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# Comparative success and empirical progress without approximate truth

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Abstract: This paper argues against a particular version of the inference from the success of a scientific theory to the claim that the theory must be approximately true to some extent. The kind of success at issue is comparative, where one theory is more empirically successful than its rival if that theory predicts phenomena that are inexplicable or anomalous according to its rival. A theory that exhibits this kind of comparative success can be seen as thereby achieving empirical progress over its rival. David Harker has developed a form of selective scientific realism based on the idea that this kind of success is evidence for the approximate truth of the parts of theories responsible for such success. Counterexamples to Harker's position are cases in which a theory is more successful than its rival in virtue of containing parts that are not even approximately true. In order to identify some counterexamples to Harker's position, this paper considers four historical cases that Greg Frost-Arnold has recently used to motivate a novel historical challenge to realism called the Problem of Misleading Evidence. This paper argues that these four cases are counterexamples to Harker's position, and that they provide a strong reason to doubt his position and the kind of success-to-truth inference that he defends.

Keywords: selective scientific realism, comparative success, empirical progress, success-to-truth inference, historical challenge to scientific realism

## 1. Introduction

The scientific realism debate concerns whether we have sufficient reason to conclude that what science tells us about unobservable entities, processes, properties, etc., is at least approximately true. Traditionally, scientific realists claim that we do, on the grounds that our best theories are quite successful when it comes to explaining and predicting various observable phenomena by appealing to unobservables that purportedly underlie those phenomena. Intuitively, it seems quite improbable that a theory could get it so right about these observable phenomena if that theory gets it fundamentally wrong when it comes to the underlying unobservables. On the other hand, antirealists claim on historical grounds that we don't have sufficient reason. The history of science contains theories that were quite successful when it comes to explaining and predicting various observable phenomena. However, many of those theories' claims about what happens at the unobservable level can't be even approximately true from the perspective of our current best theories.

Many philosophers of science have felt the pull of both of these considerations, and have attempted to defend some kind of inference from the success of a theory to its approximate truth in a way that is consistent with what we find in the history of science. They have relied primarily on two sorts of strategies in order to do so. The first strategy is to admit that not all kinds of explanatory and predictive success are indicative of approximate truth, and to identify a particular kind of success that can justify the success-to-truth inference. The second strategy is to give up the traditional realist idea that our best theories are approximately true when considered as wholes, and replace it with the selective realist idea that our best theories merely contain approximately true parts that are responsible for their success.

The most popular version of the first strategy is to focus on a particular kind of success, namely, novel predictive success.[1] The most straightforward cases of such success involve *temporal novelty*, where a prediction of a phenomenon is temporally novel if the scientists who constructed the theory that predicts that phenomenon did not know about that phenomenon at the time they constructed the theory. A good illustrative example is Einstein's prediction that light bends around massive objects.[2] There is also *use novelty*, where a prediction of a phenomenon is use novel, but not temporally novel, if scientists knew about the phenomenon but did not use it in the course of constructing the theory that predicts that phenomenon. The predictions regarding tidal phenomena that follow from Newton's theory are a good illustrative example. Realists have argued that novel predictive success is the kind of success that can justify the success-to-truth inference. As a result, the historical challenge to realism has tended to focus almost exclusively on cases in which it seems that theories that were not even approximately true exhibited novel predictive success.[3]

The second strategy is to abandon the traditional realist idea that our best theories are approximately true when considered as wholes in favor of the selective realist idea that our best theories merely contain approximately true parts.[4] Selective realists almost always combine this second strategy with the first strategy. They hypothesize that a theory's novel predictive success is due to the fact that it has parts that are at least approximately true. The basic idea is that not every part of a theory is used when deriving a successful novel prediction, and that the parts of the theory that contribute essentially to the derivation are at least approximately true. Novel predictive success justifies an inference, not to the claim that the theory is approximately true as a whole, but to the claim that it has approximately true parts. As a result, the historical challenge to realism has tended to focus on whether any not-even-approximately-true parts of theories have been responsible for any instances of novel predictive success.[5]

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[1] Musgrave (1988, p. 232), Worrall (1989, p. 114), and Psillos (1999, pp. 105-108), for example, have all attempted to defend a form of realism that focuses on novel predictive success.

[2] To be sure, Einstein was not the first to make this prediction. As Lyons (2016, 2017) points out, in 1801, Johann Georg von Soldner made the same prediction from a Newtonian perspective on the grounds that light is made of corpuscles, which have mass and are therefore subject to the force of gravity. That said, Einstein's prediction is still novel in the relevant sense because light bending was not observed until after he made his prediction. Lyons does not suggest otherwise and in fact says that Einstein's prediction "may well constitute *the* exemplary instance of novel predictive success" (2016, p. 98; Lyons' emphasis).

[3] The locus classicus is Laudan's (1981) list of theories that were successful but not even approximately true. However, Laudan's list includes a number of theories that may not have exhibited novel predictive success. Lyons (2002, 2006, 2016, 2017) has uncovered a significant number of cases of novel predictive success that are relevant to the debate. Vickers (2013) provides a useful list of relevant cases that have been identified in the literature on the realism debate (including many of Lyons' cases), as well as some cases that Vickers himself has uncovered. More recent work has uncovered additional cases—see, for example, Carman and Díez (2015), Tulodziecki (2017), Rossetter (2018), and the volume on the historical challenge to realism edited by Lyons and Vickers (2021).

[4] Selective realist positions have been developed by Kitcher (1993), Psillos (1999), Harker (2013), Vickers (2013), and Peters (2014), among others. Chakravartty's (1998, 2007) semirealism is a similar sort of position. The structural realist positions developed by, e.g., Worrall (1989), Ladyman (1998), Ladyman and Ross (2007), and French (2014) may also be considered forms of selective realism, though structural realists tend to reject this label (see, e.g., Ladyman, 2019, p. 1).

[5] With the exception of Laudan (1981), the work cited in footnote 3 focuses specifically on the historical challenge to selective realism.

So, to take stock, the realist's success-to-truth inference is now usually understood as an inference from a theory's novel predictive success to the claim that it contains approximately true parts which are responsible for its success; and the historical challenge to realism now focuses primarily on whether there are historical cases in which parts of theories that are not even approximately true are responsible for the novel predictive successes of those theories. While there is still a lot of important and exciting work being done along these lines, my aim in this paper is to consider another formulation of the success-to-truth inference and another sort of historical challenge to realism.

The success-to-truth inference that I will focus on is due to Harker (2013). While most selective realists focus on the novel predictive success of individual theories, Harker develops a selective realist position in terms of a comparative notion of success that requires examining the ways in which theories achieved progress over their rivals. According to Harker's comparative notion of success, a theory is more empirically successful than its rival if that theory predicts phenomena that are inexplicable or anomalous according to its rival. His form of selective realism is based on the idea that we should regard as approximately true those parts of theories that make them more successful than their rivals.

The historical challenge that I will focus on involves identifying historical counterexamples to Harker's selective realism. Harker clearly explains how his position ought to be assessed and what would count as a counterexample to his position. According to Harker's position, we should regard the parts of theories that are responsible for their comparative success as approximately true. Moreover, Harker's position predicts that the parts of theories that are responsible for their comparative success will be retained in successor theories. Counterexamples to his position are cases in which those parts are not retained in successor theories but rather rejected. Harker poses a challenge to antirealists who doubt his position, which is to identify enough counterexamples to undermine his position. Although Harker's position is often held up as one of the main examples of selective realism,[6] and although a number of philosophers have made some brief critical remarks about his selective realist position,[7] as far as I know, no one has attempted to meet Harker's challenge. The remainder of this paper is devoted to taking up this challenge in full.

In order to identify enough counterexamples to meet Harker's challenge, I draw upon Frost-Arnold's (2019) recent work on what he calls the Problem of Misleading Evidence. The problem is that past scientists were often faced with a total body of evidence that misleadingly confirmed theories that, by current lights, we cannot regard as even approximately true. Frost-Arnold presents a number of historical cases in order to illustrate the problem. These cases all involve comparing the misleadingly confirmed theories with rival theories, and so they are especially relevant when it comes to evaluating Harker's position. I argue that four of Frost-Arnold's cases are counterexamples to Harker's position.

The remainder of the paper is organized as follows. In section 2, I present Harker's position in more detail. In section 3, I present four of the historical cases that Frost-Arnold uses to illustrate the Problem of Misleading Evidence, and I argue that these four cases are counterexamples to Harker's position. In section 4, I argue that these counterexamples provide us with a strong reason to doubt Harker's position and the kind of success-to-truth inference that he defends. The main upshot of the argument in this paper is that the sort of comparative success and empirical progress

[6] See, for example, Lyons (2017, pp. 3215-3216), Niiniluoto (2017, p. 3306), Tulodziecki (2017, p. 1000), Vickers (2018, p. 57, fn. 4), and Dellsén (2019, p. 32).
[7] See, for example, Egg (2016, pp. 119-120), Onishi (2017, p. 4), Vickers (2017, p. 3224), Rossetter (2018, p. 8), and Saatsi (2019, p. 620, fn. 18).

that Harker focuses on does not provide a sufficient reason for concluding that the parts of theories responsible for that sort of success and progress are even approximately true.

## 2. Harker's selective realism

Harker (2013) has developed a selective realist position that differs from other selective realist positions in a number of ways. The most significant difference is that, while most selective realists focus on the novel predictive success of individual theories, Harker develops his position in terms of a comparative notion of success that focuses on the ways in which theories achieved progress over their rivals and predecessors. In this section, I first introduce Harker's notions of success and progress (section 2.1). I then show how he develops a selective realist position based on these notions (section 2.2). After that, I explain how, according to Harker, we ought to assess his position with respect to the historical record (section 2.3). Finally, I summarize Harker's main reasons for thinking that his selective realist position is preferable to other forms of selective realism, and I suggest that his position is therefore worth serious consideration (section 2.4).

### 2.1 Comparative success and empirical progress

Harker illustrates his notions of success and progress in terms of an example which centers on Rutherford's model of the atom. On Rutherford's model, the atom has a small, massive, positively-charged nucleus at its center. This nucleus is surrounded by a much larger sphere of negative charge that is distributed uniformly throughout the sphere. In other words, Rutherford did not assign any specificity to electron orbits. Earlier models of the atom could not accommodate the results of Geiger and Marsden's alpha particle scattering experiments. While most alpha particles pass through a very thin sheet of gold foil, some are deflected at angles much greater than what previous models of the atom (for example, Thomson's) predict. Rutherford's model accommodated these results because, on his model, an atom is mostly empty space, with the exception of the small, massive nucleus at the center which is capable of deflecting the alpha particles. Rutherford's model was therefore more successful than other models proposed around the same time. As Harker puts it, "Rutherford's model is regarded as *successful* principally because it could account for what rival models could not: a kind of success which foremost suggests *progress*" (2013, p. 89; Harker's emphasis).

More generally, Harker conceives of success in terms of empirical progress, and he understands both success and progress comparatively. On Harker's notion of success, a theory is more successful than a rival theory if it achieves progress over its rival. According to Harker, "theories achieve progress by predicting phenomena that are otherwise either inexplicable or anomalous" (2013, p. 90).[8] A theory therefore achieves progress over its rival if the theory successfully predicts phenomena that, from the standpoint of the rival, are anomalous or inexplicable. For Harker, relevant cases of such success include cases in which we compare a theory with its rivals, its predecessors, or previous versions of the very same theory (2013, p. 91). For ease of exposition, in what follows, I use the term 'rival' to apply, not just to competing

[8] Regarding this definition of progress, Harker (2013, p. 90) writes: "I'll suppose [this definition] for the sake of this article … I don't pretend the definition is unproblematic; nevertheless, my primary intention is to investigate the prospects of employing a comparative conception of success within the realist's argument, rather than to defend a particular model of progress." In order to evaluate Harker's position, we need to understand what this comparative conception of success is. In this paper, I will evaluate Harker's position in terms of the account of comparative success that he offers, which depends on the definition of progress that he supposes. I leave it to defenders of Harker's position to show that some alternative understanding of comparative success and progress can justify Harker's success-to-truth inference and counter the argument I offer in this paper.

theories, but also to predecessors and previous versions of the same theory. Harker emphasizes that a theory can achieve progress over its rivals only in virtue of the differences between the theory and its rivals. As he puts it, "[e]mpirical progress is never a product of shared theoretical commitments" (2013, p. 91). Moreover, as he points out, not all such differences are responsible for the progress that a theory achieves over its rivals, and it can take some work to distinguish the differences that are responsible from those that are not.

Importantly, Harker's comparative notion of success differs from novel predictive success, which, as discussed in section 1, is the sort of success that philosophers engaged in the realism debate tend to focus on. Harker's notion requires comparing theories with their rivals. In contrast, we can attribute novel predictive success to individual theories without considering their rivals. That said, if there are relevant rivals, then as soon as we bring them into the picture, we can see such cases of novel predictive success as cases in which the theory in question thereby made empirical progress over its rivals. Moreover, one of Harker's central examples of a theory that exhibited his comparative notion of success is also a commonly cited example of a theory that exhibited novel predictive success. Harker (2013, pp. 80-81, 99-101) claims that Priestley's phlogiston theory was more successful than the rival oxygen theory on the grounds that it successfully predicted that heating a calx in hydrogen would transform the calx into a metal. This particular prediction is frequently cited as an example of a successful novel prediction (Lyons, 2002, p. 70; Ladyman, 2011, p. 99; Vickers, 2013, p. 191). However, even if all instances of novel predictive success are also instances of comparative success once we bring the relevant rivals into the picture, not all instances of comparative success are instances of novel predictive success. [9] For example, the success that Rutherford's model exhibited was not novel predictive success since Rutherford used the results of Geiger and Marsden's alpha particle scattering experiments in the course of constructing his model of the atom (Harker, 2013, pp. 80, 82). More generally, the predictions that a theory must make in order to make progress over a rival theory needn't be novel predictions.

### 2.2 Selective realism

According to Harker, this comparative notion of success is a better foundation for a selective realist position than the notion of novel predictive success. Harker (2013, p. 82) states that historical challenges to selective realism "appear persuasive insofar as they are directed towards extant versions of the selective strategy." He cites Chang (2003), Lyons (2006), and Stanford (2006), all of whom provide arguments against Psillos's (1999) form of selective realism, which is based on novel predictive success. In short, Harker's view is that the focus on novel predictive success has failed to secure a tenable form of selective realism, and he proposes that his comparative notion of success is more promising in this regard. As he puts it, "[e]mpirical, scientific progress might prove

[9] A reviewer has noted that there is an issue here regarding whether there are historical cases in which a theory exhibits novel predictive success without thereby making progress over a rival theory. If there are such cases of non-comparative novel predictive success, then it's possible that such a case could be a counterexample to the standard versions of selective realism discussed in section 1, but not to Harker's position. The reviewer states that this possibility may make Harker's position conceptually weaker than other forms of selective realism in the sense that Harker's focus on comparative success narrows the range of relevant cases, as well as the range of potential counterexamples. As a result, Harker's position may have less content than other forms of selective realism. In response, I would say that, insofar as Harker's position expands the range of relevant cases to include cases of comparative, non-novel predictive success, perhaps there is an increase in conceptual strength here that, to some extent, makes up for the conceptual weakness that the reviewer notes. Since it is not my aim to defend Harker's position, a complete response to this objection falls outside of the scope of this paper.

more reliably indicative of increased truthlikeness, in light of historical considerations, than success (understood non-comparatively) has proved a reliable indicator of approximate truth" (2013, p. 90). I summarize the main reasons why Harker thinks this may be the case in section 2.4, and I believe that his position is worth taking seriously. Moreover, given that, in recent years, the realism debate has focused primarily on non-comparative conceptions of success, Harker's position has not received the attention that I believe it deserves. That said, the main upshot of my argument in this paper is that Harker's selective realism is no more tenable or promising than the extant versions based on novel predictive success. In order to make this argument, it will be necessary to go into a bit more detail regarding Harker's position.

Harker's focus on increased truthlikeness concerns one of the main elements of his position, which is the convergent realist element (2013, p. 93). Harker argues that theories that are more successful than their rivals (in the sense that they achieve progress over their rivals) are more truthlike than their rivals. Over time, lineages of theories converge on the truth. Importantly, a theory can be more truthlike than its rivals even if that theory, as a whole, is not even approximately true.

The other main element of Harker's position is the selective realist element (2013, p. 92). Harker argues that, when a theory achieves progress over its rivals, it is not generally the case that all parts of the theory are responsible for the progress achieved. The selective realist element of Harker's position amounts to identifying the parts of theories in virtue of which they achieve progress over their rivals, and singling out those parts as deserving of realist commitment. As Harker puts it:

> the empirically based reasons for preferring a replacement theory (or a revised version of an existing theory) serve both as evidence of empirical progress and justification for endorsing the new theoretical posits that are responsible for the perceived progress. (2013, p. 92)

In other words, those new theoretical posits are the parts of theories that are at least approximately true, and that we would expect to be preserved when the theory is modified or replaced by another theory. Realist commitment does not extend to parts of theories that have not managed to contribute to empirical progress.

It's worth emphasizing that the convergent realist and selective realist elements of Harker's position are closely intertwined. According to Harker, theories that achieve progress over their rivals are more truthlike than their rivals because they contain approximately true parts that their rivals lack. Harker's point is that it is in virtue of these approximately true parts that a theory is able to achieve progress over its rivals. And a theory can have approximately true parts, and be more truthlike than its rivals in virtue of having those parts, without the theory as a whole being even approximately true.

Harker illustrates the main ideas of his selective realist position in terms of his central example of Rutherford's model of the atom. It was primarily the claim that an atom has a small, massive nucleus that allowed the model to accommodate the alpha particle scattering results. The claim that the outer sphere has a uniform charge distribution did not contribute to the model's ability to accommodate these results. Indeed, Harker considers this aspect of Rutherford's model to be a "significant theoretical error" (2013, p. 81) and notes that it resulted in deficiencies in the model that were later addressed in terms of electron orbitals. Moreover, Harker (2013, p. 91) notes that Rutherford himself considered the uniform charge distribution to be an idealization that was not supported by any empirical data. So realist commitment extends to the small, massive nucleus. That is the part of Rutherford's model that is approximately true, and in virtue of which it was more

truthlike than its rivals. Realist commitment does not extend to the uniform charge distribution since that part of the model did not contribute to the success of the model.

### 2.3 Testing Harker's selective realism

Harker clearly explains how his position ought to be assessed. As he observes, realists have typically attempted to justify the inference from the success of a theory to its approximate truth on abductive grounds in terms of the idea that the approximate truth of (parts of) a theory provides the best explanation of its success. However, Harker abandons this explanatory justification and offers a predictive justification for his version of the success-to-truth inference. The basic idea is that Harker's position entails predictions about what we ought to find when we look at cases of theory change from the history of science, and that insofar as these predictions are successful, they count as evidence for his position. More specifically, Harker presents the following two realist hypotheses and the prediction that follows from them (2013, p. 95):

> Harker's hypothesis: "Those parts of theories that generate comparative success are approximately true."
>
> Harker's auxiliary hypothesis: "Approximately true insights will be preserved across subsequent instances of theory change."
>
> Harker's prediction: "[T]he insights responsible for comparative success will have been retained within our own scientific theories."

Harker claims that, insofar as the prediction is successful, it provides evidence for the claim that I've labeled 'Harker's hypothesis.' As Harker (2013, p. 93) puts it, "[i]f the theoretical claims which generate empirical progress are retained at least approximately within subsequent theories, then a realist's inference from success to the parts that induce such success survives historical scrutiny."[10] In contrast, cases in which those theoretical claims are not at least approximately retained are counterexamples to Harker's position.[11] The idea, then, is to test Harker's prediction

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[10] Strictly speaking, the inference is not from success to parts of theories, but rather from success to the approximate truth of parts of theories. Harker is clear about this elsewhere, as when he writes: "The pattern of realist endorsement proposed thus follows the selective realist directive of inferring only the approximate truth of parts of theories" (2013, p. 93).

[11] A reviewer has suggested that Harker's position makes historical counterexamples less conceptually relevant than they are for abductive defenses of selective realism. Abductive defenses of selective realism imply retention. If the best explanation of an instance of novel predictive success is that the theoretical components responsible for that success are approximately true, then those components must be at least approximately retained in subsequent theories. In contrast, Harker's main realist hypothesis does not imply retention on its own; he needs to conjoin it with an auxiliary hypothesis in order to predict instances of retention of the parts of theories responsible for comparative success. In response, as I understand Harker's position, the main realist hypothesis and the auxiliary hypothesis are both essential parts of his position. In that case, counterexamples are not necessarily less conceptually relevant than they are for abductive defenses of selective realism. In fact, Harker would presumably maintain that historical cases are more conceptually relevant because, on his position "historical evidence plays a more direct role in defending the selective thesis" (2013, p. 95). This is because successful predictions regarding the historical record, rather than an inference to the best explanation, are supposed to justify the success-to-truth inference. However, the important point for my purposes is to evaluate Harker's position in the way that he outlines in his paper. Offering a complete response to this objection therefore goes beyond the scope of this paper.

against the historical record and determine the extent to which historical cases support or undermine his position.

2.4 Harker's reasons for preferring his form of selective realism

Harker argues that his form of selective realism is preferable to other forms of selective realism for four main reasons. The first reason concerns Harker's rejection of an explanatory justification of the success-to-truth inference in favor of a predictive justification (2013, pp. 93-96). As Harker notes, antirealists typically reject inference to the best explanation, and so the sort of explanatory justification that most selective realists offer is bound to fail when it comes to persuading their antirealist opponents. As discussed in section 2.3, Harker's realist hypothesis, when conjoined with his auxiliary hypothesis, yields predictions and enables him to attempt to justify the success-to-truth inference by means of successful predictions. He can thus avoid the standard explanatory justification of the success-to-truth inference. Although my ultimate goal is to argue that Harker's position makes the wrong predictions in a number of cases, I do think that his rejection of an explanatory justification in favor of a predictive one is a step in the right direction. Harker notes that both realists and antirealists evaluate theories in terms of whether they make correct predictions. As Harker (2013, p. 95) puts it:

> If the selective realist can predict patterns of accumulation and preservation within the history of science that the antirealist cannot, then we have reasons to endorse selective realism relative to standards of acceptance that even the antirealist seems obliged to accept.

Even antirealists who reject inference to the best explanation can embrace Harker's proposal for testing his form of selective realism.

Harker's second reason concerns the convergent realist element of his position (2013, p. 93). If Harker is correct, then what we should find over time is that, as there is more and more empirical progress, more and more approximately true posits are introduced and subsequently retained in our theories. Hence, we have increased truthlikeness, and lineages of theories converge on the truth, even if we cannot conclude that our current best theories are approximately true when considered as wholes. This picture of the development of science as converging on the truth follows directly from Harker's focus on comparative success and empirical progress. As Harker points out, selective realists who focus on non-comparative forms of success (e.g., the novel predictive success of individual theories) must develop an independent argument for convergence.

The third reason concerns four criteria that, according to Harker, any form of selective realism should satisfy (2013, pp. 82-88, 96-98). They can be summarized as follows:

> (1) The Discrimination Criterion: Selective realists need a general criterion, applicable to any theory, that can be used to distinguish different constituents of a theory. Examples include Kitcher's (1993, p. 149) distinction between working and presuppositional posits, and structural realists' distinction between structure and non-structure.
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> (2) The Differential Confirmation Criterion: Selective realists need to "provide a differential confirmatory relation that justifies a realist attitude towards only that class of theoretical constituents that is identified for realist approval" (Harker, 2013, p. 84), e.g., working posits (for Kitcher) or the structural content of the theory (for structural realists). Selective realists typically try to do this by showing that these are the constituents responsible for the success of the theory.

(3) The Non-Whiggish History Criterion: When attempting to identify the constituents responsible for success, selective realists must not make improper use of current scientific knowledge. The worry here is that it would be question begging to assume that only those parts of past theories that resemble current theories were responsible for the success of past theories.

(4) The Epistemic Accessibility Criterion: When we use a general criterion to distinguish the constituents of a theory that deserve realist approval from those that do not, it must be possible to know that particular constituents satisfy that criterion. Harker's example of a failure of epistemic accessibility is Psillos' (1999, p. 105) criterion for determining whether a theoretical constituent deserves credit for generating a successful prediction. Lyons (2006, pp. 542-543) argues that Psillos' criterion is inapplicable. In that case, we could never know whether a particular theoretical constituent is responsible for a theory's success.

In short, Harker argues that his position does a better job satisfying these criteria than other selective realist positions. More specifically, Harker (2013, p. 97) claims that "[c]onceiving of success in comparative terms thus opens space for a selective realist thesis that satisfies my four criteria." In order to get a sense of what Harker has in mind, I'll briefly summarize what he says regarding two of these criteria, namely, (2) the Differential Confirmation Criterion and (3) the Non-Whiggish History Criterion.

Regarding the Differential Confirmation Criterion, Harker (2013, p. 96) argues that his focus on comparative success enables him to satisfy this criterion. The progress that a theory makes over its rivals is evidence for the new theoretical posits responsible for that progress; it is not evidence for any posits that the theory shares with its rivals, nor for any new posits that are not responsible for progress. Moreover, Harker (2013, pp. 84-85, 97) argues that his position thereby fares better than other positions. His main example is Worrall's (1989) structural realism, and he draws on Psillos' (1999, pp. 145-155) critique of this position in order to make the point. The basic idea is that, even if Worrall is able to demonstrate structural continuity over theory change, doing so does not show that predictive success differentially confirms a theory's structural (rather than non-structural) content. In contrast, a differential confirmatory relation follows rather straightforwardly from Harker's comparative notion of success.

Regarding the Non-Whiggish History Criterion, once again, Harker (2013, p. 97) argues that his focus on comparative success enables him to satisfy this criterion. His basic idea is that we can evaluate the success of a particular theory from the standpoint of older theories. In order to identify the particular posits that are responsible for the success of the theory over its rivals, we focus on the ways in which that theory differs from its rivals. For example, we can evaluate the success of Rutherford's model from the perspective of Thomson's model. Harker concludes that his position can avoid inappropriate use of current scientific knowledge. In contrast, the temptation to make use of current scientific knowledge to identify theoretical posits responsible for success is greater for selective realists who employ a non-comparative notion of success.

Finally, Harker's fourth reason for preferring his position to other selective realist positions is its ability to accommodate the historical record (2013, pp. 98-103). Harker provides some brief, suggestive sketches of how his position can deal with Fresnel's optical aether theory, Priestley's phlogiston theory (which I discuss in more detail in section 3.1), and Darwin's, Galton's and Weissmann's theories of generation and inheritance. He also makes some very brief comments regarding developments in evolutionary biology and chemistry, as well as the history of the electron.

Harker (2013, p. 93) states that it is the satisfaction of the four criteria and the historical record that will ultimately provide the strongest support for his position, and he considers his points about convergence and the predictive justification of the success-to-truth inference as two further advantages. My own view is that there is much to debate regarding the point about convergence as well as Harker's claim that his position satisfies the four criteria better than other selective realist positions. That said, I won't debate these issues here, and I believe that Harker has, at the very least, succeeded in making a prima facie case that his position deserves serious consideration. Moreover, I think it's commendable that he proposes to test his position via predictions regarding the historical record, and to thereby adopt a form of argument that has a better chance of persuading antirealists than an inference to the best explanation. However, his brief sketches of the historical cases, while suggestive, are not entirely conclusive, and I'll now turn to arguing that the historical record does not, in fact, support Harker's position.

## 3. Counterexamples to Harker's selective realism

In order to identify some counterexamples to Harker's position, I begin by discussing a case that Harker (2013, p. 101) himself labels a "genuine counterexample" to his position, which centers on Priestley's phlogiston theory (section 3.1). I then turn my focus to a novel historical challenge to realism that Frost-Arnold (2019) has recently formulated within the context of what he calls "the Problem of Misleading Evidence." I explain why this historical challenge is a promising source of additional counterexamples to Harker's position (section 3.2). Finally, I show that four of the cases that Frost-Arnold uses to illustrate the Problem of Misleading Evidence resemble Harker's phlogiston case in the relevant respects, and are therefore counterexamples to Harker's position (section 3.3).

### 3.1 Harker's counterexample

Harker clearly indicates what sort of case would count as a counterexample to his position and identifies one such counterexample. This counterexample centers on Priestley's phlogiston theory (Harker, 2013, pp. 80-81, 99-101). In the early 1780s, Priestley's theory successfully predicted that heating a calx in hydrogen, which Priestley identified as pure phlogiston, would transform the calx into a metal. The rival oxygen theory did not entail this prediction, and did not offer a compelling explanation of this phenomenon until a few years later. When it was noticed that the quantity of water increases in this experiment, the oxygen theory could explain that this water was the result of hydrogen combining with the oxygen in the calx/oxide. That said, Priestley's theory was, at least in the early 1780s, more successful. Harker (2013, p. 101) labels this case a "genuine counterexample" on the grounds that "empirical progress was achieved in a manner that relied on a theoretical assumption that can no longer be regarded as approximately true." More specifically, Harker (2013, p. 101) claims that "the commitment to combustion as a process of emission," which is the not-even-approximately-true assumption of the phlogiston theory, "led to progress that was not achievable on the assumption that combustion involves absorption," which is the assumption of the rival oxygen theory. And since the commitment to combustion as a process of emission has not been retained within our own scientific theories, we have a counterexample to Harker's position. However, Harker clearly states that a single counterexample doesn't pose a serious threat to his position. In his view, so long as his position can accommodate most historical cases, a single counterexample doesn't pose a problem. Harker closes his discussion of this counterexample by posing a challenge: "Antirealists who doubt my proposal on historical grounds are challenged to provide further examples that resemble Priestley's" (2013, p. 101).

Since my goal is to take up this challenge and identify some further counterexamples, it's important to be clear about what sort of case, in general, qualifies as a counterexample to Harker's position. Recall that Harker understands his comparative conception of success in terms of the way

in which theories make progress over their rivals. Moreover, only the differences between a theory and its rivals can be responsible for the progress that a theory makes over its rivals. The differences that matter are what Harker calls "the new theoretical posits that are responsible for the perceived progress" (2013, p. 92). The idea is that these new theoretical posits are the changes that scientists make to existing theories in order to predict otherwise inexplicable or anomalous phenomena. In general, cases in which new theoretical posits exhibit this sort of success but are not retained within our current theories are counterexamples to Harker's position.

However, there may seem to be a conflict between this understanding of counterexamples as involving new theoretical posits and Harker's claim that the Priestley case is a genuine counterexample to his position. After all, the phlogiston theorists' commitment to combustion as a process of emission was an older posit than the oxygen theorists' commitment to combustion as a process of absorption. Perhaps, then, we have a form of comparative success; but since we don't have a new theoretical posit, we don't have the sort of empirical progress that Harker thinks is indicative of increased truthlikeness.

I think that this apparent conflict can be resolved in the following way. While it's true that Harker often claims that it is new theoretical posits that are responsible for empirical progress, he also frequently puts the same point in terms of changes: "Theories cannot achieve empirical progress without recommending *changes* to existing theories" (2013, p. 91; Harker's emphasis). Such changes may involve the introduction of new posits or theories. But as the Priestley case makes clear, changes that induce progress may also involve abandoning a newer posit (e.g., combustion as a process of absorption) in favor of an older one (e.g., combustion as a process of emission). In light of the Priestley case, we ought to understand what counts as a change, and what counts as new, in a broad sense that includes returning to older posits and theories as well as introducing new ones.

To be sure, Harker could have attempted to dismiss the Priestley case on the grounds that it does not involve the introduction of a new posit. However, he did not, and I believe that this is the correct move because doing so would have at least two drawbacks. First, Harker would lose access to cases that could potentially support his position. For example, since versions of the kinetic theory of heat predate the caloric theory of heat,[12] Harker may not be able to appeal to progress that the kinetic theory made over the caloric theory in the nineteenth century because the relevant theoretical posits may not be new. Second, one might think that Harker arbitrarily excludes empirical progress that results from abandoning newer posits in favor of older ones. After all, it is not clear why the novelty of a posit has epistemic significance for the sort of success-to-truth inference that Harker advocates. It seems that what really matters is the sort of success that Harker focuses on rather than whether the posit that generates such success is new.

In what follows, I'll assess Harker's position by focusing on the theoretical posits, whether old or new, that were responsible for any comparative success and empirical progress that a theory made over its rivals. Cases in which such posits have been retained in our current theories provide support for Harker's position, while cases in which such posits have not been retained in our current theories are counterexamples to Harker's position.

### 3.2 Frost-Arnold's Problem of Misleading Evidence

In order to identify some additional counterexamples to Harker's position, I'll draw on Frost-Arnold's Problem of Misleading Evidence (PME). In short, the PME is that "[t]he total body of evidence used by scientists at a particular time was often unrepresentative or otherwise misleading"

[12] See Frost-Arnold (2019, p. 914). I discuss this case in more detail in section 3.3.

(2019, p. 910). What, exactly, is misleading evidence? Frost-Arnold presents the following gloss of misleading evidence: "evidence *E* (incrementally) confirms hypothesis *H*, but *H* is not (even approximately) true" (2019, pp. 910-911). He does not present this gloss as a definition, and he acknowledges that he does not have a definition of misleading evidence. He cites Fallis and Lewis (2014) in support of the claim that "this simple and intuitive gloss of misleadingness enjoys wide acceptance in the philosophical literature but faces problems in certain cases" (Frost-Arnold, 2019, p. 911, fn. 4). And he goes on to emphasize that "all that is necessary for present purposes is that the historical examples [that he describes] are actually cases of misleading evidence" (2019, p. 911, fn. 4).

Based on the PME, Frost-Arnold proposes the following "Misleading Evidence Induction" (2019, p. 912):

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

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

This inductive argument is Frost-Arnold's historical challenge to the realist. If we have good reason to believe that our current total evidence sets are misleading, then we don't have good reason to believe that our current best theories are even approximately true. My argument does not depend on the inference from the premise to the conclusion, and so I needn't join Frost-Arnold in defending the Misleading Evidence Induction. For the purposes of my argument, what really matter are the historical cases that Frost-Arnold uses to motivate the premise of this inductive argument, which is just the PME.

Frost-Arnold presents the PME as an explanation of why, in at least some cases, scientists endorsed theories that, by present lights, are not even approximately true. The alternative explanation that Frost-Arnold considers is Stanford's (2006) Problem of Unconceived Alternatives (PUA), which Frost-Arnold (2019, p. 909) formulates as follows: "Scientists are unable (or at least unlikely) to conceive of all respectable theoretical explanations for the available data." According to the PUA, scientists endorsed theories that are, by present lights, not even approximately true because they failed to conceive of all of the alternatives to that theory. Frost-Arnold admits that the PUA may provide the best explanation of some historical cases. That said, he argues that the PUA cannot explain cases in which scientists conceived of the rival theories that approximate our current theories and nonetheless endorsed theories that, by present lights, are not even approximately true. In contrast, the PME can explain such cases on the grounds that the total evidence misleadingly confirmed the not-even-approximately-true theory over its (by present lights) approximately true rival.

Some of the cases that Frost-Arnold uses the PME to explain are worth considering as potential counterexamples to Harker's position. The relevant cases are cases in which scientists endorse a not-even-approximately-true theory over a (by present lights) approximately true rival that the scientists in question explicitly considered. Recall that the sorts of cases that are counterexamples to Harker's position are cases in which a theory makes empirical progress over its rival in virtue of a theoretical posit that is, by present lights, not even approximately true. Suppose that, in a particular case, the evidence misleadingly confirms a theoretical posit that is not even approximately true and, moreover, does so in virtue of the empirical progress that the posit generates with respect to a rival theory. Such a case would both illustrate the PME and qualify as a

counterexample that threatens Harker's position. As a result, the cases that Frost-Arnold discusses are a good source of potential counterexamples to Harker's position.[13]

That said, some care needs to be taken since it is unclear whether counterexamples to Harker's position are, in general, coextensive with cases in which the total evidence misleadingly confirms a not-even-approximately-true theory over its, by present lights, approximately true rival. In order to show that they are coextensive, we would, at the very least, need a definition of misleading evidence, and Frost-Arnold does not provide one. We would also need a conception of confirmation to use for understanding how Harker's position is supposed to satisfy the Differential Confirmation Criterion in terms of the idea that empirical progress confirms only the parts of theories that generate such progress (Harker, 2013, pp. 84, 96-97). However, Harker himself doesn't commit to a particular conception of confirmation. Moreover, whatever conception of confirmation we use to understand Harker's Differential Confirmation Criterion would have to be the same conception we use to understand the notion of confirmation in the definition of misleading evidence. In other words, we would have to select a conception of confirmation that yields an adequate understanding of both misleading evidence and of the Differential Confirmation Criterion. Perhaps this can be done, but doing so would require addressing a number of controversial issues regarding the nature of evidence and confirmation.

In what follows, I'll avoid this issue. I'll remain noncommittal about whether the two sorts of cases are, in general, coextensive. Instead, I'll argue that four of the cases that Frost-Arnold uses to illustrate the PME are cases in which a theory makes empirical progress over its rival in virtue of a theoretical posit that has not been retained in our current theories. In other words, these four cases are counterexamples to Harker's position.

Before turning to these four cases, I want to emphasize that Frost-Arnold's cases are not the only source of potential counterexamples to Harker's position. The lists of successful-but-not-even-approximately-true theories that, e.g., Lyons (2002) and Vickers (2013) have compiled are also important sources of potential counterexamples. But since Lyons and Vickers tend to focus on the non-comparative success of individual theories, it would take more work to identify relevant rivals and demonstrate empirical progress and comparative success in these cases. If this can be done and we uncover additional counterexamples to Harker's position, that would, of course, add to my case against his position. I've chosen to focus on Frost-Arnold's cases because the scientists in these cases conceived of the (by present lights) approximately true rival theories and yet preferred their own not-even-approximately-true theories due to misleading evidence. Hence, these cases are explicitly comparative, and demonstrating empirical progress and comparative success is therefore more straightforward.

### 3.3 Four additional counterexamples

Frost-Arnold discusses a number of historical cases. His main goal is to use these cases to illustrate and motivate the PME. He does not use them to raise problems for selective realists in particular. But, as I'll now show, four of his cases are counterexamples to Harker's form of selective realism.

One of Frost-Arnold's cases centers on Ptolemy's geostatic model of the universe (2019, p. 911). Ptolemy knew that, if the earth orbits the sun, then we should be able to observe stellar parallax. However, ancient astronomers did not observe stellar parallax—in fact, astronomers did not observe it until the nineteenth century. Ancient astronomers considered two possible reasons why they did not observe stellar parallax. As Frost-Arnold puts it, "either the earth is not going

[13] In contrast, Stanford's PUA is less relevant for assessing Harker's position since cases in which the alternative is unconceived lack the sort of comparative success on which Harker focuses.

around the sun or twice the distance from the earth to the sun is as nothing with respect to the size of the cosmos" (2019, p. 911). Frost-Arnold maintains that it was reasonable for ancient astronomers to reject the second possible reason based on the evidence that was available at the time, in which case the absence of stellar parallax is misleading evidence for Ptolemy's geostatic model.

The Ptolemy case is a counterexample to Harker's position. The rival theory in this case is the geokinetic model which, as Frost-Arnold (2019, p. 911) points out, Ptolemy considered. The misleading evidence includes the fact that stellar parallax had not been observed. This fact is an anomaly for the geokinetic model since, on that model, stellar parallax should either be observable or the stars are significantly further away from the earth than ancient astronomers had reason to think. The geostatic model predicts the fact that stellar parallax is not observable. So the geostatic model was more empirically successful than the geokinetic model. The part of the geostatic model in virtue of which it predicts this fact is the part that claims that the earth is stationary. But that part of the geostatic model is not even approximately true and it is not retained in our current theories.

A second case that Frost-Arnold discusses concerns theories of spontaneous generation (2019, pp. 911-912). In the mid-eighteenth century, John Needham and Comte de Buffon performed a number of experiments in which they sealed containers of gravy, boiled them (so as to kill anything that might be living inside), and then examined the gravy under a microscope. Needham and Buffon observed small moving particles, which they interpreted to be living creatures. Lazzaro Spallanzani attempted to replicate their results but failed to observe the small moving particles that Needham and Buffon observed. Another scientist at the time, namely, von Gleichen-Russworm, tried and failed to replicate Spallanzani's results with a microscope that was better than Spallanzani's, and as a result agreed with Needham and Buffon regarding the dispute. In fact, Needham's and Buffon's microscopes were superior to Spallanzani's—unlike Spallanzani's microscope, Needham's and Buffon's microscopes could detect bacteria in Brownian motion, which is most likely what they observed. Although Needham and Buffon made more accurate observations, their observations were misleading evidence for spontaneous generation.

This spontaneous generation case is also a counterexample to Harker's position. In this example, the theory is that living things spontaneously generate from matter, and the rival theory, which Needham and Buffon considered (Frost-Arnold, 2019, p. 914), denies spontaneous generation. The misleading evidence includes Needham's and Buffon's observations of small particles in motion. Since Brownian motion was not well understood until the early twentieth century, it was reasonable for Needham and Buffon to interpret this motion as a sign of life. Such signs of life are anomalies if one denies that living things spontaneously generate from matter; but they are exactly the sort of thing that the theory of spontaneous generation predicts. Hence, the theory of spontaneous generation was more empirically successful than its rival. But once again, the part of that theory in virtue of which it predicts this fact, namely, the posit that living things spontaneously generate from matter, is not even approximately true and it is not retained in our current theories.

A third case that Frost-Arnold (2019, pp. 912-914) discusses is the example of the caloric theory of heat and its rival, the kinetic theory. Frost-Arnold points out that Daniel Bernoulli formulated what was more or less the modern version of the kinetic theory in 1738. Caloric theorists in the late eighteenth and early nineteenth centuries considered the kinetic theory but rejected it in favor of the caloric theory due to misleading evidence. The misleading evidence in this case includes the fact that heat can be transmitted through empty space (for example, from the sun to the earth). Empty space contains no molecules in motion, and so this fact is an anomaly for the kinetic theory. But if heat is a substance (caloric), then heat can travel through (otherwise) empty space. So the caloric theory was more successful in virtue of the claim that heat is a substance rather

than a form of motion, which is a posit that is not even approximately true and is not retained in our current theories. Hence, the caloric theory of heat is another counterexample to Harker's position.

Fourth, Frost-Arnold (2019, pp. 913-914) briefly discusses the evidential situation regarding Wegener's continental drift theory. Historians generally agree that the evidence available in the early twentieth century favored the rival theory that did not attribute motion to the continents. Evidence that the continents do not move was misleading, and Wegener's opponents therefore rejected his theory because of misleading evidence. This is another plausible case in which the rival theory was more successful in virtue of a mistaken theoretical posit, namely, that the continents are stationary. In that case, this example regarding Wegener's continental drift theory is another counterexample to Harker's position.[14]

Recall that the challenge that Harker (2013, p. 101) poses to antirealists is to find additional historical cases that are similar to the Priestley case. In the Priestley case, empirical progress was due to the posit that combustion is a process of emission. More generally, in order to qualify as counterexamples to Harker's position, such cases must involve theoretical posits that are not retained in our current theories, and that nonetheless led to empirical progress. At this point, it should be clear that the four cases that I've taken from Frost-Arnold's discussion of the PME all involve such posits: the posit that the earth is stationary, the posit of spontaneous generation, the posit that heat is a substance, and the posit that the continents are stationary. Hence, we have four cases that resemble the Priestley case in the relevant respects, and that therefore qualify as counterexamples to Harker's position.

It's worth pointing out a sense in which these counterexamples are stronger than Harker's own counterexample regarding the Priestley case. While Harker admits this case as a genuine counterexample, he also identifies a sense in which it is somewhat weak. Within about two years, chemists noticed that the quantity of water increases during the experiment, and Lavoisier proposed an explanation of the increase in water that was more compelling than Priestley's. Harker (2013, p. 101, fn. 24) concludes that "[p]hlogiston theory thus very quickly lost the one empirical edge it had held over absorption-based theories of combustion." He proposes "that if historical examples are to provide any *sincere* opposition to selective realism, the interpretation of such results should persist within a scientific community for longer" (2013, p. 101, fn. 24; Harker's emphasis).

It is therefore significant that the theories in Frost-Arnold's cases maintained their empirical edge for longer periods of time. Evidence for the geokinetic model and against the geostatic model (e.g., observations of stellar parallax in 1838 and of the phases of Venus in 1610) was not available until centuries after Ptolemy proposed his model (Frost-Arnold, 2019, p. 912). The controversy over spontaneous generation involving Spallanzani, Needham, and Buffon lasted from 1765 to 1780 (Frost-Arnold, 2019, p. 911). It took much of the first half of the nineteenth century for scientists to settle on the kinetic theory of heat and reject the caloric theory (Frost-Arnold, 2019, pp. 912-914). Finally, according to Šešelja and Weber (2012, p. 148), philosophers and historians of science have

[14] Frost-Arnold (2019, p. 913) also discusses three other cases: the Galilean transformations, classical mechanics, and the existence of superconducting materials. I've set these cases aside because I think it's more difficult to show that they are counterexamples to Harker's selective realism. One might argue that the Galilean transformations and classical mechanics are approximately true and are therefore neither misleading evidence cases nor counterexamples to Harker's position. And one might argue that, even if the superconducting materials case is a misleading evidence case, the issue concerning superconducting materials is an issue regarding empirical regularities rather than theories, in which case it is not relevant to assessing Harker's position.

generally agreed that it was rational to reject Wegener's theory in the early twentieth century and to withhold acceptance of continental drift until the 1960s, when it had developed into plate tectonics.

## 4. Assessing Harker's selective realism

Once we add Harker's own counterexample (the Priestley case) to the four counterexamples from section 3.3, we have a handful of plausible counterexamples to Harker's selective realism. How, then, should we assess his selective realist position?

As discussed in section 2.3, Harker's position predicts that "the insights responsible for comparative success will have been retained within our own scientific theories" (2013, p. 95). He is clear that we ought to assess his position in terms of the extent to which this prediction is successful, which requires examining historical cases. He makes it clear what sorts of historical cases would count as counterexamples to his position, and he provides the Priestley case as an illustrative example. However, he maintains that a single counterexample does not pose a problem for his position. According to Harker, "realism is an inappropriate attitude to adopt (at least in light of historical considerations) only if enough historical successes align in the relevant sense with Priestley's to undermine the realist inference" (2013, pp. 81-82). Harker claims that his selective realist position merely requires that "instances of empirical progress that result from theoretical assumptions, now considered mistaken, are sufficiently rare, and the instances of progress that result from subsequently retained insights sufficiently numerous" (2013, p. 101). The crucial issue is whether his position makes successful predictions about most cases of theory change in the history of science. If it does, then his position has quite a bit of evidence in its favor, and if there are only a few counterexamples, they can be treated as outliers. Harker therefore challenges "[a]ntirealists who doubt [his] proposal on historical grounds … to provide further examples that resemble Priestley's" (2013, p. 101), i.e., further counterexamples.

The handful of counterexamples discussed in section 3 provides us with a strong reason to doubt Harker's proposal. The main issue concerns whether instances of empirical progress that result from mistaken theoretical assumptions are sufficiently rare, and instances of progress that result from retained theoretical assumptions are sufficiently numerous. Harker himself discusses only a handful of historical cases. In addition to the Priestley case (which is a counterexample) and the Rutherford case discussed in section 2, he briefly discusses Fresnel's optical aether theory (2013, pp. 98-99) and Darwin's, Galton's, and Weissman's theories of generation and inheritance (2013, pp. 101-102). And he makes some very brief remarks regarding developments in evolutionary biology and chemistry as well as the history of the electron (2013, pp. 102-103). Even if everything that Harker says about those cases is correct, then we have only a handful of cases that are favorable to Harker's position. Given that we now have a handful of counterexamples to his position as well, we shouldn't be too confident that instances of empirical progress that result from mistaken theoretical assumptions are sufficiently rare, and instances of progress that result from retained theoretical assumptions are sufficiently numerous.

While the handful of counterexamples discussed in section 3 provides us with a strong reason to doubt Harker's proposal, I don't think it provides us with a decisive reason to reject it. The number of cases examined so far is relatively small, and it would be unwise to extrapolate from these hand-picked cases to a more general conclusion about the relative numbers of instances of progress that result from mistaken and retained assumptions, respectively. If it could be shown that Harker's position really does make the correct predictions regarding the vast majority of cases, i.e., enough cases to show that counterexamples are rare, then we could treat those counterexamples as outliers that don't threaten Harker's position. However, it remains to be seen whether Harker's position can succeed in this regard.

## 5. Conclusion

Based on the argument that I've presented in this paper, we have a strong reason to doubt, though perhaps not a decisive reason to reject, Harker's selective realist position. The more general upshot of this argument concerns cases in which theories make empirical progress over their rivals by successfully predicting phenomena that are inexplicable or anomalous according to their rivals. Even if this sort of progress is due to specific parts of theories that we can identify, such progress may not be good evidence for the approximate truth of those parts of theories in virtue of which they make progress over their rivals. After all, as we've seen from the counterexamples discussed in section 3, the progress that theories make over their rivals may be due to parts of those theories that are not even approximately true.

I want to conclude with a few brief remarks about my assessment of Harker's selective realism within the context of a more general assessment of selective realism. First, the counterexamples that I identified in section 3.3 are not counterexamples to forms of selective realism that focus exclusively on novel predictive success since I have not identified any instances of novel predictive success in these counterexamples. More generally, the sort of comparative success that Harker focuses on needn't be novel, and so counterexamples to Harker's position needn't be counterexamples to forms of selective realism that focus on novel predictive success.

Second, it is possible to have counterexamples that potentially threaten both Harker's selective realism and forms of selective realism that focus on novel predictive success. The Priestley case is such an example because it exhibits both novel predictive success and the sort of comparative success on which Harker focuses. More generally, in order for counterexamples to Harker's position to threaten other forms of selective realism, one would have to show that those counterexamples involve novel predictive success.

Third, it's at least possible for there to be a counterexample to forms of selective realism that focus on novel predictive success but not to Harker's position. Suppose we have a case in which a not-even-approximately-true part of a theory generates an instance of novel predictive success, but the theory has no rivals or previous versions. This sort of case would be a counterexample to forms of selective realism that focus on novel predictive success. However, if there are no rivals or previous versions, then we don't have any comparative success or empirical progress. As a result, this sort of case is not a counterexample to Harker's position.

I'll close by emphasizing three points. First, my argument against Harker's selective realism won't generalize into an argument against other forms of selective realism. In that case, it's appropriate to limit my conclusions to Harker's position. Second, confronting Harker's position with the full range of relevant cases requires examining cases of comparative, non-novel predictive success. For some time, the trend in the realism debate has been to ignore comparative success and focus almost exclusively on novel predictive success. Given this trend, as well as the fact that forms of selective realism that focus exclusively on novel predictive success face serious challenges from the historical record, I believe that it's worth considering Harker's position that comparative success, whether novel or not, is indicative of increased truthlikeness. Third, in this paper, I've tried to take Harker's position seriously and evaluate it on Harker's own terms. But after doing so, I believe we have a strong reason to doubt that the sort of comparative success and empirical progress that Harker focuses on are indicative of increased truthlikeness.

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