

A Historically Informed *Modus Ponens* Against Scientific Realism: Articulation, Critique, and Restoration

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There are two primary arguments against scientific realism, one pertaining to underdetermination, the other to the history of science. While these arguments are usually treated as altogether distinct, P. Kyle Stanford's 'problem of unconceived alternatives' constitutes one kind of synthesis: I propose that Stanford's argument is best understood as a broad modus ponens underdetermination argument, into which he has inserted a unique (and doubly inductive) variant of the historical pessimistic induction. After articulating three criticisms against Stanford's argument and the evidence that he offers, I contend that, as it stands, Stanford's argument poses no threat to contemporary scientific realism. Nonetheless, upon identifying two useful insights present in Stanford's general strategy, I offer an alternative variant of the modus ponens underdetermination argument, one that, although historically informed by science, requires no inductive premises. I contend that this non-inductive but historically informed variant of the modus ponens clarifies and considerably strengthens the case against scientific realism.

1. Articulation: Underdetermination, History, and a *Modus Ponens* against Scientific Realism

Scientific realists claim that we can justifiably believe that our successful scientific theories are (at least approximately) true—truth, for the realists, being no less attributable to assertions about unobservables than to assertions about observables. There are two primary arguments against scientific realism, the argument from underdetermination and the pessimistic meta-induction. In a recent book (Stanford 2006b) and a series of articles (Stanford 2005, 2006a, 2006c), P. Kyle Stanford challenges scientific realism with an argument that, though differing from the standard versions of these

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arguments, shares key characteristics of each. Drawing on the insights of Pierre Duhem and Lawrence Sklar, Stanford dubs his general concern with realism ‘the problem of unconceived alternatives’. His argument calls for serious consideration, and I will critically assess it in section 2. First, owing to its complexity, I must articulate it with some care.

Stanford characterises realists as holding ‘that our best scientific theories are successful because the descriptions they offer of otherwise inaccessible domains of nature are at least probably and/or approximately true’ (Stanford 2006a, 121). He notes that, for realists, the justification for believing a theory rests on ‘abductive or eliminative’ arguments: ‘we think it offers the best available explanation for the empirical evidence we have and . . . we regard rival or competing explanations of that same evidence as convincingly eliminated or discredited’. Seeking to challenge both this foundation for realism and realism generally, Stanford looks to Duhem, who, he says, ‘eloquently noted long ago, such an abductive or eliminative inferential procedure will only guide us to the truth about nature if the truth is among the competing explanations or hypotheses we are considering in the first place’ (Stanford 2006a, 122). Duhem asks:

Do two hypotheses in physics ever constitute . . . a strict dilemma? Shall we ever dare to assert that no other hypothesis is imaginable? Light may be a swarm of projectiles, or it may be a vibratory motion whose waves are propagated in a medium; is it forbidden to be anything else at all? (Quoted in Stanford 2006a, 122)

Stanford writes, ‘What seems to have worried Duhem is the possibility that there might be equally well confirmed alternative hypotheses . . . we simply have not conceived of in the first place’ (Stanford 2006a, 122). Stanford takes this possibility to pose a serious threat to realism. If we have reason to believe that there are unconceived alternatives, ‘then we cannot responsibly infer that’ our ‘best or only explanation . . . is even likely to represent the truth of the matter’; ‘we cannot justifiably regard the products of . . . eliminative or abductive inferences as even probably or approximately true’ (Stanford 2006a, 143). John Stuart Mill anticipated this Duhemian concern. Regarding the theory that light is a wave travelling through an ether, Mill writes:

Most thinkers of any degree of sobriety allow, that an hypothesis of this kind is not to be received as probably true because it accounts for all the known phenomena, since this is a condition sometimes fulfilled tolerably well by two conflicting hypotheses . . . while there are probably a thousand more which are equally possible, but which, for want of anything analogous in our experience, our minds are unfitted to conceive. (Mill 1867, 296)

While this point differs in subtle ways from Duhem’s, let us note the similarities; and, setting aside how Mill and Duhem may have wished to qualify the following statement, let us dub it ‘the Mill–Duhem conditional’:¹

If we have reason to believe that *T* has unconceived alternatives, we are not justified in believing that *T* is probably or approximately true.

Employing this conditional as a premise, Stanford attempts to establish its antecedent, thereby offering a clean *modus ponens*—by which I mean hereafter an argument such

that, in its broad structure, it takes the form of a *modus ponens*—for the claim that we are not justified in believing that our present scientific theories are probably or approximately true.

Stanford sees his argument as a whole as posing a ‘much more serious threat’ than the ‘traditional arguments in support of underdetermination’ and ‘the pessimistic induction’ (Stanford 2006a, 122). However, a *modus ponens* containing both a Mill–Duhem conditional and an affirmed antecedent can fairly be said to stand as a variant of the underdetermination argument. Moreover, as we will now see, the argument Stanford invokes to secure the antecedent is, at least in structure, a variant of the pessimistic induction, another argument anticipated by Mill, who writes:

every age having held many opinions which subsequent ages have deemed not only false but absurd . . . it is as certain that many opinions, now general, will be rejected by future ages, as it is that many, once general, are rejected by the present. (Mill [1859] 1998, 23)

The standard contemporary pessimistic induction is often spelled out as follows:

PMI-A:

- (1) We now see that many successful past theories were false.
- (2) Therefore, we have reason to believe that our successful contemporary theories are false.

Notice that, given the premise, and given that contemporary science has replaced the past theories to which the premise refers, there were alternatives to each of those past theories, albeit (presumably) unconceived at the time. Hence the standard pessimistic induction entails another variant regarding alternatives:

PMI-B:

- (1) We now see that for many successful past theories there were alternatives.
- (2) Therefore, we have reason to believe that our successful contemporary theories have alternatives.

An argument of this sort may be what Lawrence Sklar has in mind when, reflecting ‘upon historical scientific experience’, he suggests ‘that there are innumerable alternatives to our best present theories’ that ‘would save the data equally well’ (Sklar 1981, 18–19). While Stanford’s attempt to support the antecedent is similar in both structure and content to PMI-B, he adds a key refinement to Sklar’s historical insight regarding alternatives:

If the historical evidence confirms that *past practitioners have indeed routinely failed to conceive of well-confirmed alternative hypotheses* of this sort that were sufficiently serious as to be actually accepted by later scientific communities, then *we have every reason to believe that there are similar alternatives to our own contemporary scientific theories* that remain *presently* unconceived, even if we cannot specify or describe them further. (Stanford 2006a, 123; my italics)

First, we see that the alternatives whose existence Stanford is concerned to reveal are both ‘sufficiently serious’ and ‘well confirmed by the evidence available at the time’.² Second, Stanford emphasises as a favourable and distinguishing feature of his

argument that it pertains to ‘the theorists rather than the theories of past science’. While it should be clear that the broader argument still pivots on claims about theories themselves, we can agree with Stanford that the ‘scope and character’ of the empirical success our present theories may achieve does nothing to imply that ‘today’s scientists are any better’ than ‘the greatest scientific minds of the past’ (Stanford 2006a, 123). And, remaining mindful of the sort of alternatives with which Stanford is concerned, we can insert his unique pessimism regarding theorists into this ‘new induction’, as he calls it, which resides in (2) and (3) below. Spelling out the argument:

- (1) If we have reason to believe that a contemporary scientific theory, *T*, has unconceived alternatives, we are not justified in believing that *T* is probably or approximately true.
- (2) We now see that, for many successful past theories, (a) scientists ‘routinely failed to conceive of’ alternatives, yet (b) there were such alternatives.
- (3) We ‘have every reason to believe’ that contemporary scientists also fail to conceive of alternatives (via induction from 2a).
- (4) We have reason to believe that *T* has unconceived alternatives (via induction from 2b, given 2b’s asserted correlation with 2a, and given 3).
- (5) Therefore, we are not justified in believing that *T* is probably or approximately true.

Step (1) of this argument is the Mill–Duhem conditional; steps (2) and (3) constitute Stanford’s new pessimistic induction, hereafter ‘PMI-C’. Step (4) affirms the antecedent of (1). Together these steps purport to establish the non-realist conclusion (5).

Notice that PMI-A and PMI-B above treat success not as something inferred but as something given. Hence, each contains only one inductive connection: from the falsity of (or the existence of alternatives to) past theories we infer the falsity of (or the existence of alternatives to) present theories. By contrast, in Stanford’s pessimistic induction, PMI-C, the claim that contemporary scientists fail is not given; instead, it is inferred. So not only is premise (4) grounded inductively, (3) is as well. In other words, in Stanford’s argument, there are two inductions binding (2) to (4): the first connects (2a) to (3); the second connects (2b) to (4)—via (2b)’s putative correlation with (2a) and the connection between (2a) and (3). Finally, we see that (4) is coupled with the Mill–Duhem conditional, (1), to round out a *modus ponens* towards (5). Putting all this together: I am inviting the reader to see this argument as a broad *modus ponens* underdetermination argument, into which Stanford has inserted his unique and doubly inductive variant of the pessimistic induction. (Although I will introduce a modified version of this argument below, the *modus ponens* will stand as a framework for each version; accordingly, I will refer to such arguments generally as *modus ponens*.) With the structure of Stanford’s argument clarified, we can now apply to it the critical attention it merits.

2. Critique: Three Concerns Regarding Stanford’s Argument

In this section I will introduce three key criticisms against, or concerns with, Stanford’s argument, briefly summarised here. First, Stanford’s ‘failure thesis’ (in 2a and 3) is

superfluous, if not false. Second, even setting aside that thesis, Stanford's evidence and argument, as they stand, miss the mark against contemporary realism. Third, Stanford's own argument against realism faces the very threat Stanford himself voices against standard underdetermination arguments: with or without the failure thesis, Stanford has not grounded his induction(s).

2.1. *The Falsity, or At Least Superfluity, of the Failure Thesis*

Consider first what we can call Stanford's 'failure thesis', found in (2a) and (3): because scientists of the past 'routinely failed to conceive of' alternatives to past theories, we have good reason to think that contemporary scientists fail in the same way. To eliminate this thesis is to discard what Stanford himself deems the distinguishing mark of his argument—a thesis about 'practitioners', about 'the theorists rather than the theories' (Stanford 2006a, 123)—and constitutes a return to Sklar's (1981) PMI-B as that which secures the Mill–Duhem conditional. Stanford's failure thesis in particular, then, calls for some attention.

An initial, natural interpretation of the failure thesis—the claim that the scientific community has failed or fails to generate alternatives—is that the scientific community made or makes a bona fide *effort* to conceive of alternatives but did or does not conceive of them. Natural though it may be, I submit that this interpretation renders the failure thesis false—not because scientists generally do develop alternatives, but because scientists are not, in the context of concern, pressed or even generally endeavouring to do so: the property of theories on which the realism debate hinges is success; the theories of concern are those that are successful; and when in possession of a successful theory, the scientific community does not, and many will claim should not, dedicate its time, energy, and resources to the development of alternatives.³ Rather than interpreting Stanford's ubiquitously employed failure thesis this way, then, we are prompted to take it to mean only (and perhaps less naturally)⁴ that *scientists did or do not conceive of alternatives*. Note that the expression 'alternative to T ' is meant by Stanford to denote, not just some random idea that may altogether conflict with the evidence, but a genuine alternative, a theory that could not have been eliminated by the evidence at the time, etc. And of course Stanford recognises that he cannot merely show that scientists did not think of such alternatives. What he must also show, and endeavours to show, is (2b), that there were alternatives to the historical hypotheses with which he is concerned. Securing (2b) is required not merely because, without doing so, the argument leaves us without grounds for believing that T has alternatives; I add that without securing (2b) realists would consider themselves invited to *deny* the existence of alternatives by claiming that the non-existence of alternatives stands as the best explanation for cases in which no alternatives were brought forward. It should be clear that merely showing that scientists did not and do not think of alternatives (establishing only 2a and 3) is altogether insufficient for securing (4). Nor, however, is it necessary, or even helpful. Whether the alternatives did or did not occur to some specific past scientist is irrelevant: conceived yet *ignored* alternatives pose no less a threat than those that are unconceived. Since

what matters is only that there have been alternatives, it looks as though Stanford's failure thesis—that past scientists 'routinely failed to conceive of well-confirmed alternative hypotheses'—does little more than distract Stanford from the issue at hand.⁵ On the first interpretation, the failure thesis is false; on the second interpretation, the thesis is, in itself, superfluous. Moreover, discarding the specific pessimism of the failure thesis and its corresponding induction (from 2a to 3) we discard what Stanford sees as a key distinguishing feature of his argument, an empirical claim about *theorists*.

2.2. *Missing the Mark Against Contemporary Realism*

What needs to be shown is (2b), that past theories had genuine alternatives (and ultimately 4, that we can say the same situation holds for contemporary theories). Casting aside Stanford's (false or superfluous) failure thesis, we are back to Sklar's (1981) PMI-B. Nonetheless, we can ask whether Stanford has provided support for this argument, i.e. whether he has shown that for past theories there were genuine alternatives. Regrettably, Stanford has not made clear that the historical examples to which he does appeal will stand even as singular instances of the kind of alternatives he needs to provide in the context of the contemporary debate. A key problem lies, not so much with the alternatives he discusses, but with the theories to which they are alternatives. Dedicating his discussion to what theorists wrote regarding alternatives (which he takes as evidence for what they *did not* think about), Stanford neglects the altogether crucial task of ensuring that the theories whose alternatives he identifies are themselves relevant to the realism debate.

To make clear the weight of this second key problem, and to clarify just what is at issue in the scientific realism debate, we must remedy some oversights above. In our quest to spell out, not scientific realism *per se*, but Stanford's articulation of scientific realism specifically, we allowed ourselves to suppress some key components of contemporary realism. Realists claim to offer an empirical hypothesis, which they claim we can justifiably believe. Were the realist hypothesis merely '*T* is true', realists would simply be claiming that we can justifiably believe *T*. However, the realist hypothesis contains two crucial qualifications. First, realists qualify the attributed semantic property: they allow that *T* only approximates the truth. This requires a realist hypothesis that is unambiguously distinct from *T* itself—one whose referent is *T* and one that cannot be reduced to *T*'s description of physical objects. Second, and of course, contemporary realists do not claim we can justifiably believe that '*T* is approximately true' irrespective of *T*'s other properties. The realist hypothesis pertains, not to just any *T* or even to a specific *T*, but to a restricted class of scientific theories. If Stanford is to challenge contemporary realism, the theories for which he claims such alternatives are available—those whose alternatives he claims were 'unconceived'—must be members of this restricted class. They must be theories to which contemporary scientific realists are willing to commit themselves, i.e. proper candidates for the realist hypothesis. Crucially, and contrary to the impression that may have been fostered

above by following Stanford's (2006a) introduction, the restricted class is not: theories that stand as the best among *any* set of competitors.

Rather, refined as it has been over the past quarter century, the contemporary realist hypothesis stipulates that theories attain *novel success*—where the phenomena predicted are (at least) not used in generating the theory: *theories attaining novel success are at least approximately true*. (I first introduced the concern that Stanford's work neglects novel success in Lyons 2006, 544n10, and raised it again in Lyons 2009b, 148, 151n11.)⁶ It is the no-miracles argument that is generally deployed to justify belief in this hypothesis. And it is specifically novel success that, barring miracles, contemporary realists claim a patently false theory could not achieve. Readily conceding that theories that merely accommodate data may be false, these realists (must) embrace the idea that patently false theories have genuine alternatives, namely the true and approximately true alternatives. With these points noted, it is clear that the discovery of alternatives to theories that merely accommodate data—along with any induction made from them—poses no threat to contemporary realism. And explicitly identifying novel successes (that were achieved by the original theories) is altogether required for Stanford's argument.

Regrettably, throughout his case studies Stanford pays little attention to the question of whether those theories to which he claims there were alternatives were successful. Although he dedicates a chapter and a paper to Darwin's pangenesis hypothesis, and another chapter and paper to Galton's 'stirp' theory (claiming that both had unconceived alternatives), Stanford does not make evident that these theories even accommodate the data.⁷ And even if we charitably take his discussion to imply that they achieve this much, he neglects the crucial task of showing that these theories qualify as candidates for the realist hypothesis (or any realist application of the no-miracles argument) by attaining novel success. In his book (Stanford 2006b), the comments offered on novel success, with one exception (to be noted), lie outside of his central case studies (which occupy pages 51–140). In most of these comments, he is simply acknowledging that realists have come to emphasise novel success (Stanford 2006b, 10, 43, 158; 162–163n18, 19, 22; 186n6). In a few others, he reflects on why instrumentalists value such successes (Stanford 2006b, 206–207; 213n10). After concluding his case studies, he reintroduces Laudan's list of successful theories that cannot be approximately true and the historical argument from false (as opposed to alternative) theories. There he does mention 'the classic textbook example of novel predictive success', Poisson's novel prediction (Stanford 2006b, 146, 159). In that context, rather than the context of his 'new induction', he remarks,

As we've seen, *we might fairly add one or more nineteenth-century theories of inheritance and generation to Laudan's list that at one time enjoyed the kinds of empirical support that have traditionally led their defenders to suppose that they must be true . . .* (Stanford 2006b, 146; my italics)

As I note below, suggesting this possibility will not serve as evidence against contemporary realism. Among the theories to which Stanford has shown there are alternatives, he has only explicitly pointed to one (Weismann's—Stanford 2006b, 130) as

making a single novel prediction. (Although he mentions what appears to be one more novel prediction—Meckel’s prediction of the appearance of ‘gill slits in human embryonic development’—he breezes past it, perhaps because it was ‘interpreted differently’ when confirmed: Stanford 2006b, 56.) As we have seen, however, Stanford is ultimately making an induction from past to present theories (from 2b to 4)⁸—and showing that a relevant theory (or two) had alternatives will not even hint at the sort of inductive foundation his ‘sweeping historical thesis’ (Stanford 2006b, 51) demands. I will discuss the realist challenge regarding the quantity of positive instances below (which will amount to a third serious problem for Stanford’s argument). What is clear here is that, at the very least, we need an explicit indication that each theory whose alternatives Stanford has identified has achieved novel success. (NB: Whether *the alternatives* must be shown to achieve novel success is a distinct question, which I will address in section 3.3.) Noting that ‘we might fairly’ say some of these theories achieve what ‘traditionally led their defenders’ to suppose ‘they must be true’ (Stanford 2006b, 146)—this will hardly suffice. Putting all this another way, on the casual construal of realism offered by Stanford, belief in (a) *theories* is justified because (b) *those theories* offer ‘the best available explanation for’ (c) ‘the empirical evidence’ (Stanford 2006a, 122). However, as we have now seen, what realism claims is that we can believe (a) ‘theories achieving novel success are approximately true’ on the grounds that (b) *approximate truth* offers the only or best explanation for (c) *the novel success* of theories. Stanford’s construal misidentifies that which is (a) believed, (b) doing the explaining, and (c) being explained.⁹ Nearly 30 years ago Alan Musgrave very explicitly broke from ‘traditional’ requirements to challenge Laudan’s list: ‘few, arguably none, of the theories cited had any novel predictive success’ (Musgrave 1985, 211n10). In fact, Stanford himself expresses disapproval of the standard pessimistic induction because it ‘ignores important differences between the degrees or varieties of success enjoyed by past theories and those of our own day’ (Stanford 2006a, 122). I am afraid that Stanford, insufficiently concerning himself with the ‘character’ of empirical success, has all but ignored the crucial difference to which contemporary realists point.¹⁰ While I think Musgrave’s claim against Laudan’s list has been refuted elsewhere (Lyons 2002), and while I would not *deny* that the theories Stanford cites achieve novel success, Stanford must show that Musgrave’s claim cannot be easily redirected at Stanford’s own ‘list’ of theories whose alternatives his otherwise extensively documented case studies seek to reveal.¹¹

Somewhat surprisingly, we now see that, when success is limited to what Stanford has shown to obtain in his case studies, even if each premise in the broad *modus ponens* has been established, that argument misses its mark against the realist. What the argument needs and does not have are premises ensuring that the theories at issue are those to which realists will commit themselves. We are prompted to divide premise (1) as follows:

- (1a) *T* qualifies as a candidate for the contemporary realist hypothesis, i.e. *T* is the type of theory that realists claim we can justifiably believe to be approximately true (e.g. *T* has enjoyed novel success).

- (1b) If, however, we have reason to believe that *T* has alternatives, then (in contrast to what contemporary scientific realist's claim) we are not justified in believing that *T* is probably or approximately true.

And Stanford's claim regarding past theories, premise 2 in his argument, must also pertain specifically to theories that qualify as candidates for the realist hypothesis. Without providing grounds for accepting (1a) and an appropriately revised (2), the argument is a non-starter, posing no 'clear and present danger to scientific realism' (Stanford 2006a, 123).

2.3. *Ungrounded Induction(s)*

There is one further problem (hinted at above). In response to the argument from underdetermination, Stanford writes, 'surely one or even a few such convincing cases do not provide sufficient warrant for concluding that genuine or serious empirical equivalence is a ubiquitous phenomenon' (Stanford 2006b, 16). We have already seen that Stanford's new induction from and to failure must be discarded. Because Stanford explicitly deems that thesis—his thesis about the failure of 'practitioners', about 'theorists rather than . . . theories' (Stanford 2006a, 123)—to be the distinguishing feature of his argument, we are prompted to return to it here, briefly. And even setting aside the problems emphasised above, his 'failure' induction is particularly susceptible to the standard realist objection regarding the insufficient quantity of positive instances—an objection we have just seen him embrace regarding empirically equivalent alternatives. To accept even one instance of a single scientist's failure, we must accept that textual evidence can genuinely reveal what a scientist did not, and was even unable to, think about. We must accept that a scientist's denial of the existence of alternatives is not merely a rhetorical device to persuade others of his or her own hypothesis. Even if Stanford could dismiss these worries, irrespective of the degree of ingenuity a given scientist displayed at some point in respect to some particular idea, it should hardly be surprising that a particular scientist was unable to devise alternatives to a hypothesis that same scientist favoured or even advanced. And regardless of how, say, Fitzgerald's conceptual abilities were limited in respect to some set of alternatives, Fitzgerald's conceptual limitations need do nothing to inform us of Einstein's. All considered, given the length and breadth of the history of science, little contribution will be made to Stanford's 'new induction' by providing a set of anecdotes claiming that a few particular scientists (say Darwin, Galton, Weismann) among the many, at a few stages in the history of one branch of science, were unable to conceive of some alternatives to a particular theory each emphatically favoured.

We might ease the force of this challenge significantly by discarding Stanford's failure thesis (in PMI-C), as I have argued we must, and concerning ourselves solely with finding alternatives (as in PMI-B). Rather than being required to show that many scientists in many different periods failed to find alternatives to a given past *T*, we need only look back to identify a single alternative to *T*. Nonetheless, as noted above, *T* must qualify for a realist commitment, by, for instance, achieving novel

success. And as indicated, forging a solid induction to the conclusion that qualifying contemporary theories have alternatives will indeed require that we find many instances of historical theories that both achieved novel success and have alternatives. Until we find them, the realist standard objection regarding the quantity of positive instances (again, embraced by Stanford against empirical equivalents) will retain at least as much force as it has against the traditional argument from underdetermination and the standard pessimistic induction.

3. Restoration: An Alternative Non-inductive, yet Historically Informed, *Modus Ponens* against Scientific Realism

Although I have now criticised Stanford's particular proposal and the evidence he offers to support it, I do sympathise with his conclusion; and my critique is meant to be constructive, to open the way for a restoration of the *modus ponens*. Moreover, I agree with Stanford that the history of science is crucial to the question of scientific realism. In fact, I think that at least two important lessons for the realism debate can be drawn from Stanford's (and Sklar's) discussion. The first: the empirical equivalence component of the standard underdetermination argument is unnecessarily demanding. The second, although the primary arguments against realism—the argument from underdetermination and the historical argument—tend to be treated in the literature as wholly distinct, important relations hold between them; in particular, we can discern competitors by looking to the history of science. I will draw on and build upon the second lesson after defending the first.

3.1. Empirical Distinguishability and the Historically Informed Recognition of Competitors

It will not suffice for realists to respond only to, say, van Fraassen's (1980) very particular variant of non-realism by resting their defences against the problem of underdetermination on the question of whether there are empirically equivalent theories. Other non-realists such as Laudan (1981) recognise from the history of science that the most genuine threats to scientific realism are empirically distinct. Accordingly, with Sklar and Stanford, we must acknowledge that empirically equivalent theories are hardly the only alternative theories to threaten realism. Just as the key premise to the historical argument pertains to theories that are empirically distinct from our contemporary theories, so can the key premise for underdetermination. Nonetheless, because much of the literature on underdetermination assumes (or at least appears to assume) that the *real* threat of underdetermination arises only when competitors are empirically equivalent to our favoured theory, it is important to go beyond Stanford and Sklar and explicate just why such an assumption is false.

As a preliminary step, we are prompted to specify just what it is about empirical equivalence that is taken to be so threatening. It is the following: irrespective of the number of observations made or tests performed, there is no point in the future at

which we can empirically eliminate the competitors; empirically equivalent competitors are such that they are empirically ineliminable as competitors to our favoured scientific theory; and because they are empirically ineliminable they render our favoured scientific theory such that it is in a state of temporally unrestricted underdetermination. Two points are in order. First, realism is, in every instance, about belief at time, t . And potential future tests of competitors—at *t-plus-a-century*, or even *t-plus-a-decade*—do not in themselves have positive bearing on belief in a theory over its competitors at time, t . That given, even empirically non-equivalent competitors that have not yet been empirically eliminated (and which are such as to render our favoured theories patently false) still threaten the realist claim that we can be justified in believing our favoured theory over its competitors. Although the underdetermination in such an instance is temporally restricted, that underdetermination still bears strongly on the claim that we can justifiably believe a theory at time, t . Of course, at least descriptively or psychologically, we take temporally unrestricted underdetermination to be *more* threatening than temporally restricted underdetermination, since in the former case, as noted, the competitors are empirically ineliminable: there is no test that may be performed *at any time* that will eliminate the set of competitors and thereby the underdetermination. This, however, brings me to my second, and more important point in defending the significance of empirically distinct competitors: although empirical equivalence is sufficient for empirical ineliminability, and, so, sufficient for temporally unrestricted underdetermination, empirical equivalence is not a necessary condition for either. For in cases such that, irrespective of the kinds of tests we perform, at any time t , there will be indefinitely many competitors, our favoured scientific theory is and will always be faced with empirically ineliminable competitors. In such a situation, although each competitor may be such that, individually, it is empirically distinct from our favoured theory, the underdetermination that results from indefinitely many such competitors remains temporally unrestricted. Hence, when it comes to whether we can claim we are justified in believing a given T , situations in which our theory has indefinitely many empirically distinct competitors pose no less a threat than those situations in which our theory has empirically equivalent competitors.

Given the two points I have just made, I dare say that the very common demand for empirical equivalence is superfluous and that, throughout the history of the realism debate, that demand has ultimately been a distraction, one in favour of realism. Approaching the general point from another direction, empirical distinguishability makes a difference only *after* distinguishing tests have been performed, and if our theory is such that it will always have indefinitely many competitors, the distinguishing tests can never be performed for the entire set of competitors. The threat to epistemic realism—the claim that we can justifiably believe our favoured successful theory—remains. (In fact, although I am placing less emphasis on my first point in defence of the significance of empirically distinct competitors, that point reveals that the threat to realism—the view that we can be justified in believing a theory at time t —remains even if the empirically distinguishing tests are merely such that, at time t , they have yet to be performed.)

Having now explicated grounds for the claim that, in the key premise for underdetermination, a set of empirically distinct competitors can be no less threatening than empirically equivalent competitors, we can now turn to the second important lesson to be drawn from Stanford's (and Sklar's) approach: that important relations hold between the primary arguments against realism—the argument from underdetermination and the historical argument—in particular, that we can discern competitors by looking to the history of science.¹² Even setting aside Stanford's failure thesis, his argument involves perusing the historical literature to find alternatives that were actually developed and advocated by past scientists (but unconceived previously, or at the time, by others) and then, via an induction, concluding that we have similar such alternatives to our theories today.

I will now argue, however, that the history of science can be invoked to reveal competitors without binding us to the particulars of this approach. After first providing a snapshot of an alternative approach—a *historically informed but non-inductive* approach—to revealing that, for any theory that accounts for some set of phenomena, there are competitors *such that we cannot justifiably deny that they are approximately true*, I will articulate and illustrate the approach in more detail. In short form: we empirically identify successful past theories, typical candidates for the historical argument—say, phlogiston theory, caloric theory, ether theory, etc. (while I will not be using these as examples below, I show in Lyons 2002 that each of these attained novel success); we empirically identify successful contemporary theories; and we empirically *identify and analyse the relations* that stand between our empirically identified past and present successful theories. Although our approach is historically informed by science itself, we do not inductively infer from the fact that these past theories had competitors to the fact that present theories have competitors. Rather, among those relations that we empirically identify between past and present theories, we isolate those historically exemplified relations that are such that they extend to any theory that is related to phenomena in the way that scientific theories are non-contentiously required—according to both realists and antirealists—to be related to phenomena. And from among the latter subset of relations, we further isolate those relations (such as the historically exemplified relation specified below) that can be instantiated in indefinitely many ways.

In more detail, towards the empirical identification of one such relation between past and present theories (followed by a case in which it is historically exemplified), let any one of the historically successful but now rejected theories be *T*. According to contemporary science, *T* (approximately) predicts that a certain set of observed phenomena obtains. However, again according to contemporary science, in certain situations, the phenomena behave in a manner that significantly diverges from *T*'s predictions. Note here that, while 'scientific seriousness' is relevant only insofar as it pertains to whether or not a theory can be approximately true, that property is ensured by employing contemporary science to articulate the details of the divergence. Contemporary science itself reveals a competitor, *CT*, which, though contradicting *T*, shares those predictions successfully made by *T*. The following is expressed by *CT*:

The phenomena are (approximately) as T predicts, except in situations S , in which case the phenomena behave in manner M .

For a case in which this expression is exemplified by the relationship between a historically successful past theory and a contemporary theory, one can insert for T Kepler's deep theory of the *anima motrix*. Because this article is concerned with the question of whether our theories have competitors whose approximate truth we cannot justifiably deny, the historical element I am emphasising here is the *relationship between* historical and contemporary theories, rather than, say, the historical *process* involved in attaining historical and/or contemporary theories. Nonetheless, some historical contextualisation (drawing on Lyons 2006) will be helpful here. In Kepler's ([1596] 1981) *Mysterium Cosmographicum*, he formulated his deep theory of the *anima motrix*, a theory that causally explained some of the primary features of planetary motion. Because that text was written years before Kepler even met Brahe, it is clear that Kepler's deep theory itself was not put forward to accommodate Brahe's detailed data. And what we find is that, directly employing foundational posits of Kepler's deep theory of the *anima motrix*, Kepler made a series of temporally novel predictions. Included among the novel predictions he makes are predictions about the sun: the sun spins; it spins in the direction of planetary motion; it spins along the plane of the ecliptic; and it spins faster than any of the planets revolve around it. Further, Kepler's deep theory was centrally deployed in arriving at his laws, which were themselves central to the unprecedented success of the Rudolphine Tables. Kepler's laws led to, and continue to lead to, innumerable successful predictions pertaining to the behaviour of, not only Mars and the Earth, but also Mercury, Venus, Saturn, and Jupiter. Early on, Kepler achieved further significant novel predictive successes (pertaining to relations between the Earth, Sun, and planets), predicting, not only two planetary transits—the Mercurial, and the rare and irregular Venusian, transit—but also a separation between the two transits of less than a month. And his laws led to, and continue to lead to, a multitude of novel successful predictions regarding the then undiscovered planets, Uranus and Neptune, as well as legions of other bodies in the solar system and beyond. Kepler's deep theory and the reasoning he employed in arriving at these novel predictions centrally included the following posits, each of which is patently false by contemporary lights: the sun is a divine being and/or the centre of the universe; the natural state of the planets is rest; there is a non-attractive emanation, the *anima motrix*, coming from the sun that pushes the planets forward in their paths; the planets have an inclination to be at rest, and to thereby resist the solar push, and this contributes to their slowing speed when more distant from the sun; the force that pushes the planets is a 'directive' magnetic force, etc. While the attainment of Newton's theory very directly deployed what we now call Kepler's laws, the resultant Newtonian system ultimately contradicted, not only Kepler's laws, strictly speaking; crucially, that Newtonian system deemed dramatically false the key components of the deep theory that Kepler had employed to arrive at Kepler's laws. Moreover, constituents that Newton (and later Newtonians) centrally employed in the derivation of their own temporally novel predictions are such that, by presently lights, e.g. general relativity, those

deployed constituents are not even approximately true. (For more detail on the reasoning process involved in Kepler's and Newton's theories, where the focus is not on the question of competitors, see Lyons 2006.)¹³

Instantiating our expression above, then, one can take for *T* Kepler's deep theory of the *anima motrix*. And one can include as *S* occasions in which, say, Jupiter approaches Saturn, and occasions in which a planet's orbit is particularly close to the sun, etc.; adding as *M*, say, 'non-Keplerian perturbations', 'the advancement of Mercury's perihelion', etc. No doubt, employing contemporary science to articulate *S* and *M* in their full particulars, these will be awkward and non-simple assertions. Notice, however, that, unless the realist wants to sacrifice the possibility of deeming contemporary science approximately true, even if the realist claims that such competitors (in this case, competitors to Kepler's theory) lack certain explanatory virtues, the realist must concede that the absence of such virtues does *nothing* to provide grounds for denying the approximate truth of these competitors.

Just as the above expression reveals that there are rivals for the truth of such past theories, it also reveals that there are such rivals to any contemporary theory that accounts for some set of phenomena.¹⁴ Instantiating *T* with any accepted contemporary theory, the remaining clauses can include any *S* that has not (yet) been acknowledged as obtaining and any *M* that significantly differs from the behaviour that *T* describes. There are indefinitely many options and combinations, all of which will share *T*'s predictions about observed phenomena. (The point here is that *there are competitors* that assert that *S* and *M* obtain. Acknowledging this is wholly distinct from asserting that *S* and *M* in fact obtain. The non-realist is patently not an epistemic realist about competitors.) While some of these competitors may be subject to future empirical elimination, indefinitely many such competitors will remain at any time.¹⁵ Notably, *this situation is not 'transient'*—as the underdetermination that arises from empirically distinct competitors is often said to be. (Nor do Sklar and Stanford argue that there are indefinitely many competitors in the situations they describe.) Above I have articulated why underdetermination looms even if our favoured theory has empirically non-equivalent competitors: faced with indefinitely many competitors, we are faced with empirically ineliminable competitors; and faced with the latter, our situation is one of temporally unrestricted underdetermination—precisely as would be the case were we to show that each theory has empirical equivalent competitors. And just as *CT* above patently contradicts past theories—and, for example, since *M* can dramatically differ from the behaviour described by *T*—indefinitely many of these competitors will patently contradict contemporary theory *T*. Crucially, since *CT* above remains a candidate for truth in contemporary science, we simply cannot discard theories revealed by the above expression (be they competitors to past or to present theories) as false, or as lacking 'scientific seriousness', let alone as what Stanford terms 'Cartesian fantasies' (Stanford 2006b, 13). Despite the realist's desire to apply such charges, such charges are no more applicable to these competitors than they are to expressions of *CT*. In fact, since contemporary science accepts the above expression of *CT*, it is clear that competitors so revealed do nothing in themselves to deny the uniformity of nature or require that the world is non-simple. We

need concede little more than that some such competitors may be incomplete, but that particular concession is taken to be applicable to even our best theories, general relativity and quantum electrodynamics among them. And neither a lack of completeness nor, as indicated above, a lack of explanatory virtues gives us any grounds to deny the approximate truth of these competitors, nor, therefore, grounds to block their employment in the premise of an underdetermination argument. By the recognition of relations between contemporary and past theories, then, the history of science allows us to recognise a set of theories that compete against our favoured contemporary theories, competitors such that we cannot justifiably deny that they are approximately true. (Although the present discussion flags only one class of historically exemplified relations, it is nonetheless important to recognise that other kinds of relations may be available and put to the same use.)¹⁶

3.2. Clarification and Elaboration

A key part of what I am suggesting is that historical studies are informative in a direction that runs contrary to realist desires. In particular, what we find in case studies such as the one I am pointing to here, the Kepler–Newton contemporary transition set, is a greater degree of flexibility in the history of science than realists tend to suppose with regard to, and casually impose upon, theory acceptability. For instance, we see in that transition set that a genuine competitor (i.e. in our historical analysis, a later, e.g. contemporary, theory) can be far more complex in its empirical assertions than its predecessor: it can retain rough approximations to what its predecessor takes to be empirical laws, even extending those approximations to some domains; however, in any number of various other domains, it can significantly diverge from its predecessor's empirical claims regarding as yet unobserved phenomena. Compared against Kepler's theory, our contemporary picture does this with respect to non-Keplerian perturbations along planetary paths; it also does this with respect to galactic behaviour—where Keplerian–Newtonian (and for that matter, strictly speaking, relativistic) predictions are defied empirically (prompting in our contemporary picture, for instance, the positing of dark matter halos). Further, not only does our contemporary picture deny Newtonian behaviour of entities that approximate the speed of light and that exist at the level of the microcosm (as is often said), it also denies Newtonian behaviour for perfectly macrocosmic 'slow-moving' objects such as Mercury and neutron stars, etc. While I will say more on this below, we note here the important historical recognition that the empirical predictions of competitors can differ greatly from those made by their predecessors and, extending this to our present concern, our favoured contemporary theories.

Additionally implicit in the historical points that I am emphasising regarding the relation between past and present theories is the fact that we cannot preclude competitors on the ground that they fail to accord with background theories *in place*. (Put another way: we cannot preclude competitors merely because they require their own set of background assumptions.) If such coherence-with-background

requirements (to which realists tend to casually pay lip-service) were in place, those requirements would have prohibited small scale modifications of the kind we see in the history of quantum theory, with the theorising of Plank, Einstein, Bohr, de Broglie, etc. (as noted in Lyons 2011), as well as large-scale transitions (see, for instance, Feyerabend 1963; Swinburne 1997; Khalifa 2010), as we see in the shifts from, say, the Aristotelian to the Newtonian to the relativistic and quantum mechanical systems. By the casually expressed realist ‘rule’ that a theory must cohere with the background theory in place, none of these theory changes would have been permissible. Hence, while the historical emphasis in this article pertains to the *existence* of competitors, our recognition of the existence of the kind of competitors I am flagging here is bolstered by the related (but distinct) historical recognition that a competitor need not cohere with a background system in place—that competitors often have, and so can, come with their own background system, one that replaces that of their predecessors (or our favoured contemporary theory). Although the quest for truth may lead us to desire that a competitor cohere with *a* background system, it does not require coherence with the background system in place.

Now I am not denying that historical analysis can be informatively *restrictive* when it comes to our analysis of *which theory is to be chosen* in science. (I discuss such restrictions in Lyons 2012.) For instance, there is historical/empirical support for the claim that competitors that posit empirical divergence from their competitors *often* also provide a deep account of, explanation for, those divergences. A minor point here is that, although this may be said to be a *preference* in theory choice, we also realise from the history of science that we cannot deem deep explanations to be a *requirement* for theory choice *per se*, given for instance, nineteenth-century thermodynamics, the Copenhagen interpretation of quantum mechanics, beta decay, the need for renormalisation, quantum fluctuations, etc. Most importantly, however, what is at issue here, in the context of the realism debate specifically, is not the question of which if any competitor can be justifiably chosen or pursued by scientists; it is rather the question of whether a competitor is such that we can justifiably deny that it is (approximately) true. (I have detailed the importance of this distinction in Lyons 2009a.) Hence, even when the history of science does suggest *limitations*, those limitations pertain generally only to theory choice, and not to the matter at hand, the scientific realism debate. Here we bolster my point at the end of the last section regarding the permissibility of incompleteness: although a competitor *may* offer a deeper unifying account of the empirical complexities that it posits beyond those predicted by our favoured theory (or a predecessor), we cannot impose on the competitors (of concern in the realism debate) the *requirement* that they do provide a deeper account—for we cannot justifiably deny the (approximate) truth of those that do not.

With regard to this issue of the extent to which competitors can diverge—supraempirically as well as empirically—from our favoured theories, a further less historically based point is in order: easy though it may be, given our realist inclinations, we cannot allow ourselves to forget that contemporary science itself tells us that the universe is 13.8 billion years old. Given our psychological propensity for epistemic confidence, we might easily overlook the fact that according to science itself only infinitesimally

tiny slices of the spacetime continuum have actually been observed and documented in contemporary, and, for that matter, the entire history of, science. Anthropocentric as we are, we may tend to assume that there is little ‘room’ for variation in empirical predictions, but, when we conjoin what is historically evident in the Kepler–Newton contemporary transition set, as above, to the recognition that the sample size of our database is infinitesimally small relative to the universe about which our theories make assertions, the room for dramatic variation in the empirical predictions of competitors, while retaining empirical approximation to thus far observed phenomena, is immeasurably vast. In short, combining points above regarding the history of science with the point just made, and liberating ourselves as we now have from the demand for empirical equivalence, it is clear that a competitor can be such that the documented empirical claims at time, t , constitute only an incredibly minimal portion of the set of its empirical predictions—and that it can nonetheless posit very different behaviours in any number of domains beyond that small portion, along the way rendering the deeper core components of its predecessor (or our favoured theory) patently false.

I have just argued that the historical analysis of the kind I am proposing reveals that we are not permitted to require of competitors that they refrain from significant empirical divergence, that they cohere with background systems in place, or that they explain their empirical or supraempirical divergences from our favoured theories. This is due in part to the fact that such rules have been defied historically; but it is also, and most importantly, due to the fact that the question of concern here, the context of the realism debate, is whether we can justifiably deny that a given alternative theory is approximately true. Of course the latter question *does* impose restrictions on which alternatives qualify as genuine competitors: for instance, alternatives that are *self-contradictory* will not (as they stand) qualify as genuine competitors: we can justifiably deny their truth. And I am not claiming that self-contradictory theories pose a threat to realists. (While even internal contradictions can on occasion be remedied by minor theoretical modifications, that point in favour of my general thesis is not an issue in need of emphasis or articulation here.) Rather, the kind of theories I am emphasising, as competitors are those that are not self-contradictory. With that small caveat, in this context, I am emphasising that historical information regarding the relation between theories can inform us of the existence of competitors. But, beyond the demand that competitors share confirmed empirical predictions with their predecessors (or with our favoured theories) and the demand that competitors are not self-contradictory, the historical evaluation does not restrict us in terms of what can and what cannot be approximately true. And given that our concern is with the realism debate, competitors can be excluded only if they have properties such that we can justifiably deny that they are approximately true.

3.3. *The Alternative Non-inductive, yet Historically Informed, Modus Ponens Against Realism*

We have then a general method for revealing competitors that is at once historically informed and non-inductive. Taking each of these two key points turn, our method

is *historically informed* given the empirical/historical recognition of, for instance, the following:

- (a) a particular past theory (e.g. as above, Kepler's deep theory, *K*) qualifies as a successful scientific theory by the realist's criterion;
- (b) contemporary theory, *CT*, qualifies as a successful theory by the realist's criterion;
- (c) the particular past successful theory (e.g. *K*) is such that its deep content does not even approximate *CT*;
- (d) the particular past successful theory (e.g. *K*) has been replaced by *CT*;
- (e) nonetheless, *CT* and the particular past successful theory (e.g. *K*) bear the relation described above.

While each of these is recognised empirically, the relations between the past and present theories, in (c) and (e), are identified by syntactic analysis; nonetheless, these relations are not 'unscientific' fantasies, but historically exemplified relations, empirically identified to obtain between an empirically identified past scientific theory and an empirically identified contemporary scientific theory.

Further, although historically informed by scientific theories, our method also (and nevertheless) remains *non-inductive*. For, first, it is clear that no one of the empirical claims in the set (a)–(e) is ampliative; that set contains no step beyond the empirical recognition that these particular relationships are historically exemplified, i.e. that they obtain between a past and present scientific theory. Second, adding to the set (a)–(e), upon empirically identifying that a particular contemporary theory is one that accounts for some set of phenomena (in the way that scientific theories are non-contentiously required to do so), we instantiate our historically exemplified expression with that contemporary theory. (Of course, since the particular contemporary theory we choose here might bear on a wholly different domain of phenomena, it need not be equated with *CT* in the example above.) Finally, we instantiate *S* and *M*, selecting from indefinitely many descriptions. We recognise that the same identified relation between past theory and *CT* extends, is applicable, to any particular theory that accounts for some set of phenomena; and, given the indefinitely many possibilities for *S* and *M*, we recognise that indefinitely many competitors can instantiate that relation. In short, the historically exemplified, empirically identified competitor-relation above extends by way of instantiation to those contemporary theories that are related to phenomena in the way that scientific theories are non-contentiously required (by both realists and antirealists) to be related to phenomena. And this requires no induction.

Finally, our historically informed yet wholly non-inductive method for revealing competitors leads us to an alternative non-inductive variant of the *modus ponens* underdetermination argument, one that solves each of the defeating problems I flagged with regard to Stanford's argument:

- (1a) *T* qualifies as a candidate for the realist hypothesis, i.e. the type of theory that realists claim we can justifiably believe to be approximately true (e.g. *T* has enjoyed novel success).

- (1b) If, however, we have reason to believe that *T* has competitors such that we cannot justifiably deny that they are approximately true, then (in contrast to what the contemporary scientific realist claims) we are not justified in believing that *T* is probably or approximately true.
- (2) For past theory, *T* (which qualifies as a candidate for the realist hypothesis), contemporary theory, *CT*, expresses the following competitor to *T*: the phenomena are (approximately) as *T* predicts, except in situations, *S*, in which case the phenomena behave in manner, *M*.
- (3) That expression, of which there are indefinitely many variants, equally applies to contemporary theory, *T* (as above).
- (4) We have (every) reason to believe that contemporary *T* has indefinitely many competitors such that we cannot justifiably deny that they are approximately true.
- (5) Therefore (in contrast to what the contemporary scientific realist claims), we are not justified in believing that *T* is probably or approximately true.

This version of the broad *modus ponens* retains the original general structure but crucially emphasises the issue at hand to be whether or not *T* has competitors such that we cannot justifiably deny their approximate truth; moreover, it adds premise (1a), clarifies (1b), discards Stanford's problematic failure thesis, and eliminates all inductions. Nonetheless, by way of the historically exemplified, empirically identified relation between contemporary and past successful scientific theories, this underdetermination argument retains a genuine connection to the historical argument. I contend that, in contrast with Stanford's argument, burdened as it is by his failure thesis and the two inductions he employs, this variant of the *modus ponens*, relieved of such burdens, is an underdetermination argument that realists are pressed to confront.

I argued in section 2.2 that, given the evidence Stanford provides, the realist move to novel success (is one of the three key problems that) prevents his argument from getting off the ground. So we must ask, does a similar fate await our revised *modus ponens*? (As will be recalled, my point on novelty in section 2.2 is that the theories to which Stanford claims there were alternatives (the original theories) must be shown to have achieved novel success. In short, the *modus ponens* requires a premise—included as (1a) in my revised non-inductive variant—ensuring that *T* is a theory to which realists will commit themselves. The distinct question addressed in this section is whether *the alternatives or competitors* themselves must be shown to achieve novel success.) We recognise that, because the non-inductive method reveals competitors for all theories, it reveals competitors for theories attaining novel success. Further, as noted, what matters is whether the competitors are such that we cannot justifiably deny that they are approximately true. Because for realists, truth is emphatically what it is irrespective of what scientists may have thought about, written, believed, etc., the mere fact that some of these competitors remain unarticulated does nothing to prevent them from being approximately true. Moreover, since no one can deny that accommodating theories *can be* (approximately) true, it is no requirement of *being true* (or approximately so) that a competitor make novel predictions. Hence, our variant of the *modus ponens* underdetermination argument, whose

competitor premise involves no induction, is unaffected by the realist demand for novel success.

I have attempted to capture the structure of the argument against realism that Stanford embraces, and I have subsequently revealed significant problems with it. I have also proposed and developed an alternative version of the broad *modus ponens* that is freed from embedded inductions—a version that I have argued is not threatened by the set of realist objections I have directed against Stanford. I conclude then, that a non-inductive, yet historically informed, *modus ponens* threatens scientific realism.

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Notes

- [1] As with Stanford's appeal to Duhem, this phrase should not be taken to mean 'Duhemian underdetermination'. The latter is distinct: it pertains to how alternatives can be generated and retained and in fact stands as one way by which the antecedent of the Mill–Duhem conditional might be grounded.
- [2] With Stanford's emphasis on scientific seriousness, realists will be relieved that he is not invoking 'Cartesian fantasies' (Stanford 2006b, 13) as alternatives. Nonetheless, since he is concerned with *unconceived* alternatives, 'well-confirmed' pertains not to anything scientists have 'done with' the theories, but to a relation theories have to data, etc.
- [3] Moreover, it is implausible that anyone can point to a litany of failures in the history of cases in which the scientific community genuinely did dedicate its time, energy, and resources towards generating alternatives to what was available.
- [4] On this reading, every individual, scientist or not, continually fails at indefinitely many possible tasks he or she never genuinely sets out to achieve (e.g. parachuting into Jupiter's Red Spot).
- [5] As above, Stanford deems the failure thesis, a thesis about 'theorists rather than ... theories' (Stanford 2006a, 123), the central feature of his argument. Yet at the end of nearly 80 pages of case studies dedicated to that thesis in particular, Stanford himself, surprisingly and counter to his own efforts, concedes to the irrelevance of whether an alternative did or did not occur to some specific past scientist (Stanford 2006b, 128–129).
- [6] Since then, Saatsi and Psillos also and rightly have made the point, and Stanford has responded, in a review symposium on his book (Saatsi et al. 2009). Here I will first unfold the importance of the point; and in note 11 below, I will critique Stanford's response.
- [7] Stanford himself emphasises the failure of Darwin's theory to account for transfusion experiments, which Galton 'seems to have regarded as a decisive refutation of pangenesis'; and he notes that alternatives to Galton's theory were 'accepted by the scientific community soon after Galton's own account was developed' (Stanford 2006b, 81, 86, 100).
- [8] This is so irrespective of the first problem I pointed to—that his failure thesis is superfluous if not false—and despite his desire to focus on theorists in (2a) and (3).

- [9] When noting the one instance of novelty (attained by Weismann's theory), he writes,

Nor, it would seem, does the ability to make successful novel predictions in a given domain of theorising indicate that [realism is] beyond the reach of the problem of unconceived alternatives, despite the currency of this notion in much recent philosophy of science. (Stanford 2006b, 130)

On the contrary, I am arguing that contemporary realism *is* beyond the reach of the argument and evidence Stanford has offered.

- [10] This is clearly not to insist that, descriptively, every philosopher who fancies him or herself a realist has explicitly invoked novelty; my point does not require an exhaustive consensus among all self-proclaimed realists. (Of course there can be exceptions, for instance, those who have not taken it upon themselves to address Laudan's original historical critique of realism and/or those who, for instance, when not explicitly engaged in the realism debate, have argued that novel success does not afford greater evidential weight than accommodation.) The point is rather, first, that Stanford has offered no challenge to those realists who *do* embrace the nearly three-decade-long tradition of invoking novelty (e.g. a number of those with whom Stanford engages: Worrall 1989; Leplin 1997; Psillos 1999). Second, those realists who do not explicitly require novel success of the theories to which they commit themselves *need only* invoke this readily available response to wholly answer Stanford's challenge. Third, and finally, as is made clear at the beginning of section 2.2, realists—even those who, despite Laudan's historical argument, might hope to allow for accommodation—do not (and cannot plausibly) embrace realism as Stanford construes it, where the justification for believing a theory is merely that 'it offers the best available explanation for the empirical evidence we have . . . ' (Stanford 2006a, 122). Since realists cannot plausibly claim justification for believing *T irrespective of T's other properties* ('best' does not entail 'good'), Stanford's premise (1) must still be replaced by my premise (1a) (at the end of the present section), even if (1a) contains some well-motivated and testable restriction (following the 'e.g.') other than novel success.
- [11] As pointed out in note 6 above, Stanford (in Saatsi et al. 2009) has offered a response to this problem of novel success. Stanford first emphasises his failure thesis (2a and 3 in the schema above) and its distinctness, saying that the realist appeal to novel success 'does nothing to show that the [failed] attempts of past *theorists* are relevantly unlike the attempts of present *theorists* to' (Saatsi et al. 2009, 382) conceive of alternatives. By itself, this particular formulation of his response will not help him since, as we have seen in section 2.1, his failure thesis is false or superfluous. However, he also phrases his response in terms of (2b) (in the schema above), saying that novel success '*does nothing to show . . . that there cannot be . . . unconceived alternatives to*' theories achieving it (Saatsi et al. 2009, 382; my italics); likewise, reinvoicing Fresnel's white spot (Saatsi et al. 2009, 383) and his own two instances of possible novel success (Saatsi et al. 2009, 384) that I identified above, he writes, 'These examples further illustrate that novel predictive success is *no proof of . . . the absence* of fundamentally distinct unconceived alternatives' (Saatsi et al. 2009, 384; my italics). Unfortunately, each version of this defence—whether it is expressed in terms of his failure thesis, (2a) and (3), or in terms of (2b)—is seriously flawed as a *defence of Stanford's inductive argument and the evidence he puts forward in support of it*: the realist does not need to 'show', let alone 'prove', 'that there cannot be' or 'the absence of' unconceived alternatives. Rather the burden of providing the proper kind of evidence to support Stanford's induction(s) against the realist falls on Stanford himself. Stanford says that 'novel success' does not 'allow us to simply dismiss our *independently motivated* worries about the *possibility* and significance of unconceived alternatives' (Saatsi et al. 2009, 383; my italics). Yet, that which is 'independently motivated' here is Stanford's own argument, which does not arrive at the

mere possibility of unconceived alternatives. His argument in its full form, as we have seen, rests on two inductive premises and claims that ‘we have every reason to believe that there are similar alternatives to our own contemporary scientific theories’ (Stanford 2006a, 123; my italics). Contrary, then, to Stanford’s desires, the appeal to novel success *does allow* most contemporary realists to ‘simply dismiss’ (Saatsi et al. 2009, 383) Stanford’s induction and the chapters of evidence that Stanford offers on its behalf. See also my third point in note 10. (Another component of Saatsi et al. 2009 defence makes appeal to a different historical argument altogether, what I have elsewhere called, favoured, and defended as the ‘pessimistic meta–*modus tollens*’ against realism: Lyons 2002, 2003, 2006, 2009a, 2012. Although dismissing the idea that his ‘new induction’ rests on the standard pessimistic induction, Stanford effectively admits that his argument does depend on the meta–*modus tollens*: Saatsi et al. 2009, 383. Unfortunately, for Stanford that logically valid argument, by itself, does not justify his induction(s).)

- [12] I articulate other relations between these two arguments in Lyons (2009a), where the focus is more emphatically on realist responses to the historical argument. This section of the present article has favourable and significant bearing on the argument made there—as well as on distinct ‘underconsideration’ arguments, as defended for instance by Wray (2008) and Khalifa (2010).
- [13] I originally introduced this Kepler–Newton case (Lyons 2006) as offering a set of counterinstances to the hypothesis the deployment realist (Psillos 1999) says we can justifiably believe: ‘those theoretical constituents that are genuinely deployed toward novel predictive successes are at least approximately true’. (There I contend that the same, if not more, credit must be attributed to those theoretical constituents deployed by scientists towards accepted *generalizations*, especially when the latter have, in turn, been deployed in successful specific predictions.) Beyond the set of examples in that paper and other sets introduced in Lyons (2002; see also Vickers 2013), I suggest here that the following additional examples call for investigation, none of whose particulars, as far as I know, have been introduced as bearing historically on the scientific realism debate: Thales’s false posit that water is fundamental along with additional rejected constituents pertaining to, for instance, divinity, seminal principles, etc., were genuinely deployed towards, and culminated in, van Helmont’s successful five-year willow tree experiment. Descartes deployed his conception of God, who created extension and instilled motion into the world, to arrive at his law of the conservation of momentum. Lagrange and Leverrier made predictions about Venus that were in close accord, but they did so by deploying dramatically divergent constituents regarding Venus’s orbit and mass. The false constituent that light particles have mass was deployed by von Soldner to predict, more than a century before Einstein, that light will bend around massive objects such as the sun. This false posit was also deployed by Michell and Laplace in the pre-relativistic prediction of black holes, objects whose escape velocity is too high to allow even light—when light particles are assumed to have mass—to escape from it. Lamark, deploying the false posit that catastrophes and mass extinctions have not occurred in Earth’s history, arrived at the prediction that species change over time. Scheele, arguably, did not merely co-discover oxygen, he predicted it, and he did so by deploying a set of patently false constituents of his phlogiston theory. Deploying a set of utterly false posits regarding vortices and idle wheels in his mechanical model of the ether, Maxwell successfully predicted, not only the displacement current, but also that electromagnetic waves travel at the speed of light. Deploying the posit now seen to be false that where there is optical asymmetry there is life, Pasteur was lead towards his extended successful research programme, including, for instance, his successful novel prediction that fermentation is a biological phenomenon. Premises about the early earth now taken to be patently false were deployed towards the novel and successful Miller–Urey experiment. Einstein not only predicted the existence of what we now call ‘dark energy’, he arrived at that conclusion by centrally

deploying his now wholly rejected posit that the universe is static. (Another relevant question is the extent to which false posits have been deployed in thought-experiments that result, ultimately, in novel successes.) Although I am only indicating their significance here, I suggest that, along with other examples, each of these supports a logically valid *modus tollens* (see the point in parentheses in note 11) against the deployment realist's hypothesis. Similarly, such examples have the potential to play a role in historically grounded articulations of the kind of competitor analyses emphasised in this article.

- [14] For clarification, it should be noted that my proposal does not require that our favoured theories—which might account for new phenomena—have past counterparts. Rather what I am arguing is that new theories, including those theories accounting for new phenomena that have never been accounted for, will have the kind of competitors that I am showing any theory will have. The historical example simply extracts revealing relations from historical predecessor–successor instances, relations that can be instantiated with respect to new theories, where the competitors are to our favoured theories as the successors are to their predecessors in the historical examples.
- [15] On this point I am indebted to Peter Lipton and Nicholas Maxwell.
- [16] For a distinct example of another such relation, a relation that has the potential to be historically applied in this way, see Lyons (2011). Beyond my own examples, Schurz (2009) proposes that a specific logical theorem holds between present and past theories. While Schurz's broad thesis purports to establish a very specific logical continuity between such theories, and I am, in contrast, emphasising the use of contemporary science to articulate their *divergence* as *competitors*, given the nature of Schurz's theorem, I anticipate that the relation it captures could also be fruitfully directed towards the present concern to *generate further kinds* of competitors.

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