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Essays dedicated to Dag Westerståhl on his sixtieth birthday

Realism and Scientific Failure

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Abstract

Originally put forward by Smart and Maxwell in the 1960's, and later revived by Putnam (1975) and Boyd (1984), *the no miracle argument* for scientific realism has been much discussed. In this paper, I try to repudiate an anti-realist charge of circularity on the argument, here further scrutinized through a recent formulation due to Stathis Psillos (1999). Moreover, I attempt to turn the tables around on the anti-realists by arguing that they can hardly be said be able to explain the disheartening historical picture suggested by some of their own arguments, especially the ones pertaining to the famous *pessimistic induction*.

Do electrons exist? After all, nobody has ever touched, tasted, smelled or in any other way been in direct perceptual contact with an electron. So are we at all justified in treating electrons as anything over and above handy logical constructions—as fictions fit for predictions?

In a more general form, this kind of question is the springboard for the debate between scientific realists and scientific anti-realists. In the present paper, I will defend scientific realism from two specific lines of attack. In § 1, I set the stage by specifying the terms 'scientific realism' and 'scientific anti-realism,' as I will understand them here. § 2 deals with the implications of scientific success for the interpretation of scientific theories. In that section I try, in particular, to repudiate an anti-realist charge of circularity on an oft-mentioned argument for realism in terms of scientific success—*the no miracle argument*. The particular reading I will focus on is due to Stathis Psillos (1999). In § 3, I turn to a discussion of scientific *failure*, rather than success, and attempt to turn the tables around on the anti-realist by arguing that they can hardly be said be able to explain the depressing historical picture suggested by some of their own arguments, e.g., the ones pertaining to the famous so-called *pessimistic induction*.

1. Setting the Stage

I take the following two claims to lie at the core of the scientific realist's position:

> **The semantic claim**: The propositions of scientific theories are to be taken *at face value*.
> **The ontological claim**: The world, which the propositions of scientific theories describe, is (a) *objective*, in the sense of being as it is independently of our epistemic capacities, and (b) *independent*, in the sense of existing independently of the mental.[1]

A couple of comments are at place. Regarding the ontological claim, (a) is supposed to distinguish realism from the theories of such philosophers as Kant, according to whom the world is dependent on the synthesizing apparatus of our minds, and Kuhn, according to whom the world is partially constituted by the conceptual activities of the scientists interacting with it. (b) is supposed to distinguish realism from such theories as phenomenalism and Leibniz' theory of the world as made up of minds. Both the phenomenalist and Leibnizian world are ever so objective, but nevertheless not the kind of worlds that the realist believes in. In order not to confuse the world we are after here with any of these other worlds, I will henceforth call the world at issue in the ontological claim "the World" with a capital "W."[2]

Furthermore, the two claims taken together allow us to formulate the *ontological commitment* that the scientific realist associates with the propositions that make up scientific theories. In an unpublished paper, Stathis Psillos (forthcoming) has argued that to take a

[1] Cf. Psillos (1999, p. xix), Putnam (1978, pp. 20-21) and Laudan (1981, pp. 20-21) who put forward variants of the semantic claim, as do Boyd (1984, pp. 41-2), who also invokes an analogue of the ontological claim. The latter claim is here partly inspired by Devitt's (1984 and forthcoming) formulation of what he calls *the independence dimension* of realism. Contra some philosophers of science, I have not included any claims to the effect of science approximating the truth in my characterization of scientific realism. In this I differ from, among others, Boyd (1984, pp. 41-42). See also Putnam (1978, pp. 20-21), and Laudan (1981, pp. 20-21).

[2] Cf. Fine's "*World*," which consist in the "traditional conjunction of externality [here: objectivity] and independence" that "leads to the realist picture of an objective, external world" (Fine, 1986, p. 150).

proposition at face value is to take the truth of such propositions to be *ontologically transparent in the first instance* with respect to their truth-makers. This is not to deny that many truths are not ontologically transparent at all. All true propositions represent facts, but not all true propositions represent them perspicuously. But the crucial point here is that, on a realist construal, scientific propositions are interpreted as being transparent until further notice, in that they are taken to represent genuine facts, if true. And according to the ontological claim, these facts are genuine (although not necessarily fundamental) facts of the World. Hence, *the scientific realist's ontological commitment*:

> (OC_R): The propositions of scientific theories should be taken to be ontologically transparent in the first instance with respect to their truth-makers, where the truth-makers in question should be taken to be genuine facts of the World.

Before delving deeper into matters concerning scientific realism, I want to say a few words about how I will treat the concept of truth. By and large, the discussion between scientific realists and scientific anti-realists seems to presuppose if not a correspondence theory of truth then at least a non-epistemic one. So will I. But I say "by and large" since there are exceptions, most prominently in the later work of Hilary Putnam (e.g., 1981), who invokes an epistemic concept of truth in his defense of so-called internal realism.

Finally, I will treat realism and anti-realism as exclusive, but not jointly exhaustive, positions. This in order to not rule out, e.g., Arthur Fine's *natural ontological attitude* by definition, which, at least according to Fine himself, is neither a realism nor an anti-realism.[3] Hence, while I will treat anti-realism and instrumentalism as equivalent terms, neither is extensionally identical to non-realism

2. The No Miracle Argument for Scientific Realism

Originally put forward by J. J. C. Smart (1963 & 1968) and Grover Maxwell (1962), and later revived by Richard Boyd (e.g., 1984) and Hilary Putnam (1975), the *no miracle argument* (henceforth *NMA*) has persuaded many philosophers that scientific realism is the correct approach to scientific theories.

[3] See Fine (1986 & 1991) for Fine's own discussion of this standpoint, which will not be further discussed in the present paper.

J. J. C. Smart formulated the argument as follows, where the theory *T*' is the part of theory *T* which says only what *T* says about the macroscopic, observable phenomena:

> […] the success of *T'* is explained by the fact that the original theory *T* is true of the things that it is ostensibly about; in other words by the fact that there really are electrons or whatever is postulated by the theory *T*. If there were no such things, and if *T* were not true in a realist way, would not the success of *T*' be quite inexplicable? One would have to suppose that there were innumerable lucky accidents about the behaviour mentioned in the observational vocabulary, so that they behaved miraculously as if they were brought about by the non-existent things ostensibly talked about in the theoretical vocabulary. (Smart 1968, pp. 150-1.)

The particular argument that I will focus on (without thereby claiming that it is the standard or most common version of the argument) is the following, which is a reconstruction due to Stathis Psillos (1999) of Boyd's influential version:

> That the methods by which scientists derive and test theoretical predictions are theory laden is undisputed. Scientists use accepted background theories in order to form their expectations, to choose the relevant methods for theory-testing, to devise experimental set-ups, [etc.] […] In essence, scientific methodology is almost linearly dependent on accepted background theories: it is these theories that make scientists adopt, advance or modify their methods of interaction with the world and the procedures they use in order to make measurements and test theories.
>
> These theory-laden methods lead to correct predictions and experimental success.
>
> How are we to explain this?
>
> The best explanation of the instrumental reliability of scientific methodology is that: *the theoretical statements which assert the specific causal connections or mechanisms by virtue of which scientific methods yield successful predictions are approximately true.* (Psillos 1999, p. 78; emphasis added.)

The intuitive pull of this argument is obvious. Scientific methodology is successful in its predictions. What can better explain this success than that the connections and mechanisms, postulated by the theories applied in this methodology, correspond to (or are good approximations of) connections and mechanisms in the World?

Psillos reading is somewhat more complicated, however:

> […] it [the *NMA*] suggests that [1] the best explanation of the instrumental reliability of scientific methodology is that background theories are relevantly approximately true. [2] These background theories have themselves been typically arrived at by abductive reasoning. *Hence,* [3] *it is reasonable to believe that abductive reasoning is reliable: it tends to generate approximately true theories.* (Psillos 1999, p. 80; emphasis and bracketed figures added.)

I take it that there is an alternative *NMA*, however, or rather two related readings. The argument can, on the one hand, be read as merely an argument moving by abductive, inference to the best explanation (henceforth, *IBE*) reasoning from a premise about the success of science to a conclusion about the approximate truth of the theories applied by scientific practice, i.e., [1] in the last quote from Psillos. Let us call this the *minimal* reading of the argument. But it may, on the other hand, also be read as, in addition, appealing to an assumption about the methodology of science—an assumption about the reliability of *IBE*, invoked in order to reach the conclusion cited in [3]. And it is on this *extended* reading that the *NMA* is most plausibly considered a meta-argument (in relation to the *IBE* reasoning on the scientific object level).

Both readings have, however, been heavily criticized for being circular.[4] Let us consider Psillos' extended reading of the *NMA* first. This reading has been criticized for being circular in the sense of defending the claim that a rule of inference is reliable by using the very rule at issue. That is, the *NMA* opponent basically says, "You cannot use *IBE* to argue for the reliability of *IBE*." The circularity charge is an analogue to the one famously delivered by David Hume against some arguments for induction. And Psillos indeed tries to escape the problem posed by the objectors to the *NMA* by a distinction used already by

[4] See, e.g., Laudan (1981) and Fine (1986). Furthermore, Peter Lipton (2001) has criticized what I call the extended reading for not introducing any new evidence for the reliability of *IBE*, over and above the evidence already introduced by the minimal reading. I think Lipton is mistaken, but as I hope to show, the cost of attaining this evidence is too high in light of the problems connected to the extended reading.

Braithwaite, among others, in an attempt to defend inductive vindication of inductive learning from experience—a distinction between *premise-circularity* and *rule-circularity*.[5]

A premise-circular argument is a viciously circular argument "in the *petitio principii* sense of professing to infer a conclusion from a set of premises one of which is the conclusion itself" (Braithwaite, 1953, p. 276). A rule-circular argument, on the other hand, is an argument that asserts or implies something about the rule of inference used in the very same argument, in particular that the rule is reliable, effective, or the like. The *NMA* is quite clearly circular in the latter sense. But, Psillos claims, rule-circular arguments are not *viciously* circular.[6]

The reason for this is that no "assumptions about the reliability of a rule are present, neither explicitly nor implicitly, when an instance of this rule is used" (1999, p. 83). Psillos continues:

> When an instance of a rule is offered as the link between a set of (true) premises and a conclusion, what matters for the correctness of the conclusion is whether or not the rule *is* reliable that is, whether or not the contingent assumptions which are required to be in place in order for the rule to be reliable *are* in fact in place. (Psillos 1999, p. 83.)

The idea behind this is that we, according to Psillos, should treat reliability as a wholly objective property, in the same way some reliabilist epistemologists treat it in the debate between externalists and internalists regarding epistemic justification. In fact, Psillos is very explicit on this point, and even claims that "the issue of whether rule-circular arguments are vicious turns on the theory of justification one adopts." He continues: "Given an externalist perspective, the *NMA* does not have to assume anything about the reliability of *IBE*" (1999, p. 85). So, if *IBE is* reliable, then the *NMA* goes through (other things being equal, of course). If it *is not*, well, too bad. The point is that it is not up to *us*.[7] Hence, Psillos:

> If the rule of inference *is* reliable (this being an objective property of the rule) then, given true premises, the conclusion will also be true (or, better, likely to be true—if the rule is ampliative). Any assumptions that need to be made *about* the reliability of the rule of inference, be

[5] See Braithwaite (1953, pp. 274-278).

[6] See Psillos (1999, pp. 81-83).

[7] See Psillos (1999, pp. 83-85).

> they implicit or explicit, do not matter for the correctness of the conclusion. Hence, their defence is not necessary for the *correctness* of the conclusion. (Psillos 1999, p. 83; emphasis in original.)

The basic problem with Psillos' solution, however, is that he comes out of the battle with the conditionally weakened claim that *if IBE* is a reliable inference rule, *then* realism is probably true (in accordance with *NMA*), together with a contention to the effect that we have no reason to suspect the antecedent to be false.[8] That is, the conclusion is no longer that scientific realism is (probably) true, but a diluted conditional variant.

And it might just get worse because a further problem is that it is not obvious that we even *can* have reasons concerning the reliability of *IBE*, if reliability is supposed to be a wholly externalist affair. After all, this is one of the main upshots of the externalist-internalist debate in epistemology. As long as justification was an internal and cognitively accessible feature we could potentially find out if our reasoning was justified. But if we equate justification with externalist reliability this may not necessarily be so. And against this backdrop, the anti-realist may counter Psillos' claim that we do not have any reason to suspect the antecedent to be false by saying that this is because we in effect *cannot have* such reasons.[9]

But perhaps we do not have to take the externalist route. If I understand Psillos correctly, the crucial point about the non-vicious character of *NMA* is that it does not *presuppose* anything about the reliability of *IBE*, in analogy with how a child does not presuppose any meta logical properties of inference rules when gathering from the pleasant odor from the kitchen that dinner will soon be ready.[10]

In order to acknowledge this, and at the same time avoid the problems facing the extended reading, let us try the minimal reading instead, i.e., the reading corresponding to argument [1] in the above quote from Psillos. This reading has also been charged with being circular, or rather question begging, since it uses a rule of inference that the anti-realist does not accept. So, if the charge against the extended reading was that we are not allowed to use *IBE* in order to argue for the reliability *IBE*, the charge against the minimal reading is that

[8] See Psillos (1999, p. 85).

[9] Neither does Psillos adherence to naturalism help, since even if reliability is a natural property and in principal possible to determine through scientific practice, the status of this very practice still remains a moot point in the debate.

[10] Cf. Braithwaite (1953, pp. 281-85)

we cannot use *IBE* at all. After all, one might argue that the question at issue in the debate between scientific realists and anti-realists is whether the explanatory success of a theory lends any credibility to the further claim that it approximates truth. In other words, the controversial element of the discussion is none other than the method of inference used, *viz.* *IBE*.[11] Hence, Fine:

> […] [the scientific realist] must not offer as grounds for belief in realism its role in successful explanatory stories, on pain of begging the question […] since this is precisely the pattern of inference whose validity instrumentalism directly challenges at the level of ordinary scientific practice. (Fine 1986, p. 162.)

According to Laudan, this is why *NMA* flies in the face of the "deep-rooted conviction that the fallacy of affirming the consequent is indeed fallacious" (1981, p. 45).

This is indeed a drastic response to *NMA*. First of all, *IBE* does *not* involve affirming the consequent. *IBE* does not say that if *p* then *q*, *q*, hence *p*. What *IBE* does say, however, is that if *p* is the best explanation of the fact that *q*, then we are justified in believing that *p* in fact holds. That is, it does not say that in case the presence of a cat makes me sneeze, then we may infer from my sneezing that there is a cat around here somewhere. It does, however, say that if I sneeze and the best explanation of my sneezing is that there is a cat around here somewhere (e.g., since my colleague told me that she would bring her cat to work today; that I have no reason to believe that I have a cold; etc.), then I have good reasons to believe that there is a cat around here somewhere.

Still, the anti-realist may reject *IBE* as such, i.e., that explanatory success of a theory lends any credibility whatsoever to the further claim that the theory approximates truth. On one interpretation, this is what Bas van Fraassen (1980) does. Van Fraassen, unlike his positivist predecessors, does not deny that scientific theories are to be taken *literally*. (Cf. the *semantic claim* above.) If you believe in theories that use terms like 'electron' and 'photon,' then you are, according to van Fraassen, committed to the existence of electrons and photons. The trick is to not *believe* in them, but merely *accept* them, and "acceptance of a theory involves as belief only that it is empirically adequate" (1980, p. 12). According to van

[11] Laudan (1981) and Fine (e.g., 1986) made this charge (to my knowledge) independently. See also Hacking (1983: pp. 52-57).

Fraassen, a theory is empirically adequate if "what the theory says *about what is observable* (by us) is true" (1980, p. 18; emphasis in original). In other words, the theory saves the observable phenomena, and can hence in a sense be treated *as if* true, while the inquirers may remain agnostic as to whether it really *is* true. And it is in terms of this concept of empirical adequacy that I will henceforth construe scientific success, explanatory success, and their cognates. Against this backdrop, the anti-realist may even say that theories explain, but that accepting an explanatory theory does not even commit us to the approximate truth of the explanatory story.[12]

What does this amount to? In the above, I said that van Fraassen rejects *IBE simpliciter*, i.e., across the board, *on one interpretation*. The natural alternative is a partial rejection. A partial rejection could be construed thus: *IBE* is not a valid rule when it comes to the entities and laws postulated by scientific theories. We may indeed use *IBE* in other situations, say in our everyday interaction with the observable macroscopic world, but not when considering scientific theories. Is this a viable position? As pointed out by Boyd, the anti-realist who chooses this route must justify the proposed limitation on an otherwise legitimate principle of inference.[13] And I doubt that such a justification stands to be found here. For one thing, there is no sharp distinction between everyday interaction with the observable macroscopic world and scientific inquiry. I am not suggesting that "no *sharp* distinction" implies "no distinction *at all*," but I see no reason to believe that there is any non-question begging account (i.e., an account that does not boil down to that we do not want to be scientific realists) of why *IBE* should be subject to such restrictions; why it is a valid inference rule when it comes to chairs but not when it concerns electrons.[14]

This is not the only problem. The other problem is that van Fraassen himself sometimes rejects this interpretation, and says that he rejects *IBE* irrespective of whether the abduced hypothesis is about observables or nonobservables.[15] Now, remember van Fraassen's distinction between accepting and believing a theory. Van Fraassen has argued that there are two empirically indistinguishable forms of reasoning: *IBE* and as-if *IBE* reasoning. And

[12] Van Fraassen does not deny that the acceptance of a theory may involve a certain degree of belief that the theory is true. What he does deny is that accepting a theory involves a belief that the theory is approximately true, "which seems to mean belief that some of the member of a class centering on the mentioned theory is (exactly) true" (1980, p. 9).

[13] See Boyd (1984, p. 67).

[14] Cf. Psillos (1999, ch. 9).

[15] See Ladyman *et al.* (1997). This is also how Boyd (1984, p. 67) construes the anti-realist attack on *NMA*.

while the best explanation is taken to be true on *IBE* reasoning, the best explanation is merely taken to be as-if true on as-if *IBE* reasoning. But since we do only "believe" in theories in the sense of embracing the empirical adequacy of them (i.e., *accepting* them), the two coincide when it comes to observables. Hence, since scientific *IBE*-reasoning can just as well be construed as as-if *IBE* reasoning, the realist move to the (approximate) truth of the theories in question is supposed to be blocked.[16]

I agree, however, with Psillos that this argument by van Fraassen is spurious:

> If, as van Fraassen and his collaborators claim, the conclusion of an as-if *IBE* and of an *IBE* are equivalent when it comes to claims about observables, then there is no need to choose between them: if as-if *IBE* is reliable in its conclusions (in the restricted set of claims about observables), so is *IBE*. If one doubts the reliability of *IBE* when it comes to claims about observables, then one should also doubt the reliability of its rival which, as van Fraassen once put it, is 'apt in an anti-realist account'. Conversely, if one trusts the reliability of as-if *IBE* when it comes to claims about observables, then one should also trust the reliability of *IBE*. (Psillos 1999, p. 214.)

Hence, I take it that van Fraassen is committed to a full rejection of *IBE*. Interestingly enough, it is also in a discussion with such an *IBE* skeptic that *NMA* can indeed be seen as question begging. Consider the following analogous scenario: If you are a skeptic questioning the truth-conduciveness of, say, *modus ponens* and I try to bolster my case by using an argument applying *modus ponens*, you might want to reproach me with not playing fair. To quote Paul Boghossian: "I have certainly begged *your* question" (2001, p. 12).

In the light of such stubborn skepticism, it shall be noted that one final move is open to Psillos—one that he discusses but is not explicit about endorsing.[17] Let me illustrate it as follows: You are participating in an intellectual discussion. More or less good reasons are put forward for and against a host of different views on a host of different issues. What particular issues or views you and your interlocutors are dealing with is not relevant. It does not even have to be a very structured or well-arranged discussion. The participants may, at times, be sloppy and unclear in their reasoning and the discussions evolve in an ever so haphazard manner. The thing we ask for in order for the discussion to at all qualify as an

[16] See Ladyman *et al.* (1997).

[17] See Psillos (1999, pp. 88-9).

intellectual discussion is not a high degree of sophistication, but merely that it is based on an exchange of epistemic reasons, and that the participants involved in this exchange adhere to a few simple rules. Let us call these *rules of reasoning*.

These rules are not as clear cut and easily formulated as, say, the rules of chess or soccer, and one may be inclined to let the participants' following of these rules pertain to something along the lines of an intuition. In fact, Rudolph Carnap once talked about an "inductive intuition,"[18] which, in its empiricist guise, is a disposition to use inductive reasoning and to (fallibly) recognize whether arguments are inductively valid. Postulating a more general intuition, also pertaining to deductive and abductive reasoning and validity, may be a good means to explain why we are at all able to reason as we do and why not all intellectual activities broke down centuries ago.

But, wait a minute. Did we not just run into another instance of *IBE* reasoning? Indeed, but what this shows may be nothing other than an example related to the point made by Psillos in the following passage:

> […] evaluations [of our existing inferential practices] cannot be made from a neutral epistemological standpoint. They, too, have to employ some methods. In the final analysis, we just have to rely on some basic methods of inquiry. The fact that we make recourse to rule-circular arguments in order to defend them, if defense is necessary, is both inescapable and harmless. (Psillos 1999, p. 89.)

Postulating an abductive intuition is most plausibly done against this need for an inferential foundation where we ask no further questions and come to rest on brute intuition. Some skeptics, for whom restlessness is the prime virtue, may of course continue to ask *why* we should do this. How do we know that the basis of our rules of reasoning is reliable or secure? How are they to be justified?

But to ask such questions at *this* level is to misunderstand what it is to take part in an intellectual discussion, because if it were not for these rules of reasoning it would not be an intellectual discussion at all. It would be a mere play with words and gestures. And that is not skepticism in any meaningful sense of the term. That is reasoning gone mad. Or, one might just say, *reasoning undermining itself*. So, in a sense, the upshot here is a pragmatic

[18] See Carnap (1968, pp. 265-267).

reductio of a too skeptical questioning of the viability of some rules of reasoning and inference; if you push it too far, the revolver with the bullet of healthy skeptical doubt—indeed so vital to intellectual activity—blows up in your face. And that means, to say the least, the end of discussion.

3. The Failure of Scientific Theories—Turning the Tables Around

So, does the preceding section establish that the scientific realist is home and dry? Certainly not. Even if the discussion above can be taken to have established the plausibility of a certain conditional, namely a conditional along the lines of "if scientific theories are sufficiently successful, then we have reason to believe that they are at least approximately true," the anti-realist may still want to question whether the antecedent is true. This is not a common move, but a move that has been taken by Fine. A common move among anti-realists is however to appeal to the so-called *pessimistic induction*. Against the backdrop of a discussion of these two attacks, I want to turn the tables around in the argument against the *NMA* by showing that even *if* it should turn out that the *NMA* is viciously circular, the situation is even worse for the anti-realist—she can hardly be said to be able to make sense of the kinds of scientific evolution she typically draw upon in her arguments.

Now, according to Fine, "no further work is done by ascending from [empirical adequacy] to the realist's 'truth'." Hence, "the instrumental explanation has to be counted as better than the realist one" (Fine, 1986, p. 154). In fact, Fine even proposes that

> If the phenomena to be explained are not realist-laden, then to every good realist explanation there corresponds a better instrumentalist [i.e., anti-realist] explanation. (Fine 1986, p. 154.)

First of all, it is not obvious why the instrumental explanation has to be counted as *better* than the scientific realist's. As far as Fine's argument goes, the most plausible conclusion would be that they are on a par. But Fine probably has some kind of principle of ontological parsimony in mind—a principle, used by (among others) van Fraassen (1985), violated by the scientific realist but not by the anti-realist. Hence, the latter explanation is better, according to Fine.

Still, my feeling is that there is more to be said here. There is something lacking in the anti-realist explanation; something found lacking already by Smart:

> Consider a man (in the sixteenth century) who is a realist about the Copernican hypothesis but instrumentalist about the Ptolemaic one. He can explain the instrumental usefulness of the Ptolemaic system of epicycles because he can prove that the Ptolemaic system can produce almost the same predictions about the apparent motions of the planets as does the Copernican hypothesis. Hence the assumption of the realist truth of the Copernican hypothesis explains the instrumental usefulness of the Ptolemaic one. Such an explanation of the instrumental usefulness of certain theories would not be possible if *all* theories were regarded as merely instrumental. (Smart 1968, p. 151.)

In other words: if we adhere to anti-realism (or instrumentalism, to use Smart's term) about scientific theories, we are unable to *explain* such phenomena as predictive success, empirical adequacy, and instrumental usefulness.[19] Hence, McMullin: "The realist argues that if molecules actually exist, the sequence of events in the history of molecular theory becomes entirely intelligible. The instrumentalist has to be content with saying that it just *happened* that way" (McMullin 1991, p. 106). The anti-realist is, of course, still entitled to *use* these concepts—predictive success, empirical adequacy, etc.—and, when it comes to scientific success, appeal to the fact that the theories in question clearly works, and simply say, "Why ask further questions?" When discussing the question of why all observable planetary phenomena fit Copernicus' theory, van Fraassen actually says that the fact that "the observable phenomena exhibit these regularities, because of which they fit the theory, is merely a brute fact, and may or may not have an explanation in terms of unobservable facts 'behind the phenomena'" (van Fraassen 1980, p. 24).[20]

So why should we not just let the anti-realist get away with postulating such brute facts? Surely anti-realists—like any proponent of a philosophical theory—have to be allowed *some* primitive concepts. So why should we not grant the anti-realist the concept

[19] Van Fraassen (1980, p. 40) contests this, and claims that the success of science may be explained in Darwinian terms. See Psillos (1999, pp. 96-7) for a convincing critique of this proposal.

[20] This is the point Stanford (2000) addresses when he suggests to the anti-realist that the success of a given false theory, e.g., the Ptolemaic, indeed can be explained, namely by the fact that "its prediction are (sufficiently) close to those made by the true theoretical account of the relevant domain" (p. 275), i.e., the Copernican theory. This is an interesting suggestion but probably not a very promising one, especially in the light of Psillos (2001) critique of it.

'scientific success' as a primitive concept for explaining certain scientific events, where the scientific success in itself is given no further explanation, due to the primitive status of the corresponding concept?

After all, for the sake of scientific inquiry, the question of why a successful theory is successful *may* be uninteresting (even if I strongly doubt that it in fact is). But what about the *failure* of a scientific theory? Even granting that van Fraassen is correct in that "science, in contrast to scientific realism, does not place an overriding value on explanation in the absence of any gain for empirical results" (1980, p. 34), I doubt that any scientist would be content with an explanation in terms of that it is a brute fact that the theory did not fit the regularities. Because, if not before, I take it that scientists generally feel a need for an explanation when a theory fails, because if we know why a theory failed, we know how to avoid doing the same mistake again when constructing new theories.

Hence, the anti-realist's lack of explanatory capacity gets especially significant in the case of scientific failure. And whether they can explain failure is an interesting question not only for the reasons put forward in the preceding paragraph, but also since some arguments propounded by anti-realists indeed assume that the history of scientific inquiry is largely marked by failure. Consider, by way of example, the following passage of Fine's:

> If, for example, we could examine the myriad attempts in laboratories around the world just (literally) yesterday to turn basic science to the production of a useful instrument, then, I think, we could find failure on a massive scale, and certainly not any overall success. Further, if we study the application of science over time to a reasonably complex technology, then, even when success appears at the end of the road, it generally crowns a long history of frustration and failure. […] *I think a reasonable historical picture would be to draw each success as sitting on top of a great mountain of failures*. (Fine 1986, pp. 152-3; emphasis added.)

The upshot of Fine's argument is painfully clear: The abovementioned antecedent, stating that science is successful, is probably false. So even if we were justified in using the conditional stating that "if scientific theories are sufficiently successful, then we have reason to believe that they are at least approximately true," the antecedent nevertheless does not apply to science as we know it.[21]

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[21] Some relativist epistemologists have put forward a related argument against the claim that science is successful. See Kukla (1998, pp. 13-4) for a discussion.

I am not sure whether we would find "failure on a massive scale" if we investigated scientific inquiry as performed on a daily basis, but let us for the sake of the argument, suppose that Fine's picture is accurate. Because what is really interesting is not that realism may be able to explain (although, perhaps "circularly") the oft-alleged success of a great deal of scientific theorizing, but rather that realism also can explain why a great deal of scientific theories *failed* in empirical adequacy, while the inadequacy of the anti-realists' explanatory story gets especially obvious here.

In fact, they are hardly able to explain the picture Fine is drawing. If science constantly failed, how shall we explain such constant failure? To say that the theories used were not empirically adequate is hardly an answer. It is merely to restate the *explanandum*. Or to pick a particular example: Why did the phlogiston theory fail in due time? If the anti-realist answers the question by recourse to a primitive concept of scientific failure, she is simply refraining from giving an explanation. She can certainly say that it *did* fail—that, in the case of the phlogiston theory, it did not succeed in predicting phenomena to the same extent as the oxygen theory, and hence it was abandoned. It did, in other words, not remain empirically adequate in the end. But she cannot tell us *why*. The scientific realist, on the other hand, can: While there is such a thing as oxygen, there is no such thing as phlogiston. Again, I do not want to deny the anti-realist the right to invoke primitive concepts. What I want to do is question whether it is a good idea to invoke the particular concepts of scientific success and failure as primitive concepts.

This argument—perhaps best characterized as *an argument from scientific failure*—does in no way claim that there is a lop-sidedness between explaining success and explaining failure. What it *does* claim is that the insufficiency of the anti-realist's explanatory capacity becomes especially pertinent in the latter case. This is, of course, under the assumption that the anti-realist does not take false theories at face value, i.e., accept what I called the *semantic claim* above within the restricted domain of false theories. Such an anti-realist would indeed be entitled to say that a theory failed because it was false, without thereby being committed to inferring anything like (approximate) truth from success.

I do not know if an anti-realist would want to make such a semantic claim (nor of any anti-realist who has done it), but I can imagine that most anti-realists would think twice before making any reference to falsity, because right next to the concept of falsity lies its opposite: Truth. And as we all know, this is a concept most anti-realists want to avoid. Furthermore, the claim rejects a plausible symmetry between the explanation of successful and failed theories. Why apply one semantic to the first kind and another to the latter? I, for one, cannot come up with any reasons that do not beg the question at issue between the scientific realist and the anti-realist.

Now, what we are in effect doing when acknowledging this argument from scientific failure is also turning the tables around when it comes to the so-called *pessimistic induction* (henceforth, *PI*). P. Kyle Stanford formulates the argument thus:

> […] thinkers ranging from Pierre Duhem and Henri Poincaré to Larry Laudan have repeatedly pointed out that the history of science exhibits a parade of eminently successful theories that have nonetheless been subsequently rejected. And they ask why we would not believe that the same fate awaits our own successful theories. (Stanford 2003, p. 553.)

And this is what drove W. H. Newton-Smith to the remark that "Science, viewed *sub specie eternitatis*, can seem a depressing business" (1981 p. 183).

In order to not mistake this argument for Fine's argument above, we must note the difference in the picture drawn. While Fine suggests that scientific theories are mostly unsuccessful most of the time, the *PI* rather suggests that many scientific theories are successful but nonetheless rejected sooner or later (where I take it that the reason for rejection is that they were not successful enough in the end). And while few philosophers of science would adhere to Fine's view that science is as a whole very unsuccessful, as few would probably deny the picture drawn by the *PI*.

But the question is, of course, whether the *PI* gives us any further reason to suppose that theories should not be interpreted realistically. When confronted with *PI*, a common realist move has been to try to save the reference of terms of failed but successful theories.[22] Lately Stanford (2003) has argued, and in my opinion persuasively so, that such moves will at most result in a Pyrrhic victory, which ultimately undermines the very cause it is intended to advance, i.e., scientific realism. Therefore, I would like to try another, somewhat more head-on strategy. What I suggest is that the scientific realist may turn the tables around on the anti-realist and claim that the *PI* in fact makes a realist interpretation *plausible*. Because, why were not the failed successful theories in the end successful enough? Well, only given

[22] See Psillos (1999) and Kitcher (1993) for two attempts of this kind.

a realistic interpretation can we account for the failure at all: Taken at face-value, the entities, laws, etc., cited in the propositions that make up the scientific theory, did not correspond to a sufficiently large extent to genuine facts of the World.

I concede that this is not a fully satisfactory answer to the question, but merely the beginning of one. Among other things, the scientific realist has to explain why the lack of correspondence is of a kind that excludes correct predictions in the long run. But what I claim is that such explanations can only be delivered given a realist framework—i.e., given something along the lines of (OC_R) above. Naturally, more deliberation is needed in order to make a fully satisfactory case for the scientific realist along these lines. For now, I will confine myself to pre-empting two possible objections.

First, the anti-realist may want to disarm the argument through a compromise by claiming that some theories fail because the World "offers resistance." This will, however, not do for two reasons: (*i*) The approach is not fine-grained enough, since the World might leave quite good elbow room, and let the resistance kick in first when the theory is very bizarre. Hence, the failure of the theories that are not bizarre enough remains to be accounted for. (*ii*) More importantly, an admission in terms of the world offering resistance would, in effect, commit the anti-realist to a realist interpretation of the failed theory at issue. After all, the World can only *resist* the theory's description if the theory is *about* the World (see what I called *the ontological claim* above).

Secondly, and in light of (*ii*), the anti-realist may go Kantian and argue that scientific theories do not correspond to anything like the World, but to an interface between human cognition and an evidence transcendent world. That is, they may reject the ontological picture suggested by the objectivity part of *the ontological claim* above, and claim that clashes within the domain of this interface that accounts for the failure of scientific theories. And if the anti-realist indeed can argue persuasively for such an interface, I would be prepared to rephrase *the ontological claim* as to accommodate this.[23] I cannot, however, see that this presupposes anything other than (OC_R) above (albeit somewhat rephrased with respect to "the World"), and hence would turn out to be an anti-realism by the name only.

[23] See Putnam (1981) for an interesting and thought-provoking attempt to argue for such a neo-Kantian like picture of the world.

4. Conclusion

I have argued that what I call the minimal reading of the *NMA* could ultimately only be considered question begging given a very radical and implausible skepticism. Furthermore, I have argued that *even if* the scientific realist explanation in terms of the *NMA* had to be considered flawed, the situation is even worse for the anti-realist, since she can hardly be said to deliver an explanation at all. When faced with a success of science the anti-realist can indeed always counter the scientific realist's argument by saying, "Well, the theories are obviously empirically adequate, so why complain? Why add more ontological oil to an already smoothly running engine?", and bolster her case with an ontological principle of parsimony. And, as pointed out by McMullin: "if [the instrumentalist] is willing to leave unexplained the long-term fertility of a particular theory, there is no way for the realist to persuade him to do otherwise" (1991 p. 106). And, might I add, the *in*fertility of some, because, if I am correct, the same anti-realist move becomes even more painfully inadequate when it comes to explaining scientific *failure*. What the anti-realist can say by way of explanation is that the failed scientific theories were, in due time, defeated by more empirically adequate theories. In the end, they were not empirically adequate enough. But *why*? When it comes to answering this question, the scientific realist has the upper hand since we not only want to know *that* the theories failed and were abandoned, but also want to explain *why* they failed. And only the scientific realist has anything like the beginning of a genuine explanation: The theories did not approximate truth to a sufficiently large extent.[24]

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