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The No Miracles Intuition and the No Miracles Argument

In this paper I contrast the very modest view of the main 'consideration' supporting scientific realism taken by Poincaré and others with the much more ambitious *argument* developed by Stathis Psillos using some ideas of Hilary Putnam's and of Richard Boyd's. I argue that the attempt to produce a more ambitious argument not only fails, but was always bound to fail.

1. The no miracles intuition

Most of us tend toward scientific realism because of the amazing predictive successes enjoyed by theories in (mature) science. To take a well-worn example: the classical wave theory of light is, at root, a series of claims about an unobservable medium, the 'luminferous aether', and about unobservable periodic disturbances travelling through it; yet it turns out to follow deductively from this theory (together of course with accepted auxiliary assumptions) that, for instance, the 'shadow' of a small opaque disc held in light diverging from a point source will have an illuminated spot at its centre—a claim that can be directly empirically checked and turns out to be true.[1] 'How on earth', it seems unavoidable to ask, 'could a theory score a dramatic predictive success like that unless its claims about the reality 'underlying' the phenomena (in this case, about the unobservable luminiferous aether) are at least approximately in tune with the real underlying structure of the universe?' To assume that it *could* score such successes, while not itself even being approximately true would be, in Poincaré's words, "to attribute an inadmissible role to chance"[2].

Of course in this and similar cases, predictive success is the icing on a cake that must already be substantial. If scientists threw out enough theories simply at random, eventually one would score some predictive success 'by chance'. But other conditions are implicitly presupposed: for example, that the predictive success

1 For the historical details of this case, which are at odds with the usual philosophical presentation, see John Worrall, "Fresnel, Poisson and the white spot: the role of successful predictions in the acceptance of scientific theories", in: D. Gooding, T. Pinch and S. Shaffer (Eds.), *The Uses of Experiment*. Cambridge: Cambridge University Press, 1989, pp. 135-157.

2 Henri Poincaré, *Science and Hypothesis*, repr. New York: Dover 1952 (originally 1905), p. 150.

D. Dieks et al. (eds.), *Explanation, Prediction, and Confirmation*, The Philosophy of Science in a European Perspective 2, DOI 10.1007/978-94-007-1180-8_1,

is genuine and not brought about by some *ad hoc* accommodation of the relevant phenomenon within the theory at issue; also that the theory accounts for all the empirical success of its rivals, and so in particular for the success of its predecessor; and finally that the theory has a certain 'simplicity' or 'unity'. But provided that these conditions are met then the realist-leaning force of predictive successes like that of the white spot seems difficult to resist. As Duhem[3] put it:

> The highest test … of our holding a classification as a natural one is to ask it to indicate in advance things which the future alone will reveal. And when the experiment is made and confirms the predictions obtained from our theory, we feel strengthened in our conviction that the relations established by our reason among abstract notions truly correspond to relations among things.

Let's call the "conviction" highlighted by Duhem 'the *no miracles intuition*'. Notice that it is local: it applies to *particular* theories and their particular predictive successes. A general case for scientific realism can based on it only in a piecemeal, conjunctive way—it is reasonable to think that the general theory of relativity is approximately true because of its predictive success with, for example, the motion of Mercury, and it is reasonable to think that the photon theory of light is approximately true because of its predictive success with the photoelectric effect, and … This conjunction will not be over 'the whole of science' (whatever that is supposed to be). After all, some parts of science are frankly speculative, others highly problematic. Instead the conjunction will be over only those particular theories that have scored genuine particular predictive successes and hence elicit the no miracles intuition. No sensible scientific realist should ever have been realist about *every* theory in science, nor even about any theory that is (currently) the 'best' in its field. (It may after all, as has often been pointed out, be only 'the best of a bad lot'.) She should be realist only about theories that have scored proper predictive success, since only such success elicits the no miracles intuition and only that intuition underwrites realism.

Of course scientific realism faces many well-rehearsed problems—notably the challenge based on the history of theory change: presumably it was reasonable to think that, for example, the elastic solid ether theory of light was approximately true because of its predictive success (see above). Is this compatible with the current realist view that the still more impressively predictive photon theory of light is approximately true, given that the two theories are logically incompatible? However I lay these problems aside here.

3 Pierre Duhem, *The Aim and Structure of Physical Theory*, trans P. Wiener. Princeton, NJ: Princeton University Press 1954 (originally 1906), p. 28.

2. The 'No Miracles Argument'

Rather, the issue I want to address is whether the "conviction" pointed to by Duhem, Poincaré and others is ineliminably intuitive or can instead be backed up by some more substantial argument. After all, an intuition seems a slim reed from which to hang a philosophical position; surely an argument, if cogent, would put the realist on firmer ground.

As we have seen, the intuition applies to individual theories and so the obvious first suggestion would surely be to try to produce a form of argument aimed at underwriting the claims to (approximate) truth of such *individual* theories. This has indeed been attempted. (It is, for example, this form of the argument that Colin Howson criticises in his *Hume's Problem*[4].) But I shall not consider it here, instead going straight to the more widely touted, and altogether more ambitious, form of the argument. One that I shall argue was always a non-starter.

The first step on the downward slope was taken by Hilary Putnam who famously argued[5]:

> The positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle. That terms in mature scientific theories typically refer …, that the theories accepted in a mature science are typically approximately true, that the same term can refer to the same thing even when it occurs in different theories—these statements are viewed … as part of *the only scientific explanation of the success of science* … (emphasis added)

Putnam's idea—that scientific realism in general could be itself regarded as the (only and therefore the) best *scientific* explanation of the success of science—was in turn further elaborated by Richard Boyd and then Stathis Psillos into what Psillos calls "the explanationist defence" of scientific realism. The 'success' claim used as a premise in this argument/defence is *not* about the predictive success of particular scientific theories, but instead about the 'success' of some alleged general scientific method. (Following van Fraassen[6], this No Miracles Argument, with definite capital letters, is also sometimes called the "ultimate argument" for scientific realism.)

Psillos' 'explanationist defence' supposes that there is something called 'scientific methodology' that has proved to be 'reliable'—in that it consistently (or fairly consistently) produces theories that yield correct predictions. Moreover, this methodology depends in various ways on background theoretical assumptions. The best explanation of the 'reliability of scientific methodology' is that those theories are (approximately) true. Indeed the claim seems to be that it would be

4 Colin Howson, *Hume's Problem*. Oxford: Oxford University Press 2000.

5 Hilary Putnam, *Mathematics, Matter and Method (Philosophical Papers, Volume 1)*. Cambridge: Cambridge University Press 1975, p. 23.

6 Bas van Fraassen, *The Scientific Image*. Oxford: Clarendon Press 1980, p. 39.

inexplicable—a second-order 'miracle'—if theory-dependent scientific methodology kept producing successful scientific theories, were the theories on which that methodology is dependent not at least approximately true. As Psillos[7] emphatically puts it:

> NMA is *not* just a generalisation over scientists' [individual] abductive inferences … The explanandum of NMA is *a general feature of scientific methodology*—its reliability for yielding correct predictions. NMA asserts that the best explanation of why scientific methodology has the contingent feature of yielding correct predictions is that the theories which are implicated in this methodology are relevantly approximately true. (emphases added)

Moreover, the explanation involved in this defence of scientific realism is itself alleged to be, just as Putnam asserted, a *scientific* one. (Remember that Putnam famously claimed that scientific realism is "an overarching scientific hypothesis"[8].)

But, before asking whether this explanation of the success of scientific methodology can possibly itself be a *scientific* explanation, we should note a number of obscurities in just what the argument is supposed to be in the first place. The underlying idea seems initially clear enough: there is something called general scientific methodology that has been impressively successful (successful in producing theories that enjoy individual predictive successes); this general scientific methodology is theory-dependent in multiple ways; it would be a 'miracle' if this methodology were as successful as it is, if the theories on which it depends were not (approximately) true; on the other hand the success of the methodology would be explained if the theories on which it depends were indeed true; and moreover this is the best explanation of the success of that methodology; hence we can infer (by a meta-level 'abduction' or 'inference to the best explanation') that those theories involved in scientific methodology are indeed (approximately) true.

One thing that seems to have gone unnoticed is that the conclusion that this version of the NMA allegedly validates is *not* the (likely approximate) truth of those scientific theories that score impressive predictive success (and hence elicit the no miracles intuition)—the predictive success of our best theories is the *explanandum* in this alleged scientific explanation not the *explanans*—the *explanans* (to which we are then allegedly entitled to infer) seems to be the (approximate) truth of the *background theories* taken to be involved in helping scientific methodology produce those predictively successful theories. This seems strange. But, even laying it aside, much remains obscure. Specifically: what exactly is general scientific methodology supposed to consist in, and what role do these presupposed background theories play in it?

7 Stathis Psillos, *Scientific Realism—How Science tracks Truth*. London and New York: Routledge 1999, p. 79.

8 Hilary Putnam, *Meaning and the Moral Sciences*. Boston: Routledge and Kegan Paul 1978, p. 19.

Boyd, whose views Psillos sees himself as developing, is decidedly unclear. He takes it that Kuhn and others have shown that scientific methods are thoroughly theory-dependent—without indicating exactly how—with, however, two (partial) exceptions. Boyd argues that (a) decisions over which (observable) predicates are ‘projectable’ and (b) assessments of degrees of confirmation of a given theory both significantly depend on “the theoretical claims embodied in …[relevant] background theories” and hence in fact, or so he claims, on the assumption that those background theories are “approximately true”[9].

Psillos[10] elaborates as follows (numbers in parentheses added):

> Scientists use accepted background theories in order [1] to form their expectations, [2] to choose the relevant methods for theory-testing, [3] to calibrate instruments, [4] to assess the experimental evidence, [5] to choose among competing theories, [6] to assess newly suggested hypotheses, *etc*.

Here [1] seems to amount to Boyd’s point (a), while [2]–[6] are different aspects of Boyd’s claim (b) about ‘degree of confirmation’ being background-knowledge-dependent. What Boyd says about ‘projectability’ is rather abstract, but in so far as it applies to real science, it seems to amount to the (well-rehearsed) point that it is background theories, rather than repeated observations, that generally (though not, I think, universally) tell us which properties generalise (and also, I would add, how they may fail to generalise). So, for example, background theories tell us that all electrons have the same charge—in principle one single experiment can then fix what that charge is, and thus can sanction the generalisation that all electrons have particular charge -e. Background evolutionary-biological theories tell us how different types of the same species of bird might differ in the colour of their plumage—instead then of observing ravens haphazardly, we investigate male and female ravens, young and mature ravens, ravens living in different geographical locations, etc; if all those are black and only if they all are, then we infer that all ravens are black. But this is surely best regarded simply as a process of teasing out the full consequences (invoking, of course, auxiliary assumptions) of those underlying theories and thus of further testing them. Nothing here seems to amount to a *method* of producing new theories whose further success can be regarded as independent of the success of theories that are already accepted in science.

Much the same point surely holds for Boyd’s claim (b) about assessments of confirmation being dependent on background theories. Undoubtedly science seeks not just theories that are individually successful, but ones that also combine together successfully. A theory that is inconsistent with some already established theory and that is not independently successful will be viewed very differently

9 Richard Boyd, “The Current Status of the Scientific Realism Debate” in: Jarrett Leplin (Ed.), *Scientific Realism*. Berkeley: University of California Press 1984, pp. 41-82. Quote on p. 59.

10 op. cit., p. 78.

from one that is not (yet?) independently successful but is at least consistent with already accepted theories. Notice however that independent empirical success always seems to dominate. The fact that Copernican astronomy failed to cohere with the best available physics was not regarded by the best scientists in the 17th century as a reason to think it any the less well confirmed empirically by the independent successes it enjoyed (with, amongst others, the phenomena of planetary stations and retrogressions); but instead as a reason to look for a new physics that would be coherent with it. And, in any event, this all looks like an account of one aspect of how theories are tested once they have been articulated and nothing like an account of a 'methodology' whose reliability in *producing* successful theories can be assessed.

Finally, if we were (ill-advisedly) to think of the ways that scientists test individual theories against the background of other theories as some sort of method of producing theories, it is altogether unclear how 'reliable' that method has been—which theories are we to count? All those that anyone ever dreamed up? Or only those that survive subsequent rigorous testing? It is standard nowadays to hold that more recent philosophy of science has taken us beyond the old Reichenbach-Popper view that the contexts of discovery and of justification are quite distinct. Nowadays it is widely believed that the process of construction of theories can be rationally analysed and is *not* a "mere matter of psychology" (as Popper put it). But, however much can be said by way of logical reconstruction of how particular theories have been arrived at, still most of the action is at the appraisal stage—that is, the stage where the theory is already 'on the table' and is being subjected to stringent tests. And no matter how systematically a theory has been arrived at—by 'deduction from the phenomena' or whatever—it will of course be rejected if it fails to score (at any rate eventually) independent empirical success.

I remain unconvinced, then, of the existence of anything that can be plausibly be called 'scientific methodology in general'. Moreover, for all that we claim to have gone beyond Popper, it is surely true that scientists sometimes produce theories simply to try them out, without being in any way committed to the claim that they are likely to be predictively successful/true. Nor when they turn out not to be should the production of such tentative theories be thought of as in any way a failure—even if we did identify them as the products of some general 'scientific method'. To take one example: the idea that the anomalous motion of the perihelion of Mercury might be explained within Newtonian physics by invoking a hitherto undiscovered planet (tentatively called 'Vulcan') was of course a perfectly reasonable hypothesis. That hypothesis 'failed'—in that no evidence of the existence of such a planet could be found. But this was in no sense a failure of 'scientific method': science learned that one way of solving the Mercury problem—made plausible by background knowledge in the light of the earlier success with postulating Neptune to explain anomalies in Uranus's orbit—did not work, and so some other solution would have to be found.

But having convinced himself that the argument for realism must be at the level of some allegedly reliable ‘general scientific methodology’, Stathis Psillos necessarily views such episodes as failures and hence—even in his original treatment—is forced to weaken his position. He admits that science “has encountered many failures”[11] and so concludes that “the realist argument [i.e. his NMA] should become more local in scope”[12]. However, he cannot of course, while remaining consistent with his general position, become totally local—he continues explicitly to deny that the NMA amounts simply to a generalisation of the particular ‘abductions’ concerning particular theories in science. So he seems in the end to adopt the view that “most” products of the scientific method are successful or, perhaps (although he does not himself explicitly invoke probabilities) that the probability of a particular theory produced by the ‘scientific method’ being successful is high.

However an objectivist probabilistic approach to modelling the production of scientific theories here will not work;[13] “most” is clearly vague, and in any event we want to be realist not about ‘most’ scientific theories but (selectively) about *all* those that elicit the no miracles intuition by enjoying striking predictive success (and we should not want to endorse a realist attitude toward those that are not successful in this way). In some other passages, Psillos weakens the conclusion of his argument still further, claiming that the NMA is meant only to “defend the achievability of theoretical truth”[14]. Given his endorsement of an externalist epistemology (another aspect of his account with which I fundamentally disagree), this further weakening would only mean that science *may* deliver some theoretical assertions that are, objectively speaking and independently of what we may or may not (or may or may not rationally) believe, true. But any anti-realist—certainly van Fraassen—can agree with that! And even if we stay ‘internalist’ (as we surely should, ‘externalist epistemology’ has always seemed to me an oxymoron), the weakened claim—which would now mean that science at least on occasion delivers a theoretical assertion which it is reasonable to believe is true (or, again, better: approximately true) is surely still much *too* weak to sustain the sort of realism that seems intuitively sustainable. The realist should endorse a realist attitude toward all (and only all) those scientific theories that have been predictively successful.

Even if we were to concede that there is such a thing a scientific methodology and that it has been reliable in producing theories that are predictively successful, the problems for this approach are far from over. The idea that (i) the best explana-

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11 Ibid., p. 80.

12 Ibid.

13 For criticism of such attempts, that however should not have been taken seriously in the first place, see P. D. Magnus and Craig Callender, “Realist Ennui and the Base Rate Fallacy”, in: *Philosophy of Science,* 71, 2004, pp. 320-338. For more general criticism see John Worrall, “Miracles and Realism”, in: E. Landry and D. Rickles (Eds.), *Structure and Theory*. Springer 2010 (forthcoming).

14 op. cit., p. 79.

tion of this success is that the theories that are involved in that method are approximately true and (ii) that we are therefore entitled rationally to believe that those theories are indeed approximately true runs smack into three obvious and fundamental objections. *Firstly*, despite Putnam's explicit claim (endorsed by Boyd and seemingly by Psillos) any such explanation cannot count as *scientific*; *secondly* accepting that the argument involves a "philosophical explanation" rather than a scientific one, realism (strictly about the background theories involved in scientific method, remember) by no means clearly qualifies as even the best philosophical explanation; and *thirdly* the argument is surely circular.

Even if we conceded that 'science in general' (or at least 'mature science in general) had been 'successful', how could this proposed grand, meta-level 'abduction' or inference to the best explanation' possibly count as a *scientific* explanation of that 'success'? Scientific explanations require independent testability. Is the NMA independently testable? The nearest it might come, so far as I can tell, is via the 'prediction' that the next theory produced by the 'scientific method' will be predictively successful. (The 'prediction' that the next theory will be (approximately) true cannot of course count. Testable predictions need to be testable! 'Predictive success' is an effective notion, but truth or approximate truth is not.) But this 'prediction' (a) could easily be false without realism thereby being at all challenged or undermined: not all of the theories actually produced in science are successful and hence there is no realist case for them being true (some of them are not even *intended* (necessarily) to be candidates for truth); and (b), if it refers to theories that are actually *accepted* in science, as opposed just to proposed or considered, then it is no testable 'prediction' at all, but instead a foregone conclusion: no theory would be accepted in (mature) science unless it were predictively successful and indeed more successful than its predecessor.

Suppose it is claimed instead that realism is a better *philosophical* explanation of the success of science than its rivals—presumably because it possesses some 'explanatory virtue' different from that of empirical testability. I have many doubts about the whole notion of explanation when not directly related to empirical testability—and to talk in this way seems simply to reexpress the no miracles intuition in an obscure and misleading way. (Indeed Psillos admits[15] that it is wrong to expect that inference to the best explanation will be an inference that fits some "logical template"; but then again one wonders why, in that case, it is supposed to be any sort of real logical *inference* that takes us beyond intuition.)

And even if trade in 'philosophical explanation' is permitted, why exactly should realism be thought of as a better 'philosophical explanation' of science's success in successfully predicting new types of phenomena than, say, the constructive empiricist 'explanation'? This, mirroring Psillos' approach, would presumably claim that scientific method has been successful because the background

15 Stathis Psillos, "The Fine Structure of Inference to the Best Explanation", in: *Philosophy and Phenomenological Research* 74, 2007, pp. 441-8.

theories that it presupposes are empirically adequate. If Psillos' realist argument counts as a 'philosophical explanation' of science's success then it is difficult to see why the constructive empiricist one should not. On what grounds, then, could the realist claim hers to be the *better* explanation? Presumably only on the ground of logical strength of the 'explanans'. It is of course true that the realist claim that a theory is (let's say, strictly) true is logically stronger than the constructive empiricist claim that the theory is 'fully' empirically adequate and the suggestion is that we should always prize extra content in explanations (provided of course the extra strength does not lead to empirical refutation—no problem in this case).

But here I am in sympathy with van Fraassen[16] and Fine[17]—given that this extra content is in no way testable, this is exactly the sort of pseudo-'deeper explanation' that we should we shun. We only prize (or only ought to prize) extra content when it leads to independently checkable predictions. Psillos explicitly claims that Fine's 'explanation' of success in terms of empirical adequacy is to be dispreferred because invoking the instrumental reliability of science to explain its instrumental reliability is no sort of explanation at all. But neither is the realist 'explanation'! Following Psillos in using the hackneyed example: he complains that Fine is in the position famously ridiculed by Molière. But is the claim that opium is sleep inducing because it has dormitive virtue *and* moreover this virtue was given it by God any better an explanation than the original that just invokes dormitive virtue? And isn't the realist simply adding a non-testable add-on extra (the truth of the theory) in a completely analogous way? Explanatory brownie points are not awarded for adding content unless the extra content leads to extra testability.

Finally, the grand meta-level 'explanationist defence' of realism is circular and therefore question-begging. In essence, the explanationist defence uses inference to the best explanation to defend inference to the best explanation! Realism is the claim that our best scientific theories, which are therefore presumably the best explanations we have, are reasonably regarded as approximately true on the basis of their success in predicting new phenomenon. So the realist scientist endorses inference to the best explanation concerning particular theories; and when her realism is challenged, she is being encouraged by Psillos to respond that realism is the best position because it is the best explanation (now of the supposed general success of scientific method). But how could this possibly be convincing to a sceptic? If she accepted inferences to the best explanation she would not have been a sceptic in the first place! As Fine[18] put it the 'explanationist defence' carries no weight because it involves "the very type of argument whose cogency is the question under discussion".

16 op. cit.

17 Arthur Fine, "Unnatural Attitudes: Realist and Instrumentalist Attachments to Science", in: *Mind*, 95, 1986, pp. 149-179.

18 Arthur Fine, "Piecemeal Realism", in: *Philosophical Studies* 61, 1991, pp. 79-96. Quote on p. 82.

Fine's objection is an obvious one and so unsurprisingly has been made by a number of others (e.g. by Larry Laudan[19]). Psillos tried to avoid accepting its obvious correctness[20] by drawing a distinction (originally used by Braithwaite[21] in the (similarly doomed) attempt to argue that inductive justifications of induction are perfectly cogent) between 'rule circularity' and 'premise circularity'. If an argument for some conclusion *c* includes *c* as a premise, then the argument is 'viciously circular'; but, Psillos[22] endorses Braithwaite's opinion that 'rule circular' arguments are *not* vicious. An argument is 'rule circular' if it employs a rule of inference in taking us from its premises to its conclusion that it is justifiable as a truth-transferring rule only if certain assumptions, *including the conclusion c itself*, are themselves true.

But surely so far as the cogency of an argument goes, the only question is whether it is circular—the 'vicious' qualifier is just hot air! There seems to be complete equivalence between premise and rule circularity. In particular any premise circular argument for c can be made rule circular quite trivially: remove c from the list of premises, and, for example, add an extra rule that says you can infer X & c from any derivable statement X. Given this, how could we possibly be (rationally) less concerned about a rule circular argument than a premise circular one?

While continuing to maintain that there is an important difference between premise and rule circularity, Psillos has importantly modified his position in later writings. He now seems to admit that scientific realism is not a *scientific* explanation of anything: "The problem lies in the thought that scientific realism can be supported by the same type of argument that scientific theories are supported [by]. This is a tempting thought. But it is flawed I now think."[23] (Notice however that this does not render the above criticisms redundant since it is still Psillos's view that the NMA is to be articulated and defended as a grand meta-level 'abduction'.)

His view now is that the NMA "presupposes rather than establishes the realist frame[work]. Still *within* the realist framework, the NMA has an important role to play and this … is to offer a vindication of [inference to the best explanation]."[24]

Well, aside from the fact that no one surely ever thought that the argument *establishes* realism (as opposed to giving it some rational support), this new posi-

19 Larry Laudan, "A Confutation of Convergent Realism" in: David Papineau (Ed.) *The Philosophy of Science*, Oxford: Oxford University Press 1996, pp. 139-165.

20 Stathis Psillos, *Scientific Realism—How Science tracks Truth*, op. cit.

21 Richard B. Braithwaite, *Scientific explanation: a study of the function of theory, probability and law in science*. Cambridge: Cambridge University Press 1953.

22 op. cit., p. 82.

23 Stathis Psillos, "Choosing the Realist Framework", in: *Synthese*, DOI 10.1007/s11229-009-9606-9. Published online 30 June 2009. p. 11.

24 Ibid. This could just be seen as an elaboration of his view in *Scientific Realism* (p. 89): "In the final analysis, we just have to rely on some basic methods of inquiry. The fact that we have to make recourse to rule-circular arguments in order to defend them, if defence is necessary, is both inescapable and harmless."

tion seems to be an endorsement of the circularity charge rather than a rejoinder to it. You will, this new position allows, be moved by the NMA only if you are already an advocate of inference to the best explanation and hence already a realist. That is, surely, you won't be moved objectively speaking at all. But psychologically speaking the realist may gain extra confidence by chanting the NMA—even though it can be no news to her objectively speaking. But while preaching to the converted may make the preacher and the converted feel good, the truly converted need no preaching!

Having accepted that the NMA is not an argument in favour of realism, it is difficult to see how, in his later interpretation, it is even any sort of consideration in favour of realism—and certainly impossible to see it as a "*vindication*" of inference to the best explanation (see above quote). Psillos now asserts[25] that "the original decision to accept [the realist] framework [or any other framework while] not arbitrary [is] not a matter that answers to truth or falsity". It is difficult to see exactly what 'non-arbitrary' means here, but certainly it seems that this new position allows that someone might happen to be a realist but could equally well have chosen a rival framework—say the constructive empiricist one—and not have been in any sense wrong to do so; *and* had she made that alternative choice then the NMA would have nothing to say to her.

In contrast, the no miracles intuition favoured by Poincaré, Duhem and myself is at least intended to speak *across* frameworks. It is exactly the predictive success of some *particular* scientific theories that seems, whatever your initial philosophical point of view, ineluctably to elicit the feeling that the theory must have somehow 'latched on to' the deep structure of the universe (without of course being able to say exactly how). This obviously cannot 'establish' realism, but it does provide a very modest support for a very modest version of scientific realism—in no stronger a sense than that it sets some version of realism as the default position. This may not seem a lot, but we cannot reasonably expect anything more. We were certainly never going to get anything more from the No Miracles Argument and, as I have argued in this paper, nothing more is exactly what we get.

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25 Stathis Psillos, "Choosing the Realist Framework", op. cit., p. 6.