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How to Split a Theory: Defending Selective Realism and Convergence without Proximity

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ABSTRACT

The most influential arguments for scientific realism remain centrally concerned with an inference from scientific success to the approximate truth of successful theories. Recently, however, and in response to antirealists' objections from radical discontinuity within the history of science, the arguments have been refined. Rather than target entire theories, realists narrow their commitments to only certain *parts* of theories. Despite an initial plausibility, the selective realist strategy faces significant challenges. In this article, I outline four prerequisites for a successful selective realist defence and argue that adopting a comparative sense of success both satisfies those requirements and partially in consequence provides a more compelling, albeit more modest, realist thesis.

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1 Introduction

The concept of scientific success is central to the most favoured defences of scientific realism. There are of course a number of ways in which we might interpret the concept, even once we restrict our attention to epistemic considerations. Independently of the sense of success employed by realists, however, are issues concerning which *constituents* of successful theories deserve credit for apparent achievements. The possibility of scientific successes differentially confirming distinct parts of the same theory is central to some recent revisions to the realists' inference, developed by Kitcher ([1993], [2001]) and Psillos ([1999]).[1] To illustrate the underlying intuition, and resulting realist strategy, consider the following two cases:

(1) In the 1780s, Joseph Priestley's investigations into the nature of phlogiston produced a result of seemingly profound significance. A small quantity of calx was placed on a crucible, within a bell-jar that had been filled with a prepared gas, and the entire assemblage positioned over water. Previous work had led Priestley to identify that particular type of gas with pure phlogiston. A central tenet of the phlogiston theory was that calces required only the addition of phlogiston to transform them into metals. Thus, when Priestley heated the calx and observed, first, the gradual disappearance of the gas and, simultaneously, the calx transforming slowly into metal, better evidence for the phlogiston theory must have been hard to imagine.[2]

(2) Geiger and Marsden's alpha-particle scattering experiments led Rutherford ([1911]) to develop his nuclear model of the atom. That model described a small, electrically charged, central nucleus surrounded by a sphere of equal and opposing charge. The nuclear model, unlike older models of the atom, could account for the alpha-particle scattering data. Rutherford, however, attributed no particular structure to the orbits of the electrons and in fact assumed, for purposes of explaining the scattering results, that their combined electrical charge was uniformly distributed throughout the volume of the atom. The absence of electron orbital structure subsequently assumed significance. Important deficiencies within Rutherford's model were resolved through stipulations concerning electron orbital patterns.

[1] Other realists who endorse this general strategy include (Leplin [1997]; Niiniluoto [1999]; Sankey [2001]). The views of Psillos and Kitcher have, however, received most attention and are most rigorously developed. I'll therefore restrict my attention, in the remainder of the paper, to these two articulations of this realist strategy, with some additional brief remarks concerning structural realism.

[2] Priestley first describes the experiment in Priestly ([1783]). The relevant passage is quoted in full in Toulmin ([1957]).

From the perspective of our current theories, the work of both Priestley and Rutherford includes significant theoretical error: Priestley worked with a model of combustion that was fundamentally flawed; Rutherford's model of the atom assigned no specificity to electron orbits. Nevertheless, if we seek to identify the *source* of the described successes there appears an important distinction between the two cases. Priestley's verified prediction appears reliant on the flawed model of combustion (as well as an experimental oversight that Priestley himself later acknowledged concerning the quantity of water both before and after the heating had taken place). Rutherford's success, in contrast, seems consequent upon his description of the atom as containing a small, massive nucleus, but *independent* of commitments to the distribution of electrons' charge. Thus Priestley's success is dependent, where Rutherford's appears independent, of what are now considered their respective theoretical mistakes.

With regard to the utility of historical evidence in evaluating scientific realism, the distinction is pertinent. Laudan's ([1981]) infamous historical challenge to realism hinges on examples of past successful theories that cannot be described, by current lights, as even approximately true, thereby undermining the inference from scientific success to the approximate truth of successful theories. Some realists have responded by noting that the ostensibly unfavourable cases would prove benign if it could be argued that the successes of defunct theories were independent of their apparent theoretical mistakes. In other words, if scientific successes are products of only certain constituents of a theory, and those same constituents remain stable across subsequent cases of theory change, then no amount of discontinuity across instances of theory change can undermine the conviction that the *source* of success is at least approximately true and that success is, after all, a reliable indicator of approximate truth.[3] Herein we find motivation for developing a *selective* realist thesis, whereby realists narrow their commitments to those parts of theories that are genuinely responsible for success. If this narrowing of commitments functions as the realist suspects, then the parts of past theories that are judged responsible for past scientific successes will be preserved within current theories. Of the above examples, therefore, one might suspect that: cases such as Priestley's lend credence to antirealism (since the success appears dependent on theoretical error); cases such as Rutherford's lend credence to selective realism (since his achievement appears independent of his theoretical error); and realism is an inappropriate attitude to adopt (at least in light of historical

[3] There are, of course, other reasons to worry about the realist's inference. In this article, I'll be concerned with both the antirealist's argument based on radical changes within the history of science and, in Section 3, the objection that the realists' appeal to realism's explanatory virtues ensures the defence is inherently unpersuasive to antirealists. I won't be concerned with antirealist arguments based on underdetermination theses.

considerations) only if enough historical successes align in the relevant sense with Priestley's to undermine the realist inference.

Antirealists object to the selective strategy that the historical reconstructions offered by realists are implausible and that the distinctions drawn between the ostensibly working and idle constituents of successful theories are problematic.[4] These objections appear persuasive insofar as they are directed towards extant versions of the selective strategy, but the general approach remains worthy of further attention. Perhaps scientific success (appropriately conceived) can discriminate between the working and idle posits of past theories (appropriately distinguished) in a manner that doesn't beg the question against antirealists, and allows for plausible realist interpretations of historical examples of even radical change between successful theories and their replacements.

In this article, I use the general selective strategy as a platform for developing a distinct means of understanding and defending scientific realism. In Section 2, I outline four criteria that must be satisfied by any successful selective, realist defence. In Section 3, I suggest revisions to the concept of success employed by realists, by implication the appropriate realist conclusion to draw from instances of success, and, finally, the nature of the inference that connects success with approximate truth. In the remainder of the article, I argue that the revised thesis: (i) permits an understanding of selective realism that survives conceptual scrutiny; (ii) liberates realists from the need to defend realism's putative *explanatory* virtues; (iii) provides an immediate argument for a convergent, as well as selective, realist thesis; and, (iv) appears promising for the purposes of answering the standard, historical examples that are often regarded as torpedoing the realist's argument from success.

2 Requisites for a Selective Realist Defence

Prima facie, Rutherford's achievement with the scattering experiments is inconsequential for purposes of assessing the realist's inference. Rutherford's model neither enjoyed the novel success that many realists prize, nor obviously fails to qualify as approximately true; it is thus twice removed from a straightforward counterexample to the claim that novel success is indicative of at least approximate truth. Dismissing the case for either reason is not entirely satisfying. First, realists' tendency to elevate the epistemic significance of peculiarly novel success still lacks a convincing justification, hence we must be

[4] See, in particular (Chang [2003]; Stanford [2006]; Lyons [2006]). Chang offers historical examples in opposition to selective realism from the caloric theory, Stanford from nineteenth century developmental biology in particular, and Lyons principally from Kepler's work on planetary motion.

cautious of placing too much emphasis on such results.[5] Second, while it's not obvious that Rutherford's model can't be described as approximately true, this concession ignores the fact that at least with hindsight we recognise some aspects of the model as much better approximations to our own understanding than others. It would be significant, for the purposes of defending the realist's inference, if we could define success such that the constituents responsible for the success of Rutherford's model agreed with the ways in which our understanding of the atom has remained stable. Finally, this is a case where it appears clear that the assumption of uniform electron charge distribution was not confirmed by the successes of the model. If the selective realist cannot make sense of this case, in terms of justifying a realist endorsement for only certain parts of the theory, then it is unlikely they will be able to argue historical episodes that are ostensibly less conducive to a selective realist interpretation. Thus, although antirealists have not invoked Rutherford's success to undermine realism, it will be instructive to start with a seemingly unproblematic case.

I'll assume, therefore, that what the selective realist hopes to justify is that Rutherford's theory was successful *because* he identified that the atom has a small, massive nucleus at its centre and *despite* assuming that electron charge was uniformly distributed throughout the atom. Establishing that success was a product of particular (and subsequently retained) insights is the purpose of any selective realist thesis. Since previous selective theses have been criticized on both historical and conceptual grounds, however, it is important to seek criteria by which we can evaluate whether a given selective, realist defence is even conceptually viable. I suggest four:

(1) *The Discrimination Criterion*. The selective strategy can only work if we can discriminate between distinct constituents of successful theories. Realists cannot argue that Rutherford's claim that atoms have nuclei deserves realist endorsement, while the claim concerning uniformity of electron charge does not, without principled means of distinguishing between these claims. Structural realists distinguish structural from non-structural theoretical components; Kitcher distinguishes the presuppositional posits of a theory from the working posits. Whether either distinction is cogent is here beside the

[5] An advantage of attending only to theories that enjoy *novel* success is that it greatly reduces at least Laudan's initial list of apparent historical counterexamples to the realist's inference. However, in addition to lacking any epistemically justified distinction from other forms of scientific success, arguments that appeal to novel success have also been criticized for committing the base rate fallacy (see, for example, Magnus and Callendar [2004]). By allowing that non-novel success could justify realist attitudes, realists open themselves to a potentially greater number of historical counterexamples, but there are alternative means of strengthening the notion of success to ensure the realist's inference is at least plausible.

point–some general criterion for separation is central to any selective thesis.[6]

Endorsing only particular constituents of a successful theory is, however, insufficient for a selective realist defence. As noted, antirealists have offered historical examples of scientific theories that were successful and where the successes relied on constituents of theories that have not been retained, even approximately, within contemporary theories.[7] Such examples create trouble even for selective realist arguments. Historical interpretations can be challenged, but a more promising response concedes that, while some criteria for splitting theories cannot fulfil realist aspirations, alternative criteria can discriminate between constituents of theories in a manner that does. Different conceptions of success call for distinct analyses of what contributed towards success. This will prove crucial to my argument.

(2) *The Differential Confirmation Criterion*. To preserve the realist's inference from success in the case of Rutherford's model, it would be inadequate to note simply that physics has retained the opinion that atoms have small, massive nuclei. Rather, the constituents offered for realist interpretation must be confirmed by the successes of scientific theories, in ways that the remaining constituents are not. A successful selective realist defence must provide a *differential confirmatory relation* that justifies a realist attitude towards only that class of theoretical constituents that is identified for realist approval. Rutherford's claims about atomic nuclei—as an instance of a particular kind of theoretical claim—must be supported by the successes of his model in ways that other constituents of the model are not, if selective realism is to work in this particular case. Differential confirmatory relations are typically defended by arguing that only certain constituents of a theory are *responsible* for a theory's empirical successes.

Against Worrall's version of structural realism, Psillos ([1999], pp. 146–61) argues that, even if a distinction between the structural and non-structural content of theories can sensibly be drawn, Worrall has failed to establish that correctly identifying only the structure of some system is sufficient to generate

[6] By insisting on general criteria, I appear to be in opposition to the kind of local, experimental realism defended, for example, by Achinstein ([2002]). Appealing to work conducted by Jean Perrin, Achinstein argues that Perrin's experimental argument for the existence of molecules is sufficient for a realist attitude towards molecules. While Achinstein's proposal is interesting, his conclusion can be reconciled with historical considerations only if Achinstein can argue that Perrin's arguments are different in kind from those offered in defense of the aether, phlogiston, pangene, and so on. To offer an argument that draws the necessary distinction would be general in the sense I'm suggesting.

[7] See footnote 4 for examples.

empirical successes. It is thus unclear, continues Psillos, why an inference from scientific success to the approximate truth of a theory's structural content is justified, while an inference to the approximate truth of non-structural content is not. Worrall might rejoin that sufficient evidence of continuity of structure is reason enough to suppose that the structural content of successful theories is approximately true, but the response is inadequate for two reasons: First, there are alternative, plausible explanations for instances of continuity across theory change that do not appeal to the approximate truth of the stable elements. More importantly, Worrall clearly understands structural realism as a means of defending the realist's argument from success to approximate truth, as do other structural and selective realists. Success is supposed to provide a *reason* for believing that particular aspects of a theory are at least approximately true. Selective realism thus requires not just continuity, but a particular type of continuity: the retention of those parts that are differentially supported by the successes of our theories.

(3) *The Non-Whiggish History Criterion*. A legitimate concern for any attempt to identify which parts were responsible for the successes of replaced theories, hence appropriately confirmed by those successes, and which were idle, is that the benefit of hindsight can play an improper role. The realist hopes to persuade us that where replaced theories disagree with currently held scientific opinion, the rejected claims were irrelevant for the successes. It would be question begging if realists allowed current scientific understanding to infect their judgements concerning which parts of replaced theories were redundant. Our assessments of what did or did not contribute towards scientific achievements cannot assign a privileged status to more recent theories. Psillos ([1999], pp. 111–2) and Stanford ([2006], p. 166) each object that Kitcher's historical reconstruction of the aether theory involves whiggish history. Similarly, Chang ([2003], p. 906) argues that Psillos's analysis of the caloric theory appears plausible because he attends only to those parts of the caloric theory that resemble modern beliefs.

One of Psillos' strategies for reconciling historical considerations with scientific realism draws on the evaluations of those scientists who actually developed and articulated now replaced theories. Of aether theorists, for example, Psillos argues that there was significant ambivalence concerning the existence of a material aether. More generally, according to Psillos, scientists are themselves a reliable guide to which parts of theories are responsible for success and which are not, hence to which parts are approximately true and which are not. In the case of the aether theorists, however, Stanford ([2006], p. 175) objects that Psillos's conclusion 'can only be defended by conveniently

ignoring many [...] judgements made by leading scientists of the time'. Stanford ([2006], chs 3–5) offers detailed, additional examples from nineteenth-century biology that put significant pressure on Psillos's strategy, suggesting that the attitudes of previous generations of scientists are often wholly unreliable in distinguishing between what we would now regard as the true and false parts of replaced, successful theories. Rather than use contemporary knowledge to evaluate past *theories*, Psillos appears to have projected current understanding onto the attitudes of previous scientists. Alternative interpretations of both Kitcher's and Psillos's selective strategies are available, but these, as will be shown below, are vulnerable to equally compelling criticisms.

(4) *The Epistemic Accessibility Criterion*. Besides appealing to the judgments of scientists, Psillos ([1999], p. 110) elsewhere suggests that the constituents of theories should receive credit for success only if they satisfy specified criteria, beyond being merely part of a successful theory. More particularly, Psillos recommends that an assumption should only receive credit for the successes of a theory if it cannot be replaced by an alternate posit that is consistent with the general theory, would preserve the empirical content of that theory, and would avoid making the theory *ad hoc*.[8]

Lyons ([2006]) argues convincingly, however, that Psillos' criteria are neither relevant to the issue of determining whether some constituent of a theory contributed towards success nor epistemically accessible. The former objection is based on the observation that whether some constituent of a theory could be replaced by an alternative constituent in a non-*ad hoc* manner, and without loss of empirical content or consistency, is irrelevant to whether that constituent deserves credit for the derivation of some prediction. On any non-comparative model of confirmation it would seem that if Rutherford's commitment to an atomic nucleus deserves credit for the prediction of the alpha particle scattering results, then it deserves credit irrespective of whether an alternative posit that satisfies Psillos' criteria can be conceived.[9] Lyons's second concern illustrates a further condition for a successful realist defence: of the constituents of theories that are

[8] The relationship between Psillos's criteria for identifying which constituents are responsible for success and his historical reconstructions is unclear. Psillos does not attempt to show, for example, that the parts of theories that he claims were responsible for the successes of the aether theories and caloric theory satisfy his criteria. This problem is of course in addition to the concerns about whig history raised by Chang ([2003]) and Stanford ([2006]).

[9] The criticism demonstrates a failure by Psillos to satisfy my second criterion, above. It is thus somewhat ironic that Psillos objects against structural realists that they have failed to demonstrate how the structural parts of a theory are discriminatorily confirmed by scientific success, when the same objections applies to those parts that he, at one point, proposes for realist endorsement.

> positively highlighted in a selective realist thesis, the qualities we demand of those constituents must be *epistemically accessible*. A failure of epistemic accessibility renders the realist unable to isolate parts of theories that she can be a realist about. For Psillos, we should be realists about only those constituents that satisfy certain criteria, but it is unclear, objects Lyons, whether we could ever *know* that any constituent of any theory satisfies them.

Part of Stanford's critique of Kitcher's selective realist strategy can also be understood in terms of this fourth criterion. Kitcher ([1993], pp. 142–9) describes how we can identify parts of past theories as merely 'presuppositional' for the empirical successes of those theories. For Kitcher these posits, unlike the 'working posits', do not deserve realist endorsement. Stanford suggests we might interpret Kitcher as arguing that the *conceivability* of retaining the aether theory's empirical success without committing to the existence of a material aether renders physicists' commitment to a material aether idle, with regard to the successes of the theory. Stanford's complaint, against this interpretation, is that we can conceive of any commitment to any unobservable entity being idle for success. By this standard, no theoretical posit could ever be known to be other than idle, and hence we would have nothing to be realist about. If there are parts of theories that cannot even conceivably be replaced, then we are unable to identify those constituents.[10]

The selective realist strategy seeks to infer from scientific success to the approximate truth of certain constituents of past theories. For the strategy to answer Laudan-style concerns, first, constituents of theories should be identified via criteria that are applicable for any case of a genuinely successful theory. Second, the successes of a theory must differentially confirm those parts that are being selected for realist endorsement. Third, our understanding of the relationship between success and its sources cannot be influenced in ways that guarantee a favourable, realist reading, and in particular cannot be inappropriately informed by subsequent scientific discovery. Finally, the criteria we invoke to isolate those constituents of theories that are to be

[10] Stanford's ([2006], pp. 166–73) final analysis of Kitcher's distinction between working and presuppositional posits is that it fails for one of three reasons. Kitcher might be guilty of whiggish history. He can avoid that criticism by appealing to what's *conceivably* idle for purposes of generating success, but then has nothing left about which he can be a realist. Finally, Stanford offers a third interpretation of Kitcher's distinction, whereby presuppositional posits are those judged to have no direct causal role in the explanations and predictions provided by the theory. Stanford seems to allow that, in the case of the optical aether theories, this might preserve the realist's inference from success to approximate truth, vindicating Kitcher's strategy on this particular occasion. However, Stanford argues that, in general, this way of conceiving of the distinction will not help Kitcher either because there are many historical examples of successful theories that attributed direct causal roles to entities that are no longer recognized by contemporary science. Stanford offers phlogiston, caloric, and the vital forces of late-eighteenth and early-to-mid-nineteenth century theories of physiology and embryology as examples.

recommended for realist endorsement must render such constituents epistemically accessible. These conditions, I propose, are a minimal requirement for any selective, realist understanding of past success that is sensitive to the discontinuities that pervade the history of our sciences. It is unclear whether any currently available selective thesis satisfies them all.

The failures of previous selective theses can be illustrated by considering the possible vantages they provide for purposes of evaluating replaced theories and thereby identifying the constituents that deserve credit for empirical successes. One such vantage, explored by Psillos, is that of the successful theory itself and the theorists who worked within that framework. Historical evidence suggests, however, that theorists have often been unequivocally wedded to ideas that were subsequently regarded as hopelessly misguided. A second possible perspective is that of our own theories, but to assume these (or constituents of these) are approximately true only begs the question against antirealists. Finally, we might hope to defend the approximate truth of certain constituents of theories on the basis that no alternative is even conceivable, but then it becomes unclear whether we can justify a realist attitude towards any theoretical claim. Each of these three vantages opens the selective realist's historical re-evaluations to critical objections. Shortly, I'll describe a selective thesis that offers a new perspective on replaced theories and satisfies the above requirements for a successful realist defence. Some additional stage-setting is needed first.

3 Success, Progress, and a New Selective Realist Thesis

3.1 A new way for realists to think about scientific success

Historical considerations, at best, complicate any defence of the inference from success to approximate truth. Rutherford's success cannot straightforwardly justify the belief that his model was approximately true. Perhaps more distressing, the realist lacks justification even for maintaining the approximate truth of Rutherford's conclusion that atoms have small, massive, electrically charged nuclei. If the latter appears more deplorable, it is presumably because one might harbour reservations about describing Rutherford's model as approximately true, yet simultaneously suspect that Rutherford discovered an important truth about atoms. Such concerns highlight the usual worries that plague the concept of approximate truth, but can also, I'll suggest, motivate realists to affirm a quite distinct conception of success.

The sense of success most commonly employed by realists emphasizes the role that data play in theory construction, championing theories that have novel predictions verified.[11] I argued in (Harker [2008]) that an

[11] See, for example, (Musgrave [1988], p. 232; Worrall [1989], p. 114; Psillos [1999], pp. 106–7).

underappreciated and significant aspect to popular examples of novel success is the implication that the successful theory is *better* than available rivals.[12] Likewise for Rutherford's model, I suspect: our admiration stems not from any conviction that Rutherford discovered an approximately true model of the atom, but from the belief that Rutherford's model was, or contained, an important improvement over previous models. 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*. Conceiving of success in terms of empirical progress motivates a new way of developing the realist's argument from success. Distinct theories might range over a similar domain and might each be regarded as successful, without consideration for which theories are better than others. If progress is of central epistemic significance to realism, the previous discussions have systematically missed the mark.[13]

The distinction between comparative and non-comparative success is central to Sober's views on confirmation (for example, Sober [1994], pp. 114–35). Sober argues that confirmation is always comparative: we confirm a hypothesis by demonstrating that available evidence supports *it* more favourably than it supports a specified rival.[14] Realists need not commit themselves to Sober's view of confirmation generally, but should, I'll argue, consider employing a comparative conception of success for purposes of defending their inference to approximate truth. In the same way that empirical success can be defined independently of realist commitments, and so in principle used as a means of arguing for realist theses, so for progress, defined in terms of comparative empirical success.

3.2 Distinguishing two realist commitments

Most realists, I suppose, would endorse two theses that are rarely distinguished: first, lineages of theories are becoming more truthlike; second, at least many of our own theories are sufficiently truthlike to justify describing them as approximately true. A defence only of the former might strike many as unsatisfying, but establishing even this much is neither epistemically inconsequential nor trivially achieved. Evidence of truth-directed progress

[12] Comparative versions of novelty have been defended elsewhere (for example, Musgrave [1974]; Frankel [1979]; Nunan [1984]), though the view has proved far less popular than the use-novelty thesis.

[13] Certainly, none of the standard presentations of the realist's argument from success, for example Musgrave ([1988]), Worrall ([1989]), Psillos ([1999]), suggest that realists should construe scientific success comparatively. Critics of the realist argument also seem to understand success in thoroughly non-comparative terms.

[14] Confirmation is more typically regarded, first, as a relation between hypotheses and available evidence and, derivatively, as a means of comparing those hypotheses in terms of evidentiary support.

would represent an important victory over many forms of antirealism. Simultaneously, historical considerations create problems for any realist model of progress, even once disentangled from issues concerning the approximate truth of theories. If, however, as I've suggested, verified novel predictions and achievements like Rutherford's attract attention principally because they indicate progress, perhaps realists too should begin by defending the idea that theories are becoming more truthlike. Empirical, scientific progress might prove more reliably indicative of increased truthlikeness, in light of historical considerations, than success (understood non-comparatively) has proved a reliable indicator of approximate truth.

Introducing concepts of progress and truthlikeness creates pressure to provide more careful analyses of each. Concerning the former I'll suppose for the sake of this article that theories achieve progress by predicting phenomena that are otherwise either inexplicable or anomalous. 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.[15] Of truthlikeness, the intuitive idea is that some false models and theories are nevertheless closer to the truth than others: a model of the atom that includes a small massive nucleus, for example, is more truthlike than a model that does not include such a nucleus, *ceteris paribus*, if it's the case that atoms have small massive nuclei.[16] While a generally accepted formal conception remains elusive, the underlying intuition appears sufficiently robust to justify continuing with only an informal notion. This strategy is justified in part, again, because what's more pressing for the current thesis are reasons to believe that relating empirical progress to a realist thesis is more promising than traditional realist defences. I'll also assume in what follows that increased truthlikeness within lineages of theories can be defended by arguing for a convergent form of scientific realism.

Before describing the new selective thesis, one further clarification is important: a realist thesis that's defined in terms of theory-*lineages* might appear to be abandoning the selective realist strategy. In fact, as will become clear below, that strategy is pivotal to my thesis for what will be proposed is that the *parts* of theories which precipitate empirical progress should become the focus of a selective realist thesis. If we have reason to suppose that such parts are

[15] One reason this definition requires augmentation, in a more complete defence of the thesis, is that we don't want to credit *ad hoc* modifications to existing theories with representing empirical progress. Psillos ([1999]) defines *ad hoc* hypotheses in terms of the use-novelty thesis that I've distanced myself from. However, his is not the only horse in the race. Forster and Sober ([1994]), for example, suggest that a hypothesis is *ad hoc* if its introduction produces a loss in simplicity that is not outweighed by the gain in predictive accuracy. I think this is an important suggestion, but will postpone discussion for another occasion.

[16] The concept is already distinct from Popper's notion, by appealing to correspondence between a model or theory and its target, as opposed to Popper's view that concerns instead the quantity of true and false empirical consequences of a theory.

approximately true, then, I'll argue, we can defend both a selective and convergent scientific realism. In keeping with previous selective theses, therefore, the concept of approximate truth is not eliminated but transferred from theories to parts of theories.

Scientific success, I've observed, can be interpreted as a property that should be attributed to theories only with respect to the predictive achievements of available alternatives. Realists might therefore consider arguing, not that successful theories are approximately true, but that those theories are more truthlike. It is time to spell out such an argument in more detail.

3.3 A new selective realist thesis

Theories cannot achieve empirical progress without recommending *changes* to existing theories. Such changes might be more naturally interpreted, in some instances, as modifications or revisions to an existing theory or, alternatively, as the introduction of a new theory. Both types of change are a potential vehicle for empirical progress, however, and thus pertinent to the issue of whether empirical progress is indicative of greater truthlikeness. A convergent realist thesis cannot ignore paradigmatic instances of theory replacement, but must make plausible the view that more recent theories are more truthlike than those they replaced. By allowing that changes *to* a theory also represent a type of success with which realists should be concerned, however, the present thesis significantly expands its testability against the historical record. Typically, realists were faced with defending the view that molecular genetics, for example, is approximately true; developments within molecular genetics were irrelevant for purposes of defending that claim. On the current thesis, any empirical progress that was achieved from within the context of molecular genetics will become relevant for purposes of evaluating the realist thesis.

However we construe given changes to our theoretical frameworks, not all parts of the new system will induce progress. Empirical progress is never a product of shared theoretical commitments. For example, if Rutherford's model of the atom was more empirically adequate than previous models of the atom, it was not in virtue of those theoretical claims that it shared with older models of the atom. Nor should we assume that all unshared theoretical commitments are responsible for generating progress. By Rutherford's own admission, the assumption that electron charge was uniformly distributed through the volume of the atom was an idealization and in no sense confirmed by the data.[17]

[17] The judgment that Rutherford's progress relied on claims about nuclei, but not on the claim concerning the assumption of uniform negative charge distribution, gains credence through Rutherford's explicit recognition that the latter played no direct role in his argument. Of course we cannot rely solely on past scientists to correctly identify which parts of past theories were idle and which were responsible for progress. It will become clear below that satisfying the four criteria described in Section 2 enables us to attend less to how past scientists in fact regarded

At least in principle, therefore, we can distinguish those modifications to previous theories that improved the empirical strength of our sciences from those that didn't. Furthermore, the observations and data that speak in favour of particular modifications to our scientific understanding are precisely the observations that are predicted by the new version, but are anomalous or inexplicable for the original. In other words, *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*. Scientific progress might thus be interpreted, by the selective realist, as evidence for the approximate truth of those constituents of the new theory that precipitate progress. Our theories become empirically stronger, claims the realist, as we introduce novel theoretical insights that are approximately true; since those insights are responsible for the growth in empirical adequacy, then the latter can be offered as evidence that these novel constituents are approximately true.

The strategy closely parallels the standard realist intuition. However, rather than argue that predictive successes indicate that the successful theory is approximately true, we argue that empirical *progress* is an indicator that the theoretical insights responsible for that progress are approximately true. The proposal is distinct from previous instantiations of the selective strategy in virtue of attending to different parts of theories: what makes a theory better than available alternatives is distinct from what makes a theory empirically successful.

The new selective thesis provides a new strategy for responding to historical examples of radical ontological discontinuity. Theories of the phlogiston, aether, and so on provide important and compelling challenges to the claim that successful theories are approximately true. It's unclear whether restricting realist commitments to the 'working posits' of theories alleviates these historically based concerns. However, we might still hope to sustain the view that such theories were more truthlike than the theories they replaced while less truthlike than the theories that replaced them. We can reasonably hope to evaluate whether lineages of theories are becoming increasingly truthlike without confronting the possibility that some or all of those theories might not qualify as approximately true. The thesis is nevertheless a thoroughly realist thesis insofar as it claims that an achievement of science is the acquisition of truth concerning unobservable entities.

Recognizing that progress involves change, and that evidence of progress can be understood as reason to endorse particular changes, enables us to offer

the various constituents of a theory and more directly to how theories in fact improved upon their predecessors.

a distinctive selective realist thesis that recommends a realist attitude towards only those parts of theories that generate progress. Thus far, I've offered little reason to suppose the new realist thesis is any more plausible than previous applications of the selective strategy. It is analysis of its conceptual viability (Section 4) and fit with history (briefly, in Section 5) that I believe will ultimately recommend the thesis. Before turning to these issues, two further advantages of the thesis deserve attention.

3.4 For convergence and against relying on explanatory virtues

If 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. If, for example, Rutherford's nucleus was responsible for progress, as seems plausible, it should be endorsed as approximately true according to the view I've described. Given Rutherford's conclusion has retained currency within present models of the atom, historical considerations are cooperative. Of course Rutherford's conclusion about atomic nuclei has undergone significant further refinement and, insofar as these refinements can be shown themselves to have induced further empirical progress, they too fall within the class of claims here recommended for realist interpretation. The pattern of realist endorsement proposed thus follows the selective realist directive of inferring only the approximate truth of parts of theories. However, by attending to those constituents that generate progress, we also provide an argument for a convergent realist thesis and thereby increased truthlikeness within science. Refinements to scientific theories, models, and schema are made. Subsequent generations, let us suppose for now, both endorse those revisions and introduce additional reform themselves. Sufficient evidence for such patterns of retention and revision entails that science is cumulative and suggests that lineages of theories are converging on some limit.[18] Selective realist theses that define scientific success non-comparatively must develop independent arguments for convergence.

A further virtue of the proposal worth highlighting follows from its *prediction* that where constituents of past theories generated progress, those constituents will have been retained within current scientific theories. Yielding predictions that can be tested against the historical record is significant. When Musgrave ([1988]) discusses the realist's argument from success, he observes that *prima facie* the realist commits the fallacy of affirming the consequent.

[18] The pattern of retention and revision is thus distinct from the mere accumulation of new theoretical posits. Rather, as theories are modified, earlier ideas play a crucial role in motivating and justifying changes. It is evidence that theoretical ideas are part of ongoing refinement, producing ever greater empirical progress, which suggests convergence within science.

Theories make predictions; some such predictions are verified, representing successes for the theory; the realist appeals to the truth of those observable *consequences* in defence of their realist interpretation of the theory, seemingly committing a blatant fallacy. Musgrave's response is almost orthodoxy amongst realists: we should regard the approximate truth of theories as providing *the best explanation* for the truth of their consequences. Inferences based on explanatory considerations are not deductively valid, but they are widely regarded as an important inferential practice, thus saving realists' blushes. Musgrave himself concedes, however, that empiricists are unlikely to be swayed by realism's putative explanatory advantages. Efforts to defend realism through abductive reasoning have continued, but defending the strategy represents a further challenge for realism.[19]

A realist thesis that makes predictions avoids relying on abduction. The view I've outlined makes predictions about what patterns of continuity we should find within lineages of evolving scientific disciplines. We use empirical progress to pick out particular constituents of a theory, based on the realist attitude that progress indicates the approximate truth of the novel insights responsible for progress. The realist formulates predictions by combining this realist hypothesis with the supposition that where science has uncovered approximately true conclusions, subsequent generations will preserve those conclusions within their own theoretical frameworks. Finally, we appeal to the historical record to test whether those particular constituents have in fact been retained.

An objection that confronts any attempt to defend realism on the basis of its predictive achievements is that antirealists can also make predictions, for example that the history of science will reveal the retention of theoretical posits for as long as they remain useful. However, it is one thing to predict that *some parts* of replaced theories will be preserved across subsequent instances of theory change, quite another to predict that all those parts of replaced theories *that played a particular type of role* will be preserved. Van Fraassen ([1980], p. 67) acknowledges that empirical strength, and not just empirical adequacy, is a standard by which we should evaluate competing scientific theories. A hypothesis which predicts that particular species will survive a particular environmental disaster is stronger than a hypothesis that predicts only that some species will survive. If the predictions are verified, we have reason to prefer the former hypothesis even though both fit the observations. Standards that empiricists recognise as suitable for evaluating scientific theories can be utilized to evaluate whether realism is superior to antirealist theses in specified regards.

[19] The most sustained effort to defend realism's use of abductive reasoning is Psillos ([1999]). For criticisms of Psillos, and the strategy in general, see Doppelt ([2005]) and Frost-Arnold ([2010]). Kitcher defends the inference from success to truth in a manner that does not rely on abduction (see, in particular, his [2001]).

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. The antirealist can offer no reason for supposing that the constituents of replaced theories that induced earlier instances of empirical progress will have systematically survived all subsequent cases of theory change or modification. The realist thesis I have proposed makes just such a prediction.

In summary, selective realist theses have attempted to defend the following:

> HYP: Those parts of theories that are responsible for the verified predictions and/or explanations of the theory are approximately true.

The justification for this hypothesis is typically abductive:

> JUS: The approximate truth of the parts of theories described in HYP provides the best explanation for the successes of the theory, hence it is reasonable to assert their approximate truth.

The historical challenge that confronts the thesis is to identify the 'working posits' without relying on *post hoc* rationalizations, and show that those working posits have been retained. However, neither Psillos nor Kitcher appear to have met that challenge. I've suggested their approaches fail because they haven't identified a suitable perspective from which to evaluate past theories.

In place of this strategy, I recommend the following:

> HYP_2: Those parts of theories that generate comparative success are approximately true.[20]

The justification is not abductive, but based on conjoining HYP with a second realist hypothesis and directly testing the consequences:

> AUX: Approximately true insights will be preserved across subsequent instances of theory change.
>
> JUS_2: On the basis of HYP_2 and AUX we predict that the insights responsible for comparative success will have been retained within our own scientific theories. Verification of these predictions is evidence for HYP_2.

The historical challenge remains the same, although now historical evidence plays a more direct role in defending the selective thesis. What further distinguishes my selective thesis is that we now have a new perspective from which to conduct historical analyses. The perspective is that of *older* theories. When introducing new theories, or modifications to existing ones, theorists often provide empirically-based arguments that are designed to convince others

[20] We can ignore both those theoretical assumptions that a theory shares with its predecessor and any unshared theoretical assumptions that are idle in generating the comparative successes.

that the *changes* are worth endorsing. With regard to Rutherford's model, for example, we can evaluate the arguments that he provided for purposes of convincing his contemporaries that they should modify their understanding of atoms. Such evaluations need not assume that Rutherford's model was approximately true, nor that our own theories of the atom are approximately true, nor that explaining the alpha scattering experiments is inconceivable without a theoretical posit that approximates Rutherford's. With the new selective thesis now outlined, we can revisit the four conditions described in Section 2.

4 Requisites for a Selective Realist Defence Revisited

(1) *The Discrimination Criterion.* From the above sketch it should be apparent that, at least in principle, we can distinguish those parts of theories that have generated progress from those that have not. Lineages of theories involve numerous revisions and modifications. Some such changes are effective in yielding empirical progress; other assumptions and suggestions don't satisfy the standard. A realist thesis that is restricted to the former qualifies as a selective realist thesis.

(2) *The Differential Confirmation Criterion.* As also noted above, the appearance of progress serves as evidence for those particular changes to theoretical frameworks that generate predictive gains. Rutherford was not arguing: that there are atoms; that electrons are subatomic particles; that electrons are electrically charged, and so on. These claims were the conclusions of earlier work. Rather, Rutherford *assumed* and endorsed these earlier determinations, using them for purposes of forwarding his own argument for the view that atoms have nuclei with certain specified properties. The merits of his argument can be evaluated without concern for the evidence that was available in defence of his initial assumptions. Elements of a new theory that are already part of accepted background assumptions can be ignored for purposes of evaluating the arguments for changing our theoretical frameworks, as can assumptions that are idle in generating the new success. Changes to theoretical frameworks that induce progress thus stand in a relationship to such progress which is distinct from the relationship that obtains between those aspects of our theoretical frameworks that remain stable through cases of scientific progress or were ineffective in its production. Therein lays the evidentiary asymmetry that a selective realist thesis requires.[21]

A selective thesis that attends to comparative success thus connects success with a particular class of theoretical constituent. It is less clear whether structural realists have connected success appropriately with the structural content of a theory. Similarly, Psillos fails to show why success should redound to the credit of parts of theories *only* when no available alternative exists that is neither inconsistent with the general theory nor *ad hoc*.

(3) *The Non-Whiggish History Criterion*. By interpreting success comparatively, a theory can be evaluated from the perspective of older theories. We can evaluate the progress achieved through Rutherford's model from the perspective of models like Thomson's. Kyle Stanford (personal communication) has expressed scepticism about this proposal, suggesting that if whiggish history is a problem for previous selective realist theses, it is also a problem for this one. Certainly, I'm willing to concede that any attempt to re-evaluate past theories *can* involve the inappropriate influence of more modern scientific conclusions, but this requires only that sufficient care is taken to avoid such impropriety. If, however, Stanford's suggestion is that reconstructions of the type I have alluded to cannot in principle avoid whiggish history, then I am not convinced. In the next section I briefly consider several historical cases that have featured in the realist literature. These assessments involve no whiggish history.

(4) *The Epistemic Accessibility Criterion*. Finally, there is no obstacle to identifying a given theoretical modification as having generated empirical progress. We don't need to worry, as Psillos urges, about whether alternative posits were available that could have accounted for the same phenomena in a non-*ad hoc* manner and without loss of adequacy or consistency. If progress is achieved in virtue of some novel insight, the thesis recommends we regard that posit as approximately true. The conceivability of accounting for those same phenomena via different theoretical commitments is irrelevant to evaluating the realist thesis. By restricting our attention to those constituents of theories that are responsible for progress, we ensure that the targets of our selective thesis are epistemically accessible.

Conceiving of success in comparative terms thus opens space for a selective realist thesis that satisfies my four criteria. The thesis provides a distinct means of understanding the realist's conviction that success is an indicator of

[21] It is perhaps worth reiterating that I am not claiming that all confirmation proceeds comparatively. I am claiming that once we focus on comparative successes, then it is only those parts that induce progress that can receive credit *for the progress*.

approximate truth and a distinct means of accommodating examples of successful theories that were not, by contemporary lights, approximately true. Rather than ask how theories were predictively and explanatorily successful, we ask how such theories progressed beyond the empirical achievements of previous theories. Rather than argue that successful theories are approximately true, we defend *changes* to our scientific understanding as representing the approximate truth of those theoretical innovations that were responsible for progress; absence of radical discontinuity within this restricted domain of theoretical claims will preserve the inference against the antirealist's historical challenges. Furthermore, collectively, the systematic retention of such innovations suggests ever greater truthlikeness within lineages of theories.

5 Historical Sketches

The crucial test for any selective realist strategy is the historical record. If theories regularly achieve progress, but the novel insights responsible for such are subsequently replaced with radically different claims about the world, then the thesis I've described will fail to reconcile realism with history. The historical questions are complex and a satisfactory treatment of just one case would require more space than is suitable for this article. In what follows, I provide sketches that I hope are sufficient to at least further motivate the current thesis.

5.1 Optical aether theories

Appeals to aetherial substances were invoked from the seventeenth to the nineteenth century for purposes of explaining a wide variety of phenomena: gravitational, chemical, and electrical. It is nineteenth-century work on optical phenomena, however, that is widely regarded as providing a particularly salient challenge to the realist's inference from scientific success to approximate truth. Fresnel's work on refraction, diffraction, and polarization was successful by any standard of success that realist's are likely to find useful, yet Fresnel conceived of his work as concerning the properties of an all-pervading, elastic solid—a substance which does not exist according to modern physics. If we assume with Laudan that a theory cannot be approximately true if its central terms are non-referring, then we appear to have a nice counterexample to the realist's inference from success to approximate truth. Realists have offered a variety of responses to the case.[22] The strengths and deficiencies of these positions are not unimportant, but here I'll focus on understanding the aether theory from the perspective of the thesis I've presented.

[22] See (Hardin and Rosenberg [1982]; Worrall [1989]; Kitcher [1993]; Chakravarty [1998]; Psillos [1999]) for attempts to defend realism in the face of the challenge presented by aether theorists' work on optics.

Fresnel's achievements distinguished the new wave theory of light from the corpuscularian theories that had dominated optical work throughout much of the eighteenth century. Aspects of Fresnel's theory that played an unequivocal role in the progress Fresnel achieved include his eponymous equations and the assumption that light is a transverse (rather than longitudinal) wave. Phenomena were explicable and quantifiable predictions made possible through the introduction of these theoretical innovations. Empirical progress is thereby fully attributable to some of Fresnel's *theoretical* insights. The actual commitment to the aether, however, can be distinguished conceptually from Fresnel's equations and the assumption that light is a transverse wave. We are entitled, therefore, to ask what empirical progress was induced by the commitment to the aether that was independent of the progress achieved by the equations and the assumption that light is a transverse wave. I can find evidence of none. Assuming that transverse waves were themselves disturbances in an elastic solid aether produced no verified predictions that were not already available once light was conceived of as a transverse wave. Buchwald accords, arguing that while the aether played an important role in Fresnel's thinking the role was entirely heuristic and not '*generative* in a direct sense' (Buchwald [1989], p. 307, original emphasis). The evaluation involves no whiggish history, relying instead on an observation that could have been made by Fresnel. We might sympathize fully with those who could not conceive of waves that are unsupported. The inconceivability of unsupported waves, however, is not reason to suppose that positing the aether generated empirical progress.

Fresnel, and many who followed him in regarding light as a disturbance of the aether, produced theories of ever greater predictive adequacy and strength. A full narrative concerning how progress evolved, and which new insights were responsible for each instance, would be an enormous undertaking. However, what does seem apparent and suffices to make the current thesis worth further attention, is that nowhere does the actual commitment to the aether—once distinguished conceptually from the properties attributed to light waves—appear to have affected progress. Unless Fresnel, or anyone else, can argue that a particular constituent within some theory or model induced empirical progress, then the current thesis recommends withholding realist assent.

5.2 Phlogiston theory

The theory of phlogiston provides another seemingly compelling example for Laudan's objection. Phlogiston, after all, was a substance thought to be emitted during combustion, calcination, and respiration. The successes of the phlogiston theory were not, however, insignificant: theorists could explain

why differences in air supply enhanced or hindered combustion; why some metals displaced others within solutions; various results concerning the dissolution of metals in acids, and so on. In many cases these achievements were wholly unmatched by previous attempts to understand the properties of familiar substances and reactions. Phlogiston theory was better than the theories it replaced.

To identify the source of that progress we can again draw a conceptual distinction between claims made by the theory: first, combustion, respiration, and calcination are fundamentally the same process; second, some substances have a greater affinity for the principle of combustion than others; third, the underlying process involved the *emission* of some substance. This distinction makes possible the idea that the progress achieved by phlogiston theorists is attributable only to the first two of these claims. Pyle ([2000]) defends precisely this conclusion, arguing that the phlogiston theory 'taught chemists a lot of important and abiding truths, e.g. the fundamental identity of combustion, calcination, and respiration, the existence of a chemical balance in the biosphere between plants and animals, the possibility of transferring reducing power from one substance to another, and so on' (p. 107). For Pyle, however, the progress achieved was independent from the assumption that combustion is a process of emission.[23]

As in the case of the aether theories, the realist need not deny that belief in phlogiston had enormous heuristic value. Perhaps, as a consequence of earlier metaphysical commitments, the assumption that various reactions involved emission was almost inevitable for historical reasons. Perhaps, again due to historical contingencies, progress under this flawed assumption was a likely forerunner to Lavoisier's achievements. These historical speculations however, no matter how plausible, provide no reason for supposing that the phlogiston theory was better than earlier theories *because* of its commitment to a principle of emission.

The phlogiston theory was not approximately true. Nevertheless, historical considerations appear consistent with understanding the theory as more truth-like than any theory of oxidation reactions that had preceded it, and where the

[23] Both Schurz ([2009]) and Ladyman ([2011]) offer detailed and informative discussions of the phlogiston theory and its consequences for scientific realism. Schurz defends scientific realism by means of an analytic theorem, using the phlogiston theory, and various further historical cases as illustration of his more general thesis. Ladyman offers the phlogiston theory in further defence of the ontic version of structural realism that he has developed elsewhere (for example, Ladyman [1998], French and Ladyman [2003]). Both authors emphasize the similarity between core claims within phlogiston theory and modern chemistry, concerning the reactions that are now known as oxidation and reduction; both argue that the successes of the phlogiston theory can be understood as largely following from the discovery of these reactions and the relationship between them. The more general realist theses they defend differ in various ways from that which I've outlined here, although a more careful analysis of these differences lies beyond the scope of this article.

empirical progress achieved by the theory was a product of insights that have been retained, at least approximately. If empirical progress does not indicate increased truthlikeness then—in the case of the phlogiston theory—there should be some evidence that the commitment to combustion as a process of emission led to progress that was not achievable on the assumption that combustion involves absorption. In the case of Priestley's notable triumph, as outlined in Section 1 of his article, we have such a case: the phlogiston theory appeared better than alternative theories, insofar as only phlogiston theorists appeared able to explain the observations; the explanation relied on describing calcination, and similar processes, as a process of emission and thus the reverse process (whereby calces were transformed into metals) as a process of absorption.

The success stands as a genuine counterexample to the thesis that has been defended in this article; empirical progress was achieved in a manner that relied on a theoretical assumption that can no longer be regarded as approximately true.[24] The example is a paradigmatic illustration of the type of success that we should anticipate occurring frequently within the historical record, if we are mistaken in believing that empirical progress indicates greater truthlikeness. A single counterexample is inadequate to undermine my thesis. If 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, then historical considerations create little conflict for the idea that empirical progress is a product of theory lineages becoming increasingly truthlike. Antirealists who doubt my proposal on historical grounds are challenged to provide further examples that resemble Priestley's. Importantly, it is insufficient to observe that a successful theory included (even central) assumptions that are now regarded as false, if those assumptions were never favoured by empirically-based arguments for change.

5.3 Darwin, Galton, and Weissmann on generation and inheritance

To help ground his own inductive argument for antirealism, Stanford considers the theories of Darwin, Galton, and Weissman concerning biological

[24] Although Priestley's success is an undeniable counterexample to my positive thesis, its significance for antirealism appears to be limited. Within at most two years, Priestley recognised that the quantity of water increased during these experiments. To accommodate the observation, Priestley revised his phlogiston theory in two independent ways. Although the revised theory was consistent with the outcome of the experiment, including the increase in water, it is certainly arguable that Lavoisier had a more compelling explanation. Phlogiston theory thus very quickly lost the one empirical edge it had held over absorption-based theories of combustion (see Priestley [1785] and Toulmin [1957] for analysis). I'd submit 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.

generation and inheritance. Stanford argues that while each perceived possibilities of developmental processes that were ignored by his predecessors, each also defended theoretical claims that are now regarded as false. Furthermore, argues Stanford, these views were defended by these individuals on the basis that they could conceive of no alternative explanation. Darwin, for example, argued that development simply must involve the maturation of particles that were derived directly from parent cells, for no other theory of development was conceivable. Stanford concludes that the habit of defending scientific opinions on the basis that we cannot imagine how things could be otherwise can be shown through historical considerations to be an unreliable means of inference. Of course, this does not establish that when genuine empirical progress was achieved, the success was not a reliable indication that the novel posit responsible for progress was approximately true. In fact, on the basis of Stanford's analyses, the degree of empirical progress achieved by Darwin, Galton, and Weisberg, in terms of our understanding of ontogenic development, appears extremely minimal. The general point to observe, however, is that by construing success non-comparatively, realists and antirealists alike have not asked the right questions of past theories to evaluate whether empirical progress is a product of insights that were subsequently retained.

5.4 Stability across theory change

The thesis handles at least some historical cases that have been offered in opposition to the realist's inference from success. There are, of course, many further examples that deserve attention. As a response to antirealist arguments based on the history of science, I suggest only that the proposal is not obviously refuted. As with any selective realist thesis, however, beyond a defence of the realist's inference there is a positive aspect to the thesis which offers guidance concerning what constituents of a scientific theory a realist should endorse. Historical evidence in favour of the positive thesis emerges from studies that explicitly argue for stability across cases of theory change.

Kitcher defends cumulative interpretations of explanatory strategies that were developed within evolutionary biology and chemistry. Empirical progress is achieved, for Kitcher, as a result of refinements to scientific concepts and explanatory strategies. Those refinements are then endorsed by subsequent generations who themselves introduce additional reform. Such patterns fit neatly with the thesis I have outlined in this article.[25] Bain and Norton (2001) consider the history of the electron in the context of evaluating theory

[25] These cumulative patterns are not mentioned in Kitcher's development of his own selective realist strategy, where he understands the concept of scientific success non-comparatively. It as a result of conceiving of success in this way, I suggest, that he opens himself to the critical objections that Stanford articulates.

change as a challenge to realist attitudes. They note that theories of the electron have undergone significant and numerous changes over the past century; however, they also argue that '[a] closer look at the history reveals a sequence of theories in which an evergrowing, historically stable core of properties of the electron is discerned and in which the deficiencies of earlier theories are identified and corrected as our accounts of the electron are brought into ever closer agreement with the minutiae of experiment' (p. 453). The view again accords very well with the notion that *empirical progress* is what's relevant for reconciling history with the realist's impression that scientific success implies some measure of truth concerning unobservables.

6 Conclusions

Our understanding of the world is given to us piecemeal, where the order in which pertinent conclusions are accumulated is not always optimal. Perhaps science could have progressed by first sketching true and very general principles concerning the ontology of the natural world and the laws that govern it, then offering a sequence of theories which steadily improved upon representative accuracy, and where each new theory is immediately recognizable as a mere refinement of earlier views. This image jars violently with even the most rudimentary glance through the historical record. There is nothing conceptually problematic, however, with supposing that many scientific theories have included significant truths concerning unobservable entities, despite those models and hypotheses being embedded within broader theoretical frameworks that were fundamentally misguided. Science can become increasingly truthlike, even if our most basic assumptions are occasionally subject to radical modification.

Realists have traditionally assumed that our own theories are both approximately true and more truthlike than the theories they replaced. Contemporary realists and antirealists have lavished enormous attention on the former thesis, but often at the expense of any sustained engagement with the latter. Two of the central aims of this paper have been: first, to begin to address the imbalance; second, to argue that selective realist theses are more plausible, for both conceptual and historical reasons, when they are interpreted in terms of empirical progress rather than non-comparative scientific success.

The argument for the latter began with the observation that, as new theories achieve empirical progress over older theories, they introduce change, but only some of that change is responsible for the progress. On the hypothesis that empirical progress is an indication of increased truth-content, we can infer the approximate truth of the novel insight. If we further suppose that science is likely to cling to the (approximate) truths that it uncovers, then we should predict that, where science has achieved progress, the parts of theories

responsible will subsequently be retained. Evidence for the retention of such insights within current theories would not only reconcile the history of science with at least a convergent realism, but provide a positive argument in favour of the selective thesis. We would not thereby be justified assuming that our theories are approximately true; we would be justified believing that they are more truthlike than those that preceded them. Realism can distinguish itself from antirealist alternatives by anticipating patterns of accumulation that can be verified through historical analysis.

Better articulation of the sense of progress being employed is required, as is more careful treatment of the historical evidence. If the argument for convergent realism presented here is successful, however, it will represent a significant advance in the realism debate.

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