Realism, no meta: a realistic argument for scientific realism

In this paper I provide a novel argument for scientific realism (SR). In contrast to most recent defenses of SR, my defense of SR does not rely on the no-miracles argument (NMA). Instead, I take a more unconventional approach: I focus on the different kinds of justification available to different individuals in relation to different kinds of propositions. I maintain that this alternative focus shows that most people are warranted in believing many propositions about unobservables.

     The paper is divided into three main sections. In the first, I rehearse the main moves in the recent debate about SR. In the second, I argue that the discussion in section one enables us to see that most of the arguments in the recent debate about SR mistake their target: instead of being about SR, they are about meta-SR. I argue that what I call the JJ-principle should be rejected and, further, that if the JJ-principle *is* rejected, then meta-SR may be cleaved from SR. This enables me to advance to a position I call thin realism in the third and final section of the paper.

Section one. Summarizing the recent debate

In this section I provide a brief overview of the main argumentative moves in the recent debate about SR, beginning with a brief description of what SR is.[1]

     SR is conventionally defined in terms of unobservables. According realists, we are justified in believing what our best scientific theories tell us about unobservables. According to antirealists, we are not.

     Now this definition seems to imply that according to realists we may assert that these theories get *everything* right about the underlying nature of reality. This is sometimes referred to as naive realism. It is a useful starting point, but it is not widely held. I shall introduce more selective forms of SR momentarily.

     This definition also presupposes that there is a meaningful distinction to be made between observables and unobservables. How (and whether) that is so is a matter of debate.[2] However, such debates are not germane for my purposes. It will suffice to note that there is a *prima facie* plausible difference between observing my hand (observable) in front of my face and "observing" a positron

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[1] My overview is in the line of what followers of Lakatos might call a rational reconstruction.

[2] The *locus classicus* for this debate is (Maxwell, 1962).

(unobservable) in a cloud chamber; between observing a fire (observable) by sticking my hand in it and "observing" the early stages of the universe (unobservable) by measuring the cosmic microwave background.

The point of this is that the debate about SR is supposed to be distinct (and downstream) from debates about radical skepticism (RS) and brain-in-a-vat-like skeptical hypotheses. That is, the debate about SR is not about whether we can know that we have hands. Rather, the debate is about whether we are justified in asserting that our best scientific theories get something right about the underlying nature of reality—or whether we are justified merely in asserting that these theories "save the phenomena."

Recent discussion of SR has centered on the NMA.[3] The core idea behind the NMA is that our best scientific theories are extraordinarily successful, and the best explanation of this extraordinary success is that the theories are true. This is the best explanation because any other explanation would make the success of these theories a miracle.

Appealing to different kinds of success in the NMA will result in different forms of SR. For example, manipulative success can be used to justify what is called entity realism, a form of SR that is more selective than naive realism. The idea behind this version of the NMA is that if scientists are able to use an unobservable entity to create stable phenomena that can be investigated using independent methods, then we are justified in believing in the existence of those entities (even if not in all of the properties ascribed to those entities by the associated theories). This position is probably most famously associated with Hacking and Cartwright: to use Hacking's now perhaps hackneyed phrase, "if you can spray them, then they are real."[4]

Novel predictive success, by way of contrast, can be used to justify what is called deployment realism. The idea behind this version of the NMA is that if scientists are able to use a theory to predict novel phenomena, then we are justified in believing in the truth of the parts of the theory that are deployed in making the prediction. Note that proponents of deployment realism grant that theories often will have "idle wheels," parts that play no role in generating the predictions. The warrant conferred on the parts of the theory that are deployed does not extend (or does not extend fully) to these idle wheels. Note also that proponents of deployment realism do not insist that the novel phenomena be temporally novel: it will suffice for the phenomena to be *use*-novel, meaning that they were not used in generating the theory. In other words, the prediction should not be "cooked," for this would render the confirmation *ad hoc*.[5]

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[3] Probably most widely associated with (Putnam, 1975).

[4] (Hacking, 1983) and (Cartwright, 1983). Cartwright argues that entity realism uses "inference to the most probable cause" rather than inference to the best explanation. The appeal to probability is problematic for reasons that are discussed below. I shall not address the idea of inferring to a cause rather than an explanation except to note that in my view these two kinds of inference stand or fall together: Cartwright's simultaneous championship of the one and dismissal of the other is, I think, misguided.

[5] See, e.g., (Psillos, 1999) or (Leplin, 1997).

However, antirealists object to the NMA on a number of fronts. I shall canvass five, beginning with the problem of underdetermination, before looking briefly at a positive argument for antirealism, the pessimistic meta-induction (PMI).

The problem of underdetermination says that because there is always an empirically equivalent but inconsistent alternative for any given theory, we are not warranted in asserting the truth of either. Indeed, in a move that mirrors the NMA in reverse, some argue that because asserting merely that a theory is empirically adequate does not carry the justificatory burden of asserting that theory's truth (a justificatory burden that never can be met given the problem of underdetermination), asserting mere empirical adequacy of a theory is better than asserting truth.[6]

But realists point out that there are at least three problems with the problem of underdetermination. First, it is difficult to motivate the problem of underdetermination in a non-self-undermining way. For example, if the problem of underdetermination is motivated by induction on instances of inductive reasoning, then it straightforwardly undercuts itself.

Second, the problem of underdetermination pushes toward RS insofar as it undermines the rationale for basic beliefs like object permanence, beliefs about objects that are merely unobserved rather than unobservable.

Third and finally, realists contend that the objection is unrealistic: generally the problem faced by scientists is not a plurality of empirically equivalent theories but rather a paucity of empirically adequate theories.[7] This leads to the second antirealist objection I want to discuss, the base rate fallacy.

A base rate fallacy is an instance of faulty reasoning in which someone arrives at a faulty estimate of the probability of an event by neglecting the frequency of that event in the general population.[8] Some antirealists contend that the NMA involves just such a fallacy: the idea is that even if we do not have a plurality of theories wherewith to explain some given phenomena, as a matter of fact there are many false theories that would do so. Indeed, it is easy to show that there are many false theories that would enjoy exactly the same success as our currently best theories, so the probability that our best theories are true given that they enjoy extraordinary success is low.[9] Thus it may be seen that proponents of the NMA neglect the base rate of false successful theories, focusing instead on individuating information about our current theories and thereby committing a classic base rate fallacy.

But realists do not accept the base rate fallacy objection any more than they accept the problem of underdetermination. One prominent response is that this objection mistakes the nature of the NMA. In

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[6] (Van Fraassen, 1980).

[7] For antirealist responses, see (Dieks, 2017) or (Stanford, 2001).

[8] For a well-known discussion of this kind of fallacy in human reasoning, see (Kahneman and Tversky, 1973).

[9] (Howson 2000).

particular, realists maintain that there is a difference between the "likeliest" explanation and the "loveliest" explanation, and the NMA involves an inference to the latter rather than the former.[10] To put this another way, the goal of providing an explanation for a given phenomenon (in this case: the extraordinary success of our best scientific theories) is distinct from and not an exercise in Bayesian probability.[11] Thus, the base rate fallacy objection is based on a false presupposition: that proponents of the NMA seek to show that there is a high likelihood of our current theories being true given their extraordinary success.

This response to the base rate fallacy then leads to the third objection often raised to the NMA: that it is question-begging. In particular, antirealists do not accept inference to the best explanation (IBE) as a genuine mode of reasoning outside of inference to the likeliest explanation. Indeed, antirealists argue that the debate about SR arises precisely because they do not accept inference to the loveliest explanation. So to appeal to IBE in this guise in the NMA is to appeal to exactly what is at issue between proponents and opponents of SR.

Now realists argue that wholesale rejection of loveliest-IBE seems to steer toward RS inasmuch as, like the problem of underdetermination, it threatens beliefs about unobserved but observable things. Moreover, it is difficult to imagine a rationale for rejecting this form of IBE that would not collapse under its own weight. But the antirealist rejection of IBE is tied up with another antirealist objection: the meta *modus tollens* (MMT).

According to the MMT, study of the history of science reveals that many false scientific theories have been successful in the past: nonexistent entities have been "manipulated" to create stable and multiply investigable phenomena, and manifestly false (by modern lights) parts of theories have been deployed to generate successful predictions of novel phenomena. Thus (by *modus tollens*), even if lovely-IBE is accepted in general, it does not help in the NMA: the best explanation of our current best theories cannot be their truth.[12]

The problem with the MMT, however, is that it shows at most that loveliest-IBE is fallible. But this is something that any realist readily would concede. The only kind of reasoning that guarantees true conclusions for true premises is deductive reasoning: to admit that there are instances of loveliest-IBE that have started with true premises and ended with false conclusions is not a genuine complaint all by itself.

Moreover, to leave things there is to concede that the success of science, both past and present, is a miracle. In other words, it is something that cries out for explanation, and this is exactly where selective realisms come to the fore, for various forms of selective

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[10] (Lipton, 2004).

[11] This response to the base rate fallacy can be motivated further by pointing out that the cardinality of the set of successful false theories is the same as the cardinality of the set of successful true theories: infinite, and Bayesian probability cannot handle infinite sets.

[12] (Lyons, 2002).

realism can avoid at least some instances of the MMT by pointing out that at least some of the now-discarded parts of past theories played no role in generating past success.[13]

But this also leads to the fifth and final objection to the NMA I am going to canvass, the appeal to natural selection. Antirealists argue that the best explanation of the success of our scientific theories is not their truth but rather the environment in which they are tested. In particular, antirealists maintain that just as natural selection explains why the fittest species survive and flourish whereas others die out, so the ultra-competitive environment in which scientific theories are tested explains why our best scientific theories succeed. Thus, the appearance of genuine success in science can be explained away in the same way that the appearance of purposiveness in nature can be explained away.[14]

But realists point out that the appeal to natural selection in this context is doubly flawed. For one thing, it explains only why our theories *en masse* are (or appear) successful; it fails to explain why any individual theory is (or continues to be) successful. Thus, survival of the most successful does not fill the explanatory gap of the NMA.[15] For another thing, the appeal to natural selection is self-undermining. That is, if this explanation is accepted, then it provides grounds for thinking that its success is merely the result of natural selection rather than its truth. Thus it undercuts its own justification.

As noted above, in addition to these five objections antirealists also often advance an independent argument for their position, the PMI.[16] This argument builds on the historical considerations in the MMT. According to the PMI, the history of science is a history of failed (even if often briefly successful) ideas. A straightforward induction on the history of science therefore reveals that we should be skeptical of our current theories, for they too are likely to fail.

The problem with the PMI, however, is threefold. First, it is an instance of inductive reasoning that is supposed to generate skepticism about current instances of inductive reasoning by looking at past failed instances of inductive reasoning. This is straightforwardly self-undermining.

Second, it neglects the fact that there have been plenty of successful instances of scientific reasoning (indeed in many instances we would not have known of the failures were it not for the successes).

Third and finally, realists contend that just as new athletic records are better and, thus, more difficult to break than old ones, so new scientific theories are better and, thus, more difficult to supersede than old ones, revealing a problem internal to the PMI.[17]

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[13] This is also where notions of approximate truth and verisimilitude come into play. But these notions are notoriously difficult to make precise.

[14] (Van Fraassen, 1980).

[15] (Psillos, 1999) and (Lipton, 2004).

[16] The argument usually is traced back to (Laudan, 1981). But see (Lyons, 2002).

[17] (Leplin, 1997).

Antirealists have responses to these realist talking points and the debate rages on. Moreover, it is worth noting that the debate is shifting ever more toward the empirical, emphasizing the history of science as a means of substantiating important lessons on both sides (such as the antirealist lesson that theorists (sometimes self-consciously) make ineliminable appeal to false posits in making novel predictions). However, this is hopefully sufficient to illustrate the character of the debate. I turn now to section two.

Section two. MSR and the JJ-principle

I want to distinguish meta-scientific realism (MSR) from scientific realism *simpliciter*. The difference between these two positions has to do with what has to be justified for the position to hold. As noted in section one, according to proponents of SR, we are warranted in believing (at least some of) what our best scientific theories tell us about unobservables. According to MSR, by way of contrast, we are warranted in believing that we are warranted in believing what our best scientific theories tell us about unobservables. Or, more simply, according to MSR, we are warranted in believing that SR is true.

The *de re/de dicto* distinction can be useful in spelling out the SR/MSR distinction. A realist might assert that we are justified in believing what our best scientific theories tell us about unobservables. Taking this as a *de re* assertion, this would be an assertion of SR. Taking this as a *de dicto* assertion, this would be an assertion of MSR. This demonstrates how close the connection between SR and MSR is.

Indeed, it is precisely because the connection between SR and MSR is so tight that confusion can arise here: the two positions can be conflated. But such conflation, I think, is fatal, for the two positions are distinct notwithstanding their proximity, and I think MSR is much more difficult to defend than SR.

In order better to understand how these two positions come apart, suppose that SR and MSR were defined in terms of knowledge rather than in terms of justification. A crucial difference between knowledge and justification is that the former is generally taken to be factive whereas the latter is generally taken to be fallible.[18] Thus, if SR and MSR were defined in terms of knowledge, then, because KK—>K (i.e., knowledge that knowledge that P entails knowledge that P), MSR would entail SR. Let me explain.

Let SR be the position that we *know* (rather than merely are justified in believing) the content of what our best scientific theories tell us about unobservables. Then let MSR be the position that we know that we know this content (i.e., we know that SR is true). Then plainly if MSR holds, so does SR: if we know that we know that P, then (because knowledge is factive) we know that P.

However, I want to say three things about this. First, from this it may be seen that KK—>K follows from the facticity condition on

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[18] The knowledge/justification distinction along these lines is sometimes taken to be a species of the success/attempt distinction genus.

knowledge.[19] But the converse of KK—>K, K—>KK (often called the KK-principle), holds only if knowledge is luminous, and there are good grounds for thinking that it is not.[20] Because of this even if SR and MSR were defined in terms of knowledge (rather than justification), SR would not entail MSR and, therefore, the falsity of MSR would not entail the falsity of SR. This should be unsurprising: even a realist should concede that a practicing scientist, someone entrenched in current scientific theory, might be at sea in debates about epistemology and the philosophy of science and, thus, know various claims about unobservables while nonetheless not knowing that s/he knows these claims. I shall return to and expand on this point in section three.

Second, exactly what makes KK—>K true is what makes knowledge unsuitable for current debates about SR. That is, although *scientia* was associated with certainty in the enlightenment, the closest that contemporary realists want to get to certainty is warranted assertibility, the idea that we are warranted in making knowledge assertions. But warrant, a form of justification, is fallible, not factive. I shall return to this, too, in section three.

This leads me to my third and final point: the appeal to justification makes it even clearer that SR does not entail MSR. To get from SR to MSR, one must appeal to the principle that J—>JJ (the JJ-principle), and there are even stronger grounds for rejecting the JJ-principle than there are for rejecting the KK-principle. But if the JJ-principle is rejected, then someone might have justified beliefs about unobservables even though s/he is not warranted in believing that s/he does. This is important for me because it enables me to argue for SR without engaging in the current MSR debate. So here are two reasons why I think the JJ-principle should be rejected.

At the level of intuition, consider again the practicing scientist example from above: a scientist awash in current scientific theory but adrift in current philosophy. The point of this example is that such a scientist would have no justification for various epistemic principles s/he might appeal to in order to give warrant to his/her beliefs about unobservable entities. In our age of ultra-specialization, I suggest that this will agree with pre-theoretic intuition.

At the level of theory, note that the contrapositive of the JJ-principle is what results in the regress problem for justified belief. That is, the contrapositive of the JJ-principle says that if you do not have justification for the justification of your justified belief that P, then the justification for P is illusory. From this it seems to follow that none of our beliefs is justified, for justification always stops somewhere. But most of us think that we do have justified beliefs. So the contrapositive of the JJ-principle should be

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[19] An audience member once objected that I have the entailment relations here wrong: the facticity condition follows from KK—>K, not the other way around. I think that this objection is mistaken, but I do not think it really matters for my purposes.

[20] (Hemp, ND) or (Bosley, 1993).

jettisoned. And the JJ-principle is logically equivalent to its contrapositive, so the JJ-principle should be rejected.[21]

Now it might be thought that the JJ-principle can be retained by attacking the regress problem in a different way. In particular, it might be argued that some propositions are self-evident in the sense that to understand them is to accept them as true. Further, it might be argued that such propositions can play the role of regress-stoppers, for they are self-justifying. This, of course, is the idea behind foundationalist theories of justification, whence it might be objected that my rejection of the JJ-principle carries a heavy justificatory burden: perhaps foundationalism should be rejected, but more argument is needed than I have given in the last two paragraphs to warrant kicking aside the likes of Descartes. I would like to say two things about this.

First, foundationalism does not stand or fall with the JJ-principle. To see why, note that a foundationalist might distinguish between thin justification and thick justification. A belief has *thin* justification if it is justified. A belief has *thick* justification if but only if it is self-evident or can be derived from self-evident premises.[22] In the same way that a house might have weak foundations, a belief might have merely thin justification: it can be traced back to other propositions, but it cannot be traced back (only) to self-evident propositions.[23] Accepting thin justification as a species of justification would enable a foundationalist to reject the JJ-principle and the regress problem in one fell swoop.[24]

Second, the fact that the JJ-principle gives rise to the regress problem is not the only doctrinal reason for rejecting it. For example, note that the JJ-principle clearly does not hold for doxastic justification. Moreover, the JJ-principle makes fallibilism about justification more difficult to motivate. Thus, insofar as we want to retain fallibilism about justification, the JJ-principle probably

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[21] This argument could be reframed in terms of philosophy of language: if any theoretical principle about justification entails that most people do not have (m)any justified beliefs, then this principle is not really about justification properly speaking. At best, it is about some close analog of justification, shmustification.

[22] This distinction easily could be refined to account for degrees of justification, but such refinement is beyond the scope of the current investigation.

[23] Or perhaps it can be traced back to self-evident propositions but the propositions are not *recognized* as self-evident. From this remark it may be seen that complications arise here with regard to who is doing the "tracing." I shall overlook these complications for now, although they will come up again in section three of this paper.

[24] Thin justification and thick justification are species of the genus of (what Sayre-McCord calls) positive justification (Sayre-McCord, 1996): it is the common rejection of *permissive* justification that unites different schools of foundationalism.

should be rejected.[25] Finally, it is notable that the JJ-principle is not (immediately) self-evident, nor am I aware of any foundationalist having attempted to ground the JJ-principle in self-evident truths. This does not entail but it does suggest that the JJ-principle is self-undermining.

But if the JJ-principle is rejected as I have been advocating, then, as noted above, MSR can be separated from SR. That is, rejection of the JJ-principle makes conceptual space for a position that might be called thin realism, a position according to which we are justified in believing scientific claims about unobservables although we might not be justified in believing SR. A thin realist need not reject MSR. But s/he also might not accept it. It simply might not be on the radar.

This is important because there is an abundance of reasons for thinking that the current debate about SR is indeed just that: *about* SR and, thus, an exercise in *meta*-SR. Indeed, this seems to be recognized by many of the participants in the debate as may be seen from the names of the arguments discussed in the previous section: the pessimistic **meta**-induction and the **meta** *modus tollens*. Or consider Psillos' description of the NMA:

> NMA is a kind of *meta-abduction*. The explanandum of NMA is a general feature of scientific methodology—its reliability for yielding correct predictions. NMA asserts that the best explanation of why scientific methodology has the contingent feature of yielding correct predictions is that the theories which are implicated in this methodology are relevantly approximately true.[26]

But perhaps the most striking evidence of the meta-ness of the current debate comes from the fact that the considerations are so general: the debate is about whether scientific theories *in general* get things right about unobservables *in general*. This is not a debate about the justification of specific claims about specific kinds of unobservables; it is a general debate about whether we (in general) are generally warranted in accepting a general body of theories.

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[25] A more circuitous objection to my rejection of the JJ-principle might go as follows: surely justification is luminous even if knowledge is not. So it seems like we might be warranted in asserting: J—>KJ. But even more surely K—>J (even reliabilists appeal to a form of justification (doxastic justification)), and these two principles entail J—>JJ (and this is plainly iterable).

The problem with this objection is that even if the luminosity of justification is granted, the formalism obscures the counterintuitive nature of J—>KJ. For example, assuming that knowledge is factive and justification is not, my belief that P very well could be justified without my knowing it.

[26] (Psillos, 1999, p. 77).

Now I do not want to assert that this debate is meaningless or unimportant.[27] But I do want to suggest that if it is to continue, its participants should be very clear about where their arguments are tending and, in particular, they should be very clear about the fact that there is still ample space for *thin* realism regardless of where this debate ends up. So let me explain how I think this space can be occupied.

Section three. Focusing on individuals' relations to individual propositions in context

I want to suggest that one of the grounds for resisting the move to thin realism is that a debate about thin realism is bound to be much messier than a debate about MSR. There are two main reasons for this. Outlining them will enable me to describe and defend the position in more detail.

The first is that whereas MSR is about the justification for a single proposition, thin realism is about the justifications for a large number of propositions. That is, instead of looking for arguments to justify the claim that our best scientific theories make true claims about unobservables writ large (*modulo* selection criteria), the focus is on the actual claims that these theories make about unobservables. For instance, one might be a thin realist about a claim like "the mass of an electron is approximately 9.11×10^{-31} kg" or "gravity is an immediate action at a distance force."

As a matter of detail it might be objected that the claim about gravity being an immediate action at a distance force, although true

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[27] *pace* Fine. If I were to challenge the meaningfulness of these arguments, it would not be on the grounds that Fine cites: it would be on the grounds that they are too broad. For one thing, they rest on a poorly understood distinction between science and non-science. For another, "science" issues no unified clarion call for belief about unobservables. Indeed, "science" says nothing and there are no static scientific theories. There are simply many scientists hard at work (and, of course, many hardly at work) and, problematically for this debate, many of them issue conflicting claims about unobservables.

One way to get around the first problem would be to attack claims about unobservables regardless of their provenance. But this strategy faces three problems. First, it would deprive antirealists of the ability to insulate mathematics from the debate about unobservables, for the claim that mathematics is not a science, even if true, no longer would be germane. This strategy thereby would force antirealists to take on various positions in the philosophy of mathematics that they might not want to be burdened with.

Second, this strategy only would magnify the problem of conflicting claims about unobservables. I return to this issue in note 33 below.

Third and finally, this strategy risks making antirealism self-undermining. That is because theoretical positions, theories and propositions do not seem to be observable in any straightforward sense of the word, and if antirealism is not limited to *scientific* claims about observables, then its proponents cannot insulate their own position from the debate (by claiming that antirealism is a philosophical rather than a scientific position). In other words, if antirealism is the theoretical position that we are not justified in believing *any* claims about unobservables, then the truth of antirealism would entail that we are not justified in believing it. Surely that is a problem!

from within a Newtonian paradigm, is manifestly false according to the currently prevailing Einsteinian paradigm: according to general relativity, the "force" of gravity (and the associated "acceleration" which is caused by this "force") is actually the manifestation of the curvature of spacetime around mass-energy.[28] Exactly this objection, however, leads me to the second reason why debates about thin realism are bound to be much messier than debates about MSR.

One might be a thin realist about this claim about gravity notwithstanding the fact that it is false, and one might be so not merely from a historical perspective. That is, one might think that agents today are warranted in believing this claim every bit as much as (and, indeed, in some instances perhaps more than) agents in England in the 1690s. This is because the move to thin realism brings with it a move to individual agents and their individual justifications for individual propositions, the second factor that makes debate about thin realism much messier than debate about MSR: different kinds of individuals might have different kinds of justifications for different kinds of propositions.

Now before I expand on the implications of this, I should point out that the fracturing of the currently monolithic debate about justification-for-a-proposition into a debate about justification-for-an-individual-for-a-proposition is not entailed by the move to thin realism *per se*. Rather, it is entailed by a view about justification, one that in principle *could* be espoused by interlocutors in the current debate about MSR even though *in fact* it generally is not.

But the move to thin realism makes it much clearer (and correspondingly harder to deny) that whether an individual is justified in believing a given claim depends on facts about her epistemic situation: the evidence that is available to her, which in turn depends on facts about her circumstances and on facts about her.

For example, whether my students are justified in believing that I am wearing my glasses when I walk into the classroom on the first day of the semester depends on factors like the lighting conditions and their visual acuity. If the electricity has been cut, they might *not* be justified. But even if the lighting is normal, the blind student sitting in the back will not be justified (assuming the student has no other means). And the fact that justification depends on an individual's epistemic situation is true regardless of the content of the proposition in question (whether it is about observables or unobservables).[29]

This brings us back to a claim I made in section two: even a realist might concede that a practicing scientist might be at sea in

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[28] For helpful discussion of why Newton's project presupposed gravity as an immediate action at a distance force (notwithstanding the fact that even Newton sometimes was tempted to reduce gravity to a contact force) see section 18 of (Friedman, 2013).

[29] This example highlights one of the challenges faced by the un/observability distinction: in the current literature the distinction is based on inductive generalizations about individuals *qua* members of an ill-defined group. This risks being self-undermining and also plays into the problems associated with the meaningfulness of the current debate gestured toward in note 27 above. However, as noted in section one I am not going to pursue these issues about un/observability beyond pointing out that if the un/observability distinction falls, the antirealism/RS distinction most likely will fall with it.

debates about epistemology and the philosophy of science. Different individuals will be in different epistemic positions with regard to different kinds of propositions in the realism debate. At the broadest level we can distinguish between practicing scientists and nonscientists. The reason for this distinction is that the kinds of evidence that are available to a practicing scientist will be very different from the kinds of evidence that are available to a nonscientist.

Whereas a practicing scientist might cite various experiments that rule out alternative explanations for a given result in order to justify a given claim about an unobservable, a nonscientist might cite testimony and authority. A college student, for example, one who only has taken two semesters of physics (mechanics and electromagnetism, let us suppose), might justify a claim about unobservables on the authority of his introductory textbook and professor (and/or whatever experiments he performed in the associated labs to corroborate and reify the theories he was learning about in class). This is notable precisely because of the way physics is often taught to college students these days: Newtonian mechanics is introduced as "physics," only to be replaced in *subsequent* courses by Einsteinian and Quantum mechanics, and many students do not go on to take those subsequent courses. Instead, they walk away with a (thinly) justified belief that, e.g., gravity is an immediate action at a distance force.

However, distinguishing between practicing scientists and nonscientists is apt to mislead. It is apt to mislead because these terms refer to individuals *simpliciter* where what we need are terms that refer to individuals *in relation to a proposition.* An example will illustrate what I have in mind.

Einstein was a practicing scientist. But in relation to a claim about the Krebbs cycle or a claim about the nature of mitochondrial transmembrane proteins, Einstein would not be much better off than a nonscientist. So perhaps the distinction would be captured more accurately by appealing to the distinction between a specialist and a nonspecialist.

But at the end of the day, the point I want to make is simply that some might justify some of their beliefs on the basis of testimony and (legitimate) appeals to authority whereas others will justify those same beliefs on the basis of personal experience and argument, and both strategies can work. From this it may be seen that the version of realism I endorse, thin realism, emerges from two general considerations about epistemic justification: (1) a proposition is never justified *in itself*; a proposition is justified *in relation to an individual*;[30] and (2) what counts as justification will depend not only on the evidence available to an individual but also on that individual's epistemic position. It gets more complicated but more realistic when we layer in things like the distinction between epistemic permission (what one *may* believe) and epistemic obligation (what one *ought* to believe), non-cognitive belief formation processes and pragmatic considerations about how much due diligence

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[30] Thus I disagree with Lipton's separation of theory from theorist (Lipton, 2004, p. 167), although I readily grant that whether a theorist counts as justified in believing her theory might have little bearing on whether others count as justified in believing it.

and fact-checking any given individual is required to perform regarding any particular proposition.[31] But even without those complications, the conclusion I am pushing for is that a realistic scientific realism cannot avoid debate about whether *individuals* are justified in believing *specific* claims about unobservables. And this has two notable implications.

First, current focus on theories along with various auxiliaries is unnecessary. In the current debate, this focus has emerged because there would be no way to come up with observable results from claims about unobservables without grouping these claims into theories and taking their auxiliaries with them. But if my view is correct, then this kind of idealization is unnecessary. It is unnecessary because many people will be justified in believing many claims about unobservables on the basis of testimony and authority.

Second and relatedly, precisely because of the justificatory force of testimony and authority, *global* antirealism is untenable absent massive cultural and structural shifts in our society. That is, the claim that nobody is justified in believing any scientific claims about unobservables cannot be sustained in light of the authority accorded to scientists who make such claims. Even if an antirealist could come up with a sound argument that (i) stops short of RS and (ii) shows that any scientific claims about unobservables cannot be justified on the basis of individual experience and reasoning alone, there still would be room for justified beliefs on the basis of authoritative testimony from those who, we may suppose, are unconvinced by this argument for one reason or another.[32] As teachers (and, for many of us, as parents) this is a possibility that cannot be discounted: for much of the year it stares us in the face at least

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[31] Many would maintain that the picture I have painted needs to be substantially more complicated. For example, contextualists would argue that justification depends on the context in which an individual finds himself, and pragmatists would argue that justification depends on an individual's interests. For the present, however, I leave these issues to the side.

[32] Things get more complicated (but still not hopeless for the realist) if the testimony is in bad faith.

once or twice a week.[33] And if this is correct, then antirealism is tenable *only* in the debate about MSR.

Right about now, however, I expect both realists and antirealists in the more traditional debate will object. Realists will complain that I have conceded too much. Justifying claims on the basis of authoritative testimony when the authority might not be justified sounds like *prima facie* justification; this, they might object, is anemic realism, realism not worth defending. "Scientific theories *have* been successful," these realists might say: "just look at that computer you are typing on!" And antirealists will waive their hands dismissively: they are after bigger fish.

But any such objections would be based on a misunderstanding. I am not giving up on meta-realism: my point is simply that once the JJ-principled is jettisoned (as it should be), meta-realism can be disconnected from realism and, thus, individual claims about unobservables can be assessed on their own merits. Moreover, appealing to authoritative testimony can provide *ultima facie* justification, and it can do so for foundationalists and nonfoundationalists alike.

And if the anti/realists object that SR on the basis of authoritative testimony is not the kind of SR in which they are interested, I hasten to add that I am not arguing that antirealism fails because there are at least some people for whom beliefs in unobservables are justified on the basis of authoritative testimony: I am arguing the *global* antirealism fails for this reason. This is where my distinction between specialists and nonspecialists becomes important. So let me say more about specialists and the justification that comes from reason and experience. I think that there are four points worth making here.

The first is that we need to distinguish between the context of discovery and the context of justification. Now as this distinction is often used, justification plays no role in discovery. But that is not what I am getting at. So let me explain how this distinction plays a role in my argument for thin realism.

The idea is something like this. In the initial eureka moment of discovery, a scientist is going to count as justified in entertaining

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[33] It might be objected that if my position is correct, then people will be warranted in believing all sorts of contradictions. To take a well-worn example, general relativity and quantum mechanics disagree on various claims about unobservables such as the curvature of spacetime. But any position that entails that people are warranted in believing contradictions must be wrong. So my position must be wrong.

But this objection is too quick. I have not been arguing that all agents (or any agents) are *ultima facie* justified in believing all claims about unobservables that come out of our best scientific theories. Rather, I have been urging that the debate about scientific realism should be about specific claims about unobservables rather than about the meta-claim it currently focuses on; that a plausible theory of justification would consider propositions in relation to individuals rather than propositions in themselves; and I have been arguing that such a theory of justification will entail that most people are at least *prima facie* warranted in believing many claims about unobservables.

Given the current situation in science, I think this does mean that people are *prima facie* warranted in believing many contradictions. But given that the principle of noncontradiction will play a role in any plausible theory of justification, I do not see why this should be particularly worrying for me. However, considerations regarding opacity, recalcitrant beliefs and bounded rationality make me chary of saying more: this is a complicated issue, one that deserves a paper unto itself.

a great deal that will not withstand the criticism of reason and experience. The justificatory standards are kept low, and deliberately so in social contexts, for discovery. If you do not believe me, think about what students are told about brainstorming. It is only once the initial creative moments are over, once an individual or team has decided to move forward with an idea or two, that the justificatory burden slowly begins to rise.

The reason this is so important is that it plays into what I was saying earlier about non-cognitive belief forming mechanisms, epistemic permissions and due diligence. There is a sort of snowball effect here: a belief is formed in a low stakes environment in which that belief is permitted despite not being well justified. As the belief is subject to harsher criticisms and more stringent tests, it becomes more entrenched, and as it becomes more entrenched the justificatory burden that falls on the shoulders of the skeptic who wants to dislodge this belief by appeal to reason steadily increases.

Now an antirealist might contend that they have arguments to meet this justificatory burden. In particular, an antirealist might gesture toward some of the arguments I made in section 1 of this paper in order to show that these realist beliefs about unobservables ought to be discarded. This leads me to my second point.

As already noted, the arguments canvassed in section 1 are about MSR, not SR. If an antirealist wants to convince a specialist that a given hypothesis about unobservables is unwarranted, s/he is going to have to stop doing philosophy of science and start doing science. This is not because science is privileged in some way or because philosophy of science is not interesting or valuable. It is because scientists are investigating these propositions at the ground level, and so (assuming the JJ-principle may be discarded) aiming at the justifications for the justifications misses the target: the specialist's justification is not touched by these meta-considerations.

Now an antirealist might object that if their arguments work, then the specialist's justification is not merely thin justification; it is not justification at all. But this is far too strong and leads me to my third point.

Propositions and individuals are not created or justified in a vacuum. There are social contexts and communities of inquirers that reinforce ideas about justification. And this is highly relevant not only because it casts a shade on the claim that a specialist's justification could be no justification at all (I shall return to this momentarily) but also because even if this claim were true, the fact that a specialist's fallible but nonetheless best judgment is that some proposition about unobservables is true confers upon him/her a *prima facie* warrant for his/her belief.

Of course, an antirealist might object here too: an antirealist might argue that they need not engage scientists at the ground level in order to show that SR is unwarranted; rather, scientists need to engage at the meta level in order to show that SR is warranted. Or they might try to get at this same point in another way: we are not interested merely in whether specialists *sans phrase* are warranted in believing claims about unobservables; after all, who cares whether there is a philosophically benighted specialist who is permitted to believe claims about unobservables merely because according to his/her

best but uninterestingly uninformed judgment these claims are true? The antirealist might contend that in asking whether "we" are justified in believing claims about unobservables, we are asking about whether philosophically sophisticated specialists are warranted in believing such claims. This leads me to my fourth and final point.

Let us grant for the moment that antirealists win the day at the meta-level and that we are considering some proposition about unobservables that has passed through a sufficient number of ground level hurdles to be well accepted within the scientific community so that there is a genuine conflict between meta-level arguments and the ground level ones. After all, if the problem of underdetermination does lead all the way to RS, there is no genuine conflict here (or better: the conflict is not the kind that the antirealist needs).

Let us recall also that we are not talking about justification for a proposition in the abstract. We are talking about justification for a proposition for an individual and, in particular, for a philosophically astute specialist.

Given all of this, I contend that whether such an individual is warranted in believing in unobservables is not a simple "yes" or "no." It is an "it depends." Some individuals might be warranted in a *modus ponens*: they hold fast to their meta-level considerations and infer from them that their ground-level considerations are misguided. But others equally well might be warranted in a *modus tollens*: they hold fast to their ground level considerations and infer from them that these meta-level antirealist arguments go wrong somewhere or perhaps are simply about something else, not justification but shmustification —and this is why showing that a specialist's justification is no justification at all is simply not going to work. Moreover, with the rejection of the JJ-principle, such individuals need not have a full theory of justification to count as justified at the ground-level. They simply might believe (and be warranted in believing) that the antirealist considerations are mistaken.

Conclusion

In this paper I argued in favor of SR, the position that we are justified in believing various claims advanced by scientists about unobservables. However, unlike most participants in this debate, I did so without taking a position on the success of the NMA. Instead, I argued that (regardless of whether it succeeds) the NMA and the other arguments in the current debate are about meta-SR rather than SR *per se*. Although meta-SR can be connected to SR by means of the JJ-principle, I argued that this principle should be rejected. Instead, I argued that SR should be defended by adopting a more plausible view of epistemic justification, one that regards justification as a relation between an individual and a proposition based on his/her epistemic position. I used this to show that once relevant differences between individuals' epistemic positions are taken into account, global antirealism may be seen to be untenable. And I then argued, further, that my argument also extends to specialists, even philosophically sophisticated ones.

Thin realism might be weaker than what many realists want. But I contend that it is nonetheless pretty strong. The main point is that

realists and antirealists both need to be constrained by a realistic theory of epistemic justification. And doing so, I contend, will reveal a groundswell of justification for unobservables, justification that heretofore has been unobserved.

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