ARE UNCONCEIVED ALTERNATIVES A PROBLEM FOR SCIENTIFIC REALISM?

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Kyle Stanford starts his a recent book, *Exceeding Our Grasp*, with the claim that "the most powerful challenge to scientific realism has yet to be formulated" (2006: 9). He goes on to formulate what he takes to be that challenge, offering a version of the pessimistic meta-induction that includes elements from the underdetermination argument. I have previously labeled the meta-induction "the most powerful argument against scientific realism". I did so because "it rests on plausible claims about the history of science". Stanford brings out just how plausible such claims can be. I think his version of the meta-induction is indeed the most powerful challenge. However, I think the challenge can be met.

I shall be drawing on earlier discussions in "Scientific Realism" (2005) and *Realism and Truth* (1997). I start in section 1 by setting out what I take scientific realism to be, followed in section 2 by a brief summary of my response to the underdetermination argument against it. The paper begins in earnest in section 3 with my response to the pessimistic meta-induction. Against this background, I will turn to Stanford's argument in section 4.

1. What is Scientific Realism?

Science appears to be committed to the existence of a variety of unobservable entities - to atoms, viruses, photons, and the like - and to these entities having certain properties. The central idea of scientific realism is that science really is committed and is, for the most part, right in its commitments. As Hilary Putnam once put it, realism takes science at "face value" (1978: 37). So, for the most part, those scientific entities exist and have those properties. We might call this the "existence dimension" of realism. It is opposed by those who are skeptical that science is giving us an accurate picture of reality.

Scientific realism is not only committed to the existence of unobservable entities but also to their "mind-independence": they do not depend for their existence and nature on the cognitive activities and capacities of our minds. This is the "independence dimension of realism. This dimension is challenged by philosophers of science like Thomas Kuhn and Paul Feyerabend who think that scientific entities are somehow "constructed" by the theories we have of them. This is not a challenge I shall be concerned with in this paper.[1]

Before attempting a "definition" of scientific realism, some clarification is called for. First, talk of the commitments "of science" is vague. In the context of the realism debate it means the commitments of *current scientific theories*. The realist's attitude to past theories will be a central concern of this paper. Second, the realist holds that science is right, "for the most part". It would be foolhardy to hold that current science is not making any mistakes and no realist would hold this. Third, this caution does not seem to go far enough: it comes

[1]Elsewhere I take a very dim view of constructivism (1997, 1999, and 2001).

too close to a blanket endorsement of the claims of science. Yet scientists themselves have many epistemic attitudes to their theories. These attitudes range from outright disbelief in a few theories that are useful for predictions but known to be false, through agnosticism about exciting speculations at the frontiers, to a strong commitment to thoroughly tested and well-established theories. The realist is not less skeptical than the scientist: she is committed only to the claims of the latter theories. Furthermore, realism has a critical aspect. Theories may posit unobservables that, given their purposes, they need not posit. Realism is committed only to "essential" unobservables. In brief, realism is a cautious and critical generalization of the commitments of well-established current theories.

More clarification would be appropriate but this will have to do. Utilizing the language of the clarification we can define a doctrine of scientific realism as follows:

> **SR**: Most of the essential unobservables of well-established current scientific theories exist mind-independently.

With a commitment to the existence of a certain unobservable goes an implicit commitment to its having whatever properties are essential to its nature as that unobservable. But, beyond that, SR is noncommittal on the properties of the unobservables, on the scientific "facts". Yet the scientific realist is often committed not only to the entity realism of SR but to a stronger "fact" realism:[2]

> **SSR**: Most of the essential unobservables of well-established current scientific theories exist mind-independently and mostly have the properties attributed to them by science.

The existence dimensions of these doctrines are opposed by those who are skeptical of what science is revealing; the independence dimension is opposed by the constructivists.[3]

2. Summary of Response to the Underdetermination Argument

This appealing and influential empiricist argument against scientific realism starts from a doctrine of empirical equivalence. Let *T* be any theory committed to unobservables. Then,

> **EE**: *T* has empirically equivalent rivals.

This is taken to imply the strong underdetermination thesis:

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[2]The scare quotes around 'facts' are to indicate that the use of the term can be regarded as a mere manner of speaking, not reflecting any commitment to the existence of what many regard as very dubious entities. Ian Hacking (1983) calls this sort of doctrine "theory realism". I prefer to talk of "facts" rather than theories to emphasize that the doctrine is about the world itself not our account of it.

[3] For more on the definition of scientific realism, including resistance to including correspondence truth as part of the definition, see my 1991, 1997, 1999, and 2005.

SU: *T* has rivals that are equally supported by all possible observational evidence for it.

So, realist doctrines like SR and SSR are unjustified.

A good reason for believing EE is that there is an empiricist algorithm for constructing an equivalent rival to *T*. Consider T_o, the theory that the observational consequences of *T* are true. T_o is obviously empirically equivalent to *T*. Still, it may not count as a rival because it is consistent with *T*. That is easily fixed: *T** is the theory that T_o is true but *T* is not. *T** is an empirically equivalent rival to *T*. So EE is established.

The first step in responding to the underdetermination argument is to rule out theories like *T** that seem to have been produced in this "trick" manner. But we have to do this in a way that is not question-begging. That way starts, I have argued (1997, 2005), by emphasizing that our task is not to defend Scientific Realism from extreme "Cartesian" skepticism.

Cartesian skepticism challenges not only our belief in unobservables but our belief in observables: it challenges "realism about the external world". That challenge is both different from and prior to the challenge to Scientific Realism. It raises doubts about the very clearest cases of knowledge about observables, doubts occasioned by skeptical hypotheses such as that we are manipulated by an evil demon. The issue of scientific realism arises only once such doubts about the observable world have, somehow or other, been allayed. Allaying those doubts will involve accepting some method of nondeductive ampliative inference. Not even a theory about observables can be simply deduced from any given body of evidence; indeed, not even the very existence of an observable can be deduced "from experience". If we are to put extreme skepticism behind us and gain any knowledge about the world, we need some ampliative method of inference. Armed with that method, and confident enough about the observable world, there is thought to be a *further* problem believing what science says about unobservables. So the defense of Scientific Realism does not require that we refight the battle with extreme skepticism, just that we respond to this special skepticism about unobservables.

Against this background, I argue that *T* is indeed justified over empirically equivalent rivals like *T** because those rivals can be dismissed by whatever ampliative inferences enable us to avoid extreme skepticism. Precisely how far we can go in thus dismissing rivals remains to be seen, of course, pending an account of how to avoid extreme skepticism. Still, we can conclude that EE as it stands cannot sustain SU. If the underdetermination argument is to work, it needs to start from a stronger equivalence thesis, one that is only concerned with what we might call "genuine" rivals to *T*, ones that cannot be thus dismissed by the inferences that avoid extreme skepticism.

The next step in responding to the underdetermination argument depends on careful attention to what is meant by "empirical equivalence" in the premise EE. I argue that once we note the role of auxiliary hypotheses in testing theories, and our ability to

create evidence in novel experiments, the argument collapses. We have no good reason to believe an empirical equivalence thesis that would sustain the conclusion SU.

3 The Pessimistic Meta-Induction

The basic version of this argument is aimed at an entity realism like SR: the unobservables posited by past theories do not exist; so, probably the unobservables posited by present theories do not exist. Another version, largely dependent on the basic one, is aimed at a "fact" realism like SSR: past scientific theories are not approximately true; so, probably present theories are not approximately true. (This is an example of the convenience of exploiting the disquotational property of 'true' to talk about the world.) Both versions of the argument rest on a claim about past theories from the perspective of our present theory.[4] And the pessimistic suggestion is that, from a future perspective, we will have a similarly critical view of our present theories. Laudan (1981, 1984, 1996) has supported these claims about the past with a list of theoretical failures.

Scientific realism already concedes something to the meta-induction in exhibiting *some* skepticism about the claims of science (sec. 1). It holds that science is more or less right but not totally so. It is committed only to well-established theories not exciting speculations. It leaves room for a theoretical posit to be dismissed as inessential to the theory. According to the meta-induction, reflection on the track record of science shows that this skepticism has not gone nearly far enough.

The realist can respond to the meta-induction by attacking the premise or the inference. Concerning the premise, the realist can, on the one hand, resist the bleak assessment of the theories on Laudan's list, claiming that while some of the unobservables posited by these theories do not exist, others do; or claiming that while there is a deal of falsehood in these theories, there is a deal of truth too (Worrall 1989;[5] Kitcher 1993: 140-9; Psillos 1999: chs 5-6). On the other hand, the realist can claim that the list is unrepresentative, that other past theories do seem to be approximately true and to posit entities that do exist (McMullin 1984).

In the light of history, some skepticism about the claims of science is clearly appropriate. The argument is over how much, the mild skepticism of the realist, or the sweeping skepticism of the meta-induction. Settling the argument requires close attention to the historical details. This is not, of course, something that I have ever attempted but I have made some general remarks about the problem.

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[4]So there is a "tension" in the argument: it seems to rest on a realist view of present science and yet concludes that this realist view is mistaken; see Leplin 1997: 141-5. I suppose that we should see the meta-induction as some sort of *reductio*.

[5]Worrall takes the truth to be not about the *nature* of entities but about *structures* that contain the entities. For a critical discussion of this "structural realism", see Psillos 1999, ch. 7.

How can we *tell* whether *F*s, posited by a past theory, exist? Given the disquotational schema, "'*F*' refers iff *F*s exist", many approach this question by considering another: How do we tell whether '*F*' refers? Then in answering this question philosophers typically appeal to some theory of reference. The first problem with this common approach is that the theory of reference appealed to is usually a description theory[6] and yet we should have all learnt from the revolution in the theory of reference that a causal theory of some sort may be appropriate. This points to the second, deeper, problem: in attempting to answer the existence question by answering the reference question, the approach has its epistemic priorities all wrong. For, *we know far less about reference*, particularly about when to apply a description theory and when to apply a causal theory, than we know about what exists. In light of this, the rational procedure is to let our view of what exists guide our theories of reference rather than let our theories of reference determine what exists.[7]

So, we should not use a theory of reference to answer our existence question. How then should we answer it? Consider how, in general, we argue directly for the *non*existence of *F*s. On the basis of the established view of *F*s, we start, implicitly if not explicitly, with an assumption about the nature of *F*s: something would not be an *F unless it were G*. Then we argue that nothing is *G*. So, there are no *F*s. Very often this argument is persuasive and generally accepted. But someone might respond by denying the assumption about nature. "*F*s do not have to be *G*, they are just mistakenly thought to be *G*. So the argument proves nothing". How do we settle this disagreement? It may be difficult. We can try saying more about the established view of *F*s, but this may not do the trick. After all, the responder does not deny that *F*s are thought to be *G*, just that being *G* is part of the nature of being an *F*. And the established view may not be clear on the nature issue. We may be left with nothing but a "clash of intuitions" over that issue. In such a situation, we should wonder whether there is a genuine issue to settle: there may be no determinate matter of fact about the nature issue. If there is not, then there is no determinate matter of fact about whether the absence of *G* things establishes the nonexistence of *F*s.

Consider two humdrum examples. Most people are antirealist about witches because they believe that nothing casts spells, rides on a broomstick through the sky, and so on. Some people may be antirealist about God because they are convinced by the Problem of Evil that nothing is both all powerful and all good. But these are grounds for antirealism only if casting of spells, riding on broomsticks, and so on, and being all powerful and all good, are essential to witches and God, respectively. There may be disagreement about that.

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[6]See Kuhn (1962) for an argument along these lines. Stephen Stich (1983) and others have argued similarly for various forms of eliminativism about the mind. Stich has since recanted (1996: 3-90).

[7] My 1991, 1997, and 2009 argue for this priority. There is, of course, a truth underlying the mistaken approach: to determine whether the posits of a theory exist we have to know what those posits are and for that we have to *understand* the language of the theory (1997: 50-3). But understanding a language is a practical skill that does not require theoretical knowledge about the language, else we would understand very little (pp. 270-5).

And there is room for worry that disagreement may not be entirely over matters of fact (2009).

In light of this, we can expect that close attention to the historical details about past unobservables will reveal some ontologically determinate cases but very likely some indeterminate ones too. The determinate cases will surely include some of nonexistence; phlogiston is a good candidate. But it will surely also include some of existence; the atoms posited in the nineteenth century are good candidates.[8] So, we should conclude that the premise of the meta-induction is overstated, at least.[9] But how much is it overstated? That depends on the "success ratio" of past theories, the ratio of the determinately existents to the determinately nonexistents + indeterminates. Where is this ratio likely to leave scientific realism? To answer this we need to consider the meta-induction's inference.

I think (1997: 162-5) that there is a good reason for being dubious about the inference. Suppose that our past theories have indeed failed rather badly to get the unobservable world right. Why would that show that our present theories are failing similarly? It clearly would show this if we supposed that we are *no better* at finding out about unobservables now than we were in the past. But why suppose that? Just the opposite seems more plausible: we are now *much better* at finding out about unobservables. A naturalized epistemology would surely show that science has for two or three centuries been getting better and better at this. Scientific progress is, to a large degree, a matter of improving scientific methodologies often based on new technologies that provide new instruments for investigating the world. If this is so - and it seems fairly indubitable - then we should expect an examination of the historical details to show improvement over time in our success ratio for unobservables. If the details do show this, it will not matter to realism that the ratio for, say, two centuries ago was poor. What will matter is that we have been improving enough to now have the sort of confidence reflected by SR.[10] And if we have

[8]Also molecules and microbes; see R. Miller 1987.

[9] So the pessimistic meta-induction needs to be modified. It is important to see that, to maximize the chance of the meta-induction being effective against SR, it should take the following form: most of the unobservables posited by theories at all, or most, past times do not exist; so, probably, most of the unobservables posited by current theories do not exist. Marc Lange (2002) has shown that this form is much better than the following common one: most unobservables posited in the past do not exist; so probably most unobservables posited now do not exist. The problem with the latter form is that it commits "the turnover fallacy". Because false theories turn over much more often than true ones, its premise might be true even though, at any time, most of the unobservables posited at that time exist. (I was prescient enough, or lucky enough, to take the former form as the one to be criticized in my 1997: 161-2.)

[10]So this realist response does not take the failures of "immature" science to be irrelevant to the defense of realism, thus threatening the defense with "vacuity" (Laudan 1981: 34). Rather, it takes the relevance of a science's failures (and successes) to that defense to increase with *the degree of* that science's maturity, a degree assessed by an empirical epistemology.

been improving, but not fast enough for SR, the realist can fall back to a more moderate commitment to, say, a high proportion of the unobservables of currently well-established theories.

Improvements in scientific methodologies make it much harder to mount a case against realism than seems to have been appreciated. For the appeal to historical details has to show not only that we were nearly always wrong in our unobservable posits but that, despite methodological improvements, we have not been getting significantly righter. It seems to me most unlikely that this case can be made.[11]

4. Stanford's Unconceived Alternatives

Stanford is concerned with underdetermination, but not with the sort alleged to arise from empirical equivalence. So he is not urging a version of the argument discussed in section 2. He is concerned rather with "recurrent, transient underdetermination": "the possibility that there might be…empirically inequivalent but nonetheless well-confirmed, serious alternative among the theories that we have not yet even imagined or entertained" (p. 17). The worry here is not with alternatives that I have labeled "not genuine", ones that are Cartesian fantasies or concocted by some philosopher's algorithm. The worry is with alternatives that are as genuine as could be, "ordinary theoretical alternatives of the garden variety scientific sort that we have nonetheless simply not yet managed to conceive of in the first place" (p. 18); the worry is with "scientifically serious theoretical possibilities" (p. 31). This is "the *problem of the unconceived alternative*" (p. 18). Laurence Skar has already raised this problem in his delightfully-named article, "Do Unborn Hypotheses Have Rights?" (1981). Now, of course, there is a problem for realism only if theories *really do* face such "scientifically plausible competitors" (p. 18). Sklar assumes they do. Stanford sets out to argue that history supports this assumption: "we have repeatedly occupied a predicament of recurrent, transient underdetermination across a wide and heterogeneous variety of scientific fields and domains". This is so because "subsequent inquiry would routinely (if not invariably) reveal further, radically distinct alternatives as well confirmed by the previous available evidence as those we were inclined to accept on the strength of that evidence" (p. 19). The power of Stanford's argument comes from his examination of the historical record in support of this thesis.

Now history surely shows us that, typically, there comes a time when any theory *T1* is replaced by a successor theory *T2* that was previously unconceived. And *T2* is

[11] Jacob Busch (2006) criticizes my discussion of the pessimistic meta-induction but, as I point out in response (2006), he misplaces the onus. The meta-induction is offered as an argument against scientific realism. So there is an onus on those who propose it to show that it is a *good* argument. This requires that they establish its premise and its inference. The onus on realists is simply to show that the premise and/or the inference has *not* been established. Realists have already provided abductive arguments *for* realism and their further onus is simply to undermine arguments *against* it. I have accepted that onus (1997, 1999, 2001, 2005). I don't think that proponents of the meta-induction have fully accepted theirs.

likely to be at least as well supported by the evidence for *T1* as *T1* was. This should be uncontroversial. But Stanford goes much further: *T1* and *T2* are "radically distinct". *This is the crucial claim*. For, suppose that the entities posited by *T2* include those posited by *T1*, then clearly the change from *T1* to *T2* is no threat to an entity realism like SR. And suppose that from the perspective of *T2*, *T1* is approximately true, then clearly the change from *T1* to *T2* is no threat to a "fact" realism like SSR. Stanford's threat to scientific realism comes from his view that this sort of "continuity" is not typical in the history of science.

Despite his argument, Stanford is not generally skeptical about science: he is not suggesting that we should "never trust the deliverances of our scientific investigations" (p. 184; see also p. 37). And I have already allowed that some skepticism about the claims of science is obviously called for. The wise realist will commit only to the findings of well-established theories not to those of exciting speculations and, even there, will allow for some errors (sec. 1). Stanford is clearly calling for a more sweeping skepticism. Still, the difference between him and the realist must remain a bit uncertain. The dispute can sometimes look like one over whether a glass is half empty or half full (cf. Godfrey-Smith 2008).

As just noted, the realist can reply to the meta-induction by attacking its premise or its inference. The premise is, of course, a gloomy view of the past. And the strength of Stanford's discussion is that he has done a good job of deepening the gloom. He presents a range of cases suggesting a lack of continuity between a theory and its successor. Three chapters (3 to 5) are devoted to one case in particular, biological theories of generation and inheritance. He devotes two chapters (6 and 7) to rebutting realist attempts to lessen the gloom. A lot of this is fairly convincing and I shall not challenge him on it.[12] In any case I made no attempt to *calculate* (futilely?) the "success ratio" of past posits. Still, perhaps Stanford's discussion shows that I implied too much optimism about this ratio.

So the issue comes down to the inference. The realist's reply here is to argue that gloom about the past does not require gloom about the present. Standard versions of this reply emphasize the "immaturity" of past sciences. These versions, as Stanford notes, "point out differences in the breadth, precision, novelty, or other important features of the predictive and explanatory accomplishments of past and present theories" to invalidate the inference in the induction (p. 43). Briefly, these versions base their rejection of the inference on the claim that *present theories* are more successful than past ones. But this claim has, as Stanford nicely notes, "the lingering whiff of ad-hoc-ery" (p. 10). The version of the realist's reply that I have always preferred bases rejection of the inference on the claim that our *present methodology* is better than past ones. This version *explains why* present theories are more successful and hence removes the whiff of ad-hoc-ery.

My version of the realist's reply, summarized in section 3, is to argue that we have very good reason to believe that we have been getting better and better at learning about the

[12] Stanford is particularly harsh on realist attempts to lessen the gloom by appealing to theories of reference (2006: 147-55). Clearly this is not an attempt I would make.

unobservable world; good reason to believe that, aided by technological developments, there has been, over recent centuries, a steady improvement in the methodology of science. *That's why* our present theories are more successful. Indeed, serious sciences like physics take this improvement for granted, as methodological instruction in the classroom demonstrates. A naturalized epistemology surely supports this confidence. And, I have argued, there seems to be no basis for a pessimistic meta-induction about this epistemology (1997: 163-4). So, we have serious scientific support for the view that theories are indeed getting more successful and hence, the realist argues, more true.

Stanford responds to standard versions of the realist's reply not to mine. His response is surprisingly brief and quite inadequate. He claims that the realist's reply

> simply does not apply to the problem of unconceived alternatives or to the new induction that supports it…because the latter arguments concern the *theorists* rather than the *theories* of past and present science. That is, they point out that past theories…were at one time *the best or only theories we could come up with*, notwithstanding the *availability* of equally well-confirmed and scientifically serious alternatives….that present theorists are no better able to exhaust the space of serious, well-confirmed possible theoretical explanations of the phenomena than past theorists have turned out to be. (2006: 44)

Stanford has simply missed the point of the realist's reply. He is surely right that present scientists are not likely to be better at exhausting that theoretical space. But this is just to endorse what I earlier called "uncontroversial": every present theory is likely to have a successor that we have not yet conceived. An effective response to the realist needs to support what I earlier called "the crucial claim": *the claim that the present theory and its successor will be "radically distinct"*. For that claim is the one that threatens realism. And that claim is what the realist's reply is in effect challenging. The realist is pointing out that even if in the past theories were often radically distinct from their successors they are not likely to be now. *Continuity is the issue*. The realist reply is that discontinuity in the past does not mean discontinuity now. Stanford has failed to address this point.

I did not claim that this realist reply should remove all worries occasioned by the meta-induction, and I do not suggest this now. Still, I do think that this reply does a lot to alleviate the problem for doctrines like SR and SSR. And, as I pointed out, we can always fall back to a weaker realist position. Stanford thinks that all such fallbacks leave realism with only a "Pyrrhic" victory (p. 142). I don't think that this is so. The basic realist view that we are getting to know more and more about the unobservable world remains intact even if there remains some doubt as to just how successful we have been at this so far.

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