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# Three Tales of Scientific Success

Michela Massimi\*†

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Success-to-truth inferences have been the realist stronghold for a long time. Scientific success is the parameter by which realists claim to discern approximately true theories from false ones. But scientific success needs to be probed a bit deeper. In this article, I tell three tales of scientific success, by considering in turn success from nowhere, success from here now, and success from within. I argue for a suitable version of success from within that can do justice to the historically situated nature of our scientific knowledge. The outcome is a new way of thinking about success-to-truth inferences along perspectivalist lines.

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**1. Introduction.** A deeply entrenched realist tradition takes truth as the hallmark of scientific theories and scientific success as its assayer. Only a scientific theory that has proved successful over time can legitimately be hallmarked as true. Hence, success-to-truth inferences feature prominently in scientific realism. Miracles aside, it is the success of the theory that speaks for its truth. Or so the realist argument, in its vulgate, goes (see Putnam 1975).

But success-to-truth inferences come also in more sophisticated versions. In Psillos's (1999, 159) version, for example, Fresnel was successful (and hence right) in identifying some of the fundamental properties of the light waves (e.g., that light needs a carrier and it consists of transverse waves). But he was wrong in identifying the carrier with the molecular ether. In the structural realist version, it is the surprising success of Fresnel's equations (carried over in Maxwell's theory) that speaks for the truth of Fresnel's theory, no matter how false the underlying hypothesis of the luminiferous ether was (Worrall 1994). And in the real realist version (Kitcher 2001, 168–70), Fresnel's theory was successful—with Poisson's striking prediction of a bright spot—because of its success-

\*To contact the author, please write to: School of Philosophy, Psychology, and Language Sciences, University of Edinburgh, 3 Charles Street, Edinburgh EH8 9AD, UK; e-mail: michela.massimi@ed.ac.uk.

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ful “working posits” (e.g., electromagnetic waves of high frequency) and despite its “idle wheels” (e.g., Fresnel’s false belief about the elastic ether). In any of these realist versions, truth is inferred as the best explanation for the success of the theory at issue. Some negative tolerance is allowed in the hallmarking process. For realists of all stripes strive for approximate truth, rather than just truth.

This picture of how scientific success assays truth from falsehood has famously been challenged via powerful arguments from the history of science.<sup>1</sup> I will not review here the different arguments that have been leveled from historically minded quarters. Instead, I start with a seemingly mundane observation. Our selective realist engagement with theories of the past is to a large extent measured by how close (or, conversely, how far) we are from the theories we are judging. Fresnel’s wave theory is still close enough to our current electromagnetic theory. But Aristotle’s theory of motion, Ptolemy’s crystalline spheres, or Stahl’s phlogiston theory are more alien to us when making our realist judgments about truth-conducive inferences from success. The criterion of success we thus employ in these inferences is key to our realist judgments. I am not going to question the realist success-to-truth inferences. Instead, the main goal of this paper is to rethink the realist criterion of success so as to accommodate this mundane observation.

The observation needs some refinements, to start with. First, we need what David Harker (2013, 89) calls a “comparative conception of success.” Confirmation theory teaches us that the available evidence may support one hypothesis better than rival ones, and hence in our truth-conducive inferences we ought to look at “theory-lineages,” as Harker calls them, to assess whether a theory is comparatively successful (95):

**HYP<sub>2</sub>.** Those parts of theories that generate comparative success are approximately true.

**AUX.** Approximately true insights will be preserved across subsequent instances of theory change.

**JUS<sub>2</sub>.** On the basis of HYP<sub>2</sub> and AUX we predict that insights responsible for comparative success will have been retained within our own scientific theories. Verification of these predictions is evidence for HYP<sub>2</sub>.

I share Harker’s comparative conception of success. But I think his analysis falls short of addressing a central point, which transpires from the text in the

1. See Laudan (1981a) and, for more recent arguments, Stanford (2006) and Chang (2012).

quote above. How do we know that approximately true insights “will be preserved” across theory change?

The problem concerns the indexical nature of the personal pronoun “we” in the passage above. Does “we” mean us now? Presumably no. For we cannot make *ex post facto* predictions about what approximately true insights have been preserved in our current theories. With hindsight wisdom, we can at best make judgments about what has been preserved (as a matter of fact, not as a matter of prediction). Does “we” mean “they,” that is, our predecessors back then? Imagine Fresnel back in 1818–21 predicting that his theoretical insights about propagation of light waves would have been retained in future theories because of its comparative success (*vis-à-vis* his rivals). Fresnel and his contemporaries were in no better position to make such a prediction than to predict the elimination of the ether (given how entangled propagation of light waves was with the hypothesis of the ether back then).

Thus, a second refinement to my mundane observation is in order. I suggest rephrasing Harker’s view so that “predictions” are replaced by a posteriori judgments on theoretical insights that each scientific community at any particular time may deem worthy of retaining from theories of their predecessors. This refinement tracks theory-lineages and comparative success without privileging either our current standpoint or those of our predecessors in judgments of comparative success.

In what follows, I defend success-to-truth inferences in their historical lineages. I argue that scientific success is to some extent relative to perspectival standpoints, and it should be measured by the standards of performance adequacy typical of each standpoint. Yet, crucially, I also maintain that scientific success must be evaluable from other perspectival standpoints. My final goal is to take some preliminary steps toward a view of success across scientific perspectives, which can be faithful to the realist project, while also taking seriously the challenges coming from the history of science.

Thus, my primary objective in this paper is to clarify how theories (or parts thereof) are deemed worthy of being retained across scientific perspectives, despite the contextual (or perspectival) nature of scientific success. To achieve this objective, in section 2 I take a closer look at two influential tales of comparative success and show their respective shortcomings. In section 3, I present my perspectivist take on success from within and clarify how it is meant to improve on the previous two tales.

**2. Two Tales of Comparative Success.** From which vantage point are comparative judgments of success made? When making a judgment of the form “theory  $x$  (or parts thereof) is more successful than theory  $y$  (or parts thereof),” comparison is made between  $x$  and  $y$  against some background or vantage point, in which (presumably) some standards of performance adequacy  $s$  are embedded.

My question then is how we should understand this background or vantage point at play in judgments of comparative success. I will not be discussing standards of performance adequacy themselves (although I give a couple of examples as we go along). Instead, my focus is primarily on how to think of comparative success (and related judgments we make all the time) in terms of theory-lineages and historical development. Answering this question is crucial to understanding how scientists go about making success-to-truth inferences in any given historical period and within any given intellectual tradition. More to the point, answering this question is important to rethinking the realist commitments one may legitimately uphold, despite the situated nature of the standards of performance adequacy. In this section, I tell the tale of Fresnel's success-to-truth inferences twice, by considering two possible answers to my question above. I give my own tale of Fresnel's success story in section 3.

Consider Augustin Fresnel submitting his report in 1819 to the Parisian Académie des Sciences for the prize competition on optical diffraction. The prize commission included Gay-Lussac, Arago, Biot, Laplace, and Poisson (the last three being open supporters of Newton's corpuscularist optics). Fresnel won the prize competition, despite the skepticism of his judges. Fresnel's methodological attitude is evident from his motto *Natura simplex et fecunda* and other writings: "If this general principle in the philosophy of the physical sciences does not lead immediately to knowledge of the truth, nevertheless it can direct the efforts of the human spirit, in distancing it from systems which relate phenomena to too great a number of different causes, and in letting it prefer those which, supported on the smallest number of hypotheses, are the most fecund in consequences" (quoted in Grattan-Guinness 1990, 868).

Fresnel's story is a story of scientific success amid one of the liveliest scientific controversies between corpuscularist optics and wave optics. This story has been read again and again as a story of scientific success: of structure over substance, of working posits over idle wheels. I want to retell this story with a different question in mind: namely, by whose standards does it count as a story of scientific success? Or, alternatively, from which vantage point are judgments of comparative success (such as this one) made? And how can they last over time?

*2.1. Fresnel's Tale Number One: Success from Nowhere.* Here is a first possible answer. Fresnel's is a story of scientific success by the very standards of "Science." If we take Science (with a capital S) as the body of scientific knowledge, which endeavors to discover nature, Fresnel's wave theory of light might be regarded as an integral part of this successful endeavor. I call this the view of success from nowhere (or, the view of success from God's eye). We might never be in the position of achieving such complete true knowledge of nature, but it acts as a regulative idea of scientific inquiry to assess success and failure at any given historical time.

This view of success from nowhere has the advantage of overcoming historical contingencies about why scientific communities in particular historical periods came to embrace one theory over another. Individual successes are assessed against a regulative idea of scientific success. Thus, it is a comparative view of scientific success *sub specie aeternitatis*, so to speak. Most scientific realists, I take, have in mind this view of success.

Under this view, one may interpret Fresnel's self-declared standards of parsimony in the number of assumptions and fecundity in the consequences as standards typical of Science. These standards demanded the assent of skeptical judges such as Poisson and Biot in 1819, as much as they continue to demand our scientific assent today. They are standards of success *sub specie aeternitatis* that invariably lead to the truth (yesterday, today, and tomorrow).

Tempting as it might sound, this view is questionable. Standards for assessing scientific success (and hence the approximate truth of theories) are sensitive to historical contingencies of real communities in real historical periods. For example, Fresnel's appeal to parsimony in the number of assumptions and fecundity in the consequences is the expression of a methodological view of science that emerges in the early nineteenth century, following a long debate among supporters of David Hartley's explanationism, Thomas Reid's empiricism, and Alain-René Lesage's hypotheticalism on the specific role of ether theories (see Laudan 1981b). From Newton's *Opticks* onward, the ether was regarded as a powerful explanatory mechanism for a variety of phenomena (e.g., from the transmission of heat to electrical phenomena). A closer look at Fresnel's work soon reveals important historical contingencies about the role of the ether mechanism in assessing the success of Fresnel's results at the time.

Consider Fresnel's appeal to parsimony in the number of assumptions. Far from an eternal standard of Science, Fresnel (and his contemporaries) understood parsimony as part and parcel of the ether mechanism at work in the explanation of optical phenomena.<sup>2</sup> The Laplacians (from Biot to Poisson) believed that light propagated mechanically via the emission of corpuscles traveling in space. Fresnel, by contrast, defended a molecular view of the ether as a medium that propagated light through the vibrations of ethereal molecules under the action of central forces (including a repulsive force). It is in this context that again, 2 years later, in 1821, Fresnel introduced against the Laplacian "geometers" his model of the elastic ether, whose molecules, acting under the laws of repulsion and conservation of *vis viva*, vibrated and propagated transverse light waves. This mechanical model allowed Fresnel to give a "very simple

2. In the 1819 prize essay, Fresnel appealed precisely to the simplicity of his hypothesis against the very large number of complicated and improbable assumptions that his rival corpuscularists had to introduce to explain the phenomenon of diffraction (see Buchwald 1989, 172).

mechanical explanation of Malus's law" (quoted from Grattan-Guinness 1990, 879) and of his own earlier work on diffraction.

Fresnel still lacked an exact understanding of the mechanism of interaction between the ethereal molecules and, say, the Iceland spar, through which light waves would propagate in double refraction. A year later, in 1822, he delivered a long paper on double refraction (published only in 1827 in the *Mémoires*, the same year of Fresnel's death). The paper presented at the Académie des Sciences appears to have been welcomed by the same Laplace as of "exceptional importance" (Grattan-Guinness 1990, 896). In it, Fresnel laid down a few simple assumptions about the velocity of light rays being dependent on planes of polarization and his mechanical account of the luminous vibrations, among others. The goal was to maximize fruitfulness in the results. Indeed, Fresnel could retrieve his archenemy Biot's Laplacian analysis of double refraction (which had been presented at the Académie 3 years earlier, in March 1819; for extensive details on the history of Fresnel's 1822 work, see Buchwald 1989, chap. 11). This was the victory of Fresnel's ether model over Biot's corpuscularist optics. Parsimony in the assumptions concerning (among others) the ether mechanism,<sup>3</sup> as well as fruitfulness in the consequences that could be deduced from them (including Biot's equations for double refraction), led to Fresnel's success in 1822.<sup>4</sup> More to the point for my story, Fresnel's appeal to parsimony and fruitfulness shows how standards of scientific success are contextual and perspectival; they are not *sub specie aeternitatis*. Their being situated in a given historical and cultural context (e.g., the debate between corpuscularist optics and wave optics at the Parisian Academy in the early 1820s) allows historical communities to use these standards for assessing the relative success or failure of the available models at the time and by their own light. That Fresnel was able to retrieve Biot's sine-product law from simple assumptions about the ether model was success enough for Laplace himself to acknowledge Fresnel's results.

Success *sub specie aeternitatis* tries to disentangle the tenselessly true parts of the theory from the tensed incorrect bits (mostly to do with the ether and

3. Buchwald (1989, 306–10) also stresses, with caveats, the unifying role of the ether at the time.

4. I do not have the space to pursue in detail this historical point here. But I endorse what Grattan-Guinness (1990, 898) writes: "Of course, [Fresnel's] theory was more rich in assumptions than Fresnel had indicated—various principles of mechanics were involved in the articulation and applications of these principles, together with the particular belief that the 'elasticity of the medium' was proportional to the square of the corresponding radius vector . . . , an important source of the theorizing in terms of ellipsoids which then follows. But how *brilliant* a theory was developed. . . . It was not cast in the Parisian holy writ of differential equations, not that he seemed to care . . . and the experimental work was confined so far to topaz. But his achievement was remarkable, and fully deserved the compliments of Laplace—who maybe had just become an ex-Laplacian optician?"

its explanatory role in optical phenomena). But Fresnel's motto *Natura simplex et fecunda* did not capture standards of success from nowhere. It expressed instead a view shared at the time by wave theorists no less than Laplacians about what counted as a good optical theory by the standards of the time. And at the time assumptions about luminous vibrations in the ether were legitimately included in the standard of parsimony (as long as fruitful consequences could be deduced from them).

The lesson of our first tale is the following: standards of scientific success are always contextual and perspectival. They are the expression of methodological debates and choices of particular communities at particular times. Success from nowhere is nobody's success.

*2.2. Fresnel's Tale Number Two: Success from Here Now.* One natural reply at this point would be to rectify the blanket claim that success is ever *sub specie aeternitatis*. For example, one natural option would be to modify the scope of the claim as a claim concerning us now and our current scientific standpoint. After all, no one can claim a God's-eye view on nature. But we do have our own current scientific standpoint, and we judge success and failure in terms of it. What has lasted long enough in historical terms to reach us now seems to qualify as successful. Ether theories have long gone from our textbooks, but Fresnel's equations are still in our textbooks in some form. Thus, the former must be false, but the latter must be approximately true, if they still serve us well.

Under this view of success from here now, success is not a matter of comparison with some regulative idea of true complete knowledge of nature. Success from here now is our own success, assessed by our own lights and current standards. Fresnel's success story then becomes a story of how a partially false theory still meets standards that we value today so as to count as still approximately true by our own lights. Presumably then, it is not Fresnel's self-declared parsimony and fecundity that make his theory still successful to our own eyes (especially since parsimony for Fresnel included assumptions about the ether that we now discard as illegitimate idle wheels). Instead, Fresnel's still counts as a successful story either because his equations still feature in our textbooks or because his theory led to Poisson's novel prediction of the bright spot (and novel predictions are the sign that the theory must be true). Structural realists have appealed to the continuity in the mathematical equations; real realists and selective realists have stressed the novelty of Poisson's bright spot.

Yet, success from here now needs two caveats. First, our here now is not itself another variant of success *sub specie aeternitatis*. Our currently preferred standards for assessing the success of Fresnel's theory (be it its mathematical equations or its novel predictions) are themselves contextual and perspectival. They do not necessarily map onto the standards used by Fresnel and contemporaries at the time. It is indicative what the historian Grattan-Guinness (1990,



870) writes about the experimental confirmation of Poisson's bright spot: "There was a notable silence over this finding. Nobody else, whether undulationist or emissionist, wrote a paper on it or its possible consequences. In particular, Biot, who had described in his article with Pouillet on diffraction . . . Maraldi's somewhat similar finding (in 1725a, 139) . . . , gave it no mention in the 1819 edition of the translation of Fischer's physics textbook or in his second (1821) edition of his own *Précis* on physics." Thus, the novel prediction of the bright spot—impressive and conclusive as it may appear to our eyes—did not in fact seem to have counted as a standard for assessing the victory of Fresnel at the time.<sup>5</sup>

Second, our current standards for assessing the success of Fresnel's theory are no less provisional and transient than were Fresnel's standards at the time. Going back to my mundane observation, we judge Fresnel's theory through the lenses of Maxwell's electromagnetic theory, which we still adopt today. But from here now, Ptolemy's crystalline spheres and Stahl's phlogiston all count as unsuccessful. Yet, the indexical nature of success from here now should also remind us of the tensed character of such a comparative notion of success, which gives rise to contradictory judgments. Our success from here now does not mesh well with success from there then. Success from here now—in the mouth and hands of medieval scholars—means that Aristotle's physics and Ptolemy's crystalline spheres (among many others) counted as successful (and hence approximately true). But Aristotle's physics and Ptolemy's crystalline spheres count as false for us now. A scientific theory cannot be true at one time and false at another, by the very same comparative notion of success from here now.

The lesson of our second tale is the following. Success from here now either is our own success dressed up as *sub specie aeternitatis* or, worse, will be failure from there then.

**3. Success from Within and Success-to-Truth Inferences along Perspectivalist Lines.** What has gone wrong with our comparative notion of success? Can we improve on both success from nowhere and success from here now? A promising alternative should take on board the contextual nature of standards of scientific success, while also avoiding the problems associated with the indexical nature of success from here now. My suggestion is a view of scientific success from within. Scientific perspectives—I suggest—provide both contexts of use and contexts of assessment for scientific knowledge claims and their respective successes. Success from within is the ability

5. Andreas Baumgartner's *Die Naturlehre* in 1824 expressed a similar judgment about the superiority of Fresnel's view over the corpuscularist view, which could explain phenomena "only by coercion and additional hypotheses that violate all analogy" (quoted in Buchwald 1989, 201).

of a theory to perform adequately with respect to standards that are appropriate to the scientific perspective of the time, when assessed from the point of view of other scientific perspectives. I suggest the following definition (for more details, please see Massimi 2016a):

A scientific claim (SC) meets the criterion of success from within iff:

- a) SC expresses a proposition  $p$  at scientific perspective  $SP_1$
- b)  $p$  is true (i.e., corresponds to states of affairs in nature) and meets standards of performance adequacy in  $SP_1$  when assessed from other scientific perspectives  $SP_2, SP_3, SP_4 \dots$

This definition of success from within does justice to the realist demand for tracking nature's states of affairs (via the first part of condition  $b$ —i.e.,  $p$  is true). In other words, being true is a necessary condition for a scientific claim to classify as successful.<sup>6</sup> But it is not sufficient (as scientific realists and other kinds of realists would have it) because no one is ever in a position of assessing the success of any scientific claim from a God's-eye standpoint. Real communities advance judgments of comparative success, in given historical periods and intellectual contexts. Hence, the second part of condition  $b$  captures the perspectival component in the definition of success from within. Past theories ought to be judged in their own terms and by their very own standards (not by the standards of Science or by our own now). At the same time, their ongoing performance has to be judged adequate from the point of view of other (diachronically subsequent) scientific perspectives.

Success from within forces us to rethink the nature of success-to-truth inferences along perspectivalist lines. Scientific perspectives provide the original context of use in which relevant standards of performance adequacy are first formulated and deployed. Subsequent scientific perspectives provide contexts of assessment, from which it ought to be possible to evaluate the ongoing performance of past scientific claims by their own original standards. Given the richer informational state available to subsequent perspectives, it may be possible for later assessors to regard the performance adequacy of past scientific

6. To be clear, I am not suggesting that truth is inferred from success from within by *modus ponens*, or in some circular way. It is not the aim of this paper to question the realist view that success is somehow an indicator of truth. I have taken the realist success-to-truth inferences on board all along and granted for the purpose of this paper that success is an indicator of truth. Instead, my concern in this paper is about the notion of success itself and how we go about making comparative judgments about scientific success. I part from other varieties of realism in thinking that comparative success should be from within. Hence, I add to the default truth requirement also a reference to the ongoing performance adequacy of the scientific knowledge claim at stake across scientific perspectives.

claims as lacking in some respects. Hence, it is possible for later assessors to either retain or withdraw (in whole or in part) past scientific theories on the basis of their continuing performance adequacy (or inadequacy). This is how successful theories (or parts thereof) evolve in time and get transmitted in theory-lineages.

Thus, the ability of a scientific theory (or parts thereof) to track genuine states of affairs ultimately establishes whether the theory is approximately true or not. Yet standards of performance adequacy must also be met, for us (and our future assessors) to be able to judge the theory as still successful, and hence for believing it to be approximately true (for further details, see Massimi 2016b). Our inferences to the approximate truth of past theories (or parts thereof) are then—to some extent—due to our commitment as a scientific community to justifiably retain past scientific claims, whose performance continues to be adequate by their own original standards when assessed from later scientific perspectives.

Fresnel's optical theory is successful (and hence we believe it to be approximately true) because its propositional content in terms of periodic properties of light waves at play in a variety of optical phenomena was true (i.e., corresponded to the way in which light waves behave in reality) and it met standards of performance adequacy in the scientific perspective at the time. For example, it provided a simple explanation for optical diffraction (by contrast with corpuscularist optics), and it could be fruitfully extended to cover a range of other optical phenomena (from Biot's sine-product law for double refraction to polarization by reflection, where the so-called Fresnel's formulae still feature in our textbooks).

Parsimony in the number of assumptions and fruitfulness in the number of consequences are just two examples of a much longer list of legitimate scientific standards of performance adequacy that we today still share with Fresnel's scientific perspective. That Fresnel's theory about light waves corresponded to real behavior of light waves is necessary for it to be true. But it is not sufficient for it to count as (comparatively) successful—to the eyes of his contemporaries and to our own. For the theory ought to be judged as successful by the standards of the scientific community at the time. And the judgment of Fresnel's scientific community at the time—in the words of Laplace—continues to be our own judgment today. Fresnel's optical theory still satisfies the original standards of performance adequacy when assessed from our current scientific perspective.

We share with Fresnel's scientific perspective the standards of parsimony and fruitfulness (even if we no longer list assumptions about the ether as parsimonious, of course). And we continue to regard Fresnel's theory as meeting its own standards vis-à-vis its rivals at the time. If we were to eventually stop regarding Fresnel's optical theory as meeting these standards (maybe because we acquire a richer informational state about light wave propagation vis-à-vis

our understanding of parsimony and fruitfulness), we would stop reading Fresnel's as a success story. Does it mean that on that day Fresnel's theory will end up in the repository of simply false past theories (in good company with crystalline spheres, and what not), even if its propositional content (and the approximate truth of such content) has not changed overnight? Clearly no. The future scientific community will reassess whether to retain or withdraw Fresnel's theory in the light of its richer informational state about light wave propagation and its ongoing performance adequacy when assessed by the new scientific perspective.

Success from within can deliver a resilient perspectival notion of truth across scientific perspectives, if we consider scientific theories in their evolution and historical lineages. Success from within might well be our best antidote against both the perennial temptation of a God's-eye view (which is precluded for us) and the antirealist caricature of scientific success slipping through our fingers.

## REFERENCES

- Buchwald, J. Z. 1989. *The Rise of the Wave Theory of Light*. Chicago: University of Chicago Press.
- Chang, H. 2012. *Is Water H<sub>2</sub>O?* Dordrecht: Springer.
- Grattan-Guinness, I. 1990. *Convolutions in French Mathematics, 1800–1840*. Vol. 2, *The Turns*. Berlin: Birkhäuser.
- Harker, D. 2013. "How to Split a Theory: Defending Selective Realism and Convergence without Proximity." *British Journal for the Philosophy of Science* 64:79–106.
- Kitcher, P. 2001. "Real Realism: The Galilean Strategy." *Philosophical Review* 110:151–97.
- Laudan, L. 1981a. "A Confutation of Convergent Realism." *Philosophy of Science* 48:19–49.
- . 1981b. "The Medium and Its Message." In *Conceptions of Ether*, ed. G. Cantor and M. J. S. Hodge. Cambridge: Cambridge University Press.
- Massimi, M. 2016a. "Bringing Real Realism Back Home: A Perspectival Slant." In *The Philosophy of Philip Kitcher*, ed. M. Couch and J. Pfeifer. New York: Oxford University Press.
- . 2016b. "Four Kinds of Perspectival Truth." *Philosophy and Phenomenological Research*, published online June 10. doi:10.1111/phpr.12300.
- Psillos, S. 1999. *Scientific Realism: How Science Tracks Truth*. New York: Routledge.
- Putnam, H. 1975. "What Is Realism?" *Proceedings of the Aristotelian Society* 76:177–94.
- Stanford, P. K. 2006. *Exceeding Our Grasp*. Oxford: Oxford University Press.
- Worrall, J. 1994. "How to Remain (Reasonably) Optimistic: Scientific Realism and the 'Luminiferous Ether.'" In *PSA 1994: Proceedings of the 1994 Biennial Meeting of the Philosophy of Science Association*, vol. 1, ed. Jeffrey Barrett, 334–42. East Lansing, MI: Philosophy of Science Association.