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Psillos and Laudan

Psillos's Defense of Scientific Realism

In this paper, Psillos defends the IBE based 'no miracle argument' (NMA) for scientific realism against two main objections, 1) that it is viciously circular, and 2) that on the realist's own account of IBE, realism is not the best explanation of empirical success. Although he does consider van Fraassen's position, the defense is mainly directed against eliminative instrumentalism, which holds that scientific theories are:

merely syntactic/mathematical constructs for the organization of experimental and empirical facts, and for grouping together empirical laws and observations which would otherwise be taken to be irrelevant to one another (p. 72).

Initial plausibility arguments

Psillos begins by distinguishing between versions of the NMA that depend on IBE, Putnam-Boyd (the one on which he elaborates), and those do not depend on IBE (Smart and Maxwell), offering plausibility arguments instead.

Smart's argument is a purely a priori philosophical argument that realism is intuitively more plausible than instrumentalism, since the instrumentalism requires a 'cosmic coincidence' to explain the success of science.

I guess the idea here is that *empirical adequacy itself* is a "cosmic coincidence", which the empiricist cannot explain. But, how does the fact that T can explain T* (and T* cannot explain T*) make T a better explanation (than T*) of S? Isn't it question-begging to assume that empirical adequacy requires an explanation?

Maxwell also utilizes a notion of plausibility, but rather than arguing on philosophical grounds, he offers a Bayesian account, claiming that the prior probability of realism is higher than that of instrumentalism, and thus that there is greater confirmation of realism than instrumentalism.

Puzzling. Isn't truth logically stronger than empirical adequacy? If so, how can realism be more probable than instrumentalism? This must presuppose something like T vs T* & ~I (where I is the part that goes beyond empirical adequacy). This makes the alternatives mutually exclusive, and so it could be the case that T* & I is more probable than T* & ~I.

Psillos claims that plausibility arguments support scientific realism because they "rely on sound expectations" (p. 76). He writes that instrumentalist theories, in contrast, provide no basis for the expectation that current theories should continue to make accurate predictions in the future.

Huh? If you accept T as empirically adequate (i.e., you accept T* as true), then you have every reason to expect that it will be predictively successful in the future, don't you?

Though Psillos sees these arguments as undermining the eliminative instrumentalist position, he recognizes that they do nothing to combat positions like van Frassen's, which does not challenge realist semantics for scientific theories, but rather only the idea that rationality demands belief that the scientific theories are true.

Right. But, he also should talk a bit more about T vs T*.

Defending Explanationism

Circularity

Psillos attempts to provide further support for Boyd's 'explanationist defense of realism' (EDR), which argues that the best explanation of the empirical success of scientific theories is that they are approximately true. The EDR takes the NMA to represent an abductive inference that abduction is reliable. The NMA is thus "a kind of *meta*-abduction" (p. 79). It has been charged that this argument is "viciously circular." Psillos offers a long counter-argument focusing on the meaning of 'viciously' and on the concept of circularity itself.

Psillos distinguishes between arguments that are "premiss-circular" and those that are "rule-circular" (p. 82). Arguments are premiss-circular when the truth of part of the conclusion is presupposed in the premises. A "rule-circular" argument, in contrast, represents an instance or application of the rule of inference supported by its conclusion.

The NMA, according to Psillos, is rule-circular, not premiss-circular. The conclusion of the NMA is that the background theories of science are approximately true, which, when taken in conjunction with information that these theories were arrived at via IBE, entails that IBE is a reliable form of inference. The conclusion that the background theories of science are approximately true is not among the premises.

We should unpack this a bit. As I understand it, Psillos thinks of the high-level NMA as being *grounded* by a collection of low-level IBE arguments, where it is best *causal* explanation that's doing the work at the low-level. He talks about T asserting causal processes *C* that bring about particular phenomena *X*. And, he asks: "What else can better explain the fact that the expected (or predicted) effect *X* was brought about than that the theory *T* – which asserted the causal connections between *C* and *X* – has got these causal connections right or nearly right?" Then, I presume, he takes the high-level IBE to explain why it is that low-level IBE – as a general form of inference – is reliable. He takes the explanandum of the HLIBE to be "the reliability of scientific methodology (generally) for yielding correct predictions." So, in this sense, it is a "meta-abduction". It aims to explain why low-level IBE is reliable.

But, it seems to me he's got things backward here. He's talking, on the one hand, about the prediction of *X* from *T* being "reliable". But, he seems to want to explain why low-level IBE is

reliable. The problem is that low-level IBE does not culminate in *predictions*, but theories which explain them! He seems to be confusing two directions of inference here.

It would help to lay-out this argument a bit. As I see it, he's taking as a premise that low-level IBE's are reliable, and then he's using the high level IBE to explain the reliability in general of the low-level IBE, understood as a scientific method. I think the argument looks something like this.

(L1) T1 best causally explains P1. Therefore, T1 is a true or approximately true causal story about P1.

...

(Ln) Tn best causally explains Pn. Therefore, Tn is a true or approximately true causal story about Pn.

(L) Each of the low-level IBE inferences (Li) is reliable. Indeed, low-level IBE – *as a general method* – is reliable.

(†) The best (general) explanation of the reliability of low-level IBE (L) is that, in general, theories that best causally explain phenomena are true or approximately true.

(H) Our most causally explanatory theories are true or approximately true.

We need to ask about (1) the truth of premises (L) and (†), and (2) the cogency of the subsequent inference to (H). First, the truth of (L). *Is low-level IBE reliable? What about Hitchcock's dilemma? In order to be a best causal explanation, doesn't T have to be true (or "theoretically truth-like")? If so, don't we need to have independent reasons to believe (Ti) is theoretically truth-like in order for the inference (Li) to be reliable? On the other hand, if truth isn't required of a good causal explanation, then why think T is the best explanation of P? In particular, why think T is a better explanation of P than T* (T's empirical substructure) is? The point here is that whatever reliability the (Li) inferences have, they seem to be getting it not from in virtue of their being IBEs, but from some other source. I don't think this is an internalism/externalism issue, but more an issue about the source of the reliability of the (Li). We'll discuss this more, below.*

What about (†)? Why should we think that the best explanation of (L) is that the theories that best causally explain phenomena are true or approximately true? This is one place where a circularity charge comes in (although, Psillos doesn't seem to notice this). Can we allow the assumption that the low-level IBE inferences (Li) are reliable? It depends on what reliability means. But, if we concede that we must explain why the low-level causal stories *are* generally true or approximately true (when they best causally explain), then this does seem to beg the question against the empiricist. And, besides, as Hitchcock argues, if we do concede this, then it seems that we must have independent warrant for Ti, that does not come from the (Li) IBE at all, in which case, the "reliability" of the (Li) is of little epistemological significance. I don't see how externalism helps against this charge (it's a point about argumentative burdens in the debate between the realist and empiricist, not about what counts as knowledge). Note: this is one place where Psillos seems to be confused about the direction of the "reliability" claim. Sometimes, he talks as if he means that the theory is reliably predicting X (which does *not* beg the question against the empiricist, but is not relevant for his purposes). Other times, he talks as if he means that the low-level IBE is itself a reliable inference (this does seem question-begging). It is the latter he needs here, since this is supposed to be a *meta*-abduction. The former is irrelevant here.

Finally, what about the cogency of the inference to (H)? This is where Psillos sees the circularity charge coming in. But, it seems to me it comes-in also above in connection with (†). I actually agree with Psillos here. If we grant that low-level IBE is reliable, then I'd say that would provide warrant for the claim that the theories are true or approximately true. So, I think he's just misplaced where the circularity objection gets traction. I think it's back at the (†) stage, not in the final inference to (H), once we have *granted* everything else.

Psillos argues that rule-circular arguments are not viciously circular. His main support for this derives from adherence to an externalist epistemology. An externalist epistemology, according to Psillos, requires nothing for the justifiable use of a rule beyond its reliability in leading to true conclusions. An internalist, on the other hand, would require some independent justification of the rule, prior to its use, and thus would conclude that rule-circular arguments are viciously circular. Realists must, consequently, be externalists.

Two potential objections are recognized here. 1) Doesn't NMA rely on the assumption that IBE is reliable, making it premiss-circular?

Yes, but we should be more precise about this. It's not so much "circularity" that's the problem, but there is a question-begging assumption against the empiricist implicit in (†), I think.

2) Doesn't the fact that realists defend the reliability of IBE via NMA tacitly reflect an internalist epistemology, which would require that the rule-circularity of NMA be considered vicious?

This is on the right track. The realist and the empiricist are both engaged in providing reasons and arguments. The question is whether Psillos' argument meets his argumentative burden against the empiricist, not on what the conditions for knowledge are. We can say this in externalist terms, if he likes, but it won't help him. He needs to convince the empiricist that the argument is reliable. And, to do that, he'll need to overcome challenges like the question-begging charge and Hitchcock's dilemma.

1) Psillos argues that NMA does not require that IBE be proven reliable before it is used, but rather only that there be no reason to think that it is unreliable, since in that case its use would not be justified.

Doesn't Hitchcock's dilemma give us reason to worry about the reliability of the (Li)? If reliability allows for circularity, then it's not very useful for adjudicating disputes. And, this is something Psillos needs to do.

2) Psillos gives two options for dispensing of objection 2.

a) First, one could claim that NMA does not seek to defend IBE, but rather that it leads to a belief about the reliability of IBE whose justification is contingent upon whether or not IBE actually is reliable.

What good is this? I think Hitchcock's dilemma provides some reason to worry about the reliability of the (Li)s, unless reliability is consistent with question-begging. Either way, this is a rather lame position to sit on.

b) Or, even if one grants that NMA *does* seek to defend the reliability of IBE, this option is not excluded by externalism, but rather simply is not required. Further, if the fact that the defense of the reliability of an inferential practice relies on a rule-circular argument implied that the defense were vicious, then there would be no way to defend the reliability of basic inferences like modus ponens or induction. The suggestion is that abduction, just like deductive and inductive rules of inference, must be grounded in dispositions.

I don't see why we even get to this point. The meta-abduction only works if the explanandum is granted. But, why would an empiricist grant the explanandum?

Also, it was unclear to me how this bears on the debate. Both parties are happy with IBE for observables. The only question comes when it's applied to unobservables.

Psillos cites one further objection that has been raised by Fine. Fine cites the idea that meta-theoretic arguments must be more rigorous than those arguments used by the theory to which they refer, otherwise, meta-theory is without purpose. Thus, Fine claims, that NMA, since it is an instance of IBE, cannot be used to defend IBE. Psillos responds that this objection is too strong and is not naturalistic by drawing an analogy to the incompleteness theorem.

In the end, it's not the circularity of the inference rule that's at issue here. I think the issue is whether the realist can take (†) as an explanandum without begging the question against the realist. It's not the IBE is circular, it's that taking (†) as an explanandum is question-begging.

Is NMA the best explanation?

Here Psillos responds to further objections from Fine that instrumentalism provides a better explanation of the success of science than does approximate truth.

First, Psillos claims that Fine cannot be using IBE to infer the *truth* of instrumentalism- this would require the reliability of IBE. He argues that Fine's IBE must revolve around the notion of empirical adequacy rather than approximate truth. However, if Fine's IBE measure is empirical adequacy, which instrumentalists see as the only epistemic virtue of explanations, then there is no way to decide whether approximate truth or instrumentalism is the best explanation, since both yield the same degree of empirical adequacy. (But when the empiricists are using IBE to show that realism is inconsistent, don't they get to assume IBE epistemic values?)

Explain this to me.

Another objection leveled by Fine is that truth does not provide any explanation beyond that provided by instrumentalism. Realists must have some way to link truth to empirical success, and what can this consist in other than pragmatic reliability? The additional work that realists

purport is done by truth is really done solely by this pragmatic reliability, which instrumentalism provides for equally well. And since the instrumentalist explanation is more conservative, it is therefore better.

Psillos claims that it is not clear that the pragmatic intermediary described above exists, but if it did, it must refer to the methods, models, etc. used to make predictions. If things like these represent the pragmatic intermediary, truth is not superfluous, since it provides a basis for preferring some methods or models instead of others. (Wouldn't van Frassen's Darwinian selection mechanism do the same thing?) Secondly, Psillos claims that Fine's instrumental reliability cannot be distinguished from empirical success, and thus that on Fine's account, the explanans and explanandum are one and the same.

Fine responds to these objections with the claim that instrumental reliability entails a *disposition* toward empirical success, and that the two are therefore different. Psillos's response is to demand an explanation of this disposition, for which he offers, of course, the candidate of approximate truth.

A final possible anti-realist move, according to Psillos, would be a deflationary account of explanation whereby one simply draws an inductive inference from the past success of abduction in arriving at instrumentally reliable theories to the conclusion that future theories reached by abduction will be instrumentally reliable. Psillos's response is two-fold. First, he argues that there is no inductive evidence that, without truth, abduction will continue to be instrumentally reliable into the future. (Isn't this presupposing the truth of theories?) Thus, this argument does not explain why scientific methodology is reliable. Secondly, Psillos argues that 'induction about abduction' maintains theoretical commitments, since inductive inference requires acceptance of the truth of instrumental reliability for past theoretical generalizations, generalizations which depended on theory. (Again, what about van Frassen's Darwinism?)

Darwin

Here Psillos responds to van Frassen's Darwinian account of the success of science. He argues, citing Lipton's red-hair club, that realism provides a better explanation for the selection of theories, because realism offers a deeper account of the causal reasons for which selected theories are successful. (But isn't empirical adequacy assumed by van Frassen to be the primitive?)

Laudan's confutation of convergent realism

Laudan argues that epistemological realism is not viable as an empirical hypothesis, since the concepts of verisimilitude and reference on which it rests are problematic, and fail to be

explanatory, and since there is considerable empirical evidence that confutes, rather than supports, the realist premises.

Laudan's confutation is not directed toward 'semantic realism.' His position is not the kind of instrumentalism against which Psillos argues. He doesn't ascribe to any explanation of the success of science, because historically, as he argues, science has, by and large, been unsuccessful.

Laudan identifies five central claims for convergent epistemological realism (CER):

R1) Scientific theories (at least in the 'mature' sciences) are typically approximately true and more recent theories are closer to the truth than older theories in the same domain;

R2) The observational and theoretical terms within the theories of a mature science genuinely refer (roughly, there are substances in the world that correspond to the ontologies presumed by our best theories);

R3) Successive theories in any mature science will be such that they 'preserve' the theoretical relations and the apparent referents of earlier theories (i.e., earlier theories will be 'limiting cases' of later theories).

R4) Acceptable new theories do and should explain why their predecessors were successful insofar as they were successful.

R5) Theses (R1-R4) entail that ('mature') scientific theories should be successful; indeed, these theses constitute the best, if not the only, explanation for the success of science. The empirical success of science (in the sense of giving detailed explanations and accurate predictions) accordingly provides striking empirical confirmation for realism.

According to Laudan, CER involves two abductive arguments.

Argument 1: Inference to the best explanation from R1, R2, and R5, along with the claim that scientific theories actually are empirically successful, leads to the conclusion that the theoretical terms of science refer and that the theories of science are approximately true.

Argument 2: Inference to the best explanation from R3 and R5, along with the claim that scientists actually do attempt to preserve, and usually succeed in preserving, earlier theories as limiting cases of later theories, leads to the conclusion that the terms of earlier theories in 'mature' sciences refer and that these theories are approximately true.

Argument 1

For his refutation of argument 1, Laudan (addressing Putnam) states that the realist seemingly requires claims S1-S4.

S1) The theories in the advanced or mature sciences are successful;

S2) A theory whose central terms genuinely refer will be a successful theory;

S3) If a theory is successful, we can reasonably infer that its central terms genuinely refer;

S4) All the central terms in theories in the mature sciences do refer.

Laudan admits that, in some sense, S1 might be acceptable. However, S2-S4, he argues, are not acceptable.

S2 is refuted mainly via counterexamples:

Counterexamples to S2: the Proutian theory that hydrogen atoms make up the atoms of heavy elements, the Wegnerian theory of continental drift, Dalton's theories concerning atoms, Bohr's early electron theory. The terms of these theories refer, yet the theories were not successful.

Counterexamples to S2', a weakened version of S2, which claims only that genuine reference *usually* yields success: the sum-totals of failed atomic theories, failed wave theories of light, kinetic theories of heat, developmental theories of embryology.

S3 is refuted by the claim that reference is not enough to explain success, since many, or even most theories that refer are unsuccessful (see counterexamples to S2), as well as by counterexamples (19th C. aether theories). The realist could weaken the inference drawn in S3 to conclude only that *some* of the terms in successful theories refer, but this would be inconsistent with the realist stance that evidence for any part of a theory is evidence for the entire theory, a claim the realist needs in order to argue for the truth of deeply theoretical statements.

S4 is unwarranted without S3.

The second aspect of Laudan's refutation of argument 1 involves criticism of the notion of approximate truth. Realists argue that:

T1) if a theory is approximately true, then it will be explanatorily successful;

T2) if a theory is explanatorily successful, then it is probably approximately true.

T1 is refuted by Laudan's claim that there appears to be no independent argument for a link between approximate truth and explanatory success, and by the fact that there has been no successful account of what it means for a theory to be approximately true.

Even if T1 is granted, T2 fails because approximate truth must require genuine reference, and there is no shortage of examples of theories that were successful but failed to refer: phlogiston theories, aether theories, etc.

To combat this objection, the realist attempts to limit his or her claims to the 'mature' sciences; however, this notion of 'maturity' is quite dubious. And even if the idea of maturity is granted,

how does the realist explain the historical success of theories that fail to be ‘mature’ on the realist definition? It seems that the realist fails to explain a significant aspect of the historical success of science. In addition, there are numerous examples in ‘mature’ sciences, like physics, of successful theories whose terms failed to refer. Finally, there have been theories that are genuinely referential and empirically successful, like geological theories denying continental drift, which no one would call approximately true.

Argument 2

Argument 2 involves the notion of convergence to truthlikeness, rather than predication. Laudan refutes argument 2 on three grounds, again, mainly through counterexamples.

First, Laudan argues that scientists often do not *attempt* to preserve earlier theories as limiting cases in new theories. Counterexamples include Lyell’s dispensation of catastrophist causes in developing uniformitarian geology and Darwin’s failure to preserve the mechanisms of Lamarckian evolution.

Second, independent of any attempt, it is not the case that later theories actually do preserve earlier theories as limiting cases. Counterexamples include the fact that Copernican astronomy did not preserve the mechanisms of Ptolemaic astronomy, Newton’s physics did not maintain Cartesian theories, etc. The problem for the realist is that maintenance of earlier theories as limiting cases for new theories requires wholesale adoption of the ontologies of earlier theories, since it precisely ontological truth for which the realist is arguing. In addition, the fact that limiting relations exist between earlier and later theories does not entail that the earlier theory, as a whole, is a limiting case of the later theory.

Third, theories cannot converge in the fashion described by the realist, since any change in theory requires a change in ontology, which implies that each of the theories will have unique consequences, leading to the conclusion that new theories cannot preserve all of the consequences of earlier theories.

A more moderate realism?

Laudan next focuses on refutation of R4, the claim that new theories should explain why earlier theories were successful, which leads to a weaker form of realism. He argues that the ability of a theory to explain the success of a predecessor is neither necessary nor sufficient for increased empirical success, which leaves one at a loss as to which theory actually is better. Additionally, a comparison of theories with respect to predictive success provides no basis for *epistemic* realism.

Conclusions

Ultimately, Laudan concludes that the realist largely begs the question against the anti-realist, since the realist argues that epistemic realism is true by virtue of the fact that its consequences are true. (Contra Psillos's arguments that realism is only *rule-circular*). The epistemic realist fails to subject his or her empirical hypothesis to rigorous testing comparable to that which he or she would require for any other scientific theory (Perhaps contra Psillos's argument that the meta-theory cannot be required to be more rigorous than the theory it describes).