**Should a Historically Motivated Anti-Realist Be a Stanfordite?**

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Abstract: Suppose one believes that the historical record of science provides good evidence against scientific realism. Should one adopt Kyle Stanford’s specific version of this view, based on the Problem of Unconceived Alternatives (PUA)? I present reasons for answering this question in the negative. In particular, Stanford’s challenge cannot use many of the *prima facie* strongest pieces of historical evidence against realism, namely: (i) superseded theories whose successors were explicitly conceived, and (ii) superseded theories that were not the result of elimination-of-alternatives inferences. Attempts to accommodate (i) and (ii) within Stanford’s framework are incompatible with other commitments Stanford holds, such as anti-realism being piecemeal instead of global. As separate lines of criticism, I argue that there are problems with both Stanford’s claim that the PUA is the most important challenge to realism, and with his view of instrumentalist theory endorsement.

**1. Introduction**

Many opponents of scientific realism appeal to the history of science as evidence for their position. The most important recent development in this tradition is probably Kyle Stanford’s Problem of Unconceived Alternatives (PUA), which underwrites his New Induction (NI) over the history of science. According to the PUA, “our cognitive constitutions or faculties are not well-suited to exhausting the kinds of spaces of serious alternative theoretical possibilities from which our fundamental theories of nature are drawn” (2006, p. 45). In other words, in ‘fundamental’ scientific theorizing, scientists lack the cognitive ability to devise all plausible hypotheses that would explain the evidence available to them. The NI states that historical scientists often failed to exhaust the space of scientifically respectable hypotheses that would explain the data available to them at that historical time; therefore, present scientists are probably failing similarly. For example, General Relativity can explain all the data that was available to Newton, but Einstein’s theory was not conceived until the early 20th Century. Stanford claims this creates a problem for realism, because many fundamental scientific theories are inferred via elimination of alternatives (also known as ‘disjunctive syllogism’) (2006, p. 28). In an elimination-of-alternatives (more briefly, ‘eliminative’) inference, a supposedly exhaustive list of hypotheses (*H1* ∨ *H2* ∨ … ∨ *Hn*) is proposed, and all are eliminated (¬*H2*, … ¬*Hn*) except one (*H1*); we conclude the single remaining hypothesis is correct. But the NI provides a historically based reason to believe that, at least for ‘fundamental’ domains of scientific theorizing, the list of hypotheses probably does *not* contain a true hypothesis, so the disjunction is probably untrue. Therefore, in those fundamental domains such an argument would be unsound, and thus we lack sufficient evidence to believe scientific theory *H1* is true. Call this the *PUA-based argument against realism*.

Many critics of Stanford’s position reject anti-realism. The present paper takes a different approach: suppose one *is* moved by how many scientific theories have been consigned to the ‘dustbin of history,’ and accordingly wishes to be a historically motivated anti-realist. Should one accept Stanford’s particular version of this general position? This essay presents reasons for a negative answer: the PUA-based argument against realism omits much of the best historical evidence against scientific realism, and as a result delivers an unnecessarily restricted version of anti-realism. In particular, many discarded theories that are *prima facie* strong evidence against realism involve either *conceived* alternatives or non-eliminative (‘projective,’ in Stanford’s terminology) inferences. Section 2 describes historical examples of such conceived alternatives. Section 3 lists theories that were inferred projectively, but were later discarded. Along the way, I argue that the most natural attempts to accommodate these cases to the PUA either fail on their own terms or contravene some of Stanford’s other commitments.

Now, Stanford might immediately reply that he can simply accept my claims in §§2-3 without contradiction, while maintaining the NI and PUA. I accept that the NI and PUA, considered in isolation, are consistent with §§2-3. However, section 4 argues that some of Stanford’s other central claims *are* in tension with those points. In particular, someone who accepts the main contentions of §§2-3 must reject Stanford’s claims about the importance of the PUA, and relinquish the existence of an epistemic distinction between projective inferences and eliminative ones—the distinction Stanford relies on to make his instrumentalism piecemeal or selective, instead of global. Section 5 presents a different set of potential problems for another aspect of Stanford’s position, specifically, his claims about the proper cognitive attitude an anti-realist should take towards our current best theories. I argue that his key claims are either untenable taken on their own, or collapse into the Constructive Empiricist’s view of theory acceptance.

**2.** **The Problem of *Conceived* Alternatives**

I grant that the NI is evidence against realism. However, if a historically motivated anti-realist restricts her evidence to cases where the alternatives to the prevailing theory were unconceived, then she omits some of the *prima facie* best historical evidence for anti-realism. Much of the most compelling evidence for historically based anti-realism involves cases where the successor theory was conceived explicitly—and explicitly rejected at that earlier time as inferior to the now-discarded theory.

*2.1. Examples*

A set of three interrelated examples of conceived alternatives appears in Book I of Ptolemy’s *Almagest*. In I.5, Ptolemy argues that the Earth must be at the center of the universe, by assuming for reductio that it is not, and deducing claims that contradict accepted observations (specifically, the observation that an observer anywhere on the Earth always sees half of the zodiac). In I.7, he considers the hypothesis that the Earth is moving from one place to another, and argues that it is impossible, given the arguments in I.5 (for if it were moving from one place to another, it could not be at the center all the time). The absence of stellar parallax is further evidence that the earth does not move from one place to another. Later in I.7, Ptolemy considers the hypothesis that the Earth rotates on its axis. He admits that this hypothesis is consistent with the celestial phenomena, but argues that no version of this hypothesis is consistent with terrestrial phenomena.

These three hypotheses—that the Earth is not at the center of the universe, has a translational motion, and rotates daily—were explicitly conceived by Ptolemy. He rejected each of them because he thought the balance of the evidence told against them. This shows that even when scientists evaluate a hypothesis that later scientists will come to accept as superior, the earlier scientists can reject it. The NI cannot appeal to this case, or ones like it, as evidence against realism, since these cases involve *conceived* alternatives that are later accepted by the scientific community.

These three Ptolemaic hypotheses are not isolated instances. The hypothesis that the heat of a body consists in the motion of that body’s parts was in the same situation from the early-to-mid 1700’s until about 1840.[[1]](#footnote-1) During the 17th Century, several luminaries of the Scientific Revolution defended the view that heat was the motion of component corpuscles. However, as the 1700’s progressed, many leading researchers of the time came to regard this hypothesis as inferior to the view that heat was some sort of substance or material.[[2]](#footnote-2) These material caloric theorists had certainly considered the view that heat was motion of the constituent particles—since they had read, and were reacting to, the mechanical philosophers of the previous generation—but they rejected that earlier view. Enlightenment caloric theorists lodged several arguments against the mechanical view of their predecessors (Brush 1976, pp. 28-30); perhaps most compelling was the fact that we can observe heat diffusing across a vacuum, but of course the motion of particles cannot be transmitted across a space containing no particles.

There are further examples, such as the mutability of species. Linnaeus concisely argues that “no new species are produced” (1964 [1735], p. 18), arguing against the successor hypothesis that new species are created. Wegener’s theory that the Earth began with a single *ur*-continent, followed by subsequent continental drift, provides another example. Several other candidates for further examples are considered in (Hook 2002), a collection on Gunther Stent’s notion of a ‘premature’ hypothesis; (Barber 1961) contains a classic list of hypotheses that were considered, rejected, and later accepted. To generalize from these cases: we often find scientists in the historical record providing reductio ad absurdum arguments whose opening assumption for the purposes of contradiction is a successor theory, or a corollary to the successor theory. In sum, restricting focus to the problem of *un*conceived alternatives, as Stanford does,shrinks the body of *prima facie[[3]](#footnote-3)* evidence available in support of historically motivated anti-realism—for each of these cases involves *conceived* alternatives to the then-dominant theory.

*2.2. Objection:* *These successor theories* areun*conceived, and thus are examples of the PUA*

Stanford might claim that these historical episodes *do* instantiate the PUA. I can imagine two possible grounds for this: (a) at the earlier time, the eventual successor theories were not conceived in full detail. (b) The above presentation treats theories too atomistically; if instead the unit of analysis is the whole set of related hypotheses brought to bear on the phenomena, then these cases instantiate the PUA, since the whole set of successor theories was *not* conceived at the earlier time.

If Stanford urges (a), he presumably would point out that e.g. Ptolemy does not consider Newton’s specific model of the universe in all its detail, and therefore concludes that Ptolemy’s case is part of the inductive base for the NI. A similar objection could be lodged against material caloric theorists’ explicit consideration of the view that heat is motion: the scientific revolutionaries’ view is less specific than the theory that would later supplant the material caloric theory, viz. the kinetic theory. Thus, the successor theory had not been truly conceived (since the predecessor lacked the successor’s full detail), and therefore this case is also part of the NI’s inductive base.

First, I agree that these conceived,-rejected,-then-accepted theories were often not originally conceived in the complete detail of the actual successor. However, this does not establish that the NI can use these cases as part of its inductive base. For a realist about the successor theory would say that the previously rejected theory (e.g. ‘The Earth rotates diurnally’) was nonetheless true, even if this earlier version is not maximally specific. Therefore, Stanford’s PUA-based argument against realism would founder, since the list of alternative hypotheses considered at the earlier time *does* contain a true (though admittedly not maximally detailed) hypothesis. Second, for certain historical examples, this lack-of-specificity objection is factually incorrect. For example, Daniel Bernoulli *had* proposed a theory very similar to the modern kinetic theory of gases in 1738 (Brush 1976, p. 20), a century before the kinetic theory was widely accepted.

A Stanfordite who appeals instead to (b) (viz. the objection from holism) would stress that some of Ptolemy’s arguments against the Earth’s diurnal rotation use parts of Aristotelian physics for premises. Thus, the appropriate ‘alternative’ hypothesis here is not the bare claim that the Earth rotates (which this Stanfordite grants was conceived in Ptolemy’s time), but rather the conjunction of this claim and the relevant parts of Newtonian or general relativistic dynamics. And those larger conjunctions *were* unconceived by Ptolemy, so this more holistically-conceived theory would be part of the inductive base for the NI.

This deserves two replies. First, although Ptolemy’s arguments against the Earth’s diurnal rotation appeal to Aristotelian physics, his other arguments do not. For example, the arguments from lack of stellar parallax and the fact that every observer sees half the zodiac are independent of Aristotelian physics; they only rely on celestial phenomena. Second, more generally, this objection is in tension with Stanford’s professed aversion to confirmational holism. The ‘alternatives’ that were unconceived in this response are not individual hypotheses, but whole conglomerations of theories. I will not weigh the pros and cons of confirmational holism here, but Stanford himself expresses anti-holist sentiments (2006, §2.2).[[4]](#footnote-4) However, he does not declare conformational holism definitively false, so Stanfordites might well accept such holism to rescue their view from objections.

Before beginning the next section, I should first clarify my use of the word ‘Stanfordite.’ Despite its appearance in the title of this article, I take no stand concerning what criteria are necessary and sufficient to qualify as a Stanfordite. One need not accept every claim in Stanford’s corpus to be a Stanfordite. In particular, one need not believe those claims about which Stanford himself expresses some uncertainty; this is why a holist about confirmation can be a Stanfordite. I use the term primarily to distinguish Stanford’s position from those positions that Stanford claims are importantly distinct from his view—in particular, from the ‘classic’ pessimistic induction(s) (most often associated today with Laudan) and van Fraassen’s Constructive Empiricism. So, in the way I am using ‘Stanfordite’ here, you could reject many of Stanford’s assertions and remain a Stanfordite, but only if those rejections did not convert you into e.g. an adherent of the classical pessimistic induction (from which Stanford distinguishes his view). There is most likely a continuum of positions between the conjunction of claims in *Exceeding our Grasp* and that of the classic pessimistic induction, and I have no interest in drawing a precise line distinguishing the Stanfordite and the classic pessimistic inductor. This may seem potentially inconsistent with my titular question, but the aim of the arguments in this paper is to show that: (1) Stanford’s position suffers from some problems that the classic pessimistic induction does not, and (2) amending Stanford’s position to save it from those objections pushes one much closer to the classic pessimistic induction. But, for example, this paper contains no argument that the PUA and NI fail. Rather, the view is that the PUA leaves out important evidence that many historically motivated anti-realists have used, and if one adds that evidence to the PUA and NI, one’s position will be much closer to the classical pessimistic induction.

**3. The Problem of Unconceived *Unrepresentativeness***

This section argues that many famous examples of discarded historical hypotheses resulted from projective inferences, not eliminative ones.[[5]](#footnote-5) These projective inferences were often problematic because inquirers did not realize their samples were unrepresentative of the total population in relevant ways; some variable that was not previously recognized as relevant was in fact later found to be relevant, or there was some other sort of unrecognized heterogeneity in the target population. To mimic Stanford’s terminology, we might call this the ‘Problem of Unconceived Relevant Variables,’ or the ‘Problem of Unconceived Unrepresentativeness.’ Stanford’s PUA-based argument against realism (which only applies to eliminatively-inferred theories) cannot use these cases as evidence against realism, since they do not involve eliminative inferences—even though they are *prima facie* excellent evidence for a historically based case against realism.

*3.1. Examples*

One famous example of such a case is the Galilean velocity-addition law being superseded by the Lorentz transformations at the beginning of the 20th Century.

The Galilean velocity-addition law is

*x*′ = *x* − *vt*

(where *v* is the relative velocity between the observer and the moving object). The corresponding Lorentz Transformation is:

*x*′ = (*x* − *vt*)/ √(1 − *v*2/*t*2)

Length contraction and time dilation can be derived from this. The Galilean velocity-addition law (and its corollary, that the length of a rigid body is independent of its frame of reference) was presumably, for most scientists from the 17th to the end of the 19th century, seen as the conclusion of a projective argument (Newton says as much in the General Scholium, at least if we interpret a claim’s being “rendered general by induction” as a type of projective inference). So the Galilean velocity-addition law is a discarded claim that is not the result of an eliminative inference, and thus one that Stanford cannot appeal to as part of the inductive base for the NI.

The classical hypothesis that the ‘fixed’ stars are eternal provides a second example of this general type. This hypothesis was widely held, presumably on projective grounds, until there was sufficient data to demonstrate that what we today call ‘novas’ were not changes in the Earth’s upper atmosphere. A third example is the discovery of superconductivity. Before Heike Kamerlingh Onnes discovered Mercury’s superconducting state in 1911, scientists projectively inferred that a body’s heat capacity is proportional to its temperature, and its electrical resistivity is proportional to temperature cubed, for all temperatures. Onnes observed that below a certain critical temperature, both quantities instead quickly approached zero.

*3.2 Objection: These cases are evidence for the NI*

An objector might suggest that these are simply paradigm examples of the PUA in action: Enlightenment scientists didn’t *conceive* of the possibility that the length of a moving body is inversely proportional to √(1 – *v*2/*c*2). 19th Century scientists didn’t conceive that, for certain materials, there is some temperature below which that material’s heat capacity decreases very rapidly to zero, instead of linearly. Of course, I grant that historical scientists did not conceive of these currently accepted hypotheses. However, if Stanford holds that these cases are part of the NI’s inductive base, then he must relinquish some of his other commitments; in particular, he must either (a) claim that projective inferences are subject to the PUA, or (b) hold that these apparently projective inferences are actually eliminative inferences in disguise. Stanford cannot accept either (a) or (b) without giving up some of his other core claims; specifically, the claim that his anti-realism is “piecemeal” (2006, p. 48) or selective, instead of global.

Suppose Stanford accepts (a), allowing that the examples presented in this section are projective inferences, but claims that projective inferences can be part of the NI’s inductive base. I will argue that Stanford cannot do this without frustrating his desire to draw a significant epistemic difference between projective and eliminative inferences (2010; 2011; 2006, p. 39). What is this significant epistemic difference, according to Stanford? He writes: “a consensus in favor of a given theoretical scientific belief should be regarded with considerably more suspicion when the evidence we have in support of that belief is exclusively or even just centrally abductive in character,” as opposed to projective (2010, p. 234). (Terminological note: for Stanford, ‘abductive evidence’ constitutes the premises of an eliminative inference.) This is the core of what we could call Stanford’s ‘selective instrumentalism’: “the *limited* skepticism thus motivated [by the NI] should certainly not extend to every scientific claim or hypothesis and may even have different force as applied to the scientific exploration of different domains” (2006, 37; my emphasis).[[6]](#footnote-6) And Stanford also uses this distinction when he justifies his belief that microscopic organisms exist[[7]](#footnote-7) by appealing to the fact that we have “evidence of a non-eliminative character” of their existence (2006, p. 35; see also p. 33). The fact that microscopic organisms are not eliminatively inferred would not be evidence for their existence, unless Stanford also holds that we should be realists about (empirically successful, widely adopted) theoretical claims that are *projectively* inferred.[[8]](#footnote-8) But if the PUA applies to projective inferences, then we should not be realists about the conclusions of projective arguments. Thus, it appears Stanford can only use reply (a) by giving up his ‘limited,’ selective, or “piecemeal” instrumentalism. And if he does that, then his view collapses back into the classical PI (or something very similar) (Magnus 2010, §3), at least in terms of how much ‘suspicion’ we should cast over our current scientific theories, i.e. the cognitive attitude we take towards scientific theories that we endorse. There could still be other differences with the old-fashioned PI. Specifically, even if the extent of suspicion of current scientific theories is the same for this kind of Stanfordite and e.g. someone who follows Laudan (1981), this Stanfordite’s *reasons* for that amount of suspicion—namely, the PUA (extended to cover projective inferences)—could differ from a Laudanite’s reasons (or lack thereof).

Suppose Stanford instead pursues (b), claiming that the above examples are actually eliminative inferences in disguise. Then there are two possibilities: either all apparently projective inferences are actually eliminative inferences, or there is something special about these particular apparently projective inferences that makes them eliminative inferences in disguise. If the first, i.e. all apparently projective inferences are actually eliminative inferences, then the distinction between projective and eliminative inference disappears again, and with it Stanford’s piecemeal, limited skepticism. So there must be something about these *particular* apparently projective inferences that make them eliminative. However, this appears untenable for Stanford as well. For he says that because one can “reframe” *any* projective inference as an eliminative one (2010, p. 234), the proper way to draw the line between projective and eliminative inference is by distinguishing inferences that are “*amenable* to construal as a kind of inductive projection” from those inferences that are not amenable to such construal (2010, p. 235). That is, Stanford thinks inferences that cannot be couched as projective inferences should be viewed with ‘considerably more suspicion’ than those that can. This seems to rule out the ‘eliminative inference in disguise’ route for Stanford, since all that is required for an inference to be projective in the sense Stanford considers epistemically important is that the inference *can* be framed as projective—and the above examples certainly can be framed as projective.

**4. Can’t Stanford accept the PI?**

The previous two sections aimed to show that a proponent of the NI cannot appeal to many of the ‘Greatest Hits’ in the standard historically-motivated anti-realist’s catalogue, without giving up some of Stanford’s other central commitments. Stanford might respond that he can agree with everything in the previous two sections along the following lines. Chapters 6 and 7 of *Exceeding Our Grasp* defend (more or less) the old-fashioned PI from realist criticisms. And Stanford himself says: “I view the problem of unconceived alternatives *not* as *competing* with the traditional challenges of underdetermination and the pessimistic induction so much as bringing out what was most significant and compelling about those challenges to begin with” (2006, p. 45; my emphasis). I agree that the PUA and the NI are consistent with the PI: historically based anti-realists can make the new induction, but keep the old. However, accepting the PI is inconsistent with Stanford’s piecemeal instrumentalism (4.1). Furthermore, I disagree with Stanford’s claim that the PUA captures ‘what was most significant about the PI to begin with’ (4.2).

*4.1.* *Accepting the PI is inconsistent with Stanford’s piecemeal instrumentalism*

The argument for the claim in the above sub-heading is straightforward, since Section 3.2 above presented all the pieces. Stanford wants his criticism of realism to be selective or piecemeal. This is achieved by casting “considerably more suspicion” on widely accepted, empirically successful fundamental theories that are eliminatively inferred than those that are projectively inferred. Yet §3 showed that many theories that are evidence for the PI’s inductive base were projectively inferred. Thus, anyone who accepts the PI as a persuasive argument against realism cannot (in the absence of special pleading) simultaneously endorse Stanford’s brand of piecemeal skepticism about scientific theories. Nothing I have said is evidence against there being some other sort of piecemeal antirealism; I have only argued that the projective/ eliminative distinction should not be the basis for selecting which pieces of scientific theory merit a realist attitude.

*4.2*. *Stanford over-values the PUA-based argument against realism*

Stanford claims the PUA poses the most important problem for scientific realism. He writes: “the problem of unconceived alternatives… lies at the *heart* of *any* serious objection to scientific realism” (2006, p. 200; my emphasis). This is not an isolated moment of over-exuberance; we find similar remarks elsewhere: “it is our vulnerability to the problem of unconceived alternatives … that is *most significant* to the debate over scientific realism” (p. 23; my emphasis), and “the problem of unconceived alternatives poses the *most serious* challenge to believing the claims of contemporary scientific theories” (p. 39; my emphasis). In short, Stanford considers the PUA-based argument against realism the best and most important argument against realism.

Let us examine each of Stanford’s quoted claims in turn. First, consider the assertion that the PUA ‘lies at the heart of any serious objection to scientific realism.’ If so, then if the PI is a serious objection to scientific realism (which Stanford appears to believe), then the PUA is at the heart of the PI. I do not know precisely what Stanford means by ‘*X* is at the heart of any *Y*,’ but presumably it entails ‘*X* is a necessary condition for *Y*’ (since a heart is necessary for a chordate’s life to continue). Thus, if the PUA disappeared, then there would be no serious objections to scientific realism—not even the PI. I think this is incorrect. Sections 2-3 presented classes of historical cases that are evidence for the PI but not for the NI. This shows that there is at least some evidence against realism that is independent of the PUA. So, one might infer directly that the evidence in 2-3 suffices to demonstrate that the PUA is *not* necessary for ‘any serious objection to realism.’

However, Stanford could conceivably reply that any historically based argument against realism that omitted every historical case that was not an example of the PUA is not ‘serious,’ perhaps on the grounds that it would simply leave out too many important now-discarded scientific theories. I take no position on whether there are enough cases in the historical record that instantiate the problem of conceived alternatives, or the problem of unconceived unrepresentativeness, to pose a ‘serious’ challenge to realism—primarily because I do not wish to argue about what the threshold number is for a challenge to be ‘serious.’

Nonetheless, this reply can still be answered. To say that the PUA is a necessary condition for any serious objection to realism means that if the PUA disappeared, then all the serious objections to realism would also disappear. So let us imagine that, for historical episodes that actually involved unconceived successor theories, all the later successor theories *had* instead been conceived at the earlier time. For example, imagine that quantum mechanics and general relativity had both been explicitly formulated in 1750. Now the question is: can a serious objection to realism be posed in this hypothetical alternative history? And the answer appears to be *yes*: as Magnus (2006) and Saatsi (2009, p. 359) have pointed out, given the evidence available to scientists in 1750, Newtonian mechanics is better confirmed than general relativity and quantum mechanics. (No independent evidence available in 1750 suggests that the laws of motion are drastically different at scales outside what was detectable in 1750.) So scientists in 1750 (if they are rational) would have still accepted Newtonian mechanics, even if the PUA never existed. If something similar holds for many other historical cases that actually involved unconceived successor theories, then the PUA is not necessary for a serious objection to realism (unless there is *no* serious, historically based objection to realism—a route obviously unavailable to anyone who wants to be a historically motivated anti-realist).

Let us now turn to Stanford’s claims that the PUA is the “most significant” or “most serious” challenge to realism. I believe it may be irresoluble whether the PUA or the PI is more important, since the disputants may lack a shared standard of significance or seriousness that could decide the question. But since Stanford makes this claim, I will address it. I see at least two reasons to resist it. First, if an argument leaves out much of the *prima facie* evidence for a conclusion, and correspondingly settles for a more restricted conclusion (which is the upshot of sections 2-3 with respect to the PUA-based argument against realism), then that argument is *prima facie* less important than a similar argument that draws upon more of the relevant evidence, and accordingly establishes a wider conclusion.

Second, one could argue that ‘problem of *conceived* alternatives’ cases, like the geo-eccentric hypothesis, pose a more serious problem for realism than cases that instantiate the PUA. Why? Cases like Ptolemy’s considering the Earth to be in motion and Daniel Bernoulli’s kinetic theory of gases show that even when we explicitly consider the correct theory—i.e. the truth is ‘staring us in the face’—we benighted humans can still reject it. In my opinion, that makes us look even more inept at detecting approximate truth than an inability to conceive of a true theory, when that recherché true theory is conceptually distant from our current list of live options. To be clear, I do not think these two points conclusively establish that the PI is more significant than the PUA (whatever ‘significance’ comes to). However, they do present a challenge that a Stanford must answer, if he wishes to maintain that the PUA presents the most significant challenge to realism.

**5. Stanford’s view of theory endorsement**

This section considers an entirely different concern an anti-realist might have with Stanford’s view. One question that critics of realism attempt to answer is: ‘What should be an anti-realist’s cognitive attitude towards scientific theories that she endorses?’ This is an important question, because a realist might challenge anti-realists’ intellectual right to use a theory for prediction, intervention, and control that they do not fully believe to be true.

*5.1: Stanford’s first account of theory acceptance*

Stanford’s answer to this question, following Fine, is that the anti-realist’s attitude should be that the theory is *reliable* “across the board” (2006, p. 198). The obvious next question is: what does Stanford mean by calling a theory ‘reliable’? Stanford states that “our fundamental commitment to the reliability of a given theory in turn commits us to the truth of whatever implications it may have for entities, events, and phenomena as they are conceived outside of the theory itself” (p. 199). Stanford spells out this idea as follows: “when our theories carry implications concerning features or aspects of entities, events, and phenomena to which we have some independent route of epistemic access, our belief in the instrumental reliability of those theories leads us to expect these implications to turn out to be strictly and literally true” (p. 199). This clarifies the previous quotation in two ways: first, Stanford’s conception of ‘truth’ is not deflationary or revisionary (rather, it is ‘strict and literal’). Second, and more importantly for present purposes, to ‘conceive a theory’s implications outside of that theory’ is to “have some independent route of epistemic access” to those implications, other than that theory. The immediate question is: which implications of a particular theory can be conceived outside that theory itself? That is, what are these ‘independent routes of epistemic access’? Here is Stanford’s answer:

observation and other perceptual processes will typically play an important role for instrumentalists in fixing beliefs about the natural world… because our various sensory modalities will characteristically be among the routes most commonly used by us to secure a grasp of entities, events, and phenomena in ways that are independent of the theories towards which we adopt instrumentalist attitudes. (p. 202)

This formulation contains multiple qualifications (‘typically,’ ‘characteristically,’ ‘most commonly used’)[[9]](#footnote-9); in another formulation, Stanford states this point without such qualifications: “the instrumentalist will have to frame what she actually believes… in terms of the entities, events, and phenomena familiar from our everyday experience of the middle-sized bodies of common sense” (p. 202).[[10]](#footnote-10)

*5.2: A dilemma: Stanford’s first account of theory acceptance collapses into that of the consilience-based realist or the constructive empiricist*

Now that we have seen Stanford’s basic position on the cognitive attitude anti-realists should have towards a theory they endorse, I can articulate my concern. Regardless of whether we use the qualified (‘typically’ etc.) or unqualified version of Stanford’s account of independent routes of epistemic access, Stanford’s view of theory endorsement collapses into another view he does not want to accept. On the qualified version, Stanford’s view of theory endorsement threatens to collapse into a prominent form of realism; while on the unqualified version, Stanford’s view of theory endorsement threatens to collapse into that of the Constructive Empiricist.

Let us begin with the qualified case first. I will argue that this fails, because it would lead Stanford to believe exactly the sorts of theories that the PUA-based argument against realism alleges we should not believe are true. Suppose some theory *T* entails claim *p*, and ‘our various sensory modalities’ do not grant us any independent access to *p*. Further suppose that there is some other route of independent access to *p* that is *not* observational. (I am supposing that ‘non-observational’ could include heavily theoretically-mediated ‘observations,’ such as cases like the Large Hadron Collider allowing us to ‘observe’ particle trajectories.) So on Stanford’s proposal above, an anti-realist who endorses both *T* and this other route should believe *p* is ‘strictly and literally true.’ However, this other route will be (in some broad sense) theoretical or (at least) heavily theoretically motivated, since in the lexicon of the scientific realism debate, ‘theoretical’ is usually[[11]](#footnote-11) considered ‘non-observational.’ But if *T* and this other route both generate *p*,then we have converging or consilient theoretical reasoning leading to *p* from these multiple independent sources. And this appears very similar if not identical to a prominent species of scientific realism, one which stretches back to William Whewell: we should be realists about those claims that exhibit consilience (Snyder 2005, Salmon 1984, Achinstein 2002); Chakravartty calls this the ‘corroboration’ argument for realism (2011, §2.2). That is, if two independently developed theories or experimental protocols agree on some non-observational claim, then we should believe that non-observational claim. Therefore, if Stanford holds that there are (‘atypical,’ ‘uncommon’) non-observational routes of independent epistemic access, then it appears he would have to strictly and literally believe the same parts of theories that a (prominent species of) realist would believe.[[12]](#footnote-12) Of course, Stanford does not believe any theories which are susceptible to the PUA-based argument against realism—so the qualified horn of the dilemma is not available to him.

To avoid falling into consilience-based realism, Stanford could instead adopt what I called the ‘unqualified’ view above: drop the ‘typically,’ ‘characteristically,’ etc., and assert that observation is the *only* independent route of epistemic access. (Correlatively, Stanford could assert that the only theory that is genuinely independent of higher scientific theories is what he calls the “hypothesis of bodies of common sense” (p. 201).) Unfortunately, if Stanford makes this maneuver, then he has a different problem: his view of anti-realist theory endorsement effectively collapses into van Fraassen’s, which Stanford explicitly wants to avoid. As is well-known, the Constructive Empiricist holds that “*acceptance of a theory involves as belief only that it is empirically adequate*,” and that “a theory is empirically adequate exactly if what it says about the observable things and events in the world is true” (van Fraassen 1980, p. 12). So for van Fraassen, when a Constructive Empiricist accepts a theory, she believes all the observable consequences of that theory are true. It is possible to be a Constructive Empiricist and believe some *un*observable consequences of a theory, but those beliefs go above and beyond what is required for Constructive-Empiricist theory acceptance: van Fraassen calls them supererogatory beliefs (van Fraassen 1985, p. 255; 2001, p. 168).

So on Stanford’s unqualified position, observation is our only route of theory-independent access to the world, and we should believe some consequence of a theory is strictly and literally true if we have independent epistemic access to that consequence. Thus, Stanford will believe all the observable consequences of a scientific theory—just like the Constructive Empiricist. However, Stanford wants to distance himself from this aspect of van Fraassen’s position (2006, p. 34): for example, Stanford states that he (strictly and literally) believes that microscopic organisms exist (p. 33, p. 35).[[13]](#footnote-13) But on the unqualified view, it is unclear how Stanford could ‘strictly and literally’ believe that unobservable organisms exist (I assume Stanford considers microscopic organisms unobservable). In sum, Stanford faces the following dilemma: if there are other independent routes of epistemic access besides observation, then the portions of scientific theories he literally believes will match some version of corroboration- or consilience-based realism; if there are no other routes, then the portions of scientific theories he believes will be the same as the Constructive Empiricist’s.[[14]](#footnote-14)

Conceivably, a Stanford could resist this objection as follows. Nothing in what was said above rules out the possibility that some scientific theories could be generated by making some sort of ampliative inference from observations (or the hypothesis of bodies of common sense), and thereby arrive at a new theory that passes instrumentalist epistemic muster for being considered strictly and literally true; call this theory *T*′. Now imagine Stanford is determining which implications of an arbitrary theory *T* to believe on the ‘qualified’ version of Stanfordite theory-endorsement. He now has in hand what could be another independent route to the claims entailed by *T*, namely *T*′. And he has done this without accepting that *every* time consilience or corroboration occurs, he must strictly and literally believe the corroborated theoretical claim.

I concede that this objection is not unassailable: if Stanford can produce or specify such an ampliative type of inference that meets instrumentalist epistemic standards, then I will withdraw this objection. However, the most obvious candidate for such a type of reasoning in Stanford’s texts is projective inference; I do not see any other possibilities suggested. And as we saw in §3, the projective-eliminative distinction cannot bear the epistemic weight Stanford hoped it would.

*5.3: Stanford’s second account of theory acceptance*

Finally, Stanford might grant the objection in this section, and instead rely on another part of his description of instrumentalist’s cognitive attitude of theory-endorsement. Stanford writes:

the instrumentalist will take precisely the same attitude that the realist applies in particular contexts to theories she has specific reasons to disbelieve (like Newtonian mechanics)… and simply apply that familiar attitude much more broadly than the realist does. (2006, p. 205)

That is, the instrumentalist’s cognitive attitude toward theories she currently accepts is the same as the realist’s cognitive attitude towards Newtonian mechanics and other theories that have been superseded, but are still used. This novel and interesting proposal can be regarded as independent from Stanford’s first account of anti-realist theory acceptance above, and thus immune to the preceding objection, since this proposal makes no mention of observation or other independent routes of epistemic access.

Unfortunately, this promising proposal faces significant difficulties. Imagine a realist today who competently uses Newtonian mechanics to achieve some practical goal, such as space travel. She believes that Newton’s laws will generate predictions that are good enough for her present practical purposes, but she also believes that, for some purposes, Newtonian calculations would deliver incorrect predictions—specifically, when dealing with velocities that are a sizable fraction of the speed of light, or bodies in strong gravitational fields, or at scales where quantum effects become significant. That is, in some domains and for some purposes, Newtonian mechanics is an excellent instrument, but in others it performs much worse than another available alternative—and our imagined realist believes classical mechanics has a limited domain of applicability because of observational evidence attesting to those limits. Compare this realist using classical mechanics to an instrumentalist who endorses a current scientific theory, i.e. the kind of theory a realist today would believe to be approximately true. This instrumentalist does *not* only apply this theory within a fairly well-defined domain; rather, as Stanford says, she uses it generally, since she believes our current theories “are nonetheless the best *presently available* places to look for guidance in planning our own engagement with the natural world” (p. 206). So, there is a difference between the realist’s cognitive attitude towards Newtonian mechanics and the instrumentalist’s cognitive attitude towards the scientific theories she endorses (but does not fully believe, in the way she believes the hypothesis of the bodies of common sense).

But the mere fact that some difference exists does not necessarily create problems for Stanford’s claim that ‘the instrumentalist will take precisely the same attitude that the realist applies’ to e.g. Newtonian mechanics, since the two attitudes could be species of a single genus of attitude. To show that these are different cognitive attitudes, we also need the premise that whether a cognitive attitude is *context dependent* is an essential aspect of its identity: the realist uses Newtonian mechanics only in a fairly well-defined set of contexts, whereas the instrumentalist uses our current best theories across all contexts. I will not offer a defense of this premise; instead, I will simply note that leading accounts of the distinction between the cognitive attitudes of *belief* and *acceptance* turn on context-dependency (though not in the philosophy of science literature): belief is context-independent, while acceptance is not (Cohen 1992, Bratman 1993, Engel 1998). However, if someone nonetheless still wished to maintain Stanford’s account of theory endorsement as the attitude realists take towards Newtonian mechanics, this premise would likely be the most obvious target to criticize.

**6. Conclusion**

A historically motivated anti-realist who only appeals to the PUA and NI unnecessarily limits both the evidence for her view and the scope of her anti-realist conclusion, since she omits all historical cases where either the successors were conceived, or the theories were the result of projective inferences. The most natural attempts to incorporate such cases into the PUA-based argument against realism fall afoul of Stanford’s other commitments, in particular his desire for a piecemeal anti-realism. Stanford’s claims that the PUA poses the ‘most significant’ challenge to realism faces difficulties, since (i) many superseded historical theories do not fit the pattern of the PUA, and (ii) even if humans were always able to exhaust the space of conceptual possibilities, the historical record would not appear very different (e.g. classical mechanics would still be accepted over general relativity and quantum mechanics in 1750). Additionally, Stanford’s account of theory-endorsement threatens to collapse either into consilience-based realism, or (something very close to) van Fraassen’s Constructive Empiricist view of theory-endorsement.

Finally, I should stress that nothing said thus far constitutes an all-things-considered judgment about whether a historically motivated anti-realist should accept Stanford’s views or not. Even if someone accepted every purported problem with Stanford’s position above, it is still rationally open to that person to say ‘Yes, I recognize that Stanford’s view has certain weaknesses, but every other version of historically motivated anti-realism has even worse problems, or lacks the strengths Stanford’s position enjoys.’ To respond to this, an alternative version of historically motivated anti-realism would have to be presented, and shown to have a better balance of strengths to weaknesses than Stanford’s version. That task must await another occasion.

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1. The cut-off date of 1840 comes from (Brush 1976, p. 27), but the decline was gradual: Metcalfe (1859) presents several criticisms of the hypothesis that heat is the motion of constituent parts. [↑](#footnote-ref-1)
2. Defenders of caloric sometimes admitted that the evidence did not fully demonstrate that heat was material. Joseph Black is typical: “Such an idea [*viz*.,material caloric] of the nature of heat is, therefore, the most probable of any I know; … It is, however, altogether a supposition” (Black 1803, p. 33). However, Black also says that the mechanical theory is “totally inconsistent with the phenomena” (p. 80). [↑](#footnote-ref-2)
3. The ‘*prima facie*’ is necessary, despite the fact that these cases typically appear on the historically motivated anti-realist’s list of superseded theories, because (as an anonymous referee stressed) one could conceivably argue that these historical cases are *not* actually good evidence for anti-realism. For example, one might hold that, given the current state of the realism debate, a discarded theory must have made *novel predictions* for it to be strong evidence against realism. For example, Psillos uses exactly this reasoning to dismiss Ptolemaic astronomy as evidence against his version of realism (1999, p. 105). There is obviously insufficient space here for a historically sophisticated discussion of this general concern. However, Carman and Díez (2015) argue that Ptolemaic astronomy in fact makes multiple novel predictions. And Chang (2003) among others provides evidence that the caloric theory also made novel predictions. [↑](#footnote-ref-3)
4. Stanford writes: “the extreme holist claim that all of our beliefs are confirmed solely by their inclusion in an interconnected web that accommodates experience well on the whole may already have seen its day. More recent epistemology and philosophy of science have witnessed calls for (and some proposals for) more nuanced accounts of confirmation recognizing the differential character of the evidence in support of different kinds of scientific claims” (2006, p. 39). [↑](#footnote-ref-4)
5. The view that all inductive inferences are, at root, disjunctive syllogisms has been defended (Montague 1906, p. 281). One could argue that the inference from *All A’s observed thus far have been B* to *All A’s are B* in effect eliminates any more complex explanation of why the observed A’s have been B, without explicitly listing all these more complex alternatives. Stanford himself mentions a similar possibility (2010, p. 234). [↑](#footnote-ref-5)
6. See (Magnus 2010) for discussion of the extent to which Stanford’s strategy is piecemeal. [↑](#footnote-ref-6)
7. Stanford believes this is important to his position, because on the issue of which parts of a theory an anti-realist should believe as literally true, it distinguishes his instrumentalism from Constructive Empiricism (2006, p. 34). (See Section 5 below.) [↑](#footnote-ref-7)
8. Stanford makes similar remarks about the ‘hypothesis of organic fossil origins’: “the vulnerability of the hypothesis of organic fossil origins to any serious version of the challenge posed by the PUA has been most dramatically reduced by the fact that we have managed to supplement the fundamentally abductive sorts of evidence long available in support of it with compelling further evidence that depends instead on a more straightforward sort of inductive projection” (2010, p. 221). [↑](#footnote-ref-8)
9. I cannot find, in Stanford’s text, an explanation of what the *a*typical, *un*common routes of epistemic access are, or what “the other perceptual processes” besides observation are. [↑](#footnote-ref-9)
10. This explains why Stanford says “observability is important, but only derivatively” (2006, p. 35): what is fundamentally important is access to the area of inquiry independent of the theory we are using, testing, or developing; observation is just the way our species manages to secure such independent access. For this reason, “there is nothing especially suspicious about scientific claims regarding unobservables *per se*” (p. 35). [↑](#footnote-ref-10)
11. But not always: see McMullin (2003) on ‘O-Theories.’ [↑](#footnote-ref-11)
12. One might think this is not a problematic consequence for Stanford, since he identifies himself as a piecemeal realist. This is still a problem for Stanford, because he is not a consilience-based realist: many consilience-based realists accept exactly the type of theory that the PUA-based argument against realism undermines. Many thanks to an anonymous referee for helping me think through this issue. [↑](#footnote-ref-12)
13. It should be noted that van Fraassen says that a Constructive Empiricist can draw the observable/unobservable line at a different place than where van Fraassen himself does, as long as *something* is classified as unobservable. Constructive Empiricism does not demand that e.g. the entities visible with e.g. a 10X magnification light microscope (but invisible to the naked eye) be classified as unobservable (2001, p. 163). [↑](#footnote-ref-13)
14. Stanford’s total position would still differ from van Fraassen’s, even if they agreed on which parts of theories an anti-realist must literally believe, since Stanford cites the PUA as a *reason* *why* observational claims are important to the anti-realist, whereas van Fraassen does not (though van Fraassen might happily accept this reason; it is similar to his ‘Best of a bad lot’ argument (1989, p. 143)). [↑](#footnote-ref-14)