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Historical Inductions, Old and New*

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Abstract

I review prominent historical arguments against scientific realism to indicate how they display a systematic overshooting in the conclusions drawn from the historical evidence. The root of the overshooting can be located in some critical, undue presuppositions regarding realism. I will highlight these presuppositions in connection with both Laudan's 'Old induction' and Stanford's New Induction, and then delineate a minimal realist view that does without the problematic presuppositions.

1 Introduction

After decades of debate on 'historical inductions' against scientific realism it is high time to reflect on the shape of the debate at large. Why does the debate still linger on? I think part of the answer has to do with the fact that the two sides keep approaching the epistemic issues at stake with quite different ideas about what realism *is*. We can see this by reflecting on a broad trend in the debate that displays a complex interaction between historical evidence—'testing' of realism against the historical record of science—and conceptual issues concerning realism itself.

The evolving dialectic of the realism debate involves an interesting interplay of (i) anti-realist attempts to use the history of science to undermine the viability of *particular conceptions* of realism, and (ii) realist attempts to respond by discharging realism of *those specific conceptions* by (re)conceiving it to be less vulnerable to 'historical inductions'. A broad look at the debate reveals a trend worth reflecting upon: realist responses to historical challenges indicate that anti-realists have persistently operated with an unnecessarily demanding conception of realism in mind. Many realists have criticized the historical challenges—from Laudan (1981) onwards—of unwarranted pessimism that is due to an overly strong and demanding conception of realism. In this paper I further support this criticism of historically driven anti-realism by drawing attention to the possibility of further weakening the

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epistemological commitments that more recent anti-realists—Kyle Stanford, in particular—have attributed to realism. That is, I draw attention to the possibility of epistemic commitments that fall between the targets of (Old and New) pessimistic inductions, on the one hand, and the sort of anti-realism that those inductions are being used to buttress, on the other.

The key question at stake is: how strongly does the history of science speak against realism? In answering this question it is natural to consider the logical space of possible realist positions with an aim to identify those positions that are compatible with the historical record. Indeed, the broad shape of the debate suggests that this is a good way to approach the question at stake. Historical inductions, Old and New, typically display the drawing of conclusions of unambiguously anti-realist flavour from arguments that actually only demonstrate a tension between the history of science and *realism-construed-in-a-particular-way*. Given this, it is natural to ask how much one should weaken one's realist commitments in order to avoid the force of these inductions? Instead of viewing the historical evidence that anti-realists have amassed as speaking against realism *in toto*, we should approach the question at stake more open mindedly regarding possible realist commitments that fall between anti-realism and the sort of realism that is in tension with the history of science.

My principal aim in this paper is to explore the space of possible conceptions of realism that are reconcilable with the historical record. Towards the end I will also delineate a particular 'minimal realist' attitude that is left untouched by the historical anti-realist arguments, from Laudan and Stanford alike. Here are a couple of caveats regarding the scope of this paper. Firstly, I will bracket the various issues concerning positive arguments for realism. Admittedly the full weight of the historical arguments cannot be *fully* assessed without taking into consideration the (much) bigger picture concerning the complete realist and anti-realist packages that involve other considerations for and against particular conceptions of realism. Secondly, throughout the paper I am primarily concerned with debates concerning the status of 'fundamental' theories (e.g. fundamental physics), as it is in this sphere of science that the historical evidence speaks loudest against realism. Stanford's anti-realist induction is also directly aimed at 'fundamental theories of nature'.[1] I will argue that the coherence of a minimal conception of realism shows that even with respect to fundamental theories a realist attitude can be maintained in the face of the historical evidence. I believe the realist furthermore has the wherewithal to defend less minimal realist

[1]Cf. Stanford (2006) on the scope of New induction:

> The set of scientific beliefs [...] most vulnerable to the challenge of unconceived alternatives will almost certainly include many or even all [fundamental theories] that form the very heart of our scientific conception of the world. (p. 32)

attitudes in other, less fundamental spheres of science, although I will not defend this further claim here.

2 Historical inductions against realism

There is a long tradition of using the history of science to argue against realist attitudes to science. Much of the contemporary debate traces back to Laudan's writings in the early 1980s (although the tradition goes back at least to the early 19th c. 'bankruptcy of science' debate in France). In his extraordinarily influential paper Laudan (1981) presents a forceful challenge to realism: the history of science is arguably inconsistent with certain attempts to defend realism on the basis of the success of science and its allegedly cumulative features.[2] Historical pessimism about the reach of our theoretical grasp has received a significant new twist more recently with Stanford's 'New Induction' from the history of science, challenging realism about the fundamental sciences in particular. New Induction has not convinced everyone, however, and the debate carries on unabated.

I will review these prominent historical arguments against realism so as to indicate how they display a systematic overshooting in the conclusions drawn from the historical evidence. The root of the overshooting can be located in some critical, undue presuppositions regarding realism. I will highlight these presuppositions first in connection with Laudan (1981), after which I will reflect in a similar spirit on the ensuing debate, before moving on to Stanford's New Induction.

2.1 On Laudan's 'Confutation'

In the concluding paragraph of 'A Confutation of Convergent Realism' Laudan warns the reader of a possible misrepresentation of his position:

> It is important to guard against a possible misinterpretation of this essay. Nothing I have said here refutes the possibility in principle of a realistic epistemology of science. To conclude as much would be to fall prey to the same inferential prematurity with which many realists have rejected in principle the possibility of explaining science in a non-realist way. [...] Given the present state of the art, it can only be wish fulfilment that gives rise to the claim that realism, and realism alone, explains why science works. (1981, p. 48)

It is striking that despite this explicit forewarning and the clarity of his brief overall, Laudan's has become one of the most widely misread and misrepresented essays in the philosophy of science. The litany of references to 'Laudan's pessimistic induction', and misleading caricatures of it, give

[2] Laudan's article is easily the most influential article ever written on historical challenges to realism. For example, its Google Scholar citation index is 720 (in October 2014).

an entirely wrong impression of the intended thrust of Laudan's argument. This is not merely due to overlooking Laudan's anti-inductivist inclinations. The point of Laudan's conclusion is *not* about inductive fallibility—about the in-principle compossibility of past being one way, the future being another. Laudan's point is rather to emphasise the fact that the target of his historical 'confutation' is primarily a certain set of positive *arguments for* convergent realism. Laudan leaves completely open the possibility of coming up with some *other* arguments for a suitable realist epistemology of science that is compatible with the historical record, or there being a realist sense, consistent with the history of science, in which our theories are systematically latching onto unobservable reality.[3] The ensuing epistemological upshot is naturally a cautious one (assuming the absence of an alternative defence of realism), but Laudan's conclusion is clearly *not* a matter of the history of science speaking against realism *in toto*.

Laudan's piece must be read and appreciated in relation to its historical context: the statements and defences in the 1970s of the early versions of 'explanationist' realism, according to which the convergence of science towards the truth is a necessary ingredient in making sense of science vis-à-vis both its 'otherwise miraculous' success and some of its cumulative features. The easy-to-appreciate intuition, popularised by Putnam and others, has it that 'the positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle.' (Putnam 1975, p 73). The idea that 'success of science' could thus be linked to realist commitments gained currency in the writings of Boyd, Putnam, Newton-Smith, and Niiniluoto, for example, and Laudan's paper should be read as a reaction to the realist programme that tried to cash out this intuition in the context of freshly 'naturalised' philosophy of science.[4]

Against the backdrop of the concurrent realist programme, Laudan's article is an undeniable tour de force, and the line of thought still has some bite against (some aspects of) the 'naturalistic' No-Miracles argument (cf. Saatsi 2012). But in the grand scheme of things its focus is much narrower than is often recognised, and in as far as realists are capable of reinventing their positive case for realism, they may avoid the brunt of the historical 'induc-

[3] Cf. Laudan (1981):

> I must stress again that I am *not* denying that there may be a connection between approximate truth and *predictive* success. I am only observing that until the realists show us what that connection is, they should be more reticent than they are about claiming that realism can explain the success of science. (p. 32, my emphasis)

[4] Putnam (1978) explains, for example, that

> 'As [the realists] see it ... the notions of "truth" and "reference" have a causal explanatory role in epistemology' (p. 21. Quoted in Laudan, 1981, p. 22).

tion' as Laudan presented it. The reason has to do with Laudan's construal of 'success' and 'realism': by the current lights this is arguably unmotivated and overly demanding. In the current context Laudan can be criticized of demanding too much from the realist, and of failing to consider the possibility of more minimal ways of construing realist interpretations of science. In particular, in the current context it is relatively easy to point to some critical ambiguities and unwarranted presuppositions in Laudan's argument, with respect to central issues such as the following:

(a) What does success of science amount to (for the realist)?

(b) In what sense do theories latch onto reality (according to the realist)?

(c) What kind of knowledge do we have of the unobservable reality?

Laudan needs to make certain presuppositions in these regards in order to tackle 'the central question before us[:] whether the realist's assertions about the interrelations between truth, reference and success are sound.' (ibid, p. 22) The realist assertions, in the specific form considered by Laudan, have been subsequently overshadowed by more minimal realist claims that no longer capitalise on the notions of reference and truth in the same way.

Let's focus on the notion success, first of all. The nature of 'success' is, as Laudan put it, 'the first and toughest nut to crack.' (ibid, p. 23) It is tough because Laudan finds little explicit guidance from the realists to the nature of 'success'. According to his (probably fair) interpretation of the early realist's gambit, the realist 'wants to explain [. . .] why science in general has worked so well.' (ibid: 23) With this broad understanding of 'success' in mind, Laudan then moves on to 'assume that a theory is 'successful' so long as it has worked well, i.e., so long as it has functioned in a variety of *explanatory* contexts, has led to confirmed predictions and has been of broad *explanatory* scope.' (ibid, p. 23, my emphasis) The emphasis of explanatory success becomes increasingly prominent in Laudan's article, culminating in his infamous list of prominent false theories that were considered explanatorily successful by the scientists: the crystalline spheres of astronomy; the humoral theory of medicine; the effluvial theory of static electricity; 'catastrophist' geology; the phlogiston theory of chemistry; the vital force theories of physiology; the theory of circular inertia, and such. It is on the basis of such 'explanatorily successful' theories that Laudan concludes that 'what the history of science offers us is a plethora of theories which were both successful and [...] non-referential regarding [...] central *explanatory concepts*.' (ibid, p. 33, my emphasis)

What is so special about explanatory success? Why not fixate on (a certain kind) of predictive success instead, as the driver of realist commitments? Shifting the focus from explanatory to predictive success can furthermore deal with the problem of non-referential central terms, in as far as the very centrality of those terms is primarily due to their explanatory function. From the contemporary perspective, one fails to be convinced by the contention that realism *in toto* is challenged by past false theories that were (viewed to be) explanatorily successful. For even if realism that emphasises 'explanatory success' can be thus challenged, surely we can conceive of realism in less demanding, more minimal terms by focusing on predictive success instead.[5]

This is, of course, just what many realists—since Musgrave (1985) at least—have effectively said in response to Laudan (as I will presently recall). I have laboured on the point here since Laudan's emphasis on explanatory success is yet to be properly recognised in the literature.[6]

2.2 On the debate on 'pessimistic induction'

Many arguments in the ensuing debate on Laudan's 'historical induction' exemplify the following developments on the opposing sides of the debate.

On the realist side many have argued that the impact of the historical record can be significantly lessened by (a) reducing the number of problematic cases by strengthening the criteria required for realist commitments, and (b) weakening the commitment that those more stringent criteria should elicit. The more stringent criteria involve some suitably impressive kind of empirical success that is not exhibited by the bulk of Laudan's examples. To this end, realists have evoked theoretical maturity, and novel predictions, in particular. The weakening of realist commitments, furthermore, involves some kind of highly qualified *partial* veridicality of successful theories, making conceptual room for different senses in which a theory that is clearly false on the whole can still latch onto unobservable reality in a way that satisfies some critical realist intuitions (such as those fuelling the no-miracles intuition, in particular). The anti-realist side, on the other hand, has been reinforcing the historical challenge by more scrupulous gathering of historical evidence in effort to show that some particular historical cases, at least, speak against any reasonable construal of scientific realism.

While these developments have pushed the debate forward, it is worth noting how the ambiguities regarding the key notions of 'success' and (what I call) 'partial veridicality' have persisted in the literature. To begin with, while there has been a slow convergence towards emphasising predictive

[5]In the context in which Laudan was writing it is easy to understand why he construed 'empirical success' and 'realism' as he did, since the realist writings lacked clarity in these key respects. Perhaps Laudan's presuppositions are pertinent and justified in the context in which he operated. But realism has evolved since then; the context has changed. It is, of course, somewhat ahistorical to criticise Laudan's argument independently of the details of the views he explicitly opposed to, but by now those details seem to have been since forgotten by many, both realists and anti-realists alike.

[6]For just one recent example, Wray (2013) notes the distinction between predictive and explanatory success and discusses it in a parenthetical way, but he does not acknowledge the importance of this distinction for Laudan's argument and the dialectic on the whole.

over explanatory success on the realist side (e.g. Leplin 2004, Psillos 1999), this transition has been very gradual and incomplete. Some realists who have later come to emphasise predictive success were also making references to explanatory success in a similar spirit not that long ago. For instance, while later Leplin (2004) only argues that 'a theory's predictive success can warrant belief in the unobservable entities it postulates', the earlier Leplin (1997) mixes this idea with the notion explanatory success of theories in the *Novel Defence of Scientific Realism*, where he eplicitly argues that 'the explanation of the theory's explanatory success must be that the theoretical mechanisms it deploys are what actually produces the result.' (1997, p. 64) Similarly, Psillos's (1994) early statement and defence of the divide-et-impera move, first made in connection with his case-study of the caloric, sits very much on the fence between predictive and explanatory success.[7] Furthermore, many realist commentators *still* take realist commitments to be elicited precisely by explanatory success, in a way that tallies with Laudan's argument. (Prominent realists who have carried on arguing in this vein include McMullin (1984, 1996) and Sankey (2008), for example.)[8]

All in all, a significant degree of ambiguity has persisted in the literature regarding the kind of success associated with realism, even amongst the realists themselves. In the face of this ambiguity it is unsurprising that anti-realists have not felt a need to operate with a demanding notion of success when unearthing and using history of science to challenge the realist (despite the fact that some realists have clearly tightened their criteria for 'empirical success'). Consider, for instance, the critical reaction in Chang (2003) and Stanford (2003) against Psillos's (1994, 1999) 'preservative realist' reading of the caloric theory of heat. This criticism largely hinges on an explanatory construal of 'success', and as a result the two sides are effectively talking

[7]See Psillos (1994):

> Generally, not all deep-structural claims of a theory play the same role in the derivation of predictions and in providing well-founded explanations of observable phenomena. Some theoretical claims may be used centrally in the derivations of predictions and explanations of the phenomena, some others may be 'idle'. (p. 181)

By comparison, Psillos (1996) is already much less ambiguous.

[8]See e.g. Sankey (2008):

> I seek to extend the argument of McMullin (1987) that we are warranted in taking a theory to be 'approximately true' if it exhibits 'a high degree of explanatory success' (1987, 59). McMullin takes the explanatory success of a theory to be determined by how well it satisfies the various methodological criteria of theory appraisal (1987, 54). Where a theory exhibits a high degree of explanatory success, as indicated by satisfaction of the criteria, there are good grounds to take the general kinds of entities postulated by the theory to really exist, as well as what the theory says about such entities to be broadly correct, though open to further development (1987, 59-60). (p. 106)

past each other. In Psillos (1999) the focus is on novel predictive success and those parts of the theory that are both (i) responsible for such *predictive* success, and (ii) arguably preserved in otherwise radical theory change. The two critics, by contrast, focus on parts of the theory that were both (i) responsible for the theory's *explanatory* success, and (ii) clearly not preserved, with the intended upshot that a closer reading of the history refutes Psillos's realist gambit.[9]

It is one thing to say that such criticism grounded in the explanatory 'success' of past theories is unsurprising and understandable in the light of the persisting ambiguities; it is another thing entirely to maintain that such criticism should be taken to support 'a simple but compelling challenge to scientific realism' (Stanford 2003, p. 924). For whether or not realism *in toto* stands challenged by the non-preservation of critical explanatory aspects of theories depends on what realism amounts to. Clearly the historical evidence presented does not speak against realism *in toto* in as far as there is room for realism that is more minimal than that based on the presuppositions made by Stanford and Chang.

As I have discussed so far, one of the critical presuppositions concerns the notion of 'success': there is clearly room for a more minimal realist position in that regard, leaving behind the preoccupation with 'explanatory success' and committing more selectively only on those aspects of a theory that are responsible for novel predictive success. (Indeed, Psillos (1999), for example, already occupies such a position.) Another critical presupposition concerns the scope of realist commitments, viz. specifying the realist's epistemic attitude towards a given a piece of science that is successful in the commitment-eliciting way. Again, there is room for a more minimal realist position than the critics of preservative realism have in mind. For example, one of Stanford's central misgivings about preservative realism is that its commitments are not specified in a way that is *prospectively applicable*: to convince Stanford, preservative realism should offer a recipe that can be reliably applied 'prospectively—in advance of future developments—to identify the idle features or components of scientific theories.' (2003, p. 915) Many realists, myself included, have viewed as unjustifiably strict this demand that realist commitments should be thus prospectively specifiable. Take Newton's gravitational theory, for example, and the realist claim that its predictive success is (probably) due to the theory latching onto unobservable reality in some critical, success-inducing respects. Why exactly should the realist be furthermore required to offer a recipe that the Newtonians could have in principle employed to reveal these success-inducing respects in advance of further scientific developments due to Einstein and others?

For Stanford (2003a,b) it is critical that the realist provides a prospect-

[9]In a similar spirit Cordero (2011) appeals to the essential (explanatory) role of 'ether' in 19th c. wave theories of light.

ively applicable recipe, for two interconnected reasons. First, he argues that a retrospective identification of the ways in which a theory latches onto unobservable reality trivialises the realist gambit, since it 'virtually guarantees' that those aspects of the earlier theory that are judged to be latching onto unobservable reality (by virtue of being appropriately continuous with the latest theory) can also be viewed as the aspects responsible for the theory's empirical success.[10] Secondly, Stanford (2003b) appeals to what he calls the 'trust argument': if the articulation of the realist's epistemic commitments implies that we cannot *trust* our current science in what it says about the unobservable—in the sense that we cannot pin down some aspects in which a theory is latching onto unobservable reality—then those epistemic commitments are not worthy of the realist label.

Let's start with the triviality challenge. It is true, of course, that our current theories offer the only perspective available to us on those aspects of past theories that can be deemed to have potentially latched onto unobservable reality. In as far as we have reason to take a current theory to be latching better onto unobservable reality than a past theory, the shared aspects of the theories are the only ingredients available for a realist explanation of the past theory's empirical success. This much is undeniably true of the realist's epistemic predicament; to have it otherwise would require a theory-independent access to the way the world is. But why think, as Stanford does, that it is 'virtually guaranteed' that the shared aspects—whatever they are—can be deemed to *account for* the past theory's success from a realist perspective? Why think that we cannot have a critical grasp of the realist sense of 'accounting for past successes', so as to be able to judge fairly whether or not the shared aspects genuinely do account for the past successes in terms that satisfy the realist aspirations?

Stanford has no argument to show that such a critical grasp cannot be had. What he points to, rather, are serious ambiguities in the realists' characterisations of what is doing the 'accounting'. Admittedly realists have said very little about what it takes for shared aspects to furnish a genuine realist account of past successes, and in the face of this it is natural to worry, as Stanford does, that realists end up fooling themselves by expecting every kind of continuity to furnish a realist account of past empirical successes. But such a worry is not an argument against the realist strategy *per se*; it

[10] See Stanford (2003a):

> It is the very fact that some features of a past theory survive in our present account of nature that leads the realist *both* to regard them as true *and* to believe that they were the sources of the rejected theory's success or effectiveness. So the apparent convergence of truth and the sources of success in past theories is easily explained by the simple fact that both kinds of retrospective judgments about these matters have a common source in our present beliefs about nature. (p. 914)

does not show that the strategy of responding to historical 'inductions' by retrospective identification of aspects of reality-latching is somehow intrinsically flawed. What Stanford has provided, rather, is an argument against realists' *implementations* of this strategy.

I think Stanford is right in objecting to some popular implementations of the realist strategy. In particular, many realists have pretended to be able to offer something like a general *recipe* for judging whether or not two theories share aspects that furnish a realist account of the past theory's empirical success. Thus, structural realists, for instance, have appealed to a generic notion of 'structural correspondence' to this effect, the thought being that we can *trust* the 'structural' content our current theories', while maintaining an agnostic or even sceptical attitude towards the super-structural aspects (e.g. the nature of the entities involved). A challenge to this kind of *recipe-realism* is that all attempts to pin down a general recipe along these lines have arguably resulted in a characterisation of realist commitments so vague and ill-defined as to deflate the realist claim to be able to *trust* in our current theories in the sense given by the recipe.[11] For Stanford this challenge is (part of) his 'trust argument': in presenting a recipe for stating their epistemic commitment in a way that is compatible with various different kinds of theory changes in the history of science the realist is forced to 'defend realist inferential entitlements that are so weak as to capitulate to the realist's opponent on the question of whether we can safely trust the accounts of nature given by current or future successful scientific theories' (2003b, p. 572).

Stanford's 'trust argument' has bite against the kind of recipe-realism that endeavours to offer a generic recipe for delineating the trust-worthy parts or aspects of our current theories. But if read as an argument against realism *in toto*, it again embodies an undue presupposition about what realism must amount to. Stanford operates with a particular conception of scientific realism in mind: scientific realism defends our ability to 'trust or believe what our own best scientific theories tell us about what things are like in otherwise inaccessible domains of the natural world.' (2006, p. 158)[12] Although this way of construing realism is popular and has a venerable history, there are more minimal ways of construing the scope of realist commitments.

One can defend a realist stance without the ambition of recipe-realism. In particular, the realist can instead defend her trust in the reliability of the scientific method in yielding theories that latch better and better onto the unobservable reality; trust in the corresponding objective theoretical progress of science; trust in the thesis that our best theories that make novel

[11]It is commonplace to state that structural realism is a matter of being *committed to* our theories' structural claims about the unobservable reality, or *knowing* the structure of unobservable reality.

[12]Selective realism then amounts to providing a reliable recipe for picking out the 'bits' of our current theories that we can trust.

predictions (by and large) do so by virtue of latching onto unobservable reality. These alternative ways of expressing realist commitment have a venerable history as well, and they need not collapse either to realism in Stanford's sense, or to the kind of neo-instrumentalism that Stanford proffers as an alternative to realism.[13] I will characterise a 'minimal' realism attitude towards fundamental theories in this spirit, in terms of theoretical progress, in the final section.[14] Such realism is compatible with the historical record of science, and it is not touched by Laudan's argument by virtue of rejecting some of the critical assumptions that this Old 'induction' presupposes.

2.3 New Induction

What about Stanford's New Induction—does it add to the historical pressure to give up realist aspirations altogether, in connection with 'fundamental theories'? (Stanford, 2006) The answer is no. To see why, let's recall the gist of the New induction and its target, to begin with.

(P1) *Historical fact 1:* Scientists often fail to consider all plausible explanations of some phenomenon (at least in the fundamental sciences).

(P2) *Fact about the scientific method:* Scientists often perform eliminative inferences: they endorse or commit to a particular theory after having eliminated all but one of the explanations they have managed to consider.

(P3) *Historical fact 2:* Scientists often, in the fullness of time, come up with and adopt an alternative explanation that was unconceived at the time of the earlier eliminative inference.

(C) *Conclusion:* The eliminative inferences in science are unreliable: they do not deliver trustworthy results, since they do not take into account plausible, unconceived alternative explanations.

This is clearly a new historical argument against realism. It puts an interesting new spin on the Old induction by virtue of performing an induction over *scientists* (as opposed to theories) in a more prescribed context of fundamental theorising employing eliminative inference. Due to these novelties the argument can in some ways go beyond the Old induction in its argumentative resources; it is not merely a 'novel red herring' that works against the realist only to the extent the Old induction already does (cf. Chakravartty (2007) and Magnus (2010) for nice analyses).

[13]In Saatsi (in progress *b*) I characterise and challenge recipe-realism in more general terms.

[14]In Saatsi (in progress *a*) I discuss minimal realism in more detail in the context of epistemic conceptions of scientific progress.

Nevertheless, realist responses to the Old induction substantially interact with the New induction. One way to see this is to ask: what should we make of the unreliability of eliminative inferences established by the New induction (granting the premises)? What epistemic attitudes are available towards fundamental theories arrived at by an eliminative inference? Is anti-realism now in the offing, as Stanford proposes? Drawing a squarely anti-realist conclusion again overshoots by extrapolating beyond the historical evidence that supports (C). Given the moves that a realist may already feel compelled to make in the face of the Old induction—namely, emphasising novel predictive success, and adopting a piecemeal approach to exemplifying the sense in which a realist can account for an old theory's predictive success from our current vantage point—it is not clear that the New induction imposes any further threat on the realist.

The minimal realist position that I will outline in the next section, for example, is compatible with the New induction's conclusion that eliminative inferences in fundamental science are unreliable with respect to the purported explanatory mechanisms. Notwithstanding the New induction's conclusion, a realist can maintain that eliminative inferences are reliable in the sense that the resulting novel predictive successes (if any) are due to the theories suitably latching onto reality in ways that can be retrospectively analysed from the perspective of even better theories. In the light of the realist's response to the Old induction the historical record that demonstrates the recurring occurrence of unconceived alternatives gets interpreted quite differently. For example, general relativity may be viewed as an unconceived alternative to special relativity, which in turn can be viewed as an unconceived alternative to Newtonian mechanics, but for the realist the subtle inter-theoretic relations between these theories can furnish a sense in which the earlier theories nevertheless latched onto unobservable reality in critical respects. Thus, the later theories can be viewed as alternatives (as opposed to significant improvements) to the earlier theories *only* in a somewhat curtailed sense that does not support a kind of transient underdetermination that should further trouble the realist. The premises of the New induction, as presented above, conceal the critical inter-theoretic connections that underlie the realist's positive attitude towards the outcomes of eliminative inferences in fundamental science. This gives the misleading impression that (C) should be automatically viewed as a thoroughgoing anti-realist conclusion.

Stanford's conception of realism is obviously not going to be fulfilled by the realist moves that I have envisaged above as a way of sidestepping both the Old and the New induction. As already indicated, Stanford operates with a particular notion of realism in mind, characterising realism as 'the position that the central claims of our best scientific theories about how things stand in nature must be at least probably and/or approximately true.' (2006, p. 6). The minimal realist attitude that I will presently delineate is a far cry from this kind of optimism, and one might even worry that the disagreement becomes largely terminological: what I call (minimal) realism, Stanford calls anti-realism? But recall the key question at stake: how strongly does the history of science speak against realism? In attempting to answer this question as carefully as we can, it is better not to stick to such a demanding conception of realism and use it as a foil in an argument for a strong form of anti-realism in the 'instrumentalist tradition', according to which 'the fundamental theories [...] should be regarded [...] simply as powerful conceptual tools for action and guides to further inquiry.' (Stanford 2006, p. 24) For regardless of what we choose to call a position that results by giving up the presuppositions required by Stanford's argument, the New induction is a non-sequitur as an argument for upholding the instrumentalist tradition: there are more positive epistemic attitudes towards fundamental science that directly tap into some central realist intuitions, and fall between Stanford's neo-instrumentalism and his demanding conception of realism.

3 Minimal realism

This paper is primarily about the limits of historical evidence in the realism debate, analysed by questioning and weakening the presuppositions that anti-realists have made about realism in their historical 'inductions'. My criticism regarding the overshooting of the anti-realist arguments already stands, and I trust to have already established the *possibility* of escaping these arguments without landing in empiricism or instrumentalism. We still need to elaborate and provide a positive argument for a view that thus escapes the tension with the historical record remains, however. To finish the paper I will delineate a way of thinking about minimal realism in the spirit of my remarks on historical inductions above.[15]

It is natural to understand scientific realism as being committed, at the minimum, to the claim that science as a matter of fact makes *theoretical* progress in the sense that theories better supported by scientific evidence (by and large) latch better onto unobservable reality. This way of understanding realism is broader than a popular conception of realism—a conception that Stanford also has in mind—in terms of theoretical *knowledge* of the unobservable.[16] If one thinks of theoretical progress in purely epistemic terms—in terms of accumulation of theoretical knowledge (see e.g. Bird,

[15]See also Saatsi (in progress *a*) and Saatsi (in progress *b*) for related discussion.

[16]Cf. Chakravartty (2011):

> Most commonly [realism] is described in terms of the epistemic achievements constituted by scientific theories . . . What all of approaches [to defining realism] have in common is a commitment to the idea that our best theories have a certain epistemic status: *they yield knowledge of aspects of the world, including unobservable aspects.* (p. 1, my emphasis)

2006)—then commitment to theoretical progress of science more or less entails the latter, popular conception of realism. But the epistemic conception of theoretical progress is too narrow: science can make theoretical progress that does not boil down to accumulation of knowledge. In particular, science can make theoretical progress in the sense of theories latching better and better onto reality in a way that drives theories' increasing empirical adequacy and enables them to make novel predictions. Corresponding to this broader conception of theoretical progress there is a more minimal conception of realism, understood simply as a commitment to this broader kind of theoretical progress.

The broader conception of theoretical progress is characterised in terms of theories 'latching (better) onto unobservable reality'. I have also made liberal use of the 'latching onto' locution throughout the paper. What does it mean? I answer this question in more depth elsewhere (cf. Saatsi, in progress *b*), but here is the gist of it. Take the commonplace notion that theories—or theoretical representations, or models—can, in a given respect, provide better or worse representations of reality. This provides an intuitive and relatively unproblematic starting point. One way in which a theory T' can be a better representation of reality than T, is with respect to observable phenomena, by virtue of being more empirically adequate. This sense of representational improvement in science is commonplace and acknowledged by realists and anti-realists alike. But it can furthermore be the case that the boost in empirical adequacy from T to T' is *by virtue of* T' being a better representation of the unobservable features of the world that lie behind the relevant empirical phenomena. If that is the case, then we can say that T' latches better onto unobservable reality than T—that is, T' latches onto unobservable reality in respects relevant to the improvement in empirical adequacy. It is this sense of latching onto unobservable reality, this sense of theoretical progress, that realists are minimally committed to.

What does it take for a theory to be a 'better representation' of the unobservable reality (so as to be responsible for increase in empirical adequacy)? This sense of representational improvement is not a matter of 'overall fit' between a theoretical representation and (unobservable) reality—it is rather a matter relative to the respective degrees of empirical adequacy: a 'better' representation is more veridical with respect to some aspects that matter for improving empirical adequacy, i.e. a better representation is closer to the way the world actually is in those respects. If we were equipped with knowledge of the way the world actually is, we would be able to account for the improvement in empirical adequacy in terms of objective features of the representational relations between the two theories and the world, judging the unobservable aspects of the world represented by T' to be closer to actuality, in the relevant respects, than those represented by T. Not knowing the way the world is independently of our current theories, we can still use these theories to account for our past theories' empirical success in terms of rich inter-theoretic relations between the theories, and project confidence that those realist accounts will not be undermined by future developments.

Clearly this kind of minimal realist commitment provides nothing like a general *recipe* that could be applied to a given current theory—e.g. the standard model of particle physics—to specify what unobservable features of the world we can claim to know. But recipe-realism looks like a fool's errand, and should not be viewed as a sine qua non of vindication of realism. According to minimal realism the best we can do (with respect to some fundamental theories) is to commit ourselves to general progress of science that extends also to the theoretical level. Sometimes—for example in connection with fundamental physics—theoretical progress is just a matter of theories latching better and better onto unobservable reality so as to drive the empirical and instrumental progress of science. Specific cases of inter-theoretic relations can be studied to provide a fleshed out sense of the way in which false past theories, such as Newtonian gravity, have latched onto reality so as to satisfy the minimal realist's creed that these false theories are predictively successful (to the extent they are) by virtue of 'getting something critical right' about the world. Such studies can then serve as particular, locally applicable *exemplars* of the rich ways in which we can retrospectively account for our past theories' empirical success, in the realist spirit, in terms of them having latched onto reality. We can study such exemplars, comparing and contrasting them, to get a satisfactory handle on retrospective accounting of empirical success, but this will fall much sort of yielding a realist recipe (cf. Saatsi, forthcoming a).

The locution 'latching (better) onto reality' is meant to pick out the most general notion that taps into the 'explanationist' realist agenda. It is purposefully characterised in very broad terms, so as to be compatible with various forms of scientific realism that are all in the business of accounting (in realist terms) for the empirical success of false past theories from our current perspective. Different realists offer different, more precise definitions that take a stand on exactly what kind of representational adequacy in a given scientific case can *account for* a theory's degree of empirical adequacy in a way that satisfies the realist intuitions and doesn't give the game away to the anti-realist. The advocates of structural realism, for example, claim that the empirical success of past theories can be accounted for in terms of these theories providing a veridical representation of critical *structural aspects* of reality.[17] A related but subtly different position claims that the empirical success of past theories is (sometimes) best accounted for in terms of these theories providing a veridical representation of critical *less specific properties* (Saatsi 2005). Both realist positions are committed to the claim that the degree of empirical adequacy enjoyed by Fresnel's optical ether theory can be

[17]There are various differences amongst the structural realists. For a review, see Frigg and Votsis (2011)

accounted for in terms of Fresnel's theorising latching onto reality. Similarly, the boost in empirical adequacy achieved by classical electrodynamics—itself a false theory as a classical (non-quantum) theory—can arguably be accounted for in terms of the theory latching better onto reality. These different realist positions all claim that there is an unambiguous sense of theoretical progress from Fresnel to Maxwell to Feynman; the disagreement is mainly about how to best capture the specific sense in which these theories are progressively latching onto reality. This progress is captured by minimal realism in terms that are more general and independent of particular realist preferences for precisifying the 'latching onto' notion in specific historical cases.[18]

4 Conclusion

For several decades anti-realists have argued that the history of science clearly speaks against realism. How can realists still remain unconvinced by these arguments? Instead of being an indication of dogmatism, the persistence of the disagreement can be at least partly explained by simply noting that the strength of the historical evidence against realism depends on what 'realism' is taken to be. I have argued that the prominent historical arguments against realism display a systematic overshooting in the conclusions drawn from the historical evidence. Having first identified this trend in the debate ensuing Laudan's 'Old induction', it was easier to see how Stanford's New induction overshoots as well, by drawing a squarely anti-realist conclusion from historical data that is compatible with a suitably modest realist attitude towards 'fundamental theories'.

In the abstract, the issue is just this. Anti-realists' inductions presuppose that it is essential for realism to have certain features. Having argued that realism-thus-construed is undermined in the face of the history of science, anti-realists have concluded (or have been taken to conclude) with empiricism, or instrumentalism about science. The problem is that the step from realism-thus-construed to anti-realism is too large: there are intermediate views, naturally viewed as exemplifying a key realist credo, that fall between anti-realism and realism-thus-construed. Furthermore, since these intermediate views are not undermined by the same historical record, a historically-driven anti-realism owes us an argument for jumping all the way to anti-realism.

[18]Minimal realism is also meant to be compatible with *pluralism* regarding different philosophical, meta-scientific frameworks that can be used to capture 'latching onto' in more specific terms. These include, for example, (i) the similarity approach (e.g. Giere 1988, Teller 2001); (ii) the partial isomorphism approach (e.g. da Costa and French 2003); (iii) the mathematico-logical structure approach (Worrall 2007).

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