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Abstract and Keywords

This article endeavors to identify the strongest versions of the two primary arguments against epistemic scientific realism: the historical argument—generally dubbed "the pessimistic meta-induction"—and the argument from underdetermination. It is shown that, contrary to the literature, both can be understood as historically informed but logically valid*modus tollens*arguments. After specifying the question relevant to underdetermination and showing why empirical equivalence is unnecessary, two types of competitors to contemporary scientific theories are identified, both of which are informed by science itself. With the content and structure of the two nonrealist arguments clarified, novel relations between them are uncovered, revealing the severity of their collective threat against epistemic realism and its "no-miracles" argument. The final section proposes, however, that the realist's axiological tenet "science seeks truth" is not blocked. An attempt is made to indicate the promise for a nonepistemic, purely axiological scientific realism—here dubbed "Socratic scientific realism."

Keywords: scientific realism, pessimistic meta-induction, underdetermination, empirical equivalence, no-miracles argument, modus tollens, axiological scientific realism

1 Introduction

CONTEMPORARY scientific realism embraces two core tenets, one axiological and the other epistemological. According to the axiological tenet, the primary aim of science is truth—truth, for the realists, being no less attributable to assertions about unobservables than assertions about observables. According to the epistemological tenet, we are justified in believing an "overarching empirical hypothesis," a testable meta-hypothesis, about scientific theories. For standard scientific realism, that meta-hypothesis is that our successful scientific theories are (approximately) true. The justification for believing this meta-hypothesis resides in the “no-miracles argument”: were our successful scientific theories not at least approximately true, their success would be a miracle. In other words, rejecting miracles as explanatory, the (approximate) truth of our theories is the only, and so the best, explanation of their success. Because both sides of the debate have taken the axiological tenet to stand or fall with the epistemological tenet, it is the debate over the epistemological tenet that is at the forefront of the literature. Accordingly, this article is concerned primarily with epistemic scientific realism, in particular, clarifying and reinforcing the two primary nonrealist objections against it. Only briefly, at the end, do I return to the axiological tenet.

2 The Historical Argument Against Scientific Realism

There are two primary arguments against epistemic scientific realism. In the next section, I address the argument from the underdetermination of theories by data. Here (p. 565) I discuss the historical argument against scientific realism,[1] which takes seriously the assertion that the scientific realist’s meta-hypothesis is empirically testable. At its core lies a list of successful theories that cannot, by present lights, be construed as (even approximately) true. Contemporary versions have generally drawn on Larry Laudan’s (1981) well-known list of such theories (e.g., theories positing phlogiston, caloric, the luminiferous ether, etc.). The most prevalent version of this argument is known as “the pessimistic meta-induction”: because many past successful theories have turned out to be false, we can inductively infer that our present-day theories are likely false. Accordingly, realists respond that there are insufficiently many positive instances to warrant the conclusion: referencing Laudan’s historical argument, Stathis Psillos writes, “This kind of argument can be challenged by observing that the inductive basis is not big and representative enough to warrant the pessimistic conclusion” (1999: 105). Here I discuss two alternative *noninductive* variants of the historical argument, both of which take the form of a *modus tollens*: the first I articulated earlier (Lyons 2002), and the second I introduce here.

2.1 The First Meta–*Modus Tollens*

As with the standard pessimistic induction, the core premise of the noninductive historical *modus tollens* is a list of successful theories that are not, by present lights, approximately true. In this historical argument, the list of false successful theories demonstrates not that our contemporary theories are (likely) false but that the realist meta-hypothesis is false, in which case we cannot justifiably believe it. In fact, I contend (Lyons 2001, 2002) that this is how we should understand the structure of the historical argument in Laudan (1981). (However, notably, in texts published both before [1977: 126] and after [1983: 123] "A Confutation of Convergent Realism" [1981], Laudan explicitly embraces the standard pessimistic meta-induction.) As we have seen, in its standard form the empirical meta-hypothesis the realist says we can justifiably believe is "our successful theories are approximately true." And the standard argument for that thesis is that it would be a miracle were our theories to be as successful as they are, were they not at least approximately true. The choice this realist gives us is between approximate truth and miracles, the latter being no genuine option at all. The historical threat, then, briefly stated yet properly construed, is as follows

1. If (a) that realist meta-hypothesis were true, then (b) we would have no successful theories that cannot be approximately true. (If we did, each would be a "miracle," which no one of us accepts.)
2. However, (not-b) we do have successful theories that cannot be approximately true: the list (of "miracles").
(p. 566) **3.** Therefore, (not-a) the realist meta-hypothesis is false. (And the no-miracles argument put forward to justify that meta-hypothesis is unacceptable.)

As mentioned, the historical premise has been traditionally understood to provide positive instances toward a nonrealist conclusion that our successful *scientific* theories are (likely) false. However, we now see that premise as providing counterinstances to the realist meta-hypothesis that our successful theories are approximately true. We are making no meta-induction toward an affirmation that our scientific theories are (likely) false but rather a meta–*modus tollens* that reveals the falsity of the realist meta-hypothesis (and the unacceptability of the no-miracles argument). Not only, then, are we not justified in believing the realist's meta-hypothesis; that hypothesis cannot even be accepted as a "fallible" or "defeasible"—let alone a "likely"—conjecture. It is simply false. One virtue attributable to this variant of the historical argument is that it no longer hinges on the quantitative strength of an inductive basis, and efforts to weaken that basis will not eliminate the threat. Notice also, for instance, that the *modus tollens* requires no commitment as to which specific scientific theories among the mutually contradictory successful theories are the false ones—the falsity may lie in the past theories, in the contemporary theories that directly contradict them, or both. On this view, the quest to empirically increase the quantity of instances is not to provide strength for an inductive inference. It is rather to secure the soundness of the *modus tollens*, to secure the truth of the pivotal second premise, the claim that there are counterinstances to the realist meta-hypothesis. Put another way, increasing counterinstances serves to strengthen and secure the nonampliative, nonuniversal falsifying hypothesis "some successful theories are false." An additional role served by increasing the counterinstances (implicit in the previous parenthetical notes) pertains to the very argument realists invoke to justify believing their meta-hypothesis: the greater the quantity of "miracles," the more obvious it becomes that the core claim of the no-miracles argument is false. Increasing the collection of what become for the realist inexplicable successes, we are forced to reject the realist claim to offer the only, or even merely the best, explanation of those successes.[2] Moreover, for each counterinstance, it is now the realist who fails to live up to the realist's own much-touted demand for explanation. (Later we will see that increasing the historical counterinstances serves another, still noninductive purpose, in a second *modus tollens*.) Most crucially, this construal of the historical argument renders impotent numerous defenses against it. (Notably, among plenty of others, Lange [2002], Kitcher [2001], Magnus and Callender [2004], Parsons [2005], and Stanford [2006: 10–11][3] offer other recent arguments against the inductive variant of the historical argument, each of which misses the mark against this meta–*modus tollens*.)

(p. 567) One, albeit awkward, response to this *modus tollens* is to deny my claim that the realist invokes a meta-hypothesis. In fact, surprisingly, on both sides of the contemporary realism debate realism is construed as claiming we can justifiably believe the best explanation of natural phenomena. However prevalent this construal may be, it is the result of confusion regarding both the phenomena that realism explains and what it is that explains those phenomena. With the *modus tollens* now in hand, we can make clear just how untenable realism is rendered by this misconstrual. Of course the phenomena calling for explanation are not those of the natural world, so to speak; the latter are what scientific theories purport to explain. What realism purports to explain is, rather, a property of scientific theories. Nor, of course, is that which does the explaining itself a scientific theory; what the latter explains are natural phenomena. Just as that which is explained by realism is a property of scientific theories, so is that which does the explaining: one property of scientific theories explains another.

With regard to the property to be explained: in order to justify belief that bears on the content of science, realists must appeal to a property of scientific theories, which, in some epistemically relevant way, distinguishes those theories from any other arbitrarily selected collection of statements—say, collections of possible statements about the observable and unobservable that are contrary to what is accepted by contemporary science. And here one might think, yes, that property can simply be that of being the best explanation for a given set of phenomena. However, first the explanation needs to be "good enough" (Lipton, 2004); that is, it needs to meet the criteria scientists employ for acceptance. Second, even if scientists employ strict criteria, realists cannot simply map their beliefs onto scientific acceptance. Since to believe P is to believe that P is true, to believe the accepted explanation for a set of phenomena is to believe that it is true. Thus we have already landed on a meta-hypothesis "*accepted* theories are true." Moreover, with the proper understanding of the historical argument, we realize that this meta-hypothesis is unequivocally refuted by the meta–*modus tollens,* at nearly every turn in the history of science. Success, then, cannot be equated with superiority over available competitors, even when that superiority is determined by strict criteria for scientific acceptance. The property that realism purports to explain must go even beyond those criteria: it must be a surprising property—for instance, predictive success—such that, without some other property (e.g., truth), it would be a miracle were the theory to have it.

Regarding the property that does the explaining: the *modus tollens* forces us to see that it cannot simply be the truth of the theory. Not only is "*accepted* theories are true" refuted at nearly every historical turn; it turns out there are numerous counterinstances to even "theories achieving novel predictive success are true." Although Laudan was not concerned with novelty when formulating his list, numerous novel successes are listed in Lyons (2002, 70–72): there I show that, along with false theories outside of Laudan's list, those theories positing phlogiston, caloric, ether, and so on *did* achieve novel predictive success. In fact, even if the realist could somehow justify a commitment to only our most successful contemporary theories, the latter theories are not such that they can be believed outright: despite their individual successes, general relativity and quantum (p. 568) field theory, for instance, contradict one another; they cannot both be, strictly speaking, true—at best, at least one is only approximately so. So realists are forced to invoke as the explanatory property not truth per se but something along the lines of *approximate* truth. A meta-hypothesis is required and—despite a desire for a "face-value" realism, wherein we simply believe our favorite theories—that meta-hypothesis must be something similar to "predictively successful theories are (at least approximately) true."

Because it is only contemporary theories about which most realists want to be realists, and because our realist inclinations may blind us to the relevance of the conflict between general relativity and quantum field theory, we are prompted here to bolster the point that there is a conflict. I submit that the clash between those theories is taken as a given and in fact is taken to be *the* driving problem in contemporary theoretical science, by any number of physicists—including those in as much disagreement as, on one hand, Brian Greene and Leonard Susskind, who advocate string theory, and, on the other, Lee Smolin and Peter Woit, who argue against it.[4] Additionally, profoundly relevant but generally overlooked in philosophical circles is the dramatic conflict arising from the conjunction of those otherwise successful theories and the data regarding the acceleration of the universal expansion, the value of λ. In short, that conjunction results in what may well be the greatest clash between prediction and data in the history of science: the predicted value for λ is inconceivably greater than what the data permit, off by "some 120 orders-of-magnitude!" (Frieman et al. 2008: 20), letting physicists speak for themselves. This "blatant contradiction with observations … indicates a profound discrepancy"—"between General Relativity (GR) and Quantum Field Theory (QFT)—a major problem for theoretical physics" (Ellis et al. 2011: 1). Predicting, for instance, that "there would be no solar system" (2), this "profound contradiction" amounts to a "major crisis for theoretical physics" (10). (See also Murugan et al. 2012: 2–3.) Now contrast this "major crisis"—along with, perhaps, the galactic discrepancies that have prompted the positing of dark matter—against the comparatively negligible discrepancies that ultimately led to the overthrow of Newtonian physics by general relativity: the mildly aberrant behavior of Mercury, the less than determinate results (as it now turns out) of the Eddington expedition…. Again, the primary point is that, given the relation between general relativity and quantum field theory, at least one of these otherwise predictively successful theories cannot be strictly speaking true; at best at least one is only approximately true. Since believing that T is approximately true is to believe that T is strictly speaking false, believing that T is approximately true is altogether distinct from simply believing T. We must wholly discard the thesis that realists simply believe the best explanation of phenomena, along with any lip service to face-value realism. Recognizing the untenability of these notions, it is clear that our realist cannot avoid a meta-hypothesis. And, with the *modus tollens* in hand, we see that this meta-hypothesis must be one that invokes as its explanatory correlate something along the lines of *approximate* truth and, as the correlate to be explained, a property more restrictive than scientific acceptability, (p. 569) for instance, predictive success—hereafter meaning "novel success," where the data predicted were not used in developing the theory.[5]

2.2 The Second Meta–*Modus Tollens*

We can now introduce a second, new historical meta–*modus tollens*. This variant embraces lessons from the underdetermination argument, discussed later, and from the insight provided by twentieth-century philosophy of science that evaluations of empirical theories are comparative. Here, however, what are to be compared are meta-hypotheses. Claiming that we are justified in believing the meta-hypothesis "predictively successful theories are approximately true," the scientific realist is committed to claiming justification for believing that meta-hypothesis and the no-miracles premise *over* those ampliative meta-hypotheses that oppose them. By the latter, I mean to include the following "ContraSR" meta-hypotheses:

- "predictively successful theories are *statistically unlikely* to be approximately true"
- "predictively successful theories are *not-even-approximately-true*."

Asking whether we are justified in believing the realist's meta-hypothesis and no miracles premise over ContraSRs, we must ask: Which are in *better* accord with the data? In line with what we have seen so far, if any data could stand as correlatively precise positive instances for a ContraSR, they would have to be successful theories that are *not* approximately true. That given, and given the list of predictively successful theories that are not approximately true, the ContraSRs must be credited with such positive instances (which, as noted later, need not be literally "confirming" instances). And, in the historical argument, these positive instances are identified empirically, wholly independent of any presupposition that such a ContraSR is true. Moreover, without granting victory in (p. 570) advance to the realist's hypothesis, we have no data that stand as correlatively precise negative instances of a ContraSR. By contrast, the realist's meta-hypothesis has no correlatively precise positive instances without already establishing it. And it has a set of correlatively precise negative instances, the list, again without requiring any presupposition of a ContraSR. Given the list of successful theories that are not approximately true, the ContraSRs are rendered in their relation to the data superior to the realist hypothesis, and we are not justified in believing the latter over these ContraSRs. We are not, therefore, justified in believing the scientific realist's meta-hypothesis.

One reason I have included two nonidentical meta-hypotheses as ContraSRs is to emphasize that none of this has anything to do with believing one or the other of the two. And here we must not mislead ourselves to think that there is anything intrinsically ampliative in the relevant notions of "superiority" and "relation to the data." Even if most swans are white and only a few are black, it is unproblematic to say that black swans stand as correlatively precise positive (but not literally "confirming") instances of the false hypothesis "all swans are black"; and this unproblematic assertion requires no induction back to that false hypothesis. Likewise with the ContraSRs (irrespective of whether they are true or false): it is unproblematic to say that predictively successful theories that are not approximately true stand as correlatively precise positive (but not literally "confirming") instances of the meta-hypothesis "predictively successful theories are statistically unlikely to be approximately true" or "predictively successful theories are not-even-approximately-true." This unproblematic assertion also requires no ampliative inference back to either of these ContraSRs. No induction is involved in the task of tallying correlatively precise positive/negative instances (which are not, in themselves, literally confirming or refuting) or the task of comparing hypotheses in light of those results. Whether the same can be said for the act of *choosing between* the meta-hypotheses is a distinct question and not at issue here: for the recognition of the superiority of ContraSRs in respect to the data over the realist's meta-hypothesis does nothing in itself to necessitate, in turn, an inference to the truth of, a belief in, or even the tentative choosing or acceptance of any such empirically ampliative meta-hypothesis about successful theories. Nonetheless, employing the data, we find that we are not justified in believing the realist's meta-hypothesis. In fact, the argument just presented can be expressed as follows:

1. (a) We are justified in believing the realist meta-hypothesis (given the evidence) only if (b) we *do not* have greater evidence for those that oppose it (ContraSRs).
2. However, (not-b) we *do* have greater evidence for those that oppose it (for ContraSRs): the list; (and *as above, this claim involves no induction to any ContraSR).*
3. Therefore, (not-a) it is not the case that we are justified in believing the realist's meta-hypothesis (given the evidence).

This is another *modus tollens*, related to but distinct from the original introduced previously. There we saw that increasing the evidence against the realist meta-hypothesis (p. 571) secures soundness, the second premise of the first *modus tollens*. We now see a further reason for increasing the evidence and strengthening that premise. Via ContraSR meta-hypotheses, increasing the quantity of the items on the list increases the counterevidential weight against the realist meta-hypothesis—again necessitating no inductive (or other kind of) inference to a conclusion that our contemporary successful scientific theories are statistically unlikely to be approximately true or that they are not-even-approximately true, and so on.

To close this section on the historical argument, let us say, for the moment, that in response to the second premise of the first *modus tollens*, the realist chooses to embrace the following as the meta-hypothesis we can justifiably believe: "predictively successful theories are statistically likely to be approximately true." Not only would a retreat to this statistical meta-hypothesis diminish (but not eliminate) the testability of the realist meta-hypothesis; it would also wholly concede to miracles and to a failure on the part of the realist to explain successful theories. More to the point, the first ContraSR, "predictively successful theories are statistically unlikely to be approximately true," is bolder than but entails the negation of this new realist statistical hypothesis; that given, realists would find themselves in exactly the same situation as with their original meta-hypothesis: we have greater evidence for a bolder hypothesis that entails its negation. Hence, such a weakening of the realist hypothesis makes no difference in light of this new, second *modus tollens*.[6]

3 The Argument from the Underdetermination of Theories by Data

3.1 The Competitor Thesis

Beyond the historical argument, the other central challenge to realism is the argument from the underdetermination of theories by data.[7] Its core premise is a *competitor* (p. 572) *thesis*. In basic terms: our successful theories have empirically equivalent, yet incompatible, competitors. From that, the conclusion of the underdetermination argument is drawn. In basic terms: despite their empirical success, we cannot justifiably believe that our favored theories, rather than their competitors, are (approximately) true. I argued earlier that, contrary to much of the literature, the historical argument is not properly construed as inductive; I suggest that, likewise, the argument from underdetermination calls for clarification.

The first step is to isolate that question regarding competitors that is genuinely at issue in the scientific realism debate. That question is not "*Which* theories can we justifiably believe?" or, even more carefully, "*Which* theories are those among the class of theories such that we can justifiably believe that they are approximately true?" Since answers to these questions concede that there are such theories, the questions themselves grant a victory to realism in advance of being answered. Toward the identification of the proper question, we are prompted to clarify our terminology. Although our theories may have many alternatives (including alternative formulations), we can take "distinct alternatives" to denote alternatives such that, if those alternatives are approximately true, our favored theories cannot be approximately true: were the alternatives approximately true, they would render our preferred theories patently false. Realism, claiming we can *justifiably believe* the meta-hypothesis "our successful theories are approximately true" requires that we can *justifiably deny* the approximate truth of any such distinct alternatives to our favored theories. Taking "competitors" to mean distinct alternatives that are *empirically on par* with our favored theories, we see that the relevant and legitimate question in the realism debate is *whether* our successful scientific theories have competitors *such that we cannot justifiably deny that they are approximately true*.

With the proper competitor question in hand, the second step is to recognize that the realist cannot answer the argument from underdetermination merely by denying that our favored theories have empirically *equivalent* competitors. In fact, nonrealists other than, say, Bas van Fraassen, such as Laudan (1981), Sklar (1981), and Stanford (2006), recognize that, at least historically, the most genuine threats to scientific realism are empirically distinct. Just as theories that are empirically distinct from our contemporary theories occupy the core premise of the historical argument, such theories can occupy the competitor premise of the underdetermination argument. Clarifying “empirically on par” to allow for this, we can finalize our previous definition of “competitors”: competitors are distinct alternatives whose empirical predictions accord with the observed data predicted by our favored theories. Since it is so commonly assumed that realism is threatened only by underdetermination arguments that invoke empirically equivalent competitors, we are prompted to go beyond Laudan, Sklar, and Stanford and make clear just why such an assumption arises and why it is false.

It arises because, in contrast with an empirically distinct competitor, a competitor that is empirically equivalent to our favored theory is *empirically ineliminable*: no matter how many empirical predictions are tested, there is no point in the future at which such a competitor can be empirically eliminated; and, given the fact of empirical ineliminability, our favored scientific theories face *temporally unrestricted* underdetermination. **(p. 573)** Empirical equivalence suffices for empirical ineliminability, which in turn suffices for temporally unrestricted underdetermination. It is for this reason, I suggest, that the literature is replete with concern about empirical equivalence. Nonetheless, I contend that, as long as we find the right set of empirically distinct theories, that demand is irrelevant. That is, although empirical equivalence is sufficient for empirical ineliminability and so temporally unrestricted underdetermination, it is not a necessary condition for either.

First, an obvious point, whose importance is unencumbered by its obviousness: until distinguishing tests have been performed, the empirical distinguishability of competitors does nothing whatsoever to license belief in the (approximate) truth of our favored theories. Because realism is, in every instance, about belief (in approximate truth) at time *t*, potential future tests of competitors (at *t*-plus-a-century or even *t*-plus-a-decade) offer no positive evidence in favor of our preferred theories over those competitors. This fact alone strikes at the claim that we can be justified in believing (in the approximate truth of) our favored theories—empirically distinct competitors, which are *not yet* eliminated, posing no less a threat than empirically equivalent competitors. Second, if the situation is such that our theories will always have indefinitely many competitors, the distinguishing tests can never be performed for the entire set: in that case, our favored scientific theories are and will always be faced with empirically ineliminable competitors. Even if each competitor is individually empirically distinct from a given favored theory, and thus in principle individually empirically eliminable, the underdetermination that results from indefinitely many such competitors remains temporally unrestricted. The epistemic threat is no less severe than that posed by empirically equivalent competitors. With these points, I suggest that, just as the pessimistic induction is a straw objection to, so a distraction in favor of, scientific realism, so too are irrelevant competitor questions and the superfluous demand for empirical equivalence.

We can now endeavor to identify classes of genuine competitors and so secure the relevant competitor thesis. Toward that end, it is notable that, although the historical and underdetermination arguments have generally been treated as distinct, important relations hold between them.[8] Here I develop an empirically and historically informed competitor thesis and thus a foundational premise for an argument from underdetermination. However, I endeavor to avoid any inductive inference. The general strategy I offer for doing so is as follows: we empirically identify historically exemplified competitor relations between past and present theories, and, in particular, we isolate those competitor relations that extend to any theory related to phenomena in the way that scientific theories are noncontentiously required to be related to phenomena—according (p. 574) to both realists and antirealists. From among the latter subset of competitor relations, we identify those that can be instantiated in indefinitely many ways. In what follows, I specify two such competitor relations, demonstrating along the way that we have a historically informed yet wholly noninductive way to realize that any theories we may favor have indefinitely many competitors.

3.2 Competitors, Set 1

Toward the empirical identification of one such competitor relation between past and present theories, we can look first to those theories on "the list" employed in the historical argument. We can select as T such a historically successful but now rejected theory. According to contemporary science, T (approximately) predicts that a certain set of observed phenomena obtains. However, again according to contemporary science, in certain situations, the phenomena behave in a manner that significantly diverges from T's predictions. Here it is crucial to recognize that, since we are employing contemporary science to articulate the details of the divergence and since "scientific seriousness" is relevant to the realism debate (and its proper competitor question) only insofar as it pertains to whether or not a theory could be approximately true, the competitors we are considering cannot be excluded on the grounds that they are, for instance, "Cartesian fantasies." Contemporary science itself reveals a competitor (CT), which, though contradicting T, shares those predictions successfully made by T. The following is expressed by CT:

> The phenomena are (approximately) as T predicts, except in situations S, in which case the phenomena behave in manner M.

Instantiating this expression as a relation that obtains between past and present successful theories, one can insert for T, say, Kepler's deep theory of the *anima motrix*, which includes the following posits, each of which is patently false by contemporary lights: the sun is a divine being at the center of the universe; the natural state of the planets is rest; there is a nonattractive emanation, the *anima motrix*, coming from the sun that pushes the planets forward in their paths; the planets have an inclination to be at rest and to thereby resist the solar push, and this contributes to their slowing speed when more distant from the sun; the force that pushes the planets is a "directive" magnetic force, and so on. One can include for S occasions in which, say, Jupiter approaches Saturn, and occasions in which a planet's orbit is particularly close to the sun, and so on, adding as M, say, "non-Keplerian perturbations" and "the advancement of Mercury's perihelion." Using contemporary science to articulate S and M in their full particulars, these will be awkward and utterly nonsimple assertions, and the realist will be tempted to claim that such a competitor (in this case, a competitor to Kepler's theory) lacks certain explanatory virtues. However, because this competitor is an expression of successful contemporary science and because the realist cannot sacrifice the possibility of taking successful (p. 575) theories of contemporary science to be approximately true, the realist must concede that the absence of such virtues does nothing to provide grounds for denying the approximate truth of such a competitor. We see, then, that this expression reveals that there are rivals for the truth of such past theories. However, that expression also reveals that there are such rivals to any contemporary theory that accounts for some set of phenomena. Instantiating T with any accepted contemporary theory, the remaining clauses can include any S that has not (yet) been acknowledged as obtaining and any M that significantly differs from the behavior that T describes. We can add the fact that, according to science itself, the data set we have is infinitesimally small compared to the totality of events in the 13.8-billion-year-old universe. These points noted, we recognize that there are indefinitely many options and combinations, all of which will share T's predictions about observed phenomena. Although a very small sample of these competitors may be subject to future empirical elimination, indefinitely many such competitors will remain at any time.

3.3 Competitors, Set 2

Inspired by such historical considerations, we can move forward to identify another set of competitor relations and so another set of competitors. To similarly ensure that the next competitor set is empirically informed by science itself, we recognize first that, from science itself, including both past and present science, one can extract the assertion of situations in nature such that they

(a) appear in some experiential context, or were even taken at some stage in the history of science, to exemplify invariable and foundational continuities of the natural world, but
(b) are deemed by (e.g., later or contemporary) science to be no more than the residual effects of, and contingent on, what are ultimately uncommon conditions.

After noting a few cases in which this situation is exemplified, I unpack the way in which the prevalence in contemporary science of such posited special conditions reveals an additional set of competitors to the theories of contemporary science itself. In what follows, I use "Φ" to mean "an invariable and foundational continuity of the natural world." From the Newtonian picture, the posit that the sidereal period of a celestial object is proportional to the radius cubed constitutes no Φ but instead captures a consequence of the special condition of the low mass of the planets in our solar system in relation to that of the sun. Likewise from the Newtonian picture, no Φ is captured by the posit that the rate of acceleration for all objects falling toward earth is 9.8 meters per second per second: on one hand, this posit neglects the possibility of a terminal velocity; on the other, for instance, conjoining it to Newton's posit that the moon is in freefall, entails a sudden and extraordinary collision between the earth and moon. Consider as well, say, the assertion that the fluids argon, H_20, and freon R-113, come to a "rolling" boil at 185.85°, (p. 576) 100°, and 47.6°, respectively. By contemporary lights, rather than capturing intrinsic and invariable properties of argon, H_20, and freon R-113, this claim describes mere by-products of special conditions—for example, terrestrial convection currents acting on the molecules in the fluid, the currents themselves the result of the earth's mass, the proximity of the fluid to the earth's center of mass, and so on. (In fact, in microgravity experiments aboard the space shuttle, freon R-113 has been observed to boil at a lower temperature and to result not in a "rolling" boil but rather a single bubble coalescing in the middle of the fluid; Merte et al. 1996). That the earth is at rest, that the sun passes over us, that iron filings and compass needles are drawn in the direction of Polaris, that the sun emits primarily yellow light—in themselves, and by present lights, these claims approximate no Φs but stand only as artifacts of certain conditions: respectively, for instance, the nondiscernible nature of minimally accelerating motion, the location from which we observe the earth–sun relation, a temporary phase in the cycle of geomagnetic reversals, the particular dispersal of photons that results given the oxygen, nitrogen, carbon molecules in the atmosphere, and so on.

With each of these posits, it is not that a few exceptional phenomena deny their candidacy as Φs; rather, according to contemporary science, these posits capture only what ultimately amount to uncommon situations. Extending well beyond these examples, contemporary science insists that there are innumerably many other situations such that, though they might appear (or might have once appeared) to exemplify Φs, they are nothing more than the residual consequences of particular conditions. Acknowledging the ubiquitous positing of such special situations in contemporary science affords the recognition of a second collection of competitors to our favored theories. These competitors posit that the observed phenomena predicted by our favored theory, T, and its auxiliaries are, themselves, merely the result of unique conditions. Our favored theory's empirical claims constitute the description found in manner M (a description of no Φ but only a special situation), brought about by condition C (the particularly unique condition). Competitor:

- Φ obtains.
- Φ allows for the presence and absence of condition C and, in itself, patently contradicts T.
- *Condition C obtains* (according to the theory complex within which a description of Φ is embedded) *in spatiotemporal location* l, *and causes observable entities E at* l *to behave (approximately) in manner M, as T claims*.

Any number of cosmically rare conditions can be posited for C, whose particular effect is to bring about a set of phenomena that are in approximate accord with the confirmed predictions of our favored theory. Among them are dimensions intersecting, relations between our universe and others in the multiverse, stages in our universe's expansion, perhaps even relations between our galaxy and the Great Attractor, or our solar system and a galactic center, and so on. In fact there is one such condition that is available at nearly any level of nature: C can simply be a *threshold for emergence*—met by any variable (p. 577) regarding populations/quantities of objects, masses, charges, relations between entities, and so on—where the descriptions we favor describe no more than rare emergent properties. The properties we attribute to observed phenomena have come about only because a particular threshold was reached. And upon either surpassing the narrow limits of, or dropping below, that threshold, those properties will no longer persist. Specifying such a condition allows for indefinitely many competitors that diverge dramatically from and hence patently contradict T, all of which will share T's predictions about observed phenomena: nearly any randomly chosen self-consistent set of descriptions can qualify as descriptions of Φs that are posited in the competitors to govern phenomena in the absence of C. Given the possibilities expressed in quantum mechanics and cosmology—with particle/wave duality of "entities," wormholes, Kaluza-Klein theories, branes, holographic universes, and the like—we have great leeway for competitors that strike us psychologically as absurd. From the standpoint of these competitors, our favored T's success is no more than a by-product of condition C; and T need not, from the standpoint of these competitors, describe, *to any stretch of "approximation,"* any actual Φs.

Notice that these competitors explain their predicted observable events that are unexpected by our favored theory: in the absence of condition C, a condition perhaps deeply embedded in our *extraordinarily limited experience* of the 13.8-billion-year-old universe, the phenomena predicted by our favored T will no longer obtain (and those events predicted by Φ will). Notice also that, just as there is no generally accepted causal mechanism for certain unique conditions posited in contemporary science (e.g., vacuum fluctuations or, to use an example mentioned earlier, geomagnetic reversals), the realist demand for approximate truth cannot require that the competitors specify a causal mechanism for condition C. More generally, we cannot justifiably deny the approximate truth of these competitors merely because they do not posit causal mechanisms. Nor can we justifiably deny their approximate truth on the grounds that they violate a principle of the uniformity of nature or that they require that the world be nonsimple. These competitors are subject to no such charges.

We have now seen two sets of genuine competitors, alternatives that can render our favored theories such that they are not even approximately true and that nonetheless enjoy the same success as our favored theories. Taking both sets into account, we need concede little more than that some such competitors may be incomplete. However, because that particular concession is taken to be applicable to even our best theories, general relativity and quantum field theory among them, a charge of incompleteness cannot justify a denial of their approximate truth. Of course there are no data available that favor these competitors over our current theories, but nor are there data to eliminate them in favor of our preferred theories. And that is what is key. Moreover, in line with points made earlier, the nonrealist is no epistemic realist about competitors: acknowledging that there are *competitors that assert* that, say, S, M, and/or C obtain is wholly distinct from *asserting* that S, M, and/or C in fact obtain. And liberating ourselves from the pessimistic induction to the falsity of present-day theories, as we have, the nonrealist is wholly liberated from any denial of the truth of our favored present theories. With this we have two empirically informed, yet noninductively grounded, methods for revealing the following competitor thesis, which provides a positive answer to the competitor (p. 578) question that is relevant to the scientific realism debate: our favored theories have genuine competitors such that we cannot justifiably deny that they are approximately true.

4 Taking Stock: The *Modi Tollentis* Against Scientific Realism, Where History and Underdetermination Meet

I now consider how our competitor thesis can be put to work in a set of arguments against epistemic scientific realism. Note first that, although we arrived at the competitor thesis by way of analyzing, for instance, historically exemplified relations between past and present theories, because that thesis explicitly pertains to competitors that are not past theories, when we consider it only in explicit terms, it is not historical. Closing this section, I revisit the two *modus tollens* arguments we considered earlier. Going beyond those arguments for now, our historically grounded but ultimately ahistorical competitor thesis allows me to introduce a third—this time ahistorical—*modus tollens* argument. We have seen that epistemic realists claim we can justifiably believe the empirical meta-hypothesis, “predictively successful theories are approximately true.” As noted earlier, taking “distinct alternative” to denote a theory such that, if it is (approximately) true, our favored theory cannot be (approximately) true (e.g., as specified earlier, alternatives whose posited Φs are not described by T to any stretch of “approximation”) and taking “competitor” to mean a distinct alternative whose empirical predictions accord with the observed data predicted by T, it is again clear that epistemic realism *requires* justification for denying that successful theories have any genuine competitors that are (approximately) true. We can call this requirement the epistemic realist’s noncompetitor condition (see Lyons 2009b). Recognizing that epistemic realism entails this epistemic noncompetitor condition, we have the material for a third *modus tollens* argument against realism. Its basic structure is as follows:

1. If (a) epistemic realism holds, then (b) the epistemic realist’s noncompetitor condition holds.

2. However, (not-b) the epistemic realist's noncompetitor condition does not hold.
3. Therefore, (not-a) epistemic realism does not hold.

More carefully,

1. If (a) we can justifiably believe the realist's meta-hypothesis "our predictively successful theories are approximately true," then (b) we can justifiably *deny* that our successful theories have approximately true competitors.
2. However, (not-b) we cannot justifiably deny that our predictively successful theories have approximately true competitors; in fact, on the contrary, given the (p. 579) previous argument, our predictively successful theories have indefinitely many competitors whose approximate truth we cannot justifiably deny.
3. Therefore, (not-a) it is not the case that we can justifiably believe the realist's meta-hypothesis "our predictively successful theories are approximately true."

I have suggested that there are important relations between the historical argument and the argument from underdetermination, one example being the empirically/historically informed approach we employed toward revealing the two sets of competitors, the latter of which substantiate our competitor thesis: our favored theories have indefinitely many competitors whose approximate truth we cannot justifiably deny. With the content and structure of the arguments against realism now clarified, I add here the bold posit that this empirically informed, yet ultimately ahistorical, competitor premise can be conjoined to the empirical historical premise ("the list") in the first and second *modus tollens* arguments. That is, recognizing that indefinitely many competitors share the predictive success of our favored theories, yet nonetheless patently contradict our favored theories (and one another), we must grant that indefinitely many successful theories are patently false. The first two *modus tollens* arguments, no longer limited to being historical, receive a drastic increase in the quantity of items on "the list" of counterinstances, giving them a vastly stronger, more encompassing core premise. Additionally, given the dramatic rise in the quantity of "miracles," we significantly buttress our earlier conclusion that the no-miracles justification for believing the realist meta-hypothesis—"it would be a miracle were our theories as successful as they are were they not at least approximately true"—is utterly false.[9]

Adding the competitors to our counterinstances, it is important to note that they cannot be excluded from that list for failing to make novel predictions. A full competitor's predictions are what they are irrespective of when and whether they are actually derived. (Consider for instance the frequency and importance of *discoveries* that an already accepted theory entails a given consequence.) Just as the nonarticulated nature of a full competitor does nothing to prohibit its approximate truth, it does not affect its predictions. Now the sole purpose of the previous two competitor expressions is to reveal that there are competitors, and of course those competitors that are never generated cannot be excluded for using the data in their generation. Nonetheless, for the sake of illustration, consider a computer employing the previous expressions to generate sets of competitors to a proposed theory, T. In this case, we have a set of theories, all of which contradict one another but make some of the same predictions. And any novel success attributable to T is equally attributable to every competitor in the class of those that do not diverge in respect to T's successful novel predictions. These points noted, the competitors of concern cannot be excluded from the list of counterinstances for failing to make successful novel predictions. I submit that, irrespective of the realist appeal (p. 580) to novelty, innumerably many competitors genuinely qualify as counterinstances to the realist meta-hypothesis.[10]

Finally, this conjunction of an ahistorical competitor thesis to the list of theories in the historical argument makes newly salient an important explanatory relation between the two primary arguments against epistemic realism: the fact that, historically, we find theories that are predictively successful but not even approximately true is wholly unsurprising, and even expected, given the fact of underdetermination and, in particular, given the fact that every theory has indefinitely many competitors. Now seen in their deductive light, I suggest that both the evidence and these arguments against epistemic realism are far stronger than both the epistemic realist's meta-hypothesis and the no-miracles argument that is meant to provide justification for believing that meta-hypothesis.

5 Socratic Scientific Realism

Despite such threats to epistemic realism, taking a cue from Nicholas Rescher (1987), I argue that the nonrealist is mistaken to discard the realist's most fundamental tenet: science seeks truth. I advocate a wholly *non*epistemic, purely axiological scientific realism (see Lyons 2001, 2005, 2011, 2012). This nonepistemic realism endeavors to overcome our 2,500-year-old obsession with belief[11] and to treat meta-hypotheses about science, including "science seeks truth," the same way it treats scientific theories—not as objects of belief but as tools for inquiry, tools to be deployed in the quest for truth. I take the key idea here—discarding belief in the quest for truth—to have a long pedigree. Beyond occasionally receiving lip-service as the proper scientific attitude, Rescher, Russell, Popper, and Peirce number among those offering comments that accord with it. However, I am unsure just who, if anyone, has embraced it as fully as I aspire to.[12] (p. 581) Historically tracing back the injunction "seek truth without claiming to possess it," I suppose the figure whose voice we would hear—even if he turned away from the physical world—is Socrates. This is one factor leading me toward the name I give my position in this article: *Socratic scientific realism*.

The compulsion to believe may well be what has diverted standard scientific realists from refining their empirically uninformative axiological meta-hypothesis, thereby prohibiting them from a comprehensive account of science. Specifically, realists, including Rescher, have failed to specify just which subclass of true statements science seeks. To at least indicate the core idea of my axiological postulate, it is that *science seeks to increase the XT statements in its theory complexes*. XT statements are those whose truth is *experientially concretized*—that is, true statements whose truth is made to deductively impact, is deductively pushed to and enters into, documented reports of specific experiences. This postulate shifts from "truth" to a *subclass* of true statements, from theories to complexes/systems, and from endeavoring to simply *attain* the truth to endeavoring to *increase the quantity* of true statements in the specified subclass. Moreover, explicitly including "increase" in the postulated goal, evaluation is unambiguously comparative, theory complex against theory complex. And although realist truth is not contingent on the system of statements in which a statement is embedded, the experiential concretization of a statement's truth is. Most significantly, I have shown elsewhere (Lyons 2005) that the *actual* achievement of this state, an increase in the XT statements of a theory complex, *requires* the achievement of a set of syntactic theoretical desiderata, the importance of which both sides of the debate agree: namely, an increase in empirical accuracy and consistency and an increase in, or at least the retention of, breadth of scope, testability, and number of forms of simplicity. I make no claim that we can justifiably believe that the posited primary goal has been achieved. However, since it cannot be achieved without these *otherwise potentially disparate theoretical virtue*s, my axiological meta-hypothesis offers both an explanation and, crucially, a justification for their mutual pursuit: if we do not have these necessary conditions for our primary goal, an increase in experientially concretized truth, we know we do not have what we seek. Given this relation, I contend, this meta-hypothesis lives up to what it demands: for instance, it is informative toward our understanding of "inference to the best explanation," now liberated from epistemic baggage; it provides a better account of science than nonrealist axiologies (e.g., Laudan's meta-hypothesis that science seeks problem-solving effectiveness and van Fraassen's meta-hypothesis that the aim of science is empirical adequacy; Lyons, 2005); and finally, and more generally, it dramatically improves the realist's ability to account for what is going on in science. The battle cry of Socratic scientific realism is the following: science seeks truth and does so rationally, irrespective of whether we can justifiably believe we have achieved it.

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Notes:

([1]) See Mill (1859/1998), Laudan (1981), Sklar (1981), Rescher (1987), Worrall (1989), Leplin (1997), Chakravartty (1998), Psillos (1999), Sankey (2001), Lange (2002), Lyons (2002), Kitcher (2001), Chang (2003), Stanford (2003), Magnus and Callender (2004), Parsons (2005), Lyons (2006), Stanford, (2006), Doppelt (2007), Schurz (2009), Lyons (2009a, 2009b), Harker, (2010, 2013), Vickers (2013), Peters (2014).

([2]) I discuss nonrealist explanations of success in "Explaining the Success of a Scientific Theory" (Lyons 2003).

([3]) Stanford offers instead his "new induction." In short, because past scientists failed to think of alternatives, contemporary scientists fail as well.

([4]) Ioan Muntean has brought to my attention a formal proof of the clash between quantum field theory and general relativity in Weinberg and Witten (1980).

([5]) For the sake of simplicity and clarity, here I use this standard realist meta-hypothesis as my foil for articulating the form and implications of the primary arguments against realism. However, the most sophisticated variant of realism focuses not on theories but only those theoretical constituents "responsible for" success (e.g., Psillos 1999). The meta-hypothesis becomes "those theoretical constituents that are deployed in the derivation of successful novel predictions are at least approximately true." Testing this revision constitutes a research program in itself, taking us well beyond Laudan's list. Although my goal here is not to offer historical case studies, we can note that such a program has at least been launched. Invoking the first *modus tollens* and exploring the theoretical constituents genuinely deployed by, for instance, Kepler, Newton, Leverrier, and Adams, I detail numerous counterinstances to this sophisticated meta-hypothesis (Lyons 2002, 2006), also included in a more recent list offered in Vickers (2013). (See also Lyons 2013, footnote 13.) It turns out that theories—and in particular "theoretical constituents *responsible for*"—achieving novel successes include the usual suspects, those positing phlogiston, caloric, ether, and so on but also less familiar theories such as Rankine's thermodynamic vortex theory, as well as familiar yet overlooked theories, such as Kepler's theory of the *anima motrix*. Although, here I do not use the deployment realist's far more cumbersome meta-hypothesis as my foil, the points in this article hold no less if we pivot our discussion around it.

([6]) Encouraged now by the possibility of invoking statistical likelihood, the realist may be tempted to move it, so to speak, outside of the meta-hypothesis and claim that we can justifiably believe that the meta-hypothesis "predictively successful theories are approximately true" is statistically likely. However, this meta-hypothesis has already been rendered false by my first *modus tollens*; that given, along with the fact that "statistically likely" means "statistically likely to be true," we clearly cannot justifiably believe that this meta-hypothesis is statistically likely. It is simply false.

([7]) See Mill (1843), Duhem (1914/1991), Quine (1951), Boyd (1973), van Fraassen (1980), Sklar (1981), Churchland (1985), Ben-Menahem (1990), Horwich (1991), Laudan and Leplin (1991), Leplin (1997), Kukla (1998), Psillos (1999), Okasha (2002), Lipton (2004), Chakravartty (2008), Godfrey-Smith (2008), Wray (2008), Lyons (2009b), Manchak (2009), Hoyningen-Huene (2011), Worrall (2011), Tulodziecki (2012), Lyons (2013).

([8]) Stanford (2006), drawing on Sklar, brings this important point to the fore, offering a historical *induction* to ground what I've referred to as a competitor thesis (see footnote 2). However—even setting aside the fact that Stanford does not argue for indefinitely many competitors in the situations he specifies—I show (Lyons 2013) that his argument poses no threat to contemporary realism: it fails to concern itself with the type of theories invoked in the realist's meta-hypothesis (e.g., those making successful novel predictions, a problem initially pointed to in Lyons 2006: 544, footnote 10); it rests on a problematic thesis regarding the *failure of scientists,* and it relies on not one but two dubious inductions. The arguments I articulate here face none of these problems.

([9]) Since, for instance, few of the competitors constitute instances of our theorizing, pessimistic inductions that require uniformity in the falsity of our theorizing do not inherit this support.

([10]) Notably the *deployed* constituents of concern in footnote 5 also have competitors. Cutting to the chase, consider our computer invoking, for instance, the second competitor expression as a method for generating competitors. Among the posits genuinely deployed toward that end are the following, each of which is patently false by present lights: our favored theory, T, is false; only a subclass of T's observable consequences obtain, and (nearly any sized set of specific) observable events outside of that subclass fail to accord, and can significantly clash, with observable events described by T; Φ obtains, as an invariable and foundational continuity of the natural world; Φ allows for the presence and absence of condition C; condition C obtains; it obtains in spatiotemporal location l; condition C is what allows for the subclass of T's observable consequences that do obtain, and so on. Given indefinitely many possibilities for each of the variables, we have indefinitely many competitors even to those constituents that are deployed in the derivation of novel successes.

([11]) That is, nonsyntactic belief.

([12]) Though Rescher, Russell, and Peirce, for instance, may well advocate epistemic humility, they do not follow through on bracketing belief. And as soon as Popper is comfortable discussing truth, he introduces the epistemic realist's thesis that corroboration indicates verisimilitude; moreover he at least appears to advocate believing the truth of singular empirical predictions. (Additionally, I do not embrace what I take to be the other key components of Popperianism, e.g., the denial that science employs induction, an obsession with a demarcation criterion and falsifiability, the claim that scientists seek to falsify their theories, the demand for content-retention across theory change, etc.)

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