# The Prospective Stance in Realism

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Scientific realists endeavor to secure inferences from empirical success to approximate truth by arguing that, despite the demise of empirically successful theories, the parts of those theories responsible for their success do, in fact, survive theory change. If, as some antirealists have recently suggested, successful theory parts are only identifiable in retrospect, namely, as those that have survived, then the realist approach is trivialized, for now success and survival are guaranteed to coincide. The primary aim of this article is to counter this argument by identifying successful theory parts independently from their survival.

**1. Introduction.** Scientific realists attempt to establish the approximate truth of scientific theories, or at least their progress toward truth, on the basis of inferences from empirical success. This approach has been questioned by historically motivated arguments that aim to impair the link between success and truth. They do so by simply pointing out that the history of science is littered with once-successful theories that were ultimately abandoned as false in the wake of scientific revolutions. In turn, the realists have endeavored to secure the integrity of the success-to-truth link by arguing that those parts of a theory that are responsible for its success do, in fact, survive theory change. This tactic has given rise to an ominous counterargument. If those parts of theories that are responsible for its success are only identifiable in retrospect, namely, as those that have survived, then the realist approach is trivialized, for success and survival are now guaranteed to coincide. The aim of this article is to

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counter this antirealist argument from retrospection by isolating a specific class of theory parts whose credentials for empirical success can be demonstrated independently of their survival.

**2. Scientific Realism of Late.** Suppose that theories are capable of describing certain features of the world correctly, incorrectly, or partially correctly. How do we go about finding out whether a particular theory's descriptions are at least partially correct? Or, to use the language spoken by those engaged in the scientific realism debate, how do we go about finding out whether a theory is true or approximately true? More carefully, since approximate truth may be a tall order, how do we ascertain whether a theory possesses at least some truth content? Would we not have reason to believe in a theory's (partial) truth if what it says about specific features of the world accords well with our independent measurements and observations of those features? And, conversely, would we not have reason to doubt that theory's richness in truth if its prescriptions were largely in disagreement with our measurements and observations? To sum up then, it seems that it is the successful negotiation of what can be independently checked via measurement and observation that secures our belief in a theory's truth content. This close association between what we might call 'the empirical success' of a theory, on the one hand, and its truth content, on the other, is the pillar upon which the whole edifice of modern scientific realism is erected.

Those who wish to see the scientific realist edifice in ruins understandably attempt to shatter this pillar. From the 1960s onward, some of the most prominent attempts have been historically motivated. Kuhn, Feyerabend, Laudan, and various others have gleefully indicated that the history of science is a scrap yard of empirically successful theories that are now deemed false. At around the same time, Goodman, Quine, van Fraassen, and various others have championed another type of attempts against the realist pillar, those arising in relation to arguments from the underdetermination of theory by evidence. Arguments of this latter sort seek to demonstrate the in-principle existence of rival theories that are as empirically successful as the very theories we might hold at any point in history. Indeed, historical arguments can profitably be thought of as special cases and perhaps even constructive demonstrations of the more general underdetermination arguments. The desired upshot is, of course, the same for advocates of both the historical and the underdetermination arguments, namely, to weaken the link between empirical success and truth content.

It should come as no great surprise that inferences from empirical success-to-truth content are deductively invalid. Otherwise put, empirical success, at least in its unqualified form thus far presented, is not sufficient to establish a theory's truth content.[1] The real question is whether inferences from success-to-truth content are reliable. The antirealists claim they are not, pointing to the scrap yard where one-time imperious theories lie rusting. In response, the realists claim that the reliability of success-to-truth inferences can be upheld once we realize that scrap yards contain an abundance of parts that before long are employed to outfit new theories. If the empirical success enjoyed by a theory $T$ is only generated by certain parts, and those parts get 'recycled' into $T$'s successor(s), then $T$'s demise bears no ill effects on the inferential link between empirical success and truth content. In more familiar terms, the realist claims that only parts that are inessential or idle for a theory's success get left behind in the wake of a scientific revolution. Their loss is thus inconsequential to the realist. On the contrary, the essential parts survive. It is these parts that grant their parent theory the designation 'contains some truth', and it is with their help that it can now be claimed that a successor theory or theories is/are closer to the truth. As a result, scientific realists of late scour the history of science for those essential parts that have, indeed, survived into successor theories.

Scientific realism's soft underbelly is found in that delicate matter of theoretical term reference. Many scientific realists take the successful reference of a theory's central theoretical terms to be a necessary condition for that theory's (approximate) truth. Yet, as scores of antirealists have been keen to point out, entity after entity to which central theoretical terms of past theories purportedly refer has been discarded over the course of history—think of the phlogiston, the caloric, and the ether. The scientific realists thus find themselves in a bind, for theories whose central theoretical terms turn out not to refer cannot be (approximately) true regardless of how much empirical success they enjoy. The link between success and truth is once again undermined.

Hardin and Rosenberg (1982) were among the first realists to cut the umbilical cord that stretched between (approximate) truth and referential success, which was sometimes strangling instead of sustaining them. In recent years, Psillos (1999) has performed a more delicate incision, severing only the link between (approximate) truth and the referential success of those terms that are not central. That is, he has argued that no instances of abandoned posits within successful and presumably approximately true theories should be alarming to the realist, for the simple reason that the theoretical terms corresponding to those posits were not central to their respective theories. A term $t$ is central in Psillos's account so long as it fulfills three conditions relativized to the historical period during which

1. Empiricist antirealists tend not to question the necessity of empirical success for establishing a theory's truth content.

the theory under consideration was reigning: (i) it is part of a genuinely successful theory *T*, (ii) the descriptions associated with it are indispensable in predicting and explaining phenomena in the domain of *T*, and (iii) *T*'s supporters trust that it denotes a natural kind (Psillos 1999, 129). Notice that condition (ii) restricts the permissible theories of reference compatible with this view to those that incorporate a descriptivist component. To be sure, Psillos regards pure causal accounts of reference as too liberal, since they allow a term to refer even if none of the descriptions associated with that term is true. In their place he adopts a causal-descriptivist theory: "A term t refers to an entity x if and only if x satisfies the core causal description associated with t" (296).[2] By 'core causal description', Psillos means those descriptions that would allow the corresponding posit to play the causal role expected of it. These descriptions, take note, need to be true for *t* to refer.

The realist needs an unambiguous method through which to identify those terms of past successful theories that are genuinely referential. Psillos's solution to this predicament is to demand from theoretical terms in successor theories to incorporate the core causal description associated with the corresponding terms in their predecessors. As an example of such referential stability, he cites the ether. Echoing Hardin and Rosenberg, he claims that nineteenth-century users of the term 'ether' successfully referred to what we would nowadays call the electromagnetic field. Alas for the realists, judgments about posits are not always univocal. Kitcher (1993), for example, denounces the ether as a mere presuppositional posit whose downfall is no ground for pessimism. As for the phlogiston and caloric posits, most scientific realists nowadays, Psillos and Kitcher included, take them to be nonreferential, though even here dissenting voices are not absent (e.g., Schurz 2009).

To summarize the present state of affairs, the debate over scientific realism is largely conducted at the historical level. That is, participants in this debate test the success or failure of their preferred version of realism or antirealism against the historical record of science. At this testing ground the scientific realist has recently come face-to-face with a new threat. It is to this threat that we now turn.

**3. The Poverty of Retrospection.** One of the heftiest accusations directed at realists in recent years is that they can only identify those parts of theories that are (approximately) true in an ad hoc or post hoc manner. Stanford (2003, 2006) propounds this accusation in his discussion and dismissal of realist solutions to historical challenges. In discussing Psillos's approach, for example, he accuses him of inviting "the realist to choose

2. For a critique of Psillos's account, see Votsis (2011a).

the core causal descriptions she associates with the central terms of past theories rather carefully, with one eye on current theories' claims about nature, so there is more than a whiff of ad hoc-ery about the proposal" (Stanford 2003, 559). Perhaps a more apt characterization of the accusation is post hoc-ery, as the (approximately) true parts of theories can presumably be singled out only in retrospect. Indeed, Stanford takes this accusation a step further and argues that not only (approximately) true parts but also empirically successful parts of theories can only be identified post hoc. In his own words, "the appeal to selective confirmation [i.e., the view that only certain parts of theories are empirically successful and, hence, only those parts deserve to be confirmed] will have to provide what we might call 'prospectively applicable' criteria of selective confirmation; that is, criteria that could have been applied to past theories at the time and can now be applied to our own theories in advance of any future developments to say just which parts of past theories were (and just which parts of present theories are) genuinely confirmed by the successes they enjoy" (568).

Stanford's accusation hides an argument, which we may call 'the argument from retrospection'. The argument can be reconstructed as follows: Any successful defense of realism with historical evidence requires the ability to tell which parts of theories are empirically successful and, hence, are likely to survive theory change in advance of the actual theory change. This ability is wanting. Therefore, realism cannot be defended on historical grounds.[3] Thus, even on the supposition that inferences from empirical success to (approximate) truth are generally reliable, the scientific realist is thwarted from drawing such inferences, for she cannot determine prior to their survival which parts of theories are empirically successful.

Take the ether case again. Stanford argues convincingly that leading scientists in the nineteenth century included those parts that Psillos wants to exclude from the core causal description of the ether, that is, those parts that we now consider false. Maxwell famously wedded himself to the material and mechanical nature of the ether, deeming that light and other electromagnetic radiation could not propagate without an ether so described. This is a particularly damning verdict on Psillos, precisely because he reserves an important role for the judgments scientists made about which theory parts were essential or empirically successful. Stanford concludes by saying that "the carefully considered judgments of leading scientific defenders of the theory [of electromagnetism] concerning which

3. I am in partial agreement with this conclusion. Realism cannot be defended on purely historical grounds. The best grounds for realism can be found in the ability of theories or theory parts to generate confirmed predictions in a non–ad hoc manner. For more on this, see Votsis (2011b).

of the descriptions associated with its central terms must be satisfied by an entity in order for it to play the causal role associated with the term (i.e., which features figure in the actual core causal description) have proved to be unreliable. What this suggests, of course, is that we cannot rely on our *own* judgments about which of the descriptions we associate with our own terms are *genuinely* part of their own core causal descriptions" (Stanford 2003, 561). If, in other words, scientists are unable to make correct judgments about which parts of theories are empirically successful at the time of those theories' reign, then the realists' judgments can surely do no better.

Stanford is quite resolute about the poor prospects realists have in drawing their support from the history of science. And he is not the only one. That survival through theory change cannot lend a helping hand to the realist is a point also raised by Chang (2003). To be exact, Chang argues that "preservation is far from a sufficient condition for realist acceptance" (912). That is, a theory part's survival through theory change is not a sufficient condition for that part's (approximate) truth. Although Chang offers no example of a theory part that at first survived theory change but eventually was judged false, he presents some general reasons against the sufficiency condition. I will return to these reasons in section 7. For now, I merely wish to highlight Chang's conclusion that 'preservative realism'—that is, realism that draws its support from the preservation or, as we have been calling it, 'survival' of theory parts—is a hopeless endeavor.

**4. The Prospective Stance.** The realists ought to own up to the fact that they are largely responsible for the current state of affairs. The first slipup was to let the judgments that scientists made about which theory parts were essential get in the way. Any attempt to reconstruct the epistemic attitudes of past scientists often relies on speculating about the extent to which each of them was committed to a given posit. But even in those cases, if any, where the epistemic attitudes of past scientists were unequivocal, there is no good reason to suppose that the scientists concerned could uncannily and at will divorce themselves from the deep theoretical claims of the posit they endorsed—claims that we now believe to be unfounded. That is to say, a scientific community may not always be immediately aware of which theory parts play an indispensable role in yielding the empirical success enjoyed by a specific theory. Equally, a scientific community may not always immediately recognize a theory part's dispensability.

The second slipup was to rely excessively on what the survival of theory parts can accomplish. To profess that survival is the main route to (approximate) truth is an evident case of putting the cart before the horse. Theory parts are neither (approximately) true nor empirically successful because they survive. Rather, they are likely to be (partially) true and are likely to survive because they are empirically successful. This last assertion is the original realist strategy, but it somehow got lost on some realists in the rush to tackle the historical challenges.

The insistence that parts are (partially) true because they are empirically successful takes care of the accusation that the (partial) truth of theory parts is only discernible in retrospect. It does not, however, tackle the more serious accusation that the empirical success of theory parts is likewise indiscernible. What can the realist say on this matter? The classical picture of confirmation given in section 2 provides some clues as to how to answer this question. Epistemic warrant is earned through the ability to generate confirmed predictions in a non–ad hoc manner. The only parts of theories that are unfailingly involved in the generation of confirmed predictions are its equations or, more broadly conceived, those parts of theories that are explicitly or implicitly mathematized. Let us call such parts 'mathematical'. Not all mathematical parts need be predictively successful, but predictive success, according to the view presented here, requires mathematical parts. Naturally, for the mathematical parts of theories to generate predictions, they need to be interpreted. The question then becomes how much interpretation is required to generate the confirmed predictions. The specific class of theory parts that I promised early on in the introduction can finally be unveiled, and it consists of minimally interpreted mathematical parts.[4]

Readers familiar with the realism debate will have already noticed that the emphasis on minimally interpreted mathematical parts is very much in harmony with structural realism. I have argued for this view elsewhere and continue to believe that it is the most compelling form of realism on offer. Having said this, I will not press the case for structural realism here. Suffice it to say that the minimal interpretation approach to defusing the poverty of retrospection threat is, at the very least, congenial to structural realism.

Lest there be any doubters, observe that the minimal interpretation of mathematical parts is determinable in advance of any theory change. That this is so is demonstrated by the fact that the relationship between the minimally interpreted mathematical parts and the relevant confirmed predictions is a purely inferential matter. Take a confirmed prediction $E$. The minimal interpretation of a mathematical part, let us say an equation $M$, is given by the bare minimum that needs to be assumed in order for there to be an appropriate inferential relation from $M$ to $E$. The appropriateness of an inferential relation is a matter of great urgency in confirmation theory, but it is not one that is unique to the issue of minimal interpretation and certainly not one that can be settled here. For the purposes of this

4. The notion of 'minimal interpretation' is adapted from Chakravartty (2007, 53).

article, it is enough to say that, in the ideal case, the inferential relation is deductive, but there are surely appropriate inferential relations that are probabilistic in nature.

The minimal interpretation approach allows us to occupy a prospective stance, according to which we should be able to tell in advance and with some degree of accuracy which parts of theories yield their empirical success. Crucially, realists can stick their neck out in this way and make predictions about the future course of science. We may offer a sketch of such a prediction using quantum mechanics (QM) as an example. Although the task of figuring out exactly how much interpretation is required to maintain QM's empirical success is monumental and beyond the remit of this article, we can at least begin the process of stripping it down to a bare minimum interpretation. And there is nothing surprising about what our first steps should be. The 'metaphysical' interpretations (Bohmian, many worlds, etc.) of QM's mathematical formalism that philosophers and physicists have been tussling over are expendable, for at present they contribute nothing to the empirical success of the theory itself. To the extent that the 'metaphysical' interpretations make divergent predictions, for example, about whether wave function collapse occurs, these predictions have not been confirmed and in some cases may even be incapable of confirmation. So long as we lack the deciding evidence, we may thus justifiably dispose of such interpretations. The same cannot be said of the more 'practical' interpretations of QM. Take the Schrödinger equation. It is arguably one of the most successful equations in modern physics. Among other things, it helps predict the highly unexpected phenomenon of quantum tunneling. This phenomenon is at the center of numerous applications including the ubiquitous flash memory. For the equation to generate confirmed predictions, empirical grounding must be given to its terms. In the case of the time-dependent version of the equation, that means the energy operator, the Hamiltonian operator, and the wave function. An initial rough-and-ready realist prediction, then, is as follows: whatever the successor to QM will be, if it is to be more empirically successful, it will have to incorporate a 'bare-bones' Schrödinger equation (at least in some limit form), where the 'practical' interpretations of its terms retain at least a substantial part of their meaning and where, unless new evidence is advanced, the 'metaphysical' interpretations are cast off. More specific predictions can and should be put forth by the realists. Even so, it is worth stressing that there is nothing trivial about the foregoing prediction, for there is no a priori guarantee that the successor theory to QM will satisfy any of the cited constraints.

**5. Another Look at Retrospection.** In this short section, I would like to argue that retrospectively identifying theory parts is not always a cause for concern. The case of the ether is once again illuminating. The ether was conceived of as a central posit in, among other theories, Fresnel's wave theory of light, a theory that enjoyed substantial empirical success. As we now know, the success accorded to Fresnel's theory came not from any deep metaphysical assumptions about the ether but merely from the application of a set of equations that describe the reflection and refraction of light at an interface, that is, where two media with different refractive indexes meet. The minimal interpretation of these equations requires no presupposition about a mechanical medium. No wonder, then, that realists counsel realism toward Fresnel's equations but not the ether and that the equations but not the ether survived theory change and are, for example, derivable from the mature theory of electromagnetism.

Fresnel and his contemporaries seem to have been unaware that the ether was idle. Arguably then, its idleness was only worked out in retrospect. Be that as it may, the fact remains that a mechanical medium, as specified by Fresnel and others, is redundant in the calculations that yield the numerous independently confirmed predictions. Whether we realize this before, during, or after the pertinent theory change is immaterial to the existence of the right sort of relations between Fresnel's equations (minimally interpreted) and its various confirmed predictions. Such relations, as argued in the previous section, are not historical but inferential. So long as they are present, it does not matter whether we retrospectively identify which theory parts truly carry the weight of the evidence.

A final remark worth making is that retrospection can also serve as a valuable guide to prospection. Any theory that supersedes quantum electrodynamics will have to preserve (at least in some limit) the following minimally interpreted equations: (i) Fresnel's equations, (ii) Maxwell's equations, and (iii) the equations of quantum electrodynamics. Notice that even though this is prospection by way of retrospection, realists are still sticking their necks out, making bold predictions about the future course of science.

**6. A Dispute with the Constructive Empiricist.** That parts survive because they are empirically successful is something that not only realists but apparently also antirealist empiricists can explain. Indeed, van Fraassen (2006) claims that the constructive empiricist can give a better explanation of how and why earlier theories were successful. Instead of the realist explanation that requires the surviving parts of superseded theories to have latched on to the unobservable world, his own explanation shifts the focus toward new theories, requiring from them that they imply "approximately the same predictions for the circumstances in which the older theories were confirmed and found adequately applicable" (van Fraassen 2006, 298). The explanation, notes van Fraassen, not only doubles up as a criterion for theory acceptance—a new theory must be able to make approximately the same confirmed predictions as the one it supersedes—but also satisfies the 'no miracle' intuition. Success is no longer miraculous because new theories are built to save the phenomena. If a past theory has saved some domain of phenomena, there will be pressure on the next theory to save even more phenomena in order to strictly extend the success of its predecessor. In this respect, it is not enough for the realist to argue that new theories are edging nearer to the truth because they strictly extend the success of their predecessors.

Appearances to the contrary, the constructive empiricist explanation is not on par with the realist one. Van Fraassen can only (nominally) explain why empirically successful parts survive. Moreover, he neglects the more vital question of what is required to attain success to begin with.[5] Let us abuse the Fresnel case one more time. For Fresnel's equations to generate the kinds of predictions they are famed for, their terms must be given a minimal interpretation. Among these terms, there are at least some whose minimal interpretation involves theoretical descriptions. Take the term denoting the refractive indexes of different media. These indexes qualify as unobservable properties in van Fraassen's account, for their exact specification requires measurements of the velocity of light in the relevant media that go far beyond anything we could ever detect with our unaided senses.[6] Given that the overall success of Fresnel's equations depends on the postulation of these unobservable properties, this gives realists the upper hand. Realists can now provide a prima facie plausible explanation why Fresnel's equations are successful (and are preserved) by pointing out that at least some of the entities, properties, and relations posited, namely, those on which the success of the theory depends, have latched onto the structure of the unobservable world.[7]

**7. Conservativeness or Truth?** Let us return to Chang's general reasons for rejecting the sufficiency of survival for (approximate) truth. He identifies three separate reasons why theory parts may survive a scientific revolution: (*a*) "nature continually speaks in favor of them," by which he means that they are (approximately) true, (*b*) "our own cognitive limi-

5. Van Fraassen (2006, 299 n. 29) claims that the question "*what explains the empirical success of science ueberhaupt*?" is "to my mind, a question that does not arise at all." Alas, he offers no justification for this claim.

6. To be precise, the relative refractive index of two media is given by the ratio of the velocity of light in the one divided by the velocity of light in the other.

7. Empiricists (including van Fraassen) are always keen to point out that for them there is no demand to explain the success of science. Even so, van Fraassen attempts to illustrate that empiricists have a better explanation than the realists. My arguments counter this last view, but they are not meant to convince an empiricist that explaining the success of science is a demand they should take seriously.

tations confine us to them"—what he later brands 'human limitations'—and (*c*) "we just want to keep them"—what he later brands 'conservatism' (Chang 2003, 911–12). Unless we can dismiss *b* and *c*, Chang argues, we cannot reasonably plumb for *a*.

Are *b* and *c* either independently or jointly more likely than *a* to be the reasons responsible for the survival of most theory parts? In my view, *b* and *c* are two sides of the same coin: the human limitations (cognitive or otherwise) referred to in *b* can be understood as a kind of intrinsic conservativeness that is beyond our control, whereas the kind of conservativeness found in *c* seems extrinsic and at least in principle a matter of choice. Our conservative attitude, whether it originates in *b* and *c*, is well documented in the arenas of everyday life and science. Two prime examples from the latter sphere are the centuries-old insistence that heat is a special kind of material substance and the similarly old insistence that the physical world we live in is deterministic. Alongside this conservative attitude, we also find a rebellious attitude that is also well documented. After all, both of the hypotheses just mentioned were ultimately given up by the relevant scientific communities for radically different alternatives. Indeed, sometimes scientific communities endorse alternatives in spite of their counterintuitiveness, simply because their predictive merits are too impressive to ignore—think of action-at-a-distance models of interaction as well as much of quantum and relativity physics. Aside from its role as a foil to conservatism, rebelliousness is meant to remind us that the prime mover of scientific development is (and ought to be) greater empirical success. And that is precisely what separates conservatism for its own sake from conservatism as a mere by-product of seeking to enhance empirical success. So long as "nature continually speaks in favor" of certain theory parts, as Chang himself notes—that is, so long as these theory parts pass more and more empirical tests—we have increasingly stronger reason to suppose that their survival is not a consequence of some intrinsic or extrinsic limitation but, instead, that these theory parts have somehow latched onto some aspect of the world.

The real threat to realism is not conservatism but underdetermination. If the empirical success enjoyed by a given theory part can always be replicated or bettered by a radically different theory part, then assertions about the former part's truth content are severely undermined. Whether empirically successful theories, or theory parts, are underdetermined is one of the great unresolved issues in the realism debate. But that is a topic for another paper.

**8. Conclusion.** I hope to have supplied the reader with compelling reasons for the view that at least some empirically successful parts of theories, namely, minimally interpreted mathematical parts, can be prospectively identified. Moreover, I hope to have persuaded the reader that the survival of such parts through theory change is evidence not for a constructive empiricist view of science but, rather, for a realist one. Finally, I hope to have effectively ruled out the idea that the reason for this survival is merely due to conservative tendencies.

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