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Doing Away with the No Miracles Argument*

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Abstract

The recent debate surrounding scientific realism has largely focused on the "no miracles" argument (NMA). Indeed, it seems that most contemporary realists and anti-realists have tied the case for realism to the adequacy of this argument. I argue that it is mistake for realists to let the debate be framed in this way. Realists would be well advised to abandon the NMA altogether and pursue an alternative strategy, which I call the "local strategy".

1. Introduction

The main sticking point in the contemporary debate over scientific realism is the realist's *epistemic optimism* about current science: that we are warranted in believing that our current best tested theories are at least approximately true, and so provide a broadly correct description of the observable and (crucially) unobservable features of a mind-independent world.

The standard realist argument for such optimism is the famous "no miracles" argument (NMA), which asserts that the best explanation for remarkable empirical success of our current best theories—e.g. their novel predictive accuracy and instrumental utility as background theories (producing successful experiments, methodologies, and so forth)—is that their central theoretical claims, including those concerning unobservable entities and structures, are at least approximately correct. Indeed, it seems that most contemporary realists and anti-realists have tied the case for realism to the adequacy of this argument. Recent discussion has therefore focused on the dialogue between the NMA and Laudan's (1981) equally famous objection from the pessimistic induction (PI), which holds that since the history of science is a wasteland of similarly successful yet now abandoned theories, we have no grounds for asserting that empirical

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success is best explained by approximate truth—indeed, we have more reason to expect that current successful theories will face a similar fate.

This dialogue has shaped the terms of the debate such that realism is generally taken to stand or fall with the following pair of claims about the history of science, judged necessary to retain the putative link between success and truth:

> *Approximate truth claim (AT):* Most genuinely empirically successful yet superseded theories can in fact (from the perspective of current science) be regarded as approximately true in some substantial respect.
>
> *Continuity claim (C):* The theoretical constituents that fueled the genuine empirical success of these superseded theories are, in one way or another, retained by our current best theories in the relevant domain.

This framing is evident in the discussions of prominent realists (e.g. Psillos 1999), structural realists (e.g. Worrall 1989), and anti-realists (e.g. Stanford 2006) alike.

In this paper, I will argue that it is mistake for realists to let the debate be framed in this way. Not only is the NMA far from essential to the defence of realism, it actually weakens rather than strengthens the realist cause. Instead, realists would be well advised to pursue an alternative strategy, which I call the "local strategy". This strategy has a number of significant advantages over the NMA—in particular, it has a much easier time with the PI. Realists should therefore do away with the NMA.[2]

2. An alternative strategy

Instead of mounting a global argument for realism like the NMA, which is intended to support

[2] My thesis is similar to that of Magnus and Callender (2004), who argue that the realism debate should focus on "retail" rather than "wholesale" arguments (the NMA and the PI being instances of wholesale arguments). However, their position is based on the claim (due to Colin Howson and Peter Lewis respectively) that the NMA and the PI commit the base-rate fallacy. While I agree with this move towards retail arguments, I do not think that the charge of fallaciousness will stick: Psillos (2006) shows that it depends on inappropriate probabilistic formulations of the arguments. I will show that the retail/local strategy should be preferred to the NMA, even if the NMA and PI are assumed to be non-fallacious.

epistemic optimism about all current highly successful theories in mature sciences, without our having to consider the details of these theories individually, this alternative strategy says that the defence of realism is best constructed on a case-by-case basis. The idea is that the best foundation for a realist attitude towards a particular theoretical claim of modern science (e.g. that there are atoms, that past and present organisms on earth are the product of evolution by natural selection, that the continents move laterally on tectonic plates, etc.) is the weight of the particular first-order evidence that led scientists to accept the claim in the first place. Realism is thus to be defended through close consideration of the specific theoretical claims that realists want to be realists about, the particular empirical evidence for such claims, and questions about what epistemic attitude towards these claims is licensed by this evidence, with anti-realist challenges to be rebutted as they arise.

A good example of this sort of strategy is Achinstein's (2002) discussion of Jean Perrin's early twentieth century arguments for the atomic theory, which posits that chemical substances are composed of unobservable atoms and molecules, and that Avogadro's number, N (the number of molecules in a sample of a substance whose weight in grams is equal to the molecular weight of the substance), is a constant approximately equal to 6×10^{23}. Achinstein (following an earlier suggestion from Wesley Salmon) claims that Perrin's experiments on Brownian motion provide a compelling case for realism about the atomic theory—indeed, one that convinced many of Perrin's anti-realist contemporaries. Combined with those of Gouy and others, they eliminated all admissible causes of Brownian motion other than internal molecular forces, and, by providing multiple convergent estimates of N, Perrin provided quantitative confirmation of the atomic structure of matter. Close consideration of Perrin's arguments, Achinstein argues, demonstrates that standard anti-realist objections to epistemic optimism about the theory are misplaced or rendered *ad hoc*. In particular, he is concerned to show that a van Fraassen-style agnostic empiricism about the existence of molecules is hard to maintain, given Perrin's evidence, without resorting to arguments for full-blown inductive or general epistemic scepticism—yet van Fraassen (like most contemporary anti-realists) claims only to be a *selective* sceptic about claims about unobservables.

Based on this case study, Achinstein claims that there can be a valid experimental argument for realism about a theory. The suggestion seems to be that similar local experimental arguments can be articulated for other key components of the contemporary scientific picture; hence, realists should pursue a similarly piecemeal strategy elsewhere.

Though Achinstein points realists in the right direction, he is wrong to suggest that the argument for realism in this case is purely experimental.[3] Realism is, after all, a *philosophical* position about science, and modern anti-realists do not see themselves as rejecting the evidence that scientists advance for their theories, but rather as arguing for the correct philosophical interpretation of its epistemic significance. The philosophical action in Achinstein's discussion takes place in his rebuttal of various anti-realist interpretations of Perrin's theory and evidence (such as van Fraassen's), which Perrin himself did not consider, rather than in the recitation of Perrin's evidence itself. Moreover, not all of the considerations Achinstein invokes are specific to this case. For example, he argues that we have empirical reasons to think it *ad hoc* to be sceptical about inferences from properties of observed bodies (e.g. visible Brownian particles) to properties of unobservable bodies (e.g. molecules), but not about inferences from properties of observed bodies to properties of observable but unobserved bodies, given that we can vary the features in virtue of which bodies are observable (e.g. size) and find that this makes no difference to the properties of interest (e.g. that bodies have mass), and we have no other grounds for taking unobservability to be a biasing condition.

Thus, as I characterise it, the local strategy is not simply a matter of reciting first-order evidence for the particular theoretical claim at hand, since it will require philosophical argumentation that goes over and above this evidence, and consideration of issues that may transcend the local details of specific cases.[4] However, the strategy does hold that the details of specific cases are to be centre stage. Achinstein's key insight, I think, is that it is much easier to run standard anti-realist arguments and adopt anti-realist views of current science at a high level of abstraction, when one doesn't have to consider specific cases like Perrin's in any detail. Though first-order evidence will not be sufficient to establish realism by itself, such details

[3] Saatsi (2010) and Psillos (2011b) make a similar point.

[4] As an example of the latter, there is the question of what kind of evidence will be sufficient to warrant a realist attitude? This is clearly a general philosophical question, the answer to which hangs on one's *theory* of evidence, rather than some particular evidence itself.

nonetheless provide the realist with powerful resources to support her position. Thus, from a realist perspective, the battle is best fought in close contact with such evidence, rather than on the global level where the NMA operates, where such details are glossed over.

Clearly, much more needs to be said to give a full characterisation of the local strategy, and to show that it is in fact capable of defending a realist attitude towards particular theoretical claims. My concern here, however, is to highlight the strengths of this approach relative to the NMA. I will begin by highlighting its advantages when it comes to dealing with the PI.

3. Diffusing the pessimistic induction

The NMA provides easy grist for the mill of the historically motivated pessimist because it asserts a completely general connection between empirical success and approximate truth. A list of successful yet false theories from the history of science would call this into question. The NMA-focused realist must therefore offer a defence of sweeping historical claims AT and C in order to maintain the putative link between success and truth. Whatever one's suspicions about the tenability of these claims, this is clearly a burdensome task for the realist to take on.

It is much harder, however, to formulate a PI that threatens any particular instance of the local strategy. The most obvious way to try to do so is to point out that past scientists thought they had very strong evidence for theoretical claims—the existence of the ether, phlogiston, and so forth—now judged to be false; hence, we have grounds for pessimism about current theoretical claims held to be strongly supported by evidence. But, as Roush (2010) has argued, in an insightful piece on the PI, that kind of induction obscures the fact that the *content* of the evidence we have for current theoretical claims is quite different to that which supported now abandoned claims of previous scientists. The particular experimental results appealed to by Perrin, for instance, are quite different to those appealed to by ether or phlogiston theorists, and warrant quite different theoretical conclusions. Importantly, the realist needn't deny that past scientists had (good) evidence for their claims. Since one cannot draw a conclusion about apples from an induction over oranges, the realist can undermine a PI over first-order evidence just by pointing out that the relevant scientists whose work forms the basis of the particular instance of

the local strategy built their claims on *different* sets of evidence.[5] The burden is then shifted onto the pessimist to explain why these evidential differences make no *epistemic* difference.

Clearly, the way pessimist will respond is to move to the meta-level: claims about ether and phlogiston may have been based on different sets of evidence to those that underpin Perrin's claims, but the scientists concerned presumably used the same inferential methods in arriving at such claims. Hence, a history of inferential failures motivates pessimism about the reliability of the methods that scientists (including Perrin) use in apportioning confidence in theoretical claims given the available evidence, whatever its content.

In response to such a meta-level PI, Roush argues that there has in fact been very significant methodological change over the course of recent scientific history. Experimental and statistical methods, for instance, have changed drastically in the last century, and much of this change has been driven by the recognition of past methodological failures. Hence, Roush claims that current scientists plausibly go about apportioning confidence in particular theories based on the available evidence in ways that are significantly different to those of previous scientists, again shifting the burden onto the pessimist to explain why the failures of past methods deployed by past scientists are epistemically relevant to confidence in theoretical claims that are highly warranted according to current methods, given the evidence available to us now.

One concern about this response is that if there have been such drastic changes in method, we might anticipate equally significant methodological changes in the future, undermining confidence in the reliability of current methods.[6] Moreover, much of the realism debate has been concerned with the reliability of inference to the best explanation (IBE), and one could argue that, at least at some general level, this sort of inferential method has had a relatively stable presence in the armoury of scientists (whether they have been aware of it or not), particularly when justifying claims about unobservables.

A better response, in my view, which is more in keeping with the spirit of the local strategy, is to emphasise the *contextual* nature of scientific inference. Even if, at some general level, current scientists use the same, or very similar inferential methods to those of past

[5] Within the domains once occupied by ether and phlogiston theories, successor theories are presumably also grounded in different evidence sets, including, for instance, the evidence that led to the *rejection* of their predecessors.

[6] I thank a reviewer for raising this objection.

scientists, how those methods get deployed in particular cases depends heavily on the local scientific context. This context is crucial for understanding the justification for the particular inferences scientists make. Though Perrin's argument from his multiple convergent estimations of *N* could perhaps be viewed as an IBE (the best explanation for this concordance being the existence of molecules)—hence, methodologically like, say, inferences to the existence of the ether—as Achinstein's discussion makes clear, the epistemic force of this argument can only be appreciated within the context of a rich network of background beliefs: for example, Perrin's belief that his and others' experiments had ruled out conceivable causes of Brownian motion other than internal forces, that visible Brownian particles behave like invisible molecules and so could be used to calculate *N*, that experiments on phenomena other than Brownian motion had reached similar values for *N*, and that such values for *N* are not to be expected on the denial of the atomic theory. It is these local background beliefs that underwrite Perrin's inference to the reality of molecules, given his results. Anti-realists may, of course, take issue with these beliefs, or question whether they do in fact license this inference, but these are not questions to which to which the historical track record of IBE in quite different contexts seems directly relevant.

The way the proponent of the local strategy can respond to the meta-level PI, therefore, is to argue that the grounds for epistemic optimism about a particular theory is crucially contingent on context-specific background beliefs. It is misguided, therefore, to lump together scientists' inferences from evidence in a context-independent fashion, talking about the reliability or unreliability of inferential methods such as IBE quite generally, based on their historical track record, since that obscures the particular epistemic features that underwrite them.[7]

This brief discussion is certainly not a fully adequate response to the potential historical objections to the local strategy.[8] My aim, however, has merely been to make the case that this

[7] As Saatsi (2010) points out, Achinstein's content-driven approach has some affinity with Norton's (2003) material theory of induction. According to Norton, what makes particular inductive inferences justified or unjustified is not their conformity to some formal inference schema (e.g. their being IBEs), but rather local material facts believed to obtain in the domain of inquiry. It should be noted that Achinstein himself (2010, chapter 4) does not accept Norton's claim that there are no valid universal rules of induction, but does accept that local background information is crucial in understanding the license for particular applications of inductive rules.

[8] Stanford's (2006) new formulation of the PI also has to be considered. This trades on the apparent inability of past scientists to conceive of all the conceivable alternative hypotheses equally or better supported by the available evidence, since they frequently failed to conceive of alternatives that subsequently came to be regarded as better

strategy has more powerful resources for dealing with such objections than the NMA. The local strategy invites us to pay close attention to the content of particular scientific theories and inferences, allowing realists (hopefully) to highlight relevant local features that can ground epistemic optimism, and which distinguish the theoretical claims and inferences at hand from those of past scientists. In contrast, the generality of the NMA makes it too easy for the anti-realist to invoke the history of science in the case against realism, thus encumbering the realist with the burden of defending sweeping historical claims like AT and C. Advocates of the local strategy do have to delve into the details of current science and highlight relevant epistemic differences between particular cases of current science and past science in order to fend off historically motivated pessimism. However, nothing so burdensome as AT and C need be at stake here.

4. Defending the realist framework

In recent work, Psillos, one of the most ardent defenders of the NMA, has expressed much sympathy with Achinstein's claims about Perrin—indeed, claiming that "Perrin's case is so strong that the first-order evidence for the reality of molecules takes precedent over the second-order evidence [e.g. from the PI] there might be for being sceptical about explanatory posits" (2011b: 188). However, he thinks that such local arguments for realism, while important, are necessarily incomplete. He cites two reasons for this. I'll discuss the first here, the second in the following section.

Psillos (2011b) argues that close consideration of cases such as Perrin's fail to establish what he calls the "realist framework". This is a general philosophical framework, which *allows* that we can explain observable phenomena by positing the existence of theoretical entities and structures (e.g. as micro-constituents of observable phenomena), and that claims about such entities and structures can, in principle, be confirmed by empirical observations—e.g. in virtue of their explanatory role. Perrin clearly assumed such a framework, since he thought that it was possible to construct legitimate arguments from empirical observation to the reality of molecules. But, it is hard to see how his arguments can have any force against someone who simply rejects

confirmed by that evidence. Since such cognitive limitations presumably also affect current scientists, this motivates pessimism about current theories.

such a framework. It is notable that, though Perrin was able to convince many anti-realist opponents of the atomic theory, there were holdouts like Duhem, who never accepted the reality of atoms and molecules. Duhem (1991) regarded the positing of such unobservable entities as dubious and unnecessary "metaphysics"; hence, he was never prepared to accept the legitimacy of inferences like Perrin's from observation to the existence of molecules. It seems, then, that no amount of close consideration of the available "evidence" for theoretical entities such as molecules is likely to settle the general question of the legitimacy of explanation by postulation of theoretical entities and structures, and whether or not such things can, in principle, be confirmed. Any instance of the local strategy must *presume*, but cannot independently establish, the plausibility of this realist frame.

Historically, part of the seeming attraction of the NMA has been the idea that it might have some independent pull against such hard-line anti-realisms. This is because it works with a purely instrumental notion of empirical success—novel predictive accuracy and instrumental utility *qua* background theory—that even a Duhemian instrumentalist can accept (e.g. Boyd 1984: 59). However, as Psillos (2011a, c) is now happy to concede, the very notion that we need to explain the instrumental success of theories, and that such successes can support an attitude of epistemic optimism towards claims about theoretical entities and structures is precisely what it at issue in this question of framework. A Duhemian anti-realist can very well accept the instrumental success of theories, but stop short at the inference from success to (approximate) truth, by denying the legitimacy of explanation by postulation. At worst, then, the local strategy is in the same boat as the NMA: neither strategy can provide an independent argument for the realist framework, but only for certain realist positions *within* that framework.

What this shows is that realists need different arguments for different realist claims. One argument (or set of arguments) will be required for the general realist framework—for example, Psillos (2011a) has suggested an interesting argument for the indispensability of theoretical entities based on the ideas of Schlick, Feigl, and Reichenbach. One possible position in that framework is a view—consistent with various forms of epistemological anti-realism—which holds that it is perfectly legitimate to posit theoretical entities and structures, but which holds that we lack sufficient warrant to believe that any current theoretical claim is approximately true.

Other arguments will then be required for epistemic optimism, and that is where the local strategy comes in. Importantly, within such a framework, there is no need to confine oneself to a purely instrumental conception of the grounds for epistemic optimism.

5. So what *use* is the NMA?

The second reason that Psillos cites for the incompleteness of local realist strategies, such as Achinstein's, is that he thinks that realists also need to offer a general justification for the reliability of IBE. For Psillos, the primary payoff of the NMA *within* the realist framework is that provides a (non-vicious) rule-circular justification for the reliability of IBE. His formulation of the argument has two parts:

> (A)
> (A1) Scientific methodology is theory-laden.
> (A2) These theory-laden methods lead to correct predictions and experimental success (instrumental reliability). How are we to explain this?
> (C1) The best explanation (of the instrumental reliability of scientific methodology) is this: the statements of the theory which assert the specific causal connections or mechanisms in virtue of which methods yield successful predictions are approximately true.
>
> (B)
> (B1/C1) Theories are approximately true.
> (B2) These background scientific theories have themselves been typically arrived at by abductive reasoning.
> (C2) Therefore, (it is reasonable to believe that) abductive reasoning is reliable: it tends to generate approximately true theories. (Psillos 2011c: 23-24)

In contrast, I think there is good reason to doubt that the NMA can add anything of value here. Much in line with the ideas floated at the end of Section 3, Psillos has argued that the structure and strength of IBE reasoning is determined by local context:

> IBE-type of reasoning has a fine structure that is shaped, by and large, by the context… The

> background knowledge (or, beliefs) ranks the competitors. Other background assumptions determine the part of the logical space that we look for competitors. The relevant virtues or epistemic values are fixed, etc. Given this rich context, one can conclude, for instance, that the double-helix model is the best explanation of the relevant evidence, or that the recession of the distant stars is the best explanation of the red-shift… These contextual factors can link [explanatory] loveliness and likeliness nicely, because they do not try to forge an abstract connection between them; rather the connection stands or falls together with the richness and specificity of the relevant information available. (Psillos 2007: 443).

These sentiments do not mesh well with the idea that IBE is to be justified by a global track record argument like the above formulation of the NMA. If IBE is so context-sensitive, it does not seem appropriate to talk of the reliability of IBE in general, but only of particular instances of IBE in particular contexts. Hence, rather than ask for a general justification for IBE, it seems more appropriate to ask how it is that particular contextual information licenses particular instances of IBE. It is hard to see, therefore, what justificatory role the NMA can play.

6. Doing away with the NMA

The NMA has been, and remains, the primary realist argument for epistemic optimism about current science. But, as we've seen, there is an alternative strategy—the local strategy—that has very significant advantages over the NMA. Most notably, it seems to have a much easier time with the PI, and doesn't encumber the realist with such onerous historical commitments. The local strategy leaves untouched the question of what Psillos has called the "realist framework", but then so does the NMA. In addition, the NMA does not seem well suited to the job that it has been thought to play within that framework: that of providing a general justification for IBE.

If realists were indeed to do away with the NMA, and instead utilise the local strategy for defending epistemic optimism about current science (leaving the question of the realist framework to other arguments), this would, I suggest, lead to a much better framing of the realist debate, for realist purposes. First, it is curious feature of the recent debate that, in all the papers and books devoted to the topic, one hardly sees any discussion of the particular current scientific theories that realists actually want to be realists about. In recent years, the fuss seems to be

almost exclusively over various features of the historical record—mostly a select few of the cases cited by Laudan. This is clearly an effect of the focus on the NMA. But, while I do not deny that history is relevant to the realism debate, it is surely these current theories that ought to be the focus of attention. The local strategy thus focuses the debate where it should be: on the details of specific current theories, the specific evidence for them, and questions about what epistemic attitude towards the entities and structures postulated by these theories is warranted by this evidence.

Second, the local strategy helps to put the scope and limits of realist optimism into proper focus: realism may be appropriate for some claims of current science, but not others, and we have to delve into the details of particular cases to find out which. In so doing, it helps move the debate away from the overgeneralised and unnecessarily concessive positions that realists have been inclined to adopt as a result of their attempts to rescue the NMA. Worrall's (1989) structural realism is a case in point. While it may perhaps be true that all we are entitled to be optimistic about in current science are its implications about the abstract structure of nature, it seems bizarre to make this as a general claim, as Worrall does, on the basis of a few cases from nineteenth century physics, where it seems that the NMA can be defended against the PI only at the level of structure. This looks at once like hasty over-generalisation (why should the fortunes of a few theories from the history physics be taken as a model for all of current science?) and, from a realist perspective, far too concessive to the anti-realist, made as it is without actually engaging with the non-structural claims of specific current theories.[9]

Realists should therefore do away with the NMA.

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[9] Structural realists have appealed to other kinds of arguments in support of the claim that all we can know about nature is its structure (see Ladyman 2009). However, the argument from theory change was primary in Worrall's original papers and it remains a focus of attention in the current literature.

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