The Philosophy of Science in a European Perspective

Dennis Dieks · Wenceslao J. Gonzalez
Stephan Hartmann · Thomas Uebel
Marcel Weber *Editors*

Explanation, Prediction, and Confirmation

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Preface: Explanation, Prediction, Confirmation

This volume, the second in the Springer series *Philosophy of Science in a European Perspective*, contains selected papers from the workshops organised by the ESF Research Networking Programme *PSE* (The Philosophy of Science in a European Perspective) in 2009. The opening conference of this Programme (Vienna, 18-20 December 2008; see F. Stadler et al., eds., *The Present Situation in the Philosophy of Science*, Springer, 2010—the first volume of this series) first of all identified general directions in European philosophy of science research and defined points of contact between the different research teams that are part of the Programme. In comparison, the 2009 workshops placed a stronger emphasis on the further development of individual research lines of the teams, while keeping an eye on possibilities of cooperation and cross-fertilization.

The individual *PSE* teams and their areas of research are as follows:

Team A, Formal Methods (team leader Stephan Hartmann);
Team B, Philosophy of the Natural and Life Sciences (team leader Marcel Weber);
Team C, Philosophy of the Cultural and Social Sciences (team leader Wenceslao J. González);
Team D, Philosophy of the Physical Sciences (team leader Dennis Dieks);
Team E, History of the Philosophy of Science (team leader Thomas Uebel).

Under the umbrella of the general theme Explanation, Prediction and Confirmation, these teams organised three meetings in 2009. Team B organised a workshop on "Explanation, prediction, and confirmation in biology and medicine", which took place in Konstanz from 2 to 4 October; with Marcel Weber as the local organizer and with support from the Konstanz "Zentrum für Philosophie und Wissenschaftstheorie". Team C organised a workshop on "Explanation, prediction and confirmation in the social sciences: realm and limits" at the University of Amsterdam, from 26 to 27 October; the local organiser was Marcel Boumans. Teams A, D and E organised a joint meeting entitled "Physical and philosophical perspectives on probability, explanation and time". This meeting took place at the Woudschoten Conference Center in Zeist from 19 to 20 October 2009; its local organiser was Dennis Dieks, supported by the "Institute for History and Foundations of Science" of Utrecht University. The combined presence of three research teams in this meeting offered the opportunity for holding two explicitly interdisciplinary sessions in addition to the solo sessions of the individual teams. These combined sessions focussed on areas of overlap and joint interest between Teams A and D and Teams A and E, respectively. A detailed report of the meeting can be found in the *Journal for General Philosophy of Science*, 2010, DOI 10.1007/s10838-010-9132-y; the results of the workshop of Team C are discussed in detail in the *Journal for General Philosophy of Science*, 2010, DOI 10.1007/s10838-010-9128-7.

A large audience, from all over Europe and from a variety of specialties, attended the several workshops: at the concluding lecture of the joint Zeist workshop almost one hundred people were present. Among these participants there were a substantial number of students and young scholars. The workshops thus accorded very well with the general idea behind *PSE*: establishing contacts between scholars from different European countries while furthering high level European research in the philosophy of science. Although the individual *PSE* teams focus on subjects that at first sight may seem quite different, there turned out to be many areas of overlap and common interest, with ample opportunity for joint work. For example, a connecting thread running through a substantial number of papers in this volume is the concept of *probability*: probability plays a central role in present-day discussions in formal epistemology, in the philosophy of the physical sciences, and in general methodological debates—it is central in discussions concerning explanation, prediction and confirmation. It became very clear at the meetings that such topics can profit considerably from intellectual exchange between various disciplines. Accordingly, it was decided that *PSE* should further pursue this path of cooperation and interdisciplinarity. In fact, probability will be a *Leitmotiv* in 2010, with 4 workshops on the role of probability and statistics in various disciplines; among which a joint workshop on topics of common interest to the philosophy of the life science and the philosophy of the physical sciences. The results will be available in the third volume of this *PSE* series!

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Team A
Formal Methods

John Worrall

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.

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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?

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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-

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.

Stathis Psillos

The Scope and Limits of the No Miracles Argument[1]

In this paper, I review the scope and limits of the no miracles argument. I defend and, where necessary, revise my account of it as a way to justify Inference to the Best Explanation (IBE).

1

I have argued in my (1999, chapter 4) that the no miracles argument (NMA) should be seen as a grand IBE. The way I read it, NMA is a philosophical argument which aims to defend the reliability of scientific methodology in producing approximately true theories. More specifically, I took it that NMA is a two-part (or two-stage) argument. Here is its structure.

NMA

(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.[2]

(B)

(B1/C1) Theories are approximately true.

(B2) These background scientific theories have themselves been typically arrived at by abductive reasoning.

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1 Many thanks to two anonymous readers for comments.

2 This somewhat cumbersome formulation is meant to highlight that the application IBE should be local and selective rather than global and undifferentiated. Only those parts of a theory that do play some role in the generation of the theory's successes do get some credit from the explanation of these successes. For all practical purposes, however, the conclusion (C1) might be abbreviated thus: the best explanation of the instrumental reliability of scientific methodology is that background theories are approximately true.

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(C2) Therefore, (it is reasonable to believe that) abductive reasoning is reliable: it tends to generate approximately true theories.

Given this structure, it is clear that NMA aims to defend the reliability of IBE, as a mode of reasoning. Note that the explanandum, viz., the *instrumental* reliability of scientific methodology, is distinct from the explanans, viz., the reliability (in Goldman's sense) of inference to the best explanation. As Arthur Fine aptly put it, instrumental reliability is a feature of scientific theories in virtue of which they are 'useful in getting things to work for the practical and theoretical purposes for which we might put them to use' (1991, 86). It has mostly to do with getting predictions right and with leading to empirical successes. Reliability, in the sense it is understood by epistemological externalists, is a property of a method by virtue of which it tracks the truth—that is, it tends to generate true conclusions when fed with true premises. This important distinction is confused in Jacob Busch's (2008) and a lot that follows misses the point.

It is transparent that the NMA has two conclusions (one for each part of it). The *first* (C1) is that we should accept as (approximately) true the theories that are implicated in the (best) explanation of the *instrumental* reliability of first-order scientific methodology. The *second* (C2) is that since, typically, these theories have been arrived at by means of IBE, IBE is reliable (truth-conducive). Both conclusions are necessary for fulfilling the aim of NMA.

2

A straightforward observation is that sub-argument (B) is not circular. It is *not* an instance of IBE, anyway. (B2) is a factual premise: it is meant to state a fact about how theories have been generated and accepted. It can certainly be contested. But all that matters for (C2) to follow is this:

If (C1) is true, then given a factual premise (B2), (C2) is true.

There is a missing premise, of course, viz., that if a method yields approximately true theories, this method is reliable. But this is how reliability is understood. It might be objected that all that is shown—at best—is that IBE has been reliable; not that it will be; and hence, not that it is reliable, *simpliciter*. That is fine, however. No-one claims that the problem of induction is thereby solved.

Suppose, for the sake of the argument, that (B) has a different form. For instance, here is a reconstruction of it, motivated by Busch's (2008).

(B*)
(C1/B1) Theories are approximately true.
(B2*) These theories have been typically produced by (or have been accepted on the basis of) IBE.

What is the best explanation of the connection between IBE and truth?
(C2) That IBE is reliable.

I do not think this is the right way to reconstruct my argument, the reason being that the claim that IBE is reliable does *not* explain (in this context) the connection between IBE and truth; it just sums it up. But even if it were the right way, (B*) would be an instance of IBE and not *ipso facto* circular.

How about (A) then? This is not circular either. It is an instance of IBE, but there is no reason to think that instances of IBE, in and of themselves, are circular. This clearly isn't.

Yet, the argument *as a whole* has an air of circularity. It employs/uses IBE while its (second) conclusion states that IBE (the rule or method employed at least partly for the generation of this conclusion) is reliable. Well and truly. Is this circularity vicious?

3

Vicious circularity is an epistemic charge—a viciously circular argument has no epistemic force. It cannot offer reasons to believe the conclusion. It cannot be persuasive. This has to be right. If the charge of circularity were logical and not epistemic, (if that is, a circular argument lacked validity altogether and not just epistemic force), all deductive arguments would be viciously circular. There is an obvious sense in which all deductive arguments are such that the conclusion is 'contained' in the premises—and this grounds/explains their logical validity. Hence, deductive arguments can be circular without being *viciously* circular. And similarly, *some* deductive arguments are *viciously* circular, (without thereby being invalid),--for instance: if Socrates is mortal, then Socrates is mortal; Socrates is mortal; therefore Socrates is mortal.

Premise-circularity (where the conclusion is explicitly one of the premises) is always and everywhere vicious! It cannot possibly have any epistemic force for someone who does not already accept the conclusion. NMA, insofar as it is circular, is *not* premise-circular. (C2) is *not* among the premises of (B). And (C1) is *not* among the premises of (A).

There is, however, another kind of circularity. This, as Braithwaite (1953, 276) put it "is the circularity involved in the use of a principle of inference being justified by the truth of a proposition which can only be established by the use of the same principle of inference". It can be called rule-circularity. In general, an argument has a number of premises $P_1, \ldots, P_n$. *Qua* argument, it rests on (employs/uses) a rule of inference R, by virtue of which a certain conclusion Q follows. It may be that Q has a certain content: it asserts or implies something about the rule of inference R *used* in the argument; in particular that R is reliable. So: rule-circular arguments are such that the argument itself is an instance, or involves essentially an application, of the rule of inference whose reliability is asserted in the conclusion.

If anything, NMA is rule-circular (though in an oblique sense). Part (A) yields a conclusion (C1), such that it, together with another premise (B2), yield another conclusion (C2), whose content is that the rule by means of which (C1) was arrived at is reliable. The pertinent question is whether rule-circularity is vicious. Obviously, rule circularity is *not* premise-circularity. But, one may wonder, is it still vicious in not having any epistemic force in some sense?

In my (1999), I tied this issue to the prospects of epistemological naturalism and externalism. In effect, I argued that NMA proceeds within a broad naturalistic framework in which the charge of circularity loses its bite because what is sought is not justification of inferential methods and practices (at least in the neo-Cartesian internalist sense) but their explanation and defence (in the epistemological externalist sense). It's not as if NMA should persuade a committed opponent of realism to change sides. But it can explain to all those who employ IBE, in virtue of what it is reliable; and it can possibly sway all those who are neutral on this issue.

I now think, however, that this kind of externalist defence of NMA is too narrow. What we should be after are *reasons to believe* that IBE is reliable (and not just an assertion to the effect that *if* indeed IBE is reliable, and we are externalists about justification, we are home and dry). Externalism does have a point. Reliability is a property of a rule of inference which the rule possesses (or fails to possess) independently of the reasons we have for thinking that it does (or does not). This is the point behind my claim that "NMA does not *make* IBE reliable. Nor does it add anything to its reliability, if it happens to be reliable" (1999, 83). Where I was wrong was in what immediately followed: "[NMA] merely generates a new belief about the reliability of IBE which is justified just in case IBE is reliable". NMA *does* generate a new belief (about the reliability of IBE) but this belief is not justified "just in case IBE is reliable". This is too externalist. I now think that NMA *justifies* this belief too. To see this, let us ask the broader (and interesting) question: can IBE be justified?

4

Obviously, this question has a fine structure. It depends on how exactly we understand IBE and how exactly we understand the call for justification. I have dealt with the first issue in some detail in my (2007). So I will limit myself to a few general comments towards the end of the paper. Let me focus on the second issue and let us ask again: can IBE be justified? If the answer is no, we end up with inferential scepticism. If the answer is yes, there are two options: non-inferential justification and inferential justification. A non-inferential justification of IBE, if possible at all, would have to rely on some a priori rational insight. An inferential justification of IBE would have to rely on some rule of inference.

There are obvious problems with all three options.

1. Scepticism leaves us in an inferential vacuum, which is hardly plausible.
2. Non-inferential justification presupposes something whose existence is dubious (rational insight).
3. Inferential justification has to rely on a rule of inference. If the rule is distinct, there is the issue of how the two rules are inferentially connected. If the rule is the self-same, we end up in rule-circularity.

The good news is that this is not a conceptual tangle that arises only in the case of IBE. It spills over to more basic forms of ampliative reasoning as well as to deductive logic. So IBE is in good company. Let's call this 'the good company argument'.

5

In the case of the justification of *modus ponens* (or any other genuinely fundamental rule of logic), if logical scepticism is to be forfeited, there are two options available: either non-inferential justification or inferential (rule-circular) justification. There is no non-inferential justification of modus ponens. Therefore, there is only rule-circular justification.

Indeed, any attempt to justify modus ponens by means of an argument has to employ modus ponens itself (see Dummett 1974). Why is there no non-inferential justification of modus ponens? There are many routes to this conclusion, but two stand out. The first is Quine's argument against basing logic on conventions; the second is that if non-inferential justification is meant to amount to default-reasonableness, we may well end up with a form of relativism, since what counts as default-reasonable might vary from one community to another. (For more on this, see Boghossian 2000). It follows that the rule-circular justification of IBE is in good company—with all basic forms of reasoning (including, of course, enumerative induction).

6

But couldn't *any* mode of reasoning (no matter how crazy or invalid) be justified by rule-circular arguments? Take for instance what may be called (due to Igor Douven) *Inference to the Worst Explanation*.

(IWE)
Scientific theories are generally quite unsuccessful
These theories are arrived at by application of IWE

What is the worst explanation of this?
That IWE is a reliable rule of inference

Let's call this, following Boghossian (2000, 245) *the bad company objection*. How can it be avoided? The reply here is that the employment of rule-circular arguments rests on or requires the absence of specific reasons to doubt the reliability of a rule of inference. We can call this, the *Fair-Treatment Principle*: a doxastic/inferential practice is innocent until proven guilty. This puts the onus on those who want to show guilt. I take this to be a fundamental epistemic principle. To motivate it properly would require much more space than I have now. But the basic idea is this. Traditional foundationalism has been tied to active justification, viz., to the active search for reasons for holding a belief. So any belief is suspect unless there is some good reason to hold it. The search for independent reasons for holding the belief is then necessary for its justification, since without them there is no way to ensure that the belief is rationally held. There are many reasons why active justification is too strong a condition on the rationality of belief. But in any case, there is an alternative picture of epistemology, what Gilbert Harman (1999) has called 'general conservatism'. According to this picture, no belief requires active justification in the absence of well-motivated objections to it. The rationale for this is that justification has to start from somewhere and there is no other point to start apart from where we currently are, that is from our current beliefs and inferential practices. Accordingly, unless there are specific reasons to doubt the reliability of IBE, there is no reason to forego its uses in justificatory arguments. Nor is there reason to search for an active justification of it. Things are obviously different with IWE, since there are plenty of reasons to doubt its reliability, the chief being that typically the worst explanations (whatever that means) of the explananda are not truthlike; not to mention the fact that the first premise of IWE is false.

It may be further objected that even if the *Fair-Treatment Principle* permits the employment of certain inferential rules, it fails to give us reasons to *rely* on them. I am not sure positive reasons, as opposed to the absence of reasons to doubt, are required for the employment of a rule. But in any case, it can be argued that there are some proto-reasons for the use of certain basis inferential rules. Do not forget that our basic inferential rules (including IBE, of course) are rules we value. And we value them because they are *our* rules, that is rules we employ and reply upon to form beliefs. Part of the reason why we value these rules is that they have tended to generate true beliefs—hence we have some reason to think they are reliable, or at least more reliable than competing rules (say IWE). So even if it is accepted that the employment of rule-circular arguments in favour of an inferential rule does require the presence of reasons to take the rule seriously, there are such reasons.

7

We can pursue the issue of justification by means of rule-circular arguments a bit further, by raising the issue of whether there are intuitive constraints on justification which rule-circular arguments violate.

Suppose one were to say:

(J)
No use of rule R is justified unless *X*.

What could *X* be such that rule-circular arguments violate it? The only plausible candidate for *X* which would be violated by a rule-circular argument is: R's reliability-relevant properties are proved/supported by an independent argument. So

(J*)
No use of rule R is justified unless R's reliability-relevant properties are proved or supported by an independent argument.

Even then, there is a sense in which a rule-circular argument *is* an independent argument, since it can have epistemic force for someone who has no views about the rules they employ. In other words, an independent argument need *not* be an argument of a different form. Still, this is *weak independence*, since the users of R are *disposed* to use it, even if they have no views about it. What if we opted for a strong sense of independence?

(SI)
An argument for the reliability-relevant properties of R is strongly independent if it is either different in form from R or it can sway someone who is not already disposed to using R to start using it (or to acquire this disposition).

Note that the first disjunct of this condition is question-begging. But, suppose it is not. If we take it seriously, as noted already, it would be impotent as a criterion for the justification of a basic rule of inference, since no basic inferential rule can be justified by the application of another (distinct in character) rule. Inferential scepticism would follow suit. To see this, reflect on the following claim: no use of memory is justified unless the memory's reliability-relevant properties are proved/supported by a non-memory-based argument. Whatever this supposedly independent argument might look like, it will have to be, ultimately, memory-based, since it has to be remembered!

The second disjunct of (SI) is moot. A rule-circular argument might (conceivably) sway someone to become disposed to use this very rule. Alternatively, why should it be an intuitive requirement on justification of an inferential rule that it can rationally *force* someone to start using the rule? Suppose we do require something like this. Let's call it a condition of *extra strong independence*.

(ESI)
An argument for the reliability-relevant properties of R is extra strongly independent if and only if it can be used to sway a sceptic about R.

(ESI) is clearly *not* an intuitive constraint on justification, unless massive inferential scepticism is an intuitive position—which is not. Note, *a propos*, that nowhere is it said or implied that the use of a rule R is (or should be) rationally compelling—at least if by that it is meant that there are (or should be) arguments for R that can sway the sceptic. But, clearly, the use of a rule R and its justification on the basis of a non-sceptic-suasive rule-circular argument are rationally permitted.

8

In a recent piece Valeriano Iranzo (2008) has raised further objections to my formulation of the NMA. He grants part (A) of the argument (see section 1 above), but claims that my part (B) could in fact be replaced by the following:

(I)
(I1) Background theories are approximately true (a fortiori, they are approximately empirically adequate).
(I2) Background theories have been arrived at by IBE.
(*) An inference is instrumentally reliable iff it yields a high rate of empirically adequate conclusions.
(I3) Therefore, IBE is instrumentally reliable.

This, he argues, is a version of NMA suitable for anti-realists, since it rests on a weaker premise (*) and draws a weaker conclusion than (B). Clearly (*), *qua* a definition of instrumental reliability, is weaker than the definition of reliability required for (B). Iranzo takes it that this anti-realist version of NMA is broadly within the reliabilist camp, since it defends the instrumental *reliability* of IBE. But then he goes on to claim that once (I) is seen as an option, the conclusion (I3) should lead us to *replace* the first premise (I1) with the following weaker premise:

(I1*) Background theories are empirically adequate.

There is something strange going on here. Iranzo's anti-realist NMA is a self-undermining argument. Its conclusion (I1*) weakens one of the premises that led to it, viz., (I1). If Iranzo grants part (A) of the argument, as he says he does, there are reasons to accept premise (I1), [C1 in my formulation of (A) in section 1], which in fact is the conclusion of (A). That is, there are reasons (best-explanation based reasons) to accept that background theories are approximately true and a fortiori that they are approximately empirically adequate. But, by plugging (I1) into (I) we are entitled, according to Iranzo, only to part of the content of the premise (I1), viz., its part which has to do with the empirical adequacy of theories. I think this situation borders with incoherence. The point is not that we may find out that only part of the content of a premise was necessary for the derivation of a certain conclusion. This is fine, of course. The problem is that the very reason for holding the weakened premise (I1*) was the stronger premise (I1). In particular, the very reason for holding that background theories are approximately empirically adequate is that this follows from the conclusion of (A) that they are approximately true. So, I doubt that (I) is a coherently formulated argument.

There *is* a coherent anti-realist version of NMA, but to see it we need to change part (A) of the argument too. So:

AR-NMA

(AR-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?
(AR-C1) The best explanation (of the instrumental reliability of scientific methodology) is that background theories are (approximately) empirically adequate.

(AR-B)
(AR-B1) Background theories are (approximately) empirically adequate.
(I2) Background theories have been arrived at by IBE.
(*) An inference is instrumentally reliable iff it yields a high rate of empirically adequate conclusions.
(I3) Therefore, IBE is instrumentally reliable.

Note that (AR-NMA) takes it that (A) defends empirical adequacy as the best explanation of instrumental reliability background theories. Then, it proceeds by drawing the further (weaker) conclusion that IBE is instrumentally reliable. What is wrong with (AR-NMA)? If we take seriously the obligation/permission distinction noted above, it is a rationally permitted argument. However, in this coherent formulation of (AR-NMA), the issue between it and NMA is whether (C1) or (AR-C1) is the best explanation of the instrumental reliability of background theories. In other words, is truth or empirical adequacy the best explanation? No much progress can be made on this front—though I still think that truth is a better explanation than empirical adequacy, for the reasons noted already in my (1999, chapter 4).

Perhaps some progress can be made if we take a different line of argument. (AR-NMA) wavers between two formulations, depending on how we read premise (AR-B1):