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S.I.: NEW THINKING ABOUT SCIENTIFIC REALISM

Epistemic selectivity, historical threats, and the non-epistemic tenets of scientific realism

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Abstract The scientific realism debate has now reached an entirely new level of sophistication. Faced with increasingly focused challenges, epistemic scientific realists have appropriately revised their basic meta-hypothesis that successful scientific theories are approximately true: they have emphasized criteria that render realism far more selective and, so, plausible. As a framework for discussion, I use what I take to be the most influential current variant of selective epistemic realism, deployment realism. Toward the identification of new case studies that challenge this form of realism, I break away from the standard list and look to the history of celestial mechanics, with an emphasis on twentieth century advances. I then articulate two purely deductive arguments that, I argue, properly capture the historical threat to realism. I contend that both the content and form of these novel challenges seriously threaten selective epistemic realism. I conclude on a positive note, however, arguing for selective realism at a higher level. Even in the face of threats to its epistemic tenet, scientific realism need not be rejected outright: concern with belief can be bracketed while nonetheless advocating core realist tenets. I show that, in contrast with epistemic deployment realism, a purely axiological scientific realism can account for key scientific practices made salient in my twentieth century case studies. And embracing the realists favored account of inference, inference to the best explanation, while pointing to a set of the most promising alternative selective realist meta-hypothesis, I show how testing the latter can be immensely valuable to our understanding of science.

Keywords Scientific realism · The no-miracles argument · The pessimistic meta-induction · Selective realism · Axiological scientific realism · Socratic scientific realism · The meta-*modus tollens*

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1 Introduction

Scientific realists offer an empirical meta-hypothesis about scientific theories that they claim we can justifiably believe. In its unrefined formulation, that meta-hypothesis is "successful scientific theories are (approximately) true." The justification for believing that the second correlate of this meta-hypothesis can be conjoined to the first correlate is that the second, (approximate) truth, constitutes the only explanation of the first, success: "it would be a miracle were the theory to be so successful were it not at least approximately true." And if approximate truth is not the only explanation, it is at least the best. Prompted by, for instance, Laudan (1981) to address empirical, historical challenges to that unrefined version of the realist meta-hypothesis, epistemic realists have appropriately revised the correlates of that meta-hypothesis (and accordingly, the elements of their explanatory justification). Specifically, with new criteria introduced to pick out particular constituents of scientific theories, contemporary scientific realism is now far more selective, and hence more plausible. A most prominent variant, which I will use as an initial framework, says we can justifiably believe the meta-hypothesis, "those constituents that are genuinely deployed in the derivation of successful novel predictions are approximately true." Since any tenable realism will be "selective," in want of capturing the demarcating property of this particular selective realism, looking to Kitcher, it is appropriately dubbed *deployment realism*.

In this paper, toward the identification of new case studies that challenge deployment realism, I break away from the standard list by looking to the history of celestial mechanics, emphasizing twentieth century advances. Doing so I bring to the fore an overlooked problem for realists: dramatically false idealizations are deployed in the derivation of key predictions; and, irrespective of realist lip-service to idealizations, that fact stands in significant conflict with the epistemic position they embrace. I then articulate two purely deductive arguments that, I contend properly capture the severity of the historical threat to epistemic realism. I argue, nonetheless, for a selective realism at a higher level: concern with belief can be bracketed while still advocating core realist tenets. I show that, in contrast with epistemic deployment realism, a purely axiological scientific realism can account for a key scientific practice made salient in my case studies, offering an axiological solution to the problem of idealizations. And embracing the realist's favored description of inference, inference to the best explanation, while pointing to a set of the most promising alternative selective realist meta-hypotheses, I offer a unifying solution, showing how testing the latter can be immensely valuable to our understanding of scientific inference. Doing so, I offer a realist solution to a major problem for epistemic realists: in response to epistemic concerns, realist commitments have narrowed so drastically that they are no longer able to provide an encompassing account of science in general.

2 Case studies from the history of celestial mechanics

With the above sophisticated variant of selective realism in hand, we can survey a number of historical case studies that threaten it. Breaking from appeal to theories in Laudan's now infamous list—eighteenth and nineteenth century theories positing phlogiston, caloric, ether, as well as older theories—I look to the history of celestial mechanics, emphasizing, in the end, twentieth century advances. For some momentum going into the cases I offer here, we can look to theorizing by Kepler, Newton, Leverrier and Adams, as I've detailed in Lyons (2006).[1] The novel predictions include Kepler's predictions that the sun spins, that it spins along the plane of the ecliptic, that it spins faster than the planets revolve around it, along with Kepler's prediction of Mercurial and Venusian transits etc. Added to these are novel predictions derived from Newton's theory such as non-Keplerian perturbations, the returns of Haley's comet, and gravity assists, etc. Finally, we have Leverrier's and Adams's independent predictions of the existence of a previously unrecognized planet and its location in an enormous sky when viewed from the Earth. Toward all these predictions numerous theoretical constituents were deployed that, by present lights, cannot be even approximately true. Included among them are, for Kepler, the posits that the sun is a divine being, that the natural state of celestial objects is rest, that a force emanating from the sun pushes the planets forward in their orbits, that light diminishes inversely with distance and so does the sun's push. For Newton, the deployed constituents in his (1684) include the posit that objects have an innate force, that there are only two point-mass objects in the universe, a sun and a planet, etc. Leverrier and Adams deployed numerous false constituents pertaining not only to Uranus itself, for instance, Uranus's mass, orbital period, and eccentricity, but also to the trans-Uranian planet. For instance, the orbit posited for the latter was far too eccentric, exceeding Neptune's present value by 12.5 times for Leverrier and 14 times for Adams. Leverrier's semi-major axis was off by six times the distance between the Earth and sun; Adams by seven times. The orbital periods posited by both were more than half an earth century in excess of the value now accepted for Neptune. Compared to the $44°58'$ presently attributed to the longitude of the perihelion, Leverrier posited it to be $284°45'$ and Adams posited it to be $299°11'$. The distance between the semi-major axes of Uranus and Neptune was on Leverrier's account off by nearly 17 times and on Adams's account more than 18 times the earth's distance to the sun. Leverrier's posit on the value of Neptune's mass was off by over 18 times the earth's mass and Adams's value overreached by nearly 33 times the earth's mass; for Leverrier's, that's twice as large, and for Adams's nearly three times as large, as today's value. In short, it was due to the large collection of false posits that Leverrier and Adams were able to successfully predict both the existence and the observed location of a trans-Uranian planet, never previously observed as such.

Toward later successes, note that other Newtonians—in particular, John Michell in 1784 and Johann Geor von Soldner in 1801—deployed the following constituents to obtain a collection of novel predictions whose successes would only be recognized in the twentieth century: light is not wavelike; it is made up of corpuscles; light corpuscles have mass; "I treat a light ray as a heavy body" (von Soldner 1801, p. 948); light corpuscles follow the same laws as matter; "Light rays have all the absolute [basic] properties of matter" (translator's brackets, von Soldner 1801, p. 948); the "velocity of light" of light varies (Michell 1784, p. 37); an instantaneous gravitational force

[1] In fact, a recent study suggests we can go back farther in astronomy: due to its largely unrecognized novel successes, the Ptolemaic model deserves much greater attention than realists and non-realists have tended to suppose (Carman and Díez 2015).

affects that velocity. From these constituents, Michell reasoned that a luminous massive object will pull back on the light corpuscles it emits, causing them to decrease in proportion to that object's mass and in inverse relation to the square of the distance toward a second body, say, the earth. The velocity of light corpuscles will determine the way in which those corpuscles interact with a prism, and, so, the latter can reveal a difference in corpuscular velocity. With this line of reasoning, decades before the invention of the spectroscope, we arrive at the novel prediction that information about the properties, e.g., the mass, of distant stars can be gathered by measuring "the refrangibility of the light" (51). Moreover, Michell predicts that an observable spectral shift will permit us to differentiate between the relative masses of two objects. Finally, the masses of "systems of bodies revolving about each other" (56), binary star systems, can be compared by looking for shifts in spectral lines. von Soldner similarly infers from the above constituents that, because an instantaneous action at a distance gravitational force will pull on light corpuscles, "The light ray will . . . be forced [to curve], because of the attraction of the celestial body" (von Soldner 1801, p. 940). The title of von Soldner's text expresses his prediction explicitly: "On the deviation of a light ray from its motion along a straight line through the attraction of a celestial body which it passes close by." Here we have Newtonian versions of what Einstein dubbed ("apart" from Mercurial corrections) the "only two deductions from" (1916, p. 123) his theory of general relativity "which admit of being tested by observation, to wit, the curvature of light rays by the gravitational field, and a displacement of the spectral lines of light reaching us from large stars" (p. 124). (For more details on Michell and von Soldner see my (Lyons, forthcoming)). With the momentum gathered by these successes, we can now bring into the realism debate some early—and in particular, solar and cosmological—solutions to Einstein's equation, each of which involved false constituents deployed toward some of the most important temporally novel predictive successes of the twentieth century.

The primary successes attributed to Einstein's theory—whose Newtonian counterparts we've just seen—were derived from Schwarzschild's (1916a) solution. In their textbook, the Large Scale Structure of Space-time, Hawking and Ellis write, "All the experiments which have so far been carried out to test the difference between the General Theory of Relativity and Newtonian theory are based on predictions by this solution" (1975, p. 149). They write, "The Schwarzschild solution . . . represents the spherically symmetric empty-space outside a spherically symmetric" non-rotating point mass (p. 149). Cutting to the chase—and unaffected by our unreflective lip-service to "idealizations"—a central worry is that, although these predictive successes pertain to our solar system, the exact solution deployed by Schwarzschild to obtain them constitutes a situation that simply fails to obtain with respect to that system. Among the patently false constituents on which the predictions depend are the posits that the universe consists of only a single spherically symmetric point mass, the sun, and a test particle; that the sun is time independent, "unchanged with time" (1916a, p. 11); that the sun is perfectly spherical, "spherically symmetric"; that the sun is non-rotating. To eliminate those constituents from Schwarzschild's text is to eliminate its coordinate system, and without its coordinate system that text offers no solution and no successful predictions. However, as should be clear, each such constituent is false: the sun has a radius, is oblate, has energy fluctuations, e.g., solar flames; is perturbed by planets; it fluctuates in temperature, produces and is affected by solar winds; it bulges, compresses, deforms, is not an incompressible fluid; pressure is not equal throughout; and of course it rotates, a fact taken to be central to other tests of relativity such as frame dragging. Moreover, the Schwarzschild solution includes two singularities. One has since come to be understood as genuine, which would shock early relativists. And the other has come to be understood as the result of bad coordinates, pointed out for instance by Kruskal (1960) and Szekeres (1960), independently. Just as there is no location on the earth that is north of the earth's most northern point, that coordinate singularity amounts to a false posit: it is impossible that the coordinates be realized by the sun, or for that matter, any other physical object. They map onto nothing in reality.

In a dramatic jump outward from the solar system, Einstein took to the structure of the universe in his (1917a). Very explicitly, deploying the posits that the universe is static, homogeneously filled, and spherical, he found only one way to prevent it from collapsing upon itself: he introduced a repulsive force into his equation, his cosmological constant, λ. In the aftermath of the Nobel Prize winning 1998 studies of Type Ia supernovae, the contemporary consensus has come to be, not only that such a repulsive force exists, but that, in fact, it presently dominates our universe. Although Einstein was convinced that his was the only possible cosmological solution to his equations, Willem de Sitter soon offered another in his (1917). Along with Einstein's λ, he deployed the posit that the universe is static, "technically …an equilibrium solution" (Eddington 1930b, p. 162). However, de Sitter posited not a spherical universe but a hyperbolic universe and de Sitter's metric taken "literally . . . described a completely empty universe" (Eddington 1933, p. 356).

de Sitter's metric entails consequences not to be found in Einstein's, as Eddington explains: upon allowing vibrating atoms into "de Sitter's empty world," due to the time it takes "a pulse of light emitted by an atom" to "reach the observer" (1923, p. 164), the "vibrations of an atom become slower" (157). In de Sitter's words, "the frequency of light-vibrations diminishes with increasing distance from the origin of co-ordinates" (1917, p. 26) resulting in a red shift: the "lines in the spectra of very distant stars or nebulae must" on the basis of his theory "be systematically displaced towards the red" (26). Looking back at this temporally novel prediction, Eddington wrote, "we can detect by practical measurement this slowing down of atomic vibrations, because it is preserved in the transmission of the light to us" (1923, p. 157). Discarding our presentist lenses, it is crucial to recognize that this redshift depends "not on velocity" but the distant "position" of a vibrating atom (161); it does nothing to indicate outward motion. It is not, in de Sitter's own words, a genuine "positive radial velocity" but a "spurious," (1917, p. 26) merely "apparent" (27) one. The shift would only "imitate the effect of a receding velocity" (Eddington 1933, p. 358) and "would be erroneously interpreted as a motion of recession" (1923, p. 161): the "nebulae had not these motions when they emitted the light which is now examined" (164). This effect explicitly predicted from de Sitter's model marked a stark contrast with Einstein's theory. Five years after de Sitter's prediction, Eddington takes stock of data on the nebulae: "The great preponderance of positive (receding) velocities is very striking" (Eddington 1923, p. 162), with thirty-six spiral nebulae exhibiting the shift. In fact, Hubble credited de Sitter with being "the first to mention" the redshift (1931). Most tellingly, and perhaps surprising to the common contemporary understanding, it was de Sitter's prediction, in particular, that took centrality in Hubble's (1929): "The outstanding feature here, however, is the possibility that the velocity-distance relation may represent the de Sitter effect" (168).

Beyond the solar system, then, two more of the most heralded results of twentieth century science were attained via sets of false constituents. Looking at those 1917 metrics, de Sitter writes, "we were in the position of having two possible solutions: the static universe with matter but without expansion, and the empty universe with expansion but without matter" (1932, p. 122). He says, "the actual universe . . . . differs so much from both of these that neither can be used as an appropriate grand scale model" (1932, p. 123). Eddington writes, "Einstein's solution gave the only possible condition of equilibrium of the universe, and now this proves to be unstable. De Sitter's is also reckoned technically as an equilibrium solution, but it is a bit of a fraud; being entirely empty, there is nothing in his world whose equilibrium could be upset" (1930b, p. 162). de Sitter replied, "I agree entirely with Prof. Eddington's remarks. Einstein's solution gives a world full of matter, but no motion; mine gives a world full of motion, but no matter" (1930a, p. 163). On de Sitter's solution, Eddington granted that it would be non-static were it to contain matter; "but as there isn't any matter in it that does not matter" (1930a, p. 39). de Sitter's world, as Einstein put it, "could only exist ... if there were no stars" (1917b, p. 340). A more detailed evaluation comes from de Sitter himself in 1930, where he then considers Hubble's (1929) data. There he writes, "The only acceptable explanation" of the redshift is the solution in his own (1917) paper, but he admits that it can "only be admitted if the value of" the mean density of mass in the universe "is so small" that a mean density of zero "can be considered a good approximation" (1930b, p. 170). However, he argues Einstein's solution gives, for the mean density, only one tenth of the value that had been determined by observation; and since it is "certain that" both Einstein's solution and his own "are of the same order of magnitude" (171), both solutions are unacceptable. de Sitter concludes that, neither solution "can correspond to the truth," and importantly, with respect to his own at least, "not" even as "an approximation." [italics in original] (171). Moreover, rather crucially, it is of course patently false by contemporary lights that (what we now call) the galactic red shift is, in Eddington's words "caused by the slowing down of atomic vibrations" (1923, p. 164). In short, the false posit of a static universe required the use of λ, a repulsive force, and was thereby genuinely deployed in the novel prediction of what we now call "dark energy," taken to be confirmed in 1998 and since: Einstein predicted that which is now taken to make up 70 percent of our universe. Meanwhile the patently false posits that the universe is static and contains no matter, but when atoms were put into it, their vibrations would slow—these posits were genuinely deployed in the prediction of the red shift confirmed by Hubble and others.[2]

3 The nature of the historical threat

Rather surprisingly, in contrast with my introductory paragraph, much of the literature characterizes scientific realism as the view that, since only the truth of scientific theo-

[2] Notably, some of these are, rather than highly localized and singular, properly seen as generalizations. See Lyons (2006) where, in agreement with Stathis Psillos (1999), I argue that even more credit must be attributed to those theoretical constituents deployed by scientists towards what come to be accepted generalizations.

ries can explain their success, and since believing that P is true amounts to believing P (and vice versa), we can justifiably believe our best scientific theories: we can simply take them at face value. Though this evades realist worries about "approximate truth," we realize that any genuine kind of success involves an entire system of statements; and, of course, to accept that every statement included in a scientific system is true per se is patently untenable, if for no reason but that science would have made little progress without denying constituents in the system. Further, even to map our beliefs onto only what has been accepted by scientists, we commit ourselves to believing innumerably many theoretical constituents accepted at some stage in the history of science, those indicated by Laudan's list constituting only a small sample. Our belief system would be replete with dramatic contradictions, some blatant upon even minimal consideration. Reflective realists realize they are forced to restrict their commitments to specific posits and in particular those that meet strict criteria, such as novel predictive success. But notice that forced, as one is, to impose such restrictions, the hope for face-value realism quickly erodes: the object of belief is not an accepted scientific theory/system itself but instead a meta-hypothesis about particular parts of such systems. Struggling to retain the minimal spirit of face value realism, the restricted meta-hypothesis one might claim to believe is "those constituents deployed in novel predictive successes are true." However, in light of the cases traced above, this meta-hypothesis is clearly false. As face-value realism disappears behind us, what scientific realists claim we can justifiably believe is a meta-hypothesis that picks out specific constituents as approximately true. Recognizing the need for such a meta-hypothesis, we can now gain clarity on the way in which cases of the kind above threaten deployment realism.

The historical argument against realism is ubiquitously characterized as a "pessimistic meta-induction," its premise taken to provide positive instances toward a non-realist conclusion that our successful scientific theories are (likely) false. However, I will now hope to make clear that this characterization has been little more than a pro-realist distraction: when properly construed—or, at least, when the historical cases are properly repurposed[3]—the historical argument involves neither a meta-induction nor a conclusion that our scientific theories are (likely) false (Lyons 2002). Recognizing now that it is a meta-hypothesis that the realist claims we can justifiably believe, we can see the historical argument as a (bi-layered) *modus tollens:*

1. If (a) the realist meta-hypothesis were true, then (b) none of the constituents genuinely deployed toward successes would be such that they cannot be approximately true. (If they were, on the realist's no-miracles argument, such constituents would constitute "miracles" which no one of us accepts.)
2. However, (not-b) we do find constituents genuinely deployed toward success that cannot be approximately true: the list (of "miracles").
3. Therefore, (not-a) the realist meta-hypothesis is false. (And the no-miracles argument put forward to justify that meta-hypothesis is unacceptable.)

[3] An anonymous referee has emphasized that I should express my *modus tollens* arguments (the second one will be discussed below), not as a proper construal of the historical argument, but as my own repurposing of the same sorts of examples for stronger forms of argument.

Because the realist meta-hypothesis is simply false (and the no-miracles argument unacceptable), we are not justified in believing it; it cannot even be accepted as a "fallible" or "defeasible"—let alone a "likely"—conjecture. Notice also, the *modus tollens* requires no commitment as to which specific scientific constituents among the mutually contradictory successful constituents are the false ones: the falsity may lie in the past constituents, in those of contemporary theories that directly contradict them, or both.[4] As indicated by the parentheses, this *modus tollens* argument has two layers. On neither layer is the endeavor to empirically increase the quantity of historical instances to strengthen an inductive inference. On the first layer, it is to secure the truth of the pivotal second premise, or the non-ampliative, non-universal falsifying hypothesis "Included among the set of those constituents genuinely deployed in the derivation of successful novel predictions are constituents that are not even approximately true." However, the second layer is no less important[5] and is what drives the quest to increase the items in the relevant set: the greater the quantity of "miracles," the more obvious it becomes that the core claim of the no-miracles argument is false and that the realist argument as a whole is unacceptable. That obviousness is inversely related to justificatory promise: as the former increases, the promise for justifying the realist meta-hypothesis decreases. On that note, the second layer of the *modus tollens* bears also on a retreat to approximate truth as the *best* explanation of success: 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. For each such counterinstance, it is now realism that fails to live up to its own much-touted demand for explanation.

In fact, this is just one (bi-layered) *modus tollens* challenge to realism. A second conception of the historical argument involves the comparative evaluation of the realist meta-hypothesis against what we can call "ContraSR" meta-hypotheses:

- "those constituents that are genuinely deployed in the derivation of successful novel predictions are statistically unlikely to be approximately true"
- "those constituents that are genuinely deployed in the derivation of successful novel predictions are not-even-approximately-true."

Here these ContraSRs are used merely as tools for inquiry, to which no one need commit themselves. And clearly consideration of the relation between a given hypothesis and the data involves no inference back to the given hypothesis: 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 hypoth-

[4] Effectively, then, what is meant by the shorthand above that a constituent is "patently false" is that the constituent does not approximate by any reasonable stretch contemporary constituents that replaced it, requiring no specification of where the falsity lies. Also, because the sole justification for epistemic realism requires that the realist posit explains success—and insofar as that which does the explaining must make that which is explained "a matter of course," as Peirce puts it—realists, despite temptation, cannot radically stretch their notion of approximate truth. See Lyons (2003, 2006, p. 556).

[5] In a recent critique, Psillos (2016) challenges only the first layer of my *modus tollens* on the grounds that it does not push us to look closely at the history of science. However, this critique, even if it were correct, wholly depends on neglecting the second layer, here explicitly flagged as such.

esis “all swans are black”.[6] Likewise, indifferent to whether the ContraSRs 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 those meta-hypotheses. And the historical list of deployed posits provides precisely such instances, identified empirically and wholly independent of any presupposition or inference that such a ContraSR is true. Moreover, without granting victory in advance to the realist’s meta-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. I emphasize that these restricted tasks of, first, tallying such positive/negative instances for the ContraSRs and the realist meta-hypothesis and, second, recognizing the evidential superiority of the former over the latter involve no inference to, acceptance of, let alone belief in, any such empirically ampliative meta-hypothesis about successful theories. We have then a second *modus tollens*:

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 (that is, for ContraSRs).
2. However, (not-b) we do have greater evidence for those that oppose it (that is, for ContraSRs): the list (and 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).

With the list of deployed posits toward novel successes that cannot, by present lights, be 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 the former. We are not, therefore, justified in believing the scientific realist’s meta-hypothesis. A few points are in order. First, it is admittedly logical considerations that reveal that, on the one hand, instances of the correlates of the ContraSRs can be secured without presupposing the truth of the ContraSRs, and, on the other, that such a feature is not shared by the realist meta-hypothesis. However, those facts are due to the nature of the differentiating correlates in these *empirical* hypotheses and the way in which those correlates relate to the *empirical data*.[7] Second, and nonetheless, the second premise cannot be secured in any purely “analytic” manner but only empirically/historically. Third, although the truth value of the second premise will not change once the threshold of its truth is reached, as evidence increases the second premise becomes (potentially much) stronger, more secure:[8] we see, increasingly, that the realist’s justification is

[6] I use “correlatively precise” here to capture two important factors: the first is the fact that such positive instances connect directly to the correlates themselves, with no intermediary guesswork; the second, is that, nonetheless, “correlatively precise” allows us to fully and appropriately avoid misleading talk of singular instances “confirming” the hypotheses.

[7] For instance, the differentiating correlate of the realist meta-hypothesis has a lower degree of empirical precision.

[8] There is one other way to construe this third point: increasing the evidence against the realist hypothesis permits *stronger versions* of this second *modus tollens*, where the content is changed but not the structure. For instance, in Premise 2 the “greater” evidence can shift to “far greater” and “vastly greater” evidence,

neither logically nor empirically supported but, if anything, and at best, only psychologically persuasive. And here, any psychologically residual hope the realist might maintain for justifying the realist meta-hypothesis decreases as the evidence against that meta-hypothesis increases. That is another sense in which this *modus tollens* argument is historically informed and empirical. Yet it is still not the least bit inductive.

As a final note on these *modus tollens* arguments, consider the temptation to immunize deployment realism by claiming we can justifiably believe only the meta-hypothesis that "those constituents that are genuinely deployed in the derivation of successful novel predictions are *statistically likely* to be approximately true." Though this may appear to dodge the first *modus tollens*,[9] the relevance of the following ContraSR comes quickly to the fore: "those constituents that are genuinely deployed in the derivation of successful novel predictions are *statistically unlikely* to be approximately true." This meta-hypothesis is bolder than but entails the negation of the immunizing realist meta-hypothesis. And, just as with the realist's original meta-hypothesis, we have greater evidence for a bolder hypothesis that entails the negation of our immunizing hypothesis; and again, as that evidence increases, it decreases any psychologically residual hope the realist might have for justifying belief in even this statistical meta-hypothesis. Hence, such an immunization of the realist meta-hypothesis makes no difference in light of this new, second *modus tollens*. I take that fact, even alone, as sufficient motivation for the second *modus tollens*. As can be anticipated, reconstruing the historical threat against realism in terms of such *modus tollens* arguments renders impotent numerous realist defenses against "the pessimistic meta-induction."

4 A meta-selective scientific realism

Despite these serious epistemic threats, I contend that scientific realism need not be rejected outright. Acknowledging that theoretical systems contain various individual constituents, we've seen that selective epistemic realists endeavor to formulate an appropriately restrictive meta-hypothesis that we can justifiably believe. Yet just as scientific systems consist of individual constituents, so too does scientific realism; and the selective insight can be invoked at a higher level. The epistemic tenet that we can justifiably believe a meta-hypothesis is only one constituent of scientific realism. Just as the selective realist withholds belief in the ether, my proposal is that we can fruitfully bracket concern with (non-syntactic) belief altogether—or at least our obsession with what one can justifiably believe. Bracketing the epistemic tenet, we can isolate a set of non-epistemic yet still fundamental tenets of scientific realism, whose articulations have received insufficient attention, especially as compared against the epistemic tenet around which the debate has pivoted. For instance, while scientific realists emphasize that the primary aim of science is truth, just which subclass of true claims science seeks calls for explication. Also, although realists embrace "inference

Footnote 8 continued
and the Conclusion, 3, can change accordingly: it is not the case that we are, e.g., "even close to being justified" "have any chance of being justified," etc., in believing the realist's meta-hypothesis, given the evidence.

[9] Or at least the first layer of the first *modus tollens*.

to the best explanation" as the primary mode of inference for seeking truth, it is readily admitted that we lack a proper understanding of this kind of inference. Wholly bracketing our 2500-year-plus obsession with belief, such posits about the goal of science and the inference employed toward it can themselves be seen as tools for inquiry, tools deployed in the quest for truth that call for better articulation in light of empirical data.

Discarding belief, consider first the aim of science. In quick form, my postulate[10]—which should be seen as just that—is that, in the change of its theory complexes, science seeks to increase a subclass of true claims, in particular those whose truth is *experientially concretized,* those that are deductively pushed to descriptions regarding specific experiences. One can consider the 'push' here to be the activity of theorizing, for instance, that of adding, modifying, replacing—or, as below, even temporarily invoking—auxiliary hypotheses. Add to this the familiar notion of truth preservation. Experiential concretization obtains when the truth of a given theoretical statement is made to deductively impact, is channelled through an entire deduction to, and so preserved in, a conclusion that matches a description regarding specific experiences. According to the postulate, the subclass of true statements, including those that are deeply theoretical, that science seeks to increase in its theory complexes are those that are experientially concretized.[11] While there are a number of issues relevant to this postulate and the successes discussed above,[12] here I will take the opportunity to indicate how blatantly false posits are to be understood from this purely axiological, goal-oriented, postulate; and how bracketing concern with belief can help us to understand what I suggest is a central component of scientific theorizing, that of deploying blatantly false posits toward empirical predictions. In short, an increase in experientially concretized truth can be achieved in two ways: first, by the new experiential concretization of *truths already present*; and/or, second, by *the introduction of new truths* that are experientially concretized. And recognizing those two ways by which the goal can be achieved, even blatant falsity can be deployed in the service of inquiry, and specifically in the quest for increasing a system's experientially concretized truth. That is, patently false posits can serve toward 'bringing the truth' of high level posits 'down' to statements that describe experiences. Once novel predictions are derived, even from false hypotheses, and new data is gathered, then, on this view, those mediating blatantly false constituents previously deployed are eliminated in want of increasing the experientially concretized truth of the accepted system. Avoiding an increase in blatant falsity is required by the axiological posit only in what is *accepted*; and those blatantly false constituents, though deployed toward novel successes—so such that the deployment realist is committed to believing they are approximately

[10] This position promotes the quest for truth without claiming to possess or approximate it. Elsewhere, I've dubbed this position, 'Socratic scientific realism' (Lyons 2015).

[11] For the details of this postulate and its empirically testable consequences, see my (2005).

[12] For instance, one issue that naturally arises is whether construing science as seeking an epistemically utopian goal—as I allow truth, and even an increase in experientially concretized truth, to be—sacrifices one's ability to construe science as a rational endeavor. Although I set this topic to the side here, I argue in detail in my (2005 and 2011) that it does not.

true—are no part of the *accepted* system.[13] Put otherwise, the thesis is that scientists do the predictive work, then gather up the potential truths introduced in the course of that work, leaving the blatant falsity to the side.

On this view, solutions to Einstein's field equations of the kind seen above—Schwarzschild's, Einstein's, and de Sitters—are, despite their deployment of obvious falsity, understood as attempts to experientially concretize the (potential) truth of Einstein's field equation. And more broadly, they are steps to bring about, in the overall system, an increase in experientially concretized truth. Following his initial solution, Schwarzschild (1916b) introduced his own second solution, explicitly denying some of the plainly false components he had deployed in the first.[14] Upon the later discovery of Schwarzschild's coordinate singularity, new solutions sought to eliminate it. Similarly, the later Kerr solution (1963) introduced rotation and allowed for at least non-axial asymmetry. Each such solution can be seen as part of a series of step-by-step attempts to increase in experiential concretization of the deep Einsteinian (potential) truths: each introduces, either by posit or derivation, new near-empirical-level, and other potential, truths (e.g. about relations between light and space time, frame-dragging, etc.), all of which aid in the experiential concretization of the potential truth of the field equations. What is brought into *acceptance* includes the new successful empirical posits along with other components however deep, that are not in conflict with the larger system. In significant contrast, an epistemic realist committed to believing the approximate truth of the constituents that are deployed toward novel success wholly fails to capture what is going on in the deployment of obvious falsity.[15] We are prompted to discard concern with (at least this kind of) belief and to limit our concern to choice between, and acceptance of, theoretical systems. Doing so, the deployment of falsity is seen to be, not merely unproblematic, but a pivotal strategy in the quest for truth. In basic terms, according to the axiological realist conception of science, false posits are deployed as a tool toward the attainment of not only more truth (e.g., new truths) but also toward the conceptual connection of those potential truths already in the system (e.g. the deeper theory) to (descriptions regarding specific) experiences.

In addition to the axiological postulate, another scientific realist tenet that I contend can be advocated as a tool for inquiry, even bracketing belief, is the posit that, generally, scientific inference amounts to inference to the best explanation.[16] Toward that end, we can welcome a new level of sophistication that has emerged from the scientific realism debate, and with it the set of minimal conditions for a selective epistemic realist meta-hypothesis. Such a meta-hypothesis must be, first, explanatorily relevant: to explain success, it must pick out constituents that are genuinely responsi-

[13] On the axiological realist account, those components in the *accepted* system are likewise deployed in the service of inquiry, but, in contrast, remain candidates for truth and hence for its experiential concretization.

[14] Likewise for Newton after his pre-Principia (1684) arrival at his law of universal gravitation mentioned above.

[15] That is, their explicit belief commitments stand in contradiction with any lip-service to “idealizations” they might offer unreflectively.

[16] Though I offer here no articulation of the nature of such inference, the importance of Lipton's (2004) is undiminished by its incompleteness.

ble, and so deserving of credit, for success; second, ascertainable: to have any content at all, i.e., to inform us of what we can justifiably believe (and avoid the charge of 'ad hocery'), it must allow identification of just which theoretical constituents qualify; and third, sufficiently realist: to go beyond anti-realism, it must pick out constituents that reach to a level deeper than the empirical data. Although numerous options may fail to meet these requirements, I suggest these conditions license, as with deployment realism, a subset of selective realist criteria. And we can see the historical *modus tollens* arguments as imposing a fourth condition: although a criterion may be relevant, testable, and adequately realist, we need to direct the historical data against the meta-hypothesis containing that criterion. I will suggest below that, even wholly bracketing the question of whether, upon being tested, we can justifiably believe such meta-hypotheses, those that meet the first three criteria are extraordinarily valuable. Toward that end, going beyond deployment realism discussed above, I take the following to be among the most sophisticated realist meta-hypotheses in the contemporary literature[17]:

- Hacking: The entities that have been subjected to manipulation exist. (1983)
- Worrall: The structural claims pivotal toward novel predictive success approximate those in the world. (1989)
- Carrier: The classification offered by those theories achieving novel predictive success corresponds with natural kinds in the world. (1993)
- Chakravartty: The minimal mathematical interpretation of those theoretical posits that specify detectable properties are approximately true. (1998)
- French: The deep structural components of our successful theories are at least partially isomorphic to structures in the world. (1998, 2014)
- Ladyman: The modal components of those theories achieving novel predictive success capture genuine modalities in nature. (1998, 2007)
- Worrall (later): The Ramsey sentences of those theories that achieve novel predictive success correspond with the world. (2001, 2011)
- Ruttkamp-Bloem: Those theory networks that are interactively progressive express a genuine science-world engagement; network revisions track real target systems. (2011)
- Harker: Those theoretical constituents that generate empirical, including explanatory, progress have greater truth content than those they replace. (2013)
- Vickers: Those smallest deducible parts of the theoretical constituents that are deployed in and suffice for successful novel predictions are at least approximately true. (2013)
- Peters: Those theoretical posits that unify more accurate empirical claims than are required to construct them are at least approximately true. (2014)

While each of these meta-hypotheses invites further consideration in the context in which it was originally proposed, as a candidate for justified belief, the reader may, like me, suspect that each meta-hypothesis is threatened by the *modus tollens* arguments—in which case, as we've seen, the justification for believing such meta-hypotheses is destroyed—and/or does not live up to the above three criteria required of selective real-

[17] These are articulated only roughly and only listed roughly in the order of their introduction to the debate.

ism. My aim here, however, is not undertake such detailed evaluations but to illustrate how, even bracketing belief as I propose, the testing of such meta-hypotheses can be profoundly informative toward the development of a proper and, in fact, still realist conception of science.

Specifically, mindful of the *modus tollens*, each such candidate for epistemic realism also offers an empirical meta-hypothesis about just which theoretical constituents are retained across theory change. And these content-retention meta-hypotheses can be deployed—in the way scientific hypotheses can be deployed—as tools for further empirical inquiry, and, on the axiological realist account, as part of the endeavor to attain the kind of truth science seeks. Bracketing belief, the aim of such inquiry will not be to justify belief in the meta-hypotheses but to gain a better, empirically informed account of scientific inference writ large, for instance, of criteria involved in inference to the best explanation. My proposal is that, endeavoring to convert the above meta-hypotheses into the content-retention hypotheses they offer, and testing the latter against empirical data, we can empirically develop a distribution of content-retention priorities. The resulting order of such an empirically informed priority distribution will be inversely related to the empirically attained counterinstances, where the first priority with have the fewest. So bracketing semantic belief—and in contrast with the original posits for what we can justifiably believe—these less demanding retention hypotheses are conjectural posits whose strength and so priority-ranking will vary in light of historical data. The following is merely an example priority ranking—which is itself conjectural—that should serve to illustrate what such a priority-distribution of these retention hypotheses could look like. A successor system retains those theoretical constituents of its predecessor that …

- Priority 1: *classify phenomena* (Carrier)
- Priority 2: are central to *the interactive progressiveness of the theory network* (Ruttkamp-Bloem)
- Priority 3: express *the smallest deducible parts sufficing for successful novel predictions* (Vickers)
- Priority 4: describe *modalities that bear on novel predictive success* (Ladyman)
- Priority 5: express *deep structure that bears on novel predictive success* (French)
- Priority 6: express *the minimal mathematical interpretation of detectable properties* (Chakravartty)
- Priority 7: describe *entities that have been subjected to manipulation* (Hacking)
- Priority 8: *unify more accurate empirical claims than are required to construct them* (Peters)
- Priority 9: are involved in the *generation of empirical, including explanatory, progress* (Harker)
- Priority 10: are *equated with its Ramsey sentence* (Worrall)
- Priority 11: are *deployed in novel successes* (Psillos)
- Priority n) . . .

Conjectural though this list is,[18] it is testable—to the point that one can construe such hypotheses and prioritizations as making novel predictions—and it suffices to illustrate the idea with which I will conclude.

Despite the historical threat against epistemic scientific realism, careful attention to testable selective realist meta-hypotheses and their relevant historical data holds the promise of a far more informed articulation of the non-epistemic yet nonetheless wholly realist thesis regarding the nature of inference to the best explanation. With this more informed articulation comes a meta-system that can be deployed as a tool for further inquiry. Such a meta-system holds the promise of, not only unifying the retention consequences of the ten or so distinct realist hypotheses from which they are derived, but providing a better understanding of the nature of past scientific practice. Moreover, that understanding can have prescriptive import, possibly even affording contemporary scientists themselves liberation from some of the myths (e.g. whiggism) to which they may be naively committed. Once we have such a meta-system, we can embark on a second stage: posit competitors to it, including those that are descriptively inadequate, putting them up against one another, testing them in this second stage, not against the history of science, but to see which is most conducive to, say, kinds of empirical success. Prescriptive lessons, then, may be built on more than historical scientific practice, thereby improving inquiry to an even greater degree—such advances nonetheless being the product of precisely the kind of proposal on offer here. Similar points hold for the axiological tenet. A clear conception of the goal of inquiry can be deployed to guide future inquiry. An empirically informed conception of the goal of the best science up to the present can help to inform decisions regarding future science, potentially affecting even its content. Beyond that, in parallel to the second stage above, an empirically supported axiological posit such as the one I've pointed to earlier, could be overthrown by further axiological analysis—perhaps, say, by finding a goal of inquiry that better coheres with our other goals. With such an interplay between the descriptive and prescriptive, our current state of empirical inquiry, including its products, e.g. scientific theories, could well be seen in, say, a century to have only been in its infancy—irrespective of how impressed we may be with that state today. Hence, although I challenge the view that selective realism succeeds in picking out a meta-hypothesis we can justifiably believe, I suggest here that testing such empirical meta-hypotheses, be they content-retention or axiological hypotheses, against the history of science can be immensely valuable, perhaps even toward the advancement of scientific inquiry itself. And this, I conclude—only half-jokingly—is how the scientific realism debate can save the world.

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[18] Since some of these retention hypotheses will pick out deeper constituents while others will pick out fewer constituents, comparisons of logical strength are hardly more obvious than their as-yet untested empirical strength; nonetheless, in want of an example ordering, I offer this illustrative prioritization based on rough conjectures regarding logical strength. Admittedly, it will be surprising that Ramsey sentence retention lands so low on the priority ranking, but see my (forthcoming).

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