so we cannot simply infer from what was true about past scientists to what is true about current scientists and what will be true about future scientists by induction. Finally, according to the Argument from Changing Research Interests, our best scientific theories are likely to be replaced by new theories as the research goals of scientists are achieved and their research interests change (see Sect. 5.5). Scientific realists could object to this argument by distinguishing between primary and secondary research goals. The latter may change, but the former could be stable, which means that theories could be stable as well. Indeed, according to both scientific realist and antirealist positions in the scientific realism/antirealism debate, scientists supposedly have primary research goals that do not change over time. For scientific realists, approximate truth is such a primary research goal, whereas for constructive empiricists, it is empirical adequacy.

**Glossary**

**Antirealism** An agnostic or skeptical attitude toward the theoretical posits (that is, unobservables) of scientific theories. Antirealism comes in different varieties, such as Constructive Empiricism (see Chap. 3, Sect. 3.3) and Instrumentalism (see Chap. 3, Sect. 3.2).

**Approximate truth** Closeness to the truth or truthlikeness. To say that a theory is approximately true is to say that it is close to the truth. According to some scientific realists, approximate truth is the aim of science. (See Chap. 2, Sect. 2.1.)

**Case study** A particular, detailed description of a scientific activity, a scientific practice, or an episode from the history of science. (See Chap. 2, Sect. 2.2.)

**Cherry-picking** A sample from which an inductive inference is made is said to be cherry-picked when it is not randomly selected. (See Chap. 5, Sect. 5.1.)

**Constructive Empiricism** The view that the aim of science is to construct empirically adequate theories. A theory is empirically adequate when what the theory says about what is observable (by us) is true. (See Chap. 3, Sect. 3.3.)

**Empirical success** A scientific theory is said to be empirically successful just in case it is both explanatorily successful (that is, it explains natural phenomena
that would otherwise be mysterious to us) and predictively successful (that is, it makes predictions that are borne out by observation and experimentation). (See Chap. 3, Sect. 3.1.)

**Explanationist Realism** The view that realist commitments are warranted with respect to the theoretical posits that are responsible for—or can best explain—the predictive success of our best scientific theories (also known as “Deployment Realism”). (See Chap. 3, Sect. 3.1.)

**Explanatory success** A scientific theory is said to be explanatorily successful just in case it explains natural phenomena that would otherwise be mysterious to us. (See Chap. 3, Sect. 3.1.)

**Fallacious argument** An argument whose premises fail to provide either conclusive or probable support for its conclusion (see also invalid argument and weak argument). (See Chap. 2, Sect. 2.2.)

**Hasty generalization** A fallacious inductive argument from a sample that is not representative of the general population that is the subject of the conclusion of the argument (because the sample is too small or cherry-picked rather than randomly selected). (See Chap. 2, Sect. 2.2.)

**The historical graveyard of science** The claim that, throughout the history of science, most scientific theories and theoretical posits have been abandoned, discarded, or replaced by new scientific theories and theoretical posits. (See Chap. 5, Sect. 5.1.)

**Inference to the Best Explanation (IBE)** An ampliative (or non-deductive) form of argumentation that proceeds from a phenomenon that requires an explanation to the conclusion that the best explanation for that phenomenon is probably true. (See Chap. 4, Sect. 4.1.)

**Invalid argument** A deductive argument in which the premises purport but fail to provide logically conclusive support for the conclusion. (See Chap. 1, Sect. 1.1.)

**Modus ponens** A form of argument with a conditional premise, a premise that asserts the antecedent of the conditional premise, and a conclusion that asserts the consequent of the conditional premise. That is, “if \(A\), then \(B\); \(A\); therefore, \(B\),” where \(A\) and \(B\) stand for statements. Modus ponens is a valid form of inference, and so an argument in natural language that takes this logical form is valid. On the other hand, the following logical form is invalid: “if \(A\), then \(B\), \(B\); therefore, \(A\).” It is known as the fallacy of affirming the consequent. (See Chap. 4, Sect. 4.1.)

**Modus tollens** A form of argument with a conditional premise, a premise that denies the consequent of the conditional premise, and a conclusion that denies the antecedent of the conditional premise. That is, “if \(A\), then \(B\), not \(B\); therefore, not \(A\),” where \(A\) and \(B\) stand for statements. Modus tollens is a valid form of inference, and so an argument in natural language that takes this logical form is valid. On the other hand, the following logical form is invalid: “if \(A\), then \(B\), not \(A\); therefore, not \(B\).” It is known as the fallacy of denying the antecedent. (See Chap. 5, Sect. 5.1.)

**Predictive success** A scientific theory is said to be predictively successful just in case it makes predictions that are borne out by observation and experimentation. (See Chap. 3, Sect. 3.1.)
The Problem of Unconceived Alternatives (PUA) The claim that, throughout the history of science, scientists typically occupied an epistemic position in which they could conceive of only a few theories that were well-confirmed by the available evidence, while there were alternative theories that were as well-confirmed by the available evidence as those theories that were accepted by scientists. (See Chap. 5, Sect. 5.4.)

Scientific realism An epistemically positive attitude toward those aspects of scientific theories that are worthy of belief. Scientific realism comes in different varieties, such as Explanationist Realism (see Chap. 3, Sect. 3.1), Entity Realism (see Chap. 3, Sect. 3.4), Structural Realism (see Chap. 3, Sect. 3.5), and Relative Realism (see Chap. 6, Sect. 6.1).

Weak argument A non-deductive (or inductive) argument in which the premises purport but fail to provide probable support for the conclusion. (See Chap. 1, Sect. 1.1.)

References and Further Readings

Baker, A. (2010). Inference to the best explanation. In F. Russo & J. Williamson (Eds.), Key terms in logic (pp. 37–38). London: Continuum.

Bird, A. (2007a). What is scientific progress? Noûs, 41(1), 64–89.

Campos, D. G. (2011). On the distinction between Peirce’s abduction and Lipton’s inference to the best explanation. Synthese, 180(3), 419–442.

Chakravartty, A. (2008). What you don’t know can’t hurt you: Realism and the unconceived. Philosophical Studies, 137(1), 149–158.

Chang, H. (2011). The persistence of epistemic objects through scientific change. Erkenntnis, 75(3), 413–429.

Douven, I. (2017). Abduction. In E. N. Zalta (Ed.), The Stanford encyclopedia of philosophy (Summer 2017 ed.). https://plato.stanford.edu/archives/sum2017/entries/abduction/.

Fahrbach, L. (2011). How the growth of science ends theory change. Synthese, 180(2), 139–155.

Frost-Arnold, G. (2011). From the pessimistic induction to semantic antirealism. Philosophy of Science, 78(5), 1131–1142.

Godfrey-Smith, P. (2011). Induction, samples, and kinds. In J. K. Campbell, M. O’Rourke, & M. H. Slater (Eds.), Carving nature at its joins: Topics in contemporary philosophy (pp. 33–52). Cambridge, MA: The MIT Press.

Harman, G. H. (1965). The inference to the best explanation. The Philosophical Review, 74(1), 88–95.

Hesse, M. (1976). Truth and the growth of scientific knowledge. PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association, 1976(2), 261–280.

Hurley, P. J. (2006). A concise introduction to logic (9th ed.). Belmont: Wadsworth.

Kitcher, P. (1993). The advancement of science: Science without legend, objectivity without illusions. New York: Oxford University Press.

Ladyman, J. (2002). Understanding philosophy of science. London: Routledge.

Ladyman, J. (2007). Ontological, epistemological, and methodological positions. In T. Kuipers (Ed.), General philosophy of science: Focal issues (pp. 303–376). Amsterdam: Elsevier.

Ladyman, J. (2011). Structural realism versus standard scientific realism: The case of phlogiston and dephlogisticated air. Synthese, 180(2), 87–101.

Laudan, L. (1981). A confutation of convergent realism. Philosophy of Science, 48(1), 19–49.

Lipton, P. (1993). Is the best good enough? Proceedings of the Aristotelian Society, 93(1), 89–104.
References and Further Readings

Lipton, P. (2005). The Medawar lecture 2004: The truth about science. *Philosophical Transactions of the Royal Society B, 360*(1458), 1259–1269.

Lyons, T. D. (2017). Epistemic selectivity, historical threats, and the non-epistemic tenets of scientific realism. *Synthese, 194*(9), 3203–3219.

Magnus, P. D. (2010). Inductions, red herrings, and the best explanations for the mixed record of science. *The British Journal for the Philosophy of Science, 61*(4), 803–819.

Mizrahi, M. (2013a). The pessimistic induction: A bad argument gone too far. *Synthese, 190*(15), 3209–3226.

Mizrahi, M. (2013b). The argument from underconsideration and relative realism. *International Studies in the Philosophy of Science, 27*(4), 393–407.

Mizrahi, M. (2015). Historical inductions: New cherries, same old cherry-picking. *International Studies in the Philosophy of Science, 29*(2), 129–148.

Mizrahi, M. (2016a). The history of science as a graveyard of theories: A philosophers’ myth? *International Studies in the Philosophy of Science, 30*(3), 263–278.

Mizrahi, M. (2016b). Historical inductions, unconceived alternatives, and unconceived objections. *Journal for General Philosophy of Science, 47*(1), 59–68.

Mizrahi, M. (2018). The “positive argument” for constructive empiricism and inference to the best explanation. *Journal for General Philosophy of Science, 3*, 1–6.

Mizrahi, M. (2019). An absurd consequence of Stanford’s new induction over the history of science: A reply to Sterpetti. *Axiomathes, 29*(5), 515–527.

Park, S. (2011). A confutation of the pessimistic induction. *Journal for General Philosophy of Science, 42*(1), 75–84.

Park, S. (2019). In defense of realism and selectivism from Lyons’s objections. *Foundations of Science, 24*(4), 605–615.

Psillos, S. (2007). The fine structure of inference to the best explanation. *Philosophy and Phenomenological Research, 74*(2), 441–448.

Psillos, S. (2018). Realism and theory change in science. In E. N. Zalta (Ed.), *The Stanford encyclopedia of philosophy* (Summer 2018 ed.) https://plato.stanford.edu/archives/sum2018/entries/realism-theory-change.

Putnam, H. (1975). *Mathematics, matter and method*. New York: Cambridge University Press.

Rosenberg, A. (1980). *Sociobiology and the preemption of social science*. Baltimore: Johns Hopkins University Press.

Roush, S. (2010). Optimism about the pessimistic induction. In P. D. Magnus & J. Busch (Eds.), *New waves in philosophy of science* (pp. 29–58). New York: Palgrave Macmillan.

Rowbottom, D. P. (2019). Extending the argument from unconceived alternatives: Observations, models, predictions, explanations, methods, instruments, experiments, and values. *Synthese, 196*(10), 3947–3959.

Schurz, G. (2019). *Hume’s problem solved: The optimality of meta-induction*. Cambridge, MA: The MIT Press.

Stanford, K. P. (2001). Refusing the devil’s bargain: What kind of underdetermination should we take seriously? *Philosophy of Science, 68*(S3), S1–S12.

Stanford, K. P. (2006). *Exceeding our grasp: Science, history, and the problem of unconceived alternatives*. New York: Oxford University Press.

Sterpetti, F. (2019). On Mizrahi’s argument against Stanford’s instrumentalism. *Axiomathes, 29*(2), 103–125.

Tulodziecki, D. (2017). Against selective realism(s). *Philosophy of Science, 84*(5), 996–1007.

van Fraassen, B. C. (1980). *The scientific image*. New York: Oxford University Press.

Vickers, P. (2019). Towards a realistic success-to-truth inference for scientific realism. *Synthese, 196*(2), 571–585.

Weintraub, D. A. (2007). *Is Pluto a planet? A historical journey through the solar system*. Princeton: Princeton University Press.

Wray, B. K. (2008). The argument from underconsideration as grounds for anti-realism: A defence. *International Studies in the Philosophy of Science, 22*(3), 317–326.
Wray, B. K. (2012). Epistemic privilege and the success of science. *Noûs, 46*(3), 375–385.
Wray, B. K. (2018). *Resisting scientific realism*. Cambridge: Cambridge University Press.
Wray, B. K. (2019). Discarded theories: The role of changing interests. *Synthese, 196*(2), 553–569.