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Over the last two decades, the debate over scientific realism has been dominated by two arguments that pull in contrary directions: the 'no miracle' argument and the 'pessimistic induction'. The latter suggests that the historical record destroys the realist's belief in an explanatory connection between truthlikeness and genuine empirical success. This paper analyzes the structure of the 'pessimistic induction', presents a move—the *divide et impera* move—that neutralizes it, and motivates a substantive yet realistic version of scientific realism. This move is also compared with Worrall's and Kitcher's recent reactions to the 'pessimistic induction'.

1. 'No Miracle' vs. 'Pessimistic Induction'. Over the last two decades, the debate over scientific realism has been dominated by two arguments that pull in contrary directions: the 'no miracle' argument and the 'pessimistic induction'.

According to the 'no miracle' argument, the predictive success of science would be difficult, if not impossible, to account for unless mature scientific theories were approximately true.[1] This line has been developed mostly by Boyd (cf. 1984) into a systematic defense of scientific realism. The strength of the 'no miracle' argument, though, rests on the following powerful intuition: there must be some kind of *explanatory connection* between novel predictive success and the theory's being roughly right about the world, specifically an explanatory connection which involves the claim that what the theory says about *unobservable aspects of the world* is roughly right. This is the intuition that has motivated various realists (Maxwell 1962, Smart 1963, Putnam 1975, Newton-Smith 1981, Worrall 1989), but not only them (most notably, Duhem 1906, 28–31). No matter how one tries to articulate this explanatory connection, the intuition behind the 'no miracle' argument cannot be easily made to go away.

The 'pessimistic induction', however, suggests that the 'no miracle' argument flies in the face of the history of science. Laudan's "historical gambit" (1984b, 157) consists of a list of past theories—which "could be extended *ad nauseam*"—that are characteristically false and yet once were viewed as empirically successful and fruitful (1981, 33). Then the history of science cannot possibly warrant the realist belief in an explanatory connection between truthlikeness and empirical success.

Can the 'pessimistic induction' be defeated? Can a substantive version of scientific realism be shown to be compatible with the historical record? This piece analyzes the structure of Laudan's argument, presents a move that neutralizes it, and argues that a substantive yet realistic version of scientific realism can still be defended.

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[1]For brevity, I use 'approximate truth' and 'truthlikeness' as virtually synonymous.

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2. Unpacking the 'Pessimistic Induction'. The 'pessimistic induction' is a kind of *reductio*. The target is the realist thesis that:

(A) Current successful theories are approximately true.

Laudan, however, does not directly deny that current successful theories may happen to be truthlike. His argument aims to discredit the claim that there is an explanatory connection between empirical success and truthlikeness which warrants the realist's assertion (A). In order to achieve this, the argument compares a number of past theories to current ones and claims:

(B) If current successful theories are truthlike, then past theories *cannot* be.

Past theories are deemed not to be truthlike because the entities they posited are no longer believed to exist and/or because the laws and mechanisms they postulated are not part of our current theoretical description of the world. Then, comes the 'historical gambit':

(C) These characteristically false theories were, nonetheless, empirically successful.

So, empirical success is not connected with truthlikeness and truthlikeness cannot explain success: the realist's potential warrant for (A) is defeated. As Laudan put it:

> Because [most past theories] have been based on what we now believe to be fundamentally mistaken theoretical models and structures, the realist cannot possibly hope to explain the empirical success such theories enjoyed in terms of the truthlikeness of their constituent theoretical claims. (1984a, 91–92; cf. also 1984b, 157)

How might a realist attempt to defeat this argument? The main move so far has aimed to reduce the size of Laudan's list. Realists suggest that only mature and genuinely successful theories are at issue (cf. Devitt 1984, 161–162; Boyd 1984). They are certainly right in this, especially vis-a-vis the notion of empirical success. For, as Worrall (1989, 1994) has stressed, any theoretical framework can be made to fit the phenomena—and hence to be 'successful'—by simply writing the right kind of empirical consequences into it. Hence, the relevant notion here should be understood in terms of a theory's yielding *novel* predictions. But then some theories, like the contact-action gravitational ether theories, the crystalline spheres theory and others, drop out of Laudan's list since none of them enjoyed any genuine success in this sense (cf. Worrall 1994, 335; McMullin 1987, 70). This move is meant to weaken premise (C) above: if we restrict the meta-inductive basis, it no longer warrants the conclusion that genuine success and truthlikeness are not connected.

Although it is correct that realists should not worry about all past theories that Laudan suggests, this move alone is not enough to defeat the 'pessimistic induction': it does not account for the fact that at least *some* past theories that pass all realist tests of maturity and success are still considered false. The relevant examples are the caloric theory of heat and the nineteenth-century optical ether theories. If these theories are false, despite their being both distinctively successful and mature, then the intended explanatory connection between empirical success and truthlikeness is still undermined. How can we defend this explanatory connection?

3. *Divide et Impera.* The crucial premise in Laudan's *reductio* is (B): if we hold current theories to be truthlike, then past theories are bound not to be truthlike

since they posited entities that are no longer believed to exist, and posited laws and theoretical mechanisms that have now been abandoned. Without this premise the pessimistic conclusion does not follow.

Can we defeat (B)? Here is a suggestion: it is enough to show that the success of past theories did not depend on what we now believe to be fundamentally flawed theoretical claims. Put positively, it is enough to show that the theoretical laws and mechanisms which generated the successes of past theories have been retained in our current scientific image. Let me call this the *divide et impera* move. It is based on the claim that when a theory is abandoned, its theoretical constituents, i.e. the theoretical mechanisms and laws it posited, should not be rejected *en bloc*. Some of those theoretical constituents are inconsistent with what we now accept, and therefore they have to be rejected. But not all are. Surely some of them, instead of having been abandoned, have been retained as essential constituents of subsequent theories. The *divide et impera* move suggests that if it turns out that the theoretical constituents that were responsible for the empirical success of otherwise abandoned theories are those that have been retained in our current scientific image, then a substantive version of scientific realism can still be defended.

This move dissociates genuine empirical success from characteristic falsity. Moreover, it paves the way for the 'right kind' of explanatory connection between success and truthlikeness. Laudan, realists should say, has taught us something important: on pain of being at odds with the historical record, the empirical success of a theory cannot issue an unqualified warrant for the truthlikeness of everything that the theory says. Insofar as old realists have defended this, they have been shown to be, to say the least, unrealistic. Yet, it would be equally implausible to claim that, despite its genuine success, everything that the theory says is wrong. The right assertion seems to be that the genuine empirical success of a theory does make it reasonable to believe that the theory has *truthlike constituent theoretical claims*.

Moreover, if the theoretical constituents that were responsible for the empirical success of past theories have been retained in subsequent theories, then this gives us more reason to be optimistic about their truthlikeness: that all these theoretical constituents have been shown to be invariant and stable elements of our modern scientific image; they have survived several revolutions and have contributed to the empirical success of science. This is not a conclusive argument for their truthlikeness. Yet, realists can follow Kitcher's (1993) lead and suggest that the best way to defend realism is to use the generation of stable and invariant elements in our evolving scientific image to support the view that these elements represent our best bet for what theoretical mechanisms and laws exist.

This preamble for the *divide et impera* move may resonate with two recent reactions to the 'pessimistic induction', those of Kitcher (1993) and of Worrall (1989; 1994). Both of them have defended an analogous view, viz. that realists should characterize what kinds of statements are abandoned as false and what are retained. Kitcher suggests a distinction between "presuppositional posits" and "working posits," while Worrall draws the line between the "content" of a theoretical statement, which gets superseded, and its "structure," which is retained. My position is akin to Kitcher's, although some differences will be discussed shortly. However, the *divide et impera* move is not meant to reflect or capture Worrall's structure/content distinction. Worrall's approach, although the first brave attempt to reconcile the 'no miracle' argument with the historical record, faces some rather serious problems. His 'structural realism' asserts that there is continuity in theory-change, but such continuity is restricted to the mathematical/structural claims of scientific theories while assertions about the nature of theoretical entities and mech-

anisms change radically. Discussing the transition from Fresnel to Maxwell, Worrall suggests that "there was a continuity or accumulation in the shift, but the continuity is one of *form* or *structure*, not of content" (1989, 117). Furthermore, the structural realist "insists that it is a mistake to think that we can ever 'understand' the nature of the basic furniture of the universe" (1989, 122). Laudan has actually anticipated that one "might be content with capturing only the formal mathematical relations" of the superseded theory within its successor (1981, 40). But he rightly dismissed this view as a viable *realist* answer since it amounts to the response of " 'closet' positivists." As I have shown in detail in my 1995, Worrall's position relies on an unwarranted sharp distinction between the mathematical structure of a scientific assertion and the nature of the entity or process it describes. However, in modern science structure and nature form a continuum: the nature of an entity or mechanism is given via a structural/mathematical account of its properties and relations. At any rate, in order to make his position realist, Worrall needs to show that mathematical equations that feature in successful theories represent real relations in the world which are knowable independently of their *relata.* Specifically, he needs to justify the move from the fact that some mathematical equations are retained in theory-change to the claim that they describe real relations between physical objects otherwise unknown. Worrall (1989, 1994) does not offer such an argument. But if he appeals to anything connected with the predictive success of scientific theories (e.g., a suitable version of the 'no miracle' argument), then, in order to make a case for structural realism, he first needs to show that the mathematical structure of a theory is somehow exclusively responsible for its predictive success. That, however, is not true: mathematical equations alone—devoid of their physical content—cannot give rise to any predictions. If one admits that there is substantive (not just formal) retention in theory-change, then one should also admit that some physical content 'carries over' to the successor theory. But such an admission undercuts the claim that the predictive success vindicates only the mathematical structure of a theory (cf. Psillos 1995, 27–31). In sum, the *divide et impera* move does not coincide with Worrall's structure/content distinction.

How should realists circumscribe the truthlike constituents of past genuinely successful theories? We should first emphasize that we should really focus on the *specific* successes of certain theories, like the prediction of Fresnel's theory of diffraction that if an opaque disk intercepts the rays emitted by a light source, a bright spot will appear at the centre of its shadow; or Laplace's prediction of the law of propagation of sound in air by means of the hypothesis that sound's propagation is an adiabatic process. Then we should ask the question: how were these successes brought about? In particular, which theoretical constituents essentially contributed to them? It is not, generally, the case that *no* theoretical constituents contribute to a theory's successes. Similarly, it is not, generally, the case that *all* theoretical constituents contribute (or, contribute equally) to the empirical successes of a theory. (What, for instance, was the relevant contribution of Newton's claim that the center of mass of the universe is at absolute rest?) Theoretical constituents that essentially contribute to successes are those that have an indispensable role in their generation. They are those which "really fuel the derivation"—to use one of Laudan and Leplin's recent expressions (1991, 462). When does a theoretical constituent H indispensably contribute to the generation of, say, a successful prediction? 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. Clearly, there are senses in which all theoretical assertions are

eliminable, if for instance, we take the Craig-transform of a theory, or if we 'cook up' a hypothesis H* by writing P into it. But if we impose some natural epistemic constraints on the potential replacement, if, for instance, we require that the replacement be independently motivated, non ad hoc, potentially explanatory etc., then it is not at all certain that a suitable replacement can always be found.[2] Worrall has recently noted that whenever a theory is replaced by another, "the replacing theory alone offers a constructive proof of the 'eliminability' of the earlier one" (1994, 339). Clearly, the old theory as a whole gets eliminated. Yet, Worrall's observation does not establish the eliminability of the specific theoretical constituents that contributed to the empirical successes of the superseded theory. If the *divide et impera* move is correct, then these constituents are typically those that 'carry over' to the successor theory (admittedly, sometimes, only as limiting cases of the relevant constituents of the replacing theory).

So, when it comes to explaining the specific successes of a theory by means of the claim that the theory has truthlike constituent theoretical assertions, realists should argue that the truthlike constituents are (more likely to be) those that essentially contribute to, or "fuel," these successes. Realists need only care about those constituents which contribute to successes and which can, therefore, be used to account for these successes, or their lack thereof. Analogously, the theoretical constituents that realists need not commit themselves to are precisely those that are 'idle' components, impotent to make any difference to the theory's stake for empirical success.

What is required to perform successfully the *divide et impera* move? The key to this question lies in the careful study of the structure and content of past genuinely successful theories. What is needed are careful case-studies that will attempt to

(i) identify the theoretical constituents of past genuine successful theories that essentially contributed to their successes; and
(ii) show that these constituents, far from being characteristically false, have been retained in subsequent theories of the same domain.

Clearly, if all kinds of claims that are inconsistent with what we now accept were essentially employed in the derivation of novel predictions and in the well-founded explanations of phenomena, then one cannot possibly appeal to their truthlikeness in order to explain empirical success. Then, Laudan wins. However, if it turns out that the theoretical constituents that are essentially employed are those that have 'carried over' to subsequent theories, then the 'pessimistic induction' gets blocked. Settling this issue requires detailed study of some past theories that qualify as genuinely successful. The good news for realism is that relevant studies of the several stages of the caloric theory of heat and the nineteenth century optical ether theories suggest that both of the foregoing questions admit of positive answers (cf. Psillos 1994, 1995). It turns out, for instance, that Carnot's explanation of the fact that maximum work is produced in a Carnot-cycle employed only the principle of the impossibility of perpetual motion and not the assumption that heat is a material substance (cf. Psillos 1994, 173–178). Hence, scientific realism can still be defended.

Lack of space allows me only to refer the reader to these case-studies. However, as regards the *general* argument of this paper, the details of these studies—illuminating though they may be—are not necessary. The argument so far has aimed: (a) to show that if realists successfully perform tasks (i) and (ii) above, then a case

[2]Laudan and Leplin (1991, 462–463) have defended a similar view vis-à-vis the argument from underdetermination.

can be made for scientific realism; and (b) to indicate how these tasks can be performed, in particular, what role the suggested case-studies are called to play, what issues they should focus on and how they are relevant to settling the argument between scientific realism and the 'pessimistic induction'.

Isn't the *divide et impera* move too close to Kitcher's approach? Couldn't one simply identify the idle constituents of a theory with Kitcher's "presuppositional posits" and the essentially contributing constituents with his "working posits"? These identifications may be pertinent. However, there are differences. My distinction between idle and essentially contributing constituents is meant to capture how the successes of a theory can differentially support its several theoretical constituents. Kitcher's distinction between presuppositional and working posits, however, is meant to capture the difference between referring and non-referring terms. Working posits are said to be "the putative referents of terms that occur in problem-solving schemata," while presuppositional posits are "those entities that apparently have to exist if the instances of the schemata are to be true" (1993, 149). But so put, the distinction is problematic. For, in effect, we are told that the success of a problem-solving schema does support the existence of the referents of some of the terms featuring in it, but it does not support the existence of a putative entity whose presence is required for the truth of the whole schema. But unless one shows in virtue of what success can support some existence claims but not others, all this is grist to Laudan's mill. Kitcher suggests that the putative referents of presuppositional posits, such as the ether, were only *apparently* presupposed for the truth of the relevant schemata; in fact, they turned out to be eliminable without derivational loss (1993, 145). But this suggestion is retroactive and open to the charge that it is ad hoc: the eliminable posits are those that get abandoned. Yet, as we'll see now, the *divide et impera* move can improve on Kitcher's views by avoiding this charge.

4. *'Tout bien ou rien'* Realism? The non-realist may object that, with hindsight, we can rather easily work it out so that the theoretical constituents that 'contributed' to the successes of past theories turn out to be those which were, as it happens, retained in subsequent theories. She may suggest that realists are bound to first identify the past constituents that have been retained and then proclaim that it was those (and only those) that contributed to the empirical success and which enjoyed evidential support. Can realists do better than that? Retention aside, can we independently identify the theoretical constituents that contribute to the successes of a given theory and suggest that it is only those that we can deem truthlike?

In response to this objection, we should point out that eminent scientists do this all the time. It is not that realists come, as it were, from the future to identify the theoretical constituents of past theories that were responsible for their successes. Scientists themselves tend to identify the constituents that they think responsible for the successes of their theories and this is reflected in their attitude towards their own theories. This attitude is not an all-or-nothing affair (cf. Psillos 1994). Scientists do not, normally, believe either that all that a successful theory says is truthlike or that, despite its success, nothing it says is truthlike. Rather, the likes of Lavoisier, Laplace and Carnot—to mention just a few—had a *differentiated attitude* towards their theories (in this case the caloric theory), in that they believed in the truthlikeness of some theoretical claims while they also thought that some others were too speculative, or too little supported by the evidence, to be accepted as truthlike. This differentiated attitude was guided by the manner in which the several constituents of the theory were employed in the derivation of predictions (e.g., Laplace's prediction of the correct law of the propagation of sound in air)

and in well-founded explanations of phenomena (e.g., Carnot's explanation of the fact that maximum work is produced in a Carnot-cycle). So, theoretical claims that were not essential for the successes of the theory were treated with suspicion, as for instance was the case with the assumption that heat is a material fluid. Whereas those claims that "fueled" the successes of the theory were taken to enjoy evidential support and were believed to be truthlike, as for instance was the case with the claim that heat can remain in latent form, or with the claim that the propagation of sound in air is an adiabatic—rather than an isothermal—process.

My view is that it is precisely those theoretical constituents that scientists themselves believed to contribute to the successes of their theories (and hence to be supported by the evidence) that tend to get retained in theory-change. Whereas, the constituents that do not 'carry-over' tend to be those that scientists themselves considered too speculative and unsupported to be taken seriously. If this view is right, (and, based on the details of my (1994, 1995), I think it's likely to be), then not only is the *divide et impera* move not ad hoc, but rather it gains independent plausibility from the way scientists treat their theories and the way they differentiate their commitments to their several constituent theoretical claims. If, therefore, there is a lesson that scientists should teach realists is that an all-or-nothing realism is not worth fighting for.

Another potential challenge to my argument concerns the issue of reference of abandoned theoretical terms. As Laudan has stressed, a theory cannot be truthlike unless its central terms refer (1981, 33), and, clearly, terms such as 'caloric' and 'luminiferous ether', although central in the relevant theories, do not refer.

Realists need to tread carefully here. Some tend to adopt variants of the Kripke-Putnam causal account of reference in order to argue that some abandoned theoretical terms *do* refer (cf. Hardin and Rosenberg 1982). Yet these accounts face well-known problems (cf. Laudan 1984b, Worrall 1994). Realists ought to concede that they need a good theory of reference. But before such a theory becomes available, is it possible to undercut the force of the objection at hand? There are, in fact, two rejoinders available.

First, realists should argue that not all cases of abandoned terms are troublesome. The serious cases regard terms that were indeed central in some genuinely successful theory; *central* in the sense that the advocates of the theory took the successes of the theory to warrant the claim that there are natural kinds denoted by these terms. It is only about such terms that the issue of preservation of reference is pressing. If such terms turn out to be vacuous, then there seems to be no connection between empirical success and successful reference. But not all abandoned terms were central. For instance, most of the scientists (Lavoisier, Laplace, Black, Carnot) who worked with the 'caloric' theory were very keen to suggest that there is not enough evidence to warrant the claim that the cause of rise and fall of temperature is a material substance (caloric). On the contrary, they tried to derive the laws that govern heat phenomena independently of this assumption.[3] Lavoisier and Laplace, for example, stressed: "the conservation of free heat in simple mixtures of bodies is, then, independent of those hypotheses about the nature of heat [i.e., material vs. mechanical accounts]; this is generally admitted by the physicists, and we shall adopt it in the following researches" (1780, 153; my translation). So, if a given abandoned term was not central in the above sense, realists should not be required to render it referential.

Second, realists can motivate a notion of *approximate reference.* Although they

[3]For all the relevant references and a detailed historical substantiation of these claims cf. Psillos 1994.

should concede that most abandoned terms do not refer to anything we now posit, there seems to be a sense in which some of them may approximately refer to current posits. What is this sense? It is not just that some current posit has taken up the place of an abandoned posit as the hypothetical cause of a set of phenomena. The current posit is ascribed some (but surely not all) of the *attributes* ascribed to the abandoned putative entity, attributes in virtue of which it was thought to produce its effects. Hence, although there is nothing in the world that possesses all the attributes ascribed to an abandoned posit α, there may well be a current posit β which is ascribed some (sometimes most) of the attributes ascribed to α and is also considered to be causally responsible for the same phenomena as α. If this situation occurs, (and it's at least arguable that it has occurred in the transition from luminiferous ether to the electromagnetic field), then we may be willing to say that the term intended to refer to the abandoned posit α approximately refers to the current posit β. Surely, these thoughts need careful and detailed elaboration. Yet, I hope, they are sufficiently clear to motivate and warrant the advancement of such an account.

5. A Happy Ending? Is this the end of the troubles of scientific realism? Certainly not. If successful, I have only motivated a substantive version of scientific realism that is not defeated by the 'pessimistic induction': it survives because it has learned to adapt, that is, to be more realistic in its aspirations and its commitments. It sustains an explanatory connection between genuine success and truthlikeness, but points out that claims of truthlikeness extend only to the theoretical constituents that essentially contribute to the successes of theories. The issue around realism has always been whether it is rational to believe in theoretical claims, claims that extend into the realm of the unobservable. Insofar as the argument of this paper blocks the 'pessimistic induction', it defends a substantive—if only more cautious—version of scientific realism.

Scientific realism should still be defended against other challenges: the charge that the use of inference to the best explanation in support of realism is viciously circular and the argument from underdetermination (but cf. Psillos 1996). It also still needs adequate semantics for approximate truth and reference. It's good to know, though, that there is still room for optimism—and without miracles.

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