By Otávio Bueno

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though the search for a true description of the world is a crucial feature of scientific realism, realists usually grant that we are typically unable warrantedly to assert that a given theory is true. This is particularly the case with theories that deal with unobservable entities or with aspects of the world that are spatio-temporally remote. Theories usually involve idealisations (such as point-masses or ideal gases) and simplifications (to allow, for example, the application of mathematics to the physical domain under consideration). Theories also disregard a number of distorting features, such as the presence of air-resistance in the description of the law of free fall. Furthermore, experimental results often contain errors, and predictions almost never exactly match these results. So it is not surprising that for the scientific realist it is not truth but “truth likeness [which] is the working notion of truth in science” (p. 276). If exact truth cannot be had, the realist can at least adopt truth-likeness.

But if truth-likeness plays such a role, can the realist provide a sensible account of this notion? Of course, this is a topic that has been in the realist’s agenda for a long time. And Psillos spends some time reviewing the difficulties faced by several attempts at providing a formal account of truth-likeness, including Popper’s proposal to define verisimilitude in terms of the truth content and the falsity content of a theory, the ‘possible worlds’ approach, and the ‘type-hierarchies’ view (pp. 261–275). According to Psillos, none of these accounts works. But the good news for realists is that they don’t actually need to provide a formal account of truth-likeness. There is no need to move beyond the intuitive notion of truth-likeness, which Psillos takes to be well enough understood. But what exactly is such an intuitive notion? Here is Psillos’s version (following previous works by Weston and Lewis): ‘A description D is approximately true of a state S if there is another state S’ such that S and S’ are linked by specific conditions of approximation, and D is true of S’. As Psillos points out, according to this account, a theoretical law is approximately true of the world if it is true in a world that approximates our world under certain conditions.

But wait: why exactly don’t realists need to provide a formal account of truth-likeness? In Psillos’s view, because the notion of truth-likeness (as opposed to the notion of truth) is not open to known paradoxes, such as the Liar. As we all know, the existence of such paradoxes led Tarski to formulate a formal account of truth. In the absence of corresponding paradoxes for truth-likeness, the intuitive notion is good enough.

The problem with this response is that, as characterised above, the intuitive notion of truth-likeness is defined in terms of truth. So if truth is open to paradoxes, so is the intuitive notion of truth-likeness. The realist may reply that he or she can always adopt a suitable formal account of truth to avoid the paradoxes. The difficulty with this reply is that if Psillos defines truth-likeness in terms of a formal account of truth, he can no longer claim to have an intuitive notion of truth-likeness. With truth-likeness defined in terms of formal truth, truth-likeness becomes formal too.

More importantly, the realist also needs to establish the connection between truth and truth-likeness (and one would expect something more than a simple definitional connection). It is not by chance that some realists go on to assert that truth is a limiting case of truth-likeness (see p. 273, where the claim is made that truth is a limiting case of verisimilitude). But it is unclear how the above intuitive account of truth-likeness can be used to maintain such a connection between truth and truth-likeness. How can truth be a limiting case of truth-likeness if in order to define truth-likeness the notion of truth is presupposed?

Perhaps the realist could reply that truth is not, after all, a limiting case of truth-likeness. But this seems to leave the realist with a notion of truth-likeness that isn’t of much use for realism. Realists, of course, ultimately concerned with truth. Truth-likeness is, as Psillos points out, just the “working notion of truth in science” (p. 276). The adoption of truth-likeness can be seen as a pragmatic expedient given the messiness of scientific practice. But I take it that the idea is to eventually get to truth—via truth-likeness. But how can this be done?

Well, it is not clear at all that it can be done. The difficulty here is that, according to the intuitive account, judgments about truth-likeness are thoroughly context-dependent. After all, the ‘specific conditions of approximation’ that are used in the definition of truth-likeness change from one context to another. But truth is not context-dependent—especially for the realist. To allow truth to be context dependent is, of course, to open the door to all sorts of relativisms that are anathema for a realist view. So a considerable gap between truth and truth-likeness needs to be bridged. But it is unclear how the intuitive notion of truth-likeness can be used to bridge this gap, given that it crucially depends on the thoroughly context dependent ‘specific conditions of approximation’. The intuitive notion, if anything, seems to highlight the gap.

This seems to leave the scientific realist in an unstable situation: there is the need for truth-likeness in the realist view, given the difficulties to assert the truth of a theory (due to the presence of idealisations, simplifications and so on). However, neither the formal account of truth-likeness
not the intuitive view seem to work. Given this scenario, perhaps the move to something weaker than truth, such as empirical adequacy, may not look so bad after all.

In order for Psillos to provide an account of the maturity of a scientific theory, he needs to articulate an account of the indispensability of a hypothesis to make a successful prediction. The maturity issue is, of course, crucial for the realist, given Psillos’s own formulation of scientific realism: according to the scientific realist, only mature scientific theories are expected to have terms that refer, and these are the theories that are expected to be truth-like (p. xix). But under what conditions do we say that a hypothesis indispensably contributes to the generation of a successful prediction? According to Psillos, the predictive indispensability of a hypothesis can be characterised thus:

Suppose that H together with another set of hypotheses H’ (and some auxiliaries A) entail a prediction P. H indispensably contributes to the generation of P if H’ and A alone cannot yield P and no other available hypothesis H* which is consistent with H’ and A can replace H without loss in the relevant derivation of P (p. 110).

As Psillos correctly notes, there is a sense in which no theoretical claim is indispensable to the generation of a given prediction: we can simply adopt a Craig-transform of the theory under consideration or we can “cook up” a hypothesis H* by writing P into it” (p. 110). To avoid these possibilities, Psillos advances some epistemic constraints that any putative hypothesis is required to satisfy. The constraints include requirements that a theory be independently motivated, non-ad hoc, potentially explanatory and so on. With these constraints, Psillos can then conclude: “it is not certain at all that a suitable replacement can always be found” (p. 110). I agree.

The problem, however, is that with those epistemic constraints, it is not clear that any suitable replacement can ever be found. It might be said that this is exactly what Psillos intended. After all, the point of having an account of predictive indispensability is precisely to single out one—and only one—crucial hypothesis without which the successful prediction can’t be made. And it is clear why the scientific realist needs an account as strong as this: without such a tight connection between the relevant hypothesis and the successful prediction, scientific realists would have a hard time avoiding Laudan’s well-known counterexamples of theories whose predictions seem to be successful but whose terms don’t refer (see Laudan [1981] and Laudan [1996]). To deflate Laudan’s counterexamples, Psillos strategy is to single out the class of predictively indispensable hypotheses, and claim that those are the hypotheses the scientific realist should be realist about. Given that such hypotheses are indispensable to the generation of the relevant predictions, we cannot abandon the hypotheses without losing the predictions.

But in trying to avoid Laudan’s counterexamples, Psillos’s account seems to have moved too far. For how can the scientific realist establish that a particular hypothesis indispensably contributes to the generation of a given prediction? In order to establish that, not only does one need to establish that there is a hypothesis H such that, together with other auxiliary hypotheses H’ and background assumptions A, it predicts P—which can be done—but one also needs to establish that no other ‘available’ hypothesis H* predicts P (together with H’ and A). In other words, what the realist needs to establish is that H is the one and only hypothesis that generates the prediction of P (given H’ and A). But how can the realist ever be able to establish that?

The problem here emerges from the requirement that there is no available hypothesis H*. What does the realist mean by ‘available’? If by ‘available’ it means ‘at the historical moment in which H was originally entertained’, then Psillos’s account of predictive indispensability lacks the force to overcome Laudan’s criticism. For there will be hypotheses that were predictively indispensable at one time (given that there were no available alternatives to them when the hypotheses were first formulated), but which turned out not to be indispensable at a later time (with the formulation of a new hypothesis that generated the relevant prediction). With this account, the scientific realist will be basically endorsing Laudan’s criticism!

However, if by ‘available’ the realist means ‘conceptually available’, rather than historically so, in the sense that the alternative hypothesis H* may not have actually been available at the moment in which H was first formulated, but could be entertained by some scientific community in the future, then Psillos’s account becomes way too strong, even for the realist. For how could the realist assert that a given hypothesis is indeed predictively indispensable? He or she would need to show that there is no conceptually available hypothesis H* that also generates the prediction P. But unless the hypothesis H* is inconsistent (a possibility that the scientific realist won’t take seriously anyway), a whole range of hypotheses H* are conceptually available (and they even satisfy Psillos’s epistemic constraints). In other words, the difficulty here is that, at any particular moment of time, the realist will never be able to establish that there are no conceptually available hypotheses H* that entail P—and so the realist won’t be able to establish that H indispensably contributes to the generation of P. For even if the scientific community may not be able to
conceive of H* in a particular moment, this doesn’t entail that it won’t be able to conceive of it in the future.

However, perhaps the interpretation of ‘available hypothesis’ as sheer conceptual availability is enough for the scientific realist’s needs. For it allows him or her to deal with past cases of predictive indispensability of scientific theories. Although the scientific realist is unable to claim that a given hypothesis is predictively indispensable, he or she can at least claim that a particular hypothesis is not indispensable. For if a suitable replacement H* has ever been found for a given hypothesis H, that’s all that the realist needs to claim that H wasn’t indispensable after all.

But this leaves the scientific realist in an awkward position. He or she can never be warranted in asserting that a given hypothesis is predictively indispensable, and so he or she can never be warranted in asserting that a given theory is indeed mature. At best the realist can claim that a given hypothesis isn’t predictively indispensable, and so that a given theory isn’t mature after all. What this entails is that the scientific realist is unable to assert that he or she is realist about a given hypothesis H, since in order to make this assertion, the realist will need to establish that H is predictively indispensable. At best, the realist can claim that he or she is not realist about a certain hypothesis H—supposing that H is not predictively indispensable, and so it’s only an ‘idle’ component. To say the least, it is unfortunate not to be able to assert one’s own position.

Furthermore, this generates a problem for the scientific realist. The reason why the realist needs an account of predictive indispensability is to avoid having to provide an account of success for ‘idle’ theoretical components; only the indispensable components in a theory need explanation. However, without being able to establish that a given hypothesis is indeed predictively indispensable—without being able to assert that a given hypothesis contributes essentially to the predictive success of a theory—the realist cannot claim to have provided an explanation of the success of science. For the realist cannot warrantly assert that these are the indispensable components responsible for the theory’s success—not, at least, with the account of predictive indispensability provided by Psillos.

It is unfortunate that Psillos only discusses Worrall’s epistemic version of structural realism (Worrall [1989]), neglecting Ladyman’s ontic version (Ladyman [1998]) to a two-line footnote (p. 309, note 5). For the ontic version is not open to Psillos’s charges against structural realism (see also French [1999] and French and Ladyman [2001]).

Psillos’s main complaint against structural realism is that it cannot explain the continuity of structure in scientific change without simply falling back into scientific realism. The idea is that, in isolation from a number of theoretical assumptions, a mathematical structure cannot explain anything about the world. So a commitment to structure is not enough to explain the retention of mathematical structures in theory change. According to Psillos, the same cannot be said about scientific realism. After all:

Scientific realists can explain the fact that mathematical equations have been retained in theory change by saying that they form an integral part of the well-supported and (approximately) true theoretical content of theories. But they would deny that all of what is retained is empirical content and (uninterpreted) mathematical equations. Not only is some theoretical content also retained, but scientists now have good reason to believe that the content of current theories—what they predicate of the world—is better supported by evidence, and, hence, more likely to be true (p. 147).

Moreover, Psillos continues:

The fundamental insight Worrall has, i.e. that the predictive success of a theory points to the theory’s being correct in some of its claims about the unobservable world, cannot be best served by a distinction along the lines of structure (or mathematical equations) versus nature (or theoretical content)... The best place to draw the relevant line is between essentially contributing theoretical components and ‘idle’ ones (p. 155).

It is difficult to see how Psillos can maintain that the scientific realist is able to explain the retention of mathematical structure in theory change. For, according to Psillos (see the second quotation above), to explain such structural preservation requires a distinction between “essentially contributing theoretical components and ‘idle’ ones”. But, as argued above, Psillos attempt to characterise the latter distinction fails. And so it’s not clear that the scientific realist is actually able to provide the intended explanation.

Of course, the distinction between essentially contributing theoretical components and ‘idle’ ones has no role in structural realism, and it is not a distinction that the structural realist is committed to. The explanation of the retention of mathematical structures in theory change is accomplished by a claim about the adequacy of such structures to represent the relevant structures of the world. This is one of the reasons that motivated the move to the ontic version of structural realism in the first place (see Ladyman [1998], French [1999], and French and Ladyman [2001]). In this respect,
Author’s Response

By Stathis Psillos

It’s a privilege to have one’s book discussed so thoroughly and carefully as the reviewers do with my book. So, I thank them all for their incisive and thought-provoking criticism. Lack of space doesn’t allow me to discuss their points in the detail they deserve. So, I’ll restrict my attention to two central issues that crop up in the reviews: the role of the ‘No Miracles Argument’ (NMA) in the defence of realism and the prospects for Structural Realism.

Lipton poses a strong challenge: show that NMA does some genuine extra work for realism over and above the work already done by first-order instances of Inference to the Best Explanation (IBE) that scientists use in order to form their theories about the unobservable world. Lipton and I both agree that circular vindications of inferential methods can be legitimate. His challenge is peculiar to an NMA-like vindication of IBE: unlike legitimate inductive assessments of inductive methods, an NMA-like assessment of the reliability of IBE introduces “no new evidence” for its reliability. Let me remind the reader that I take NMA to consist in two parts. The first part is that we should accept as relevant (approximately true) the theories that are implicated in the (best) explanation of the instrumental reliability of first-order scientific methodology. The second part is that since, typically, these theories are arrived at by IBE, IBE is reliable. Both parts are necessary for my version of NMA. Note that the second part has excess content over the first. The first part says nothing yet about the reliability of IBE. On the contrary, the second part issues a general statement about a mode of reasoning which, being contingent, implies that there must be a feature of the world that answers to IBE’s reliability. That the world is such that—as a contingent matter of fact—IBE tends to yield (approximately) true theories is a new general claim about the world which is not entailed by the (scientists’) first-order IBEs. In support of this, note that the general claim (that IBE is reliable), if true, would support relevant counterfactuals which no first-order claim could support.

But perhaps all this doesn’t meet Lipton’s worry that there is no new evidence for the second part of NMA—viz, that IBE is reliable—which is not already evidence for the “first-order scientific case for [the] truth [of theories]”. A lot seems to depend on how to understand the requirement of “new evidence”. But two things seem relevant here. First, the actual track-record of successful applications of IBE does offer genuine evidence for the reliability of IBE. In particular, successful novel predictions issued by first-order theories arrived at by IBE do lend extra credence to the claim that IBE is reliable. Second, the reliability of IBE offers new evidence (of a sort) for the truth of first-order scientific theories. Suppose theories $T_1, \ldots, T_n$ are accepted as true on the basis of the relevant evidence. Suppose also that one grants that the fact that these theories were arrived at by a method (viz., IBE) offers no new evidence for the claim that they are (probably) true which was not already there by considering the (first-order) evidence for them. Yet, it seems to offer some new evidence for the claim that a first-order theory $T_{n+1}$, which is arrived at by IBE (perhaps, a theory from a totally different domain), is (probably) true. The successes of $T_1, \ldots, T_n$ and the fact that they were arrived at by IBE, supports, via NMA, the view that IBE is reliable, and this works in addition to the first-order evidence for $T_{n+1}$ to make $T_{n+1}$ more credible. Generally, the fact that a theory has been arrived at by a reliable method will have extra probative force; it will add to our confidence in its (probable) truth, for its truth will also be supported (indirectly) by all the (first-order) evidence that has led scientists to accept the method as reliable.

There is one more reason why NMA has genuine excess content over and above first-order IBEs. The latter are diverse and disparate (e.g., they might admit the form of common-cause arguments, or of arguments from unification etc.). If NMA is correct, then it says something about the common deep inferential structure of the several instances of explanatory reasoning and suggests that a host of possibly disparate instances of this success-to-truth-via-explanatory-considerations mode is reliable.