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S.I. : NEW THINKING ABOUT SCIENTIFIC REALISM

Reflections on new thinking about scientific realism

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Abstract In August 2014 the Universities of Pretoria and Johannesburg hosted a major international conference in Cape Town, 'New Thinking about Scientific Realism', to assess extant discussions of the view in hopes of opening up new avenues of research, and to sow the seeds of further development and consideration of these prospective lines of inquiry. In this, the concluding essay of the Special Issue collecting some of the descendants of these earlier presentations, I extract some of the more striking themes to emerge with the aim of reflecting on their novelty and their promise for the future study of scientific realism.

Keywords Scientific realism · Selective realism · Pessimistic induction · Mathematical realism · Scientific representation · Axiological realism · Approximate truth · Epistemic stances

1 Introduction: futuristic scientific realism

Therefore the ancients say, 'Yield and overcome.' – *Tao Te Ching*, verse 22

If there is a thread that is woven through the contributions to this Special Issue on scientific realism, it is the idea that realism cannot be what it was once taken to be, and is perhaps still taken to be outside of the discussions of aficionados. Over the past few decades, thinking about how best to formulate the idea of realism in the context of the sciences (by 'realism' in what follows, I will intend scientific realism unless otherwise indicated) has offered a great deal of refinement of an older, coarser picture of

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a philosophical position according to which one should believe that scientific theories are true. Different ways of conceiving what scientific theories are, precisely, and greater attention to the use of models and other representational devices in scientific practice have refined our understanding of the vehicles of scientific knowledge. Different ways of carving up these vehicles into components which may vary with respect to their relative epistemic warrant have refined our understanding of what realists are realists about, exactly. More detailed explications of what 'true' means for the realist, in a domain of inquiry–the sciences–where absolute truths can be scarce on the ground, have enriched our understanding of its epistemic upshot. And then there is the question of whether belief is all that important to realism after all.

One might well contend that a number of these refinements were, in fact, implicit in many earlier discussions of realism. Perhaps it was simply taken for granted earlier that talk of theories and truths and beliefs *simpliciter* comprised a number of conveniently simplified ways of speaking about the core issues in dispute between realists and antirealists. While there is surely something to this contention, the devil is, as they say, in the details. In more recent explorations of these details, the very idea of realism has come into sharper focus and its tenability as a philosophical view of science has been subjected to sharper analysis, critique, and defense. The proposals under discussion here all fall under this description. At a certain level of abstraction they may all be described as attempts to focus on some refinement of the general idea of scientific realism, with the goal of demonstrating its persuasiveness or its failings and to propose a way forward. In what follows, I consider some of the main themes to emerge in these attempts and evaluate their potential to advance debates about realism.

In the next section, I ponder a number of studies preoccupied with the challenge of squaring realism with the history of science. This work focuses primarily on the idea of selective realism as a response to the challenge, and in particular on a variant of selective realism I call 'explanationism'. In Sect. 3, I turn to considerations of the nature of scientific theories and how the realist should understand their putatively representational relations to the targets of scientific inquiry. This will include a discussion of an aspect of theorizing that is often left to one side in discussions of realism, namely, its mathematical content. Finally, in Sect. 4, I examine proposals for a re-envisioning of the nature of realism itself, all of which suggest a retreat from or a replacement of commitments that are commonly taken to be essential to the position. An overarching problematic, which will surface in all three sections, is a delicate balance between moderating realism so as to render it a more tenable position in the face of significant challenges, and not weakening it to the extent that it no longer amounts to a philosophical position worthy of the name 'realism'. I will contend that there is an acute risk in much new thinking about the view that, by yielding so much in order to overcome, both the substance and the spirit of realism may be lost entirely.

2 Exorcizing the ghosts of science past

2.1 Selectivity: discontinuity and partitioning

Testifying to the lasting importance of the field of history and philosophy of science to ongoing concerns in general philosophy of science, it would appear that a healthy

portion of work concerning realism is still fixated on issues of whether and, if so, how realism can be defensible in light of the history of science. Given the impressive discontinuities we find in our best scientific descriptions of the relevant subject matters in any given domain of investigation over time, what sense is there in being a realist? Historical studies documenting cases in which earlier, empirically successful theories and models invoked theoretical entities that were subsequently viewed as failing to inhabit the actual world, or whose central theoretical principles or laws were similarly rejected, have comprised a longstanding challenge to realism. Whether in the form of an enumerative, pessimistic induction based on these cases from the past leading to skepticism about theories in the present, or simply in the form of a dissolution of the common realist association of empirical success with likely truth (or approximate truth; I will take this qualification as read in what follows and consider it in Sect. 4.1), debates about the import of history still resonate strongly in debates about realism.

As noted by Mario Alai (2016), there are different ways in which realists have sought to respond to the challenge of history. With respect to both of the formulations of this worry expressed above in particular, there are various ways in which one might take issue with the cogency of drawing lessons from the past. To take just one currently prominent example, one might argue that contemporary science in a given domain is sufficiently unlike earlier incarnations constituting past science in the relevant lineage–in terms of its methods of investigation and analysis, the empirical data available to scientists, and so on–that conclusions regarding the epistemic failings of the past do not license any conclusions, let alone pessimistic conclusions, about the present. In this way, so the suggestion goes, one may happily set aside discontinuities in the content of scientific descriptions over time in debates about realism given the fact of meta-level discontinuities over time in the nature and quality of scientific investigations themselves.

Note, however, that even if one is tempted by this line, what it generates all by itself is merely the purely defensive, negative conclusion that the falsity of past science does not license antirealism in the present. It does not itself suggest any obvious, difference-making, positive support for realism, since it is difficult to see how the mere fact that current science may be better off methodologically and otherwise in comparison to previously rejected science could establish that current science yields true descriptions, simply because of the truism that 'better' does not entail 'good enough'. Consequently, appealing to discontinuities in the nature and quality of scientific investigations does not by itself yield any sort of partitioning of scientific descriptions over time according to which instances of later (such as present) science are demonstrably privileged with respect to assessments of truth. Something more is required to give a fulsome, realist response to the pessimistic induction.

For some time now, as Alai registers, this something more has commonly been taken to be some or other form of selective realism: the idea that in many, perhaps most if not all cases, realists should not be investing in the truth of theories *simpliciter*, but rather in parts of theories that have greater epistemic warrant than theories taken as a whole. If such a strategy for realism were tenable, it would furnish a *prima facie* response to concerns arising from the history of science: while many past, empirically successful theories are clearly false *in toto*, they may nonetheless incorporate elements that are true. Selective realists thus aim to partition the content of scientific theories

and models in such a way as to identify the parts that are likely to survive in scientific description over time and into the future in virtue of their superior epistemic warrant. And while there are several proposals for how one might do this, the one that draws most attention as a response to the challenge of history in particular is the idea that those bits that are required to *explain* the empirical success of the relevant science are the bits that realists should endorse. It is thus no surprise, as we will see next, that this explanationist defense of realism is the target of new concerns steeped in historical cases.

2.2 Challenges to selective explanationism

It is typically thought to be a necessary condition for any account of selective realism that it furnish a *prospective* criterion or criteria according to which the realism-worthy parts of theories and models, if any, can be identified. That is, the account should allow one to identify the relevant parts of theories not only in past science, where identifications of elements that just happen to be found in contemporary theories as well will always be subject to lingering suspicions of (conscious or unconscious) rationalization *post hoc*. It should also allow one to identify, *at any given time*, aspects of current theories that have sufficient epistemic warrant to suggest that they are likely to survive into the future, if and when theories change in those domains.[1] According to the explanationist form of selective realism, this prospective condition is satisfied by focusing on those aspects of theories that are especially important, or seemingly crucial, or perhaps even indispensable to empirical success, where this is typically cashed out in terms of aspects of theories that are required in order to derive successful predictions.

Now, performing this sort of diagnostic work on scientific theories is almost guaranteed to be a messy affair in most interesting cases, for the simple reason that it is not typically very obvious what one should consider unimportant or dispensable to making predictions without the benefit of hindsight. For one thing, it is unclear why we should expect that even *at* a time, these kinds of judgments are generally, unanimously shared by scientists. For another thing, to the extent that they are shared, the judgment of historical actors at a time may well differ in comparison to those made by their scientific progenitors and descendants. But for the sake of argument, here, let us make the highly nontrivial assumption that some recipe or recipes for identifying the relevantly crucial or indispensable components of theories can be articulated with sufficient clarity and precision to obviate these worries. Even then, as new historical case studies suggest, it is unclear whether the explanationist can deliver the goods.

Consider, for example, Gauvain Leconte's detailed consideration (2017) of the famous case of Fresnel, Poisson, and the infamous white spot. As the well known story goes, in the course of nineteenth century theorizing about the nature of light, one of the surprising consequences that Poisson was able to derive from Fresnel's

[1] For an analogous consideration of how prospectively applicable criteria of success or failure may function to render a form of realism substantive, see David Spurrett's discussion (2015) of materialism or physicalism–a thesis or theses often linked to discussions of scientific realism.

wave theory was that a bright spot should appear in the center of a shadow cast on a screen, created by shining light on a disk in front of it. The prediction was subsequently confirmed experimentally, but what aspects of theory, precisely, are required in order to derive it? On Leconte's retelling, Poisson and Fresnel actually furnished different derivations, each invoking different theoretical constituents. To the extent that these different recourses to theoretical description share a common core, it is insufficient to derive the prediction. To make matters worse, the common core all by itself does not appear to suggest any particular ontology of light, and adding to it in the ways that Poisson and Fresnel did in order to generate the prediction of the white spot ends up making recourse to mutually incompatible assumptions regarding the nature of light.

It is in the nature of historical cases that one may attempt to contest them with counter-narratives, but my interest here is in the question of how a realist might respond to this sort of case, assuming that the details are correct. Peter Vickers (2016) offers a possible solution when he suggests that it may be possible, in reply to the pessimistic induction, to forego prospective diagnoses of aspects of theories worthy of realist commitment after all: 'even if we can't prospectively identify what warrants realist commitment in our best contemporary theories, we can prospectively identify at least some elements which do not merit realist commitment'. Taking Bohr's prediction of the frequencies of the spectral lines of ionized helium as an illustration, he argues that while on some occasions, what seemed crucially explanatory at the time turns out to be rejected in due course, realists can take solace from the fact that certain false claims that may *seem* crucial actually entail weaker claims that would serve just as well. The stronger claims are parasitic on the weaker ones: the former are useful in deriving successful empirical predictions but as it happens, they do so in virtue of entailing the latter.

Certainly, in cases where insight allows such revelations, the realist may be freed from having to defend parts of past theories that might otherwise have seemed necessary to explain their empirical success, but what does that buy? There is no obvious reply here to the pessimistic induction (unless the weaker claims are themselves endorsed), for what is gained in not having to defend some aspects of past theories is lost in the failure to offer a prospective criterion for identifying their realism-worthy content. Even in cases where historical actors might have appreciated that merely apparently essential explanatory hypotheses imply weaker ones capable of the same explanatory work, Vickers takes this to indicate only that the stronger hypotheses are not confirmed by the relevant empirical success. Thus, to the extent that realists hope to make positive knowledge claims about what they *do* (as opposed to *do not*) believe, they would still seem to depend here on some form of retrospective assessment. To append Leconte's conclusion here: 'the criterion to circumscribe the true parts of a theory can be defined only from the point of view of superseding theories'.

If this is the best the realist can do, her goose is cooked. Once realism is tied to a retrospective assessment of theories and models, it is doomed, because theories *at any time, including the present*, will then require a retrospective assessment before any positive knowledge claim can be made. In that case, unless one is in a position to assess past and present science from the perspective of something like the ideal limit of inquiry, assuming that all and only truths will be believed even then, one is never in a position to assert the kind of knowledge that realists habitually commend–and

the ideal limit of inquiry is nowhere on the horizon. Just as other forms of selective realism (entity realism; structural realism; what I call 'semirealism') must articulate in detailed and compelling ways what *warrants* belief in certain aspects of theories, as opposed to what does not, explanationism still has work to do to exorcize the ghosts of history. But there is room for optimism here too: just as I suggested earlier that noting improvements in the quality of scientific investigation (methods and data) must be bolstered by some form of selective realism, so too is the latter bolstered by the former. Combining these strategies, as Alai suggests, offers hope. The interwoven support of a number of partially supporting planks may be enough to render realism a tenable epistemology of science.

3 The representing and the represented

3.1 Mathematics: the abstract and the concrete

One of the newer facets of discussions of realism and antirealism reflects a larger groundswell of interest over the past couple of decades in issues of scientific representation. Here, a relatively recent, intensive focus on practices of scientific modeling has joined longer standing foci concerning the nature of theories and their possible semantic relationships to things in the world. In all of this discussion of theories, models, what they are precisely, and how and to what extent they represent their subject matters, the topic of specifically *mathematical* representation has occupied a somewhat awkward position on the fringe as an issue at the intersection of general philosophy of science and the philosophy of math. One the one hand, indispensability arguments of the sort commonly attributed to Quine and Putnam are sometimes proffered as no less applicable to mathematical entities (numbers, sets, groups, categories) than to entities that are more commonly viewed by realists as the unobservable entities of science (particles, fields, viruses, organelles). On the other hand, this conflation may well seem misleading, because of what seem like striking differences in the arguments standardly deployed to argue for ontological commitment to mathematical and scientific entities, respectively.

Indispensability arguments for mathematical entities typically reason from two premises: one to the effect that we should commit ontologically to those entities that are in some sense indispensable to our best science; and another to the effect that mathematical entities are indispensable in the relevant sense. Attention to the relevant senses of 'indispensable' implicit in the most influential arguments for scientific realism, however, raises questions about whether mathematical realists could plausibly have the same things in mind. When an entity realist argues that a detailed understanding of the causal interactions of entities, facilitating manipulation, interference, and control, serves as a basis for realism, that which may be described as putatively indispensable is that which interacts in appropriately causal ways. But mathematical entities are causally inefficacious. When a structural realist argues that the preservation of descriptions of relations between supposed entities serves as a basis for realism, that which may be described as putatively indispensible are physical relations implicated in the relevant science. But mathematical entities are not physical. Similarly, what is

considered indispensable by explanationists are those scientific entities, relations, etc. required to fuel empirical success.

Of course, there is no doubt that the sorts of things to which realists commit, the arguments for which may reveal implicit conceptions of indispensability (as suggested above), are often described mathematically, but this all by itself suggests at best a weaker notion of indispensability that is applicable to linguistic expression very generally: in order for human creatures to express themselves in relatively complex and systematic ways, such as those involved in articulating theories and models, we need to use natural, scientific, and mathematical languages. Perhaps this weaker notion of indispensability can be parlayed into arguments for another kind or kinds of realism–say, regarding linguistic entities very generally, like concepts (mathematical and otherwise) and/or propositions–but this would seem to engender a rather different and more general constellation of issues than those at stake in debates about *scientific* realism. If some aspect of mathematical indispensability is to be held on a par with the kinds of indispensability taken seriously in these latter debates, some less generic feature of mathematical expression, which makes a plausible connection to standard arguments for (scientific) realism, is required.

Nora Berenstain (2016) explores what might be taken as a proposal for this more specific sort of feature of mathematical description in her discussion of well known cases in which mathematical facts seem indispensible to scientific explanations. Answers to the questions of why honeycomb has a hexagonal structure, and why North American cicadas have thirteen- or seventeen-year life cycles, have mathematic kernels, expressed in terms of facts about the geometry of hexagons and the prime-ness of certain numbers. Arguably, what does the explanatory work in such cases are the *particular* mathematical structures (properties and relations) described in the explanations, not the generic fact that we employ mathematical modes of description. Berenstain suggests that it is because these structures are instantiated in physical phenomena that they are *there*, in a way amenable to realism, ready and waiting to be pressed into service in scientific explanation. I would simply point out that to appeal to relations of instantiation as the means by which bits of mathematics are "physicalized" and thus rendered more analogous to the unobservables of interest to scientific realists is to tack on a fairly weighty metaphysical thesis. (What is instantiation, after all?) If nonchalant tacking is benign, one might wonder whether describing these mathematical structures as tropes, or as nominalists do, would not serve just as well.

3.2 Relations of theories and models to the world

Turning from mathematical representations and the representation of mathematical facts to the nature of scientific representation more broadly, Steven French (2015) hints at certain opportunities in recent thinking about the latter for progress in how we think about the epistemological dimension of realism. One of these opportunities is a consequence of what he regards as a shift from thinking about the epistemology of realism in terms of relations of reference between theories and various aspects of reality, to some other sort of representational relation or relations: 'the realist's traditional reliance on reference has given way to the invocation of representation as

the appropriate framework for describing the relationship between theories and the world.' A second opportunity concerns the question of what theories are, precisely, such that they are able to fulfill the kinds of representational functions assigned to them by realists. Let us consider these possible avenues for progress in turn.

First, regarding the current absorption with issues of scientific modeling: there is no question that recent fascinations with how different kinds of models and simulations represent target systems of scientific interest have often (though not always) paid little explicit attention to traditional realist claims about truth and reference. Does this amount to a reconceptualization of the relationship between theory and world with consequences for our conception of realism? I do not think so. For one thing, note that two of the most intriguing and much discussed features of modeling–abstraction and idealization–are often analyzed in terms of truth and reference. Abstraction is commonly conceived in terms of building only some of the features that are potentially relevant to the nature or behavior of something into a model representing it; here the *prima facie* challenge to realism suggested by at-best-partial reference is clear. Idealization is commonly conceived in terms of building features into a model that distort the true natures of the things in the world to which they correspond; here the *prima facie* challenge to realism suggested by deviations from the truth is also transparent. Thus, so far as realist diagnoses of representation are concerned, it is dubious whether the new 'framework' has altered the relevant categories of analysis so much as provided new angles from which the old categories may be assessed.

Regarding the idea of setting aside considerations of reference more specifically, French does not dwell on or belabor the point, but he does put his finger on something that seems implicit in at least some recent thinking about models in science. This is perhaps a vestige of what some have viewed as a bonus of the semantic view of theories, according to which theories are simply families of models, as against the so-called syntactic view, according to which they are sets of sentences (e.g. axiomatic systems). The idea is that one may forgo having to consider relations between linguistic entities and the world (like reference relations), as may be required of realists adopting the syntactic view, in favor of different kinds of relations between models and targets such as isomorphism, homomorphism, or similarity. It is difficult to see, however, how trading the former for the latter dissolves considerations of reference. If a model bears a relation to its target in such a way as to support realism, the natural reading of this is that we have "latched onto" something in the world, which is all that the idea of reference is intended to secure. If one holds instead that realism does not in fact require successful reference, this thought too may be formulated on either view of theories. The question of whether realism is helpfully spelled out in terms of reference, then, is independent of recent forays into modeling.

A second point, on which French does dwell, is the question of how best to think about the nature of theories in the first place. Here he sees extant accounts as having floundered in various ways when pressed to explicate how precisely theories function in scientific practice–a concern that applies with some force to the representational framework engendered by focusing on models. If theories are models and models are (on one view discussed) abstract entities, for example, how are they created or discovered by scientists, let alone apprehended by those other than their creators? In place of extant views he suggests a proposal shared with Vickers which they call 'theory elim-

inativism': theories are not things as such, having some imagined ontological status unto themselves. Rather, talk of theories is simply elliptical for scientific practices, which then serve as truthmakers for theory talk. I have no objection to this and, indeed, find the deflationary spirit of the proposal attractive, but the issue with which I am concerned here is whether this offers any assistance to realism. And here I draw a blank, for merely pointing to scientific practice does not serve to articulate realist claims about scientific knowledge. In order to make *those* claims, one inevitably speaks of representations that are true or refer or bear some other relation to their subject matter. To repeat the previous moral, this cross must be borne however one thinks of theories.

4 Changing our very conception of realism

4.1 Content: on aims and approximate truth

Thus far I have considered a number of suggestions for how one might understand or amend realism (and the challenges it faces) in instructive ways–ideally, in ways that embody promising new avenues along which to engage these subjects going forward. In this section I will examine a few last recommendations, all of which have in common the aspiration of re-envisioning completely a core tenet of realism as it is usually formulated. A first example of this comes from Timothy Lyons (2016), motivated by a study of the history of twentieth century celestial mechanics. The precise motivation extracted from the study is the fact that our best theories often incorporate idealizations that turn out to be important to their empirical success, including derivations of successful predictions (Leconte makes the same observation in connection with Poisson's and Fresnel's derivations, as discussed above). Solutions to Einstein's field equations invoke point masses, perfectly spherical bodies, and so on, and these are assumptions that cannot be viewed, Lyons thinks, as approximately true. The subject of idealization is a live one, as we have seen, and it is debatable whether idealizations are in fact incompatible with notions of approximate truth. But I will leave this issue to one side here.[2]

What interests me presently is not so much the motivation of idealization as what Lyons takes this to be motivation *for*. Realism is nonetheless tenable, he suggests, as a purely axiological thesis, that is, one concerning what science aims to do: 'science seeks to increase a subclass of true claims'. As a proposal for how to understand realism, this is revisionary in that it would do away with the epistemic dimension of the position as it is commonly formulated, as involving beliefs about truth, reference, or ontology in connection with certain scientific claims, replacing all of this with an exclusive characterization of realism in terms of that for which science aims or seeks. Since no one person or group owns the term 'realism', it may be used as a term of art, but is this an attractive definition? For one thing, superimposing what can only amount to a large-scale rational reconstruction of what *science itself* (not a person,

[2] For some thoughts, see (Chakravartty 2007), chapter 8, and (2017), chapter 6. It is not sufficient, of course, to jump from the falsity of idealizations (this is definitional) to the absence of approximate truth. Since anything that is merely approximately true is *understood* to be false, significantly more is required.

whose aim might be asked for and given) *seeks to do* onto a hugely variegated set of practices and barely imaginable scales of time, space, and context may seem a rather fanciful philosophical exercise. One might think twice about defending a philosophical position defined in that way. But let us play along here for the sake of argument.

Merely aiming to do something is compatible with never achieving it–indeed, with it being *impossible* to achieve. An antirealist whose primary contention is that the sciences should not be held to produce truths of the sort typically associated with realist diagnoses of our best science, or who perhaps even maintains that science cannot in principle produce such truths, might be perfectly content to grant that in some or perhaps even all cases (if she is happy to grant the large-scale rational reconstruction this requires), science merely *aims* to furnish truths. After all, this is no threat to her antirealism. On her diagnosis, science fails in this aspiration and nothing in her understanding of the outputs of scientific investigation is contested. One might then ask: how compelling is a proposal for realism that is compatible with *anti*realism, as it is commonly construed? Again, one may define terms as one may, but it is difficult to imagine why many if any realists should be convinced to see this as a "realism" worth defending, since it takes no stand whatsoever on what is real. To the extent that some substantive epistemic thesis is generally viewed as constitutive of realism, an exclusive focus on aims seems, at best, a change of subject.

Ilkka Niiniluoto is an example of a realist who views idealization as entirely consistent with the idea of approximate truth, and his main concern (2015) is to defend the thought that scientific progress can be understood in precisely these terms, as increasing truthlikeness or verisimilitude. On this view it is sufficient for realism, he thinks, that theories make progress toward the truth, and he contrasts this with the selective realist requirement that some parts of theories be 'preserved' and thus 'accumulated' across theory change. This contrast seems to me to be misleading, however, and by noting this I hope to make an irenic point. The selective realist's notion of 'preservation' was never intended to be read in so strict a way as to rule out improvements. This is the reason, of course, that so much recourse is made to the idea that what is preserved is preserved in *some form*–for example, as recoverable in limiting case scenarios described by improved theories. What this suggests is that the core insights of realist understandings of science associated with selective realism and increasing truthlikeness are, in fact, supportive of one another, not contrary. Niiniluoto himself suggests as much when he admits that 'rival successive theories can be treated as referring to the same theoretical entities so that it makes sense to compare their degrees of truthlikeness'.

Indeed, I submit that one must go further than this. A conception of realism in terms of increasing truthlikeness *requires* supplementation by something like selective realism, for it is only the latter that suggests that our best theories are, in fact, in some way close to the truth. Increasing truthlikeness all by itself is a merely comparative notion: a theory may be more approximately true than another while both are nevertheless so horribly off the mark as to be implausible candidates for anything like a realist commitment. Granted, moving in the right direction counts for something, but it difficult to see how it could count for much unless the "degrees" of approximate truth at issue are sufficiently high as to make realism a tenable view. Perhaps it is not true that one cannot make a silk purse out of a sow's ear–with enough theory change

over enough time, descriptions may change gradually and considerably–but if one's best scientific theories at any given time are nowhere close to being silk purses and instead look very much like sows' ears, not even a well defined notion of progress will spare realist blushes. One might hope, of course, to supplement the idea of increasing truthlikeness with something weaker than selective realism, such as the axiological view of realism canvassed above. I would contend, however, that just as neither of these views is promising by itself, their sum is likewise unpromising for realism.

4.2 Scope: case by case and sometimes not at all

As is common in discussions of realism, most of this special issue focuses on specific facets of the view and antirealist appraisals, with arguments for and against. In closing I would like to zoom the lens out to consider the larger scale question of the reasonable scope and application of realism–as judged by the standards of realists themselves. This is important, I believe, because in proposing possible avenues of new thinking about realism, it is important to root these efforts in a clear grasp of how far our thinking has come already. One of the ways in which the evolution of the position is most evident is in realists' own assessments of the appropriate scope of the view. At one time, discussions of realism were conducted almost exclusively in very broad terms, which gave the perhaps mistaken impression that it is a global or universal view concerning all of science. 'Scientific theories are true', 'their central terms genuinely refer', 'the ontologies they suggest are correct', and so on may give the impression of universality, even while a great deal of work over the past few decades has focused on earnest attempts to qualify these statements and to describe the conditions under which they are plausible.

It is in this spirit that Juha Saatsi (2015) bemoans what he describes as 'recipe realisms': 'epistemological commitments in the abstract, recipe like, regarding all/most/many theories in mature science'; 'a perspective on science that is sweeping and monolithic in its realist outlook'. In contrast he recommends a 'piecemeal' approach to thinking about what the details of a realist commitment might be in any given case. I can only applaud this thought, though I suspect I take it to be more present in the currently evolved state of realist discussion than Saatsi would agree. When entity realists recommend belief in certain entities, but only conditionally upon an apparent ability to exploit our causal knowledge of them in manipulation, intervention, and control, and not so much otherwise, are they not suggesting a piecemeal approach to scientific theories and models? One might offer the same analysis, *mutatis mutandis*, in connection with structural realist claims about certain mathematical relations preserved across theory change, and my own, semirealist position which grounds realist ontology on a bedrock of well detected properties, and explanationist claims about explanatorily essential aspects of theories. These are not, I take it, recipes offered with the expectation that they will or must describe all or perhaps even most science. They are attempts to illuminate forms of epistemic warrant that are present or absent in any given case.

Even if I am right, however, that much contemporary realism is self-avowedly non-universal in the sense of not seeking to embrace all aspects of even our best science, are

the most popular recipes nonetheless 'sweeping and monolithic'? Well, to the extent that they are *successful* illuminations of epistemic warrant, they will apply wherever they apply and will in that sense be sweeping, but this seems irreproachable. Of course, saying this is to say nothing about whether these proposals are *in fact* successful in pinpointing epistemic warrant, as their advocates contend, but that is another issue, debates about which are still unfolding. This also leaves open the interesting question of whether more than one of these proposals may be correct, whether in application to different cases or perhaps even to one and the same part of science. The answer to the first sub-question here suggested, of whether different recipes for realism may apply in different cases, is relatively straightforward (as indicated above): it simply depends on whether the relevant recipes are, in fact, defensible in virtue of their having identified some genuine forms of epistemic warrant, and on whether these forms of warrant are present in the relevant cases.

The second sub-question, about whether more than one account of realism might apply to one and the same case has intriguing potential, I think, to generate new thinking about realism: while some of these proposals are standardly presented as being mutually exclusive, let me advocate here an openness to potentially fruitful intersections and meetings of the mind between realists of different stripes. For example, in a given case, it may turn out that the most important explanatory parts of a theory are well detected properties, or entities, or structures. It may be that in a given case, the knowledge of an entity that is most warranted is some structural feature of it or its relations. It is precisely because different prescriptions for realism may be applicable to different sorts of cases–exactly what Saatsi suggests–and furthermore because there is likely, I suspect, some promising and substantive overlap between these prescriptions in cases where they are simultaneously applicable, that I do not agree that 'realism without recipes is still realism enough'. Even on an appropriately piecemeal approach, the recipes are still tasty when the ingredients of the case are right and the cook is skillful. Cases of scientific theorizing and modeling bear all manner of similarities and differences to one another that are ripe for analysis by realists.

To go yet further, if one is willing to entertain the idea that the landscape of realism is likely more complex than any one account of it has, until now, suggested on its own, there is one last step to take toward what seems to me a compelling view of the landscape of realism *and antirealism*, taken together as contrasting attitudes toward the output of the sciences. Building on recent work on the idea of epistemic stances underlying realism and antirealism, Curtis Forbes (2016) argues that grappling with the complexities of cases may help one to determine, in a better informed and more reflective way than is possible otherwise, whether to be a realist or an antirealist. He views at least some debates between those on opposite sides as having reached a stalemate, and suggests that we focus now instead on the issue of which position best fits the epistemic (and other) values of a given person. Exploring nineteenth century electrodynamics, he contends that different approaches to the science exemplified by different scientists favored different kinds of scientific practice and, ultimately, different possibilities for knowledge–of measurements, unification, and new phenomena. Seeing how the values of scientists are correlated with putative forms of knowledge in actual cases may help philosophers to see which epistemic commitments are best suited to their own values.

I take several morals from these considerations of the proper scope and application of realism. After searching critique and defense these last few decades, it is past time that we reject the notion that realism can furnish a plausible, global or universal interpretation of all of even our best theories and models. The details of cases are messy and there are highly variable degrees of similarity between different scientific contexts of practice and knowledge. One consequence of this is that adopting a realist attitude toward the content of theories does not entail that one believes all such content; one may believe only those aspects, including unobservable aspects, regarding which one thinks belief is warranted, thus espousing a realism about those things in particular (or not, as the case may be). Another consequence is that to the extent that we are able to discern patterns of epistemic warrant across different sorts of cases–this is what I take a number of proposals for realism to be doing, in their own ways–we should recognize that the strength of these kinds of warrant comes in degrees. Yet another consequence is that we should pay attention to cases where these different forms of warrant come together, and where they come apart.

To distill a conclusion from all of this new thinking about realism, once we pay attention to the messy details of science itself, it is only right that our conclusions about where and where not to apply our conceptions of realism should vary across its breadth. On my own view (which I have not argued for here, but which has certainly informed my discussion and critique of these papers), scientific realism is an attitude toward theories and models that is selective and admits of varying strengths, which is in keeping with the variability we find in the selectivity and strengths of evidence and inference exemplified by the sciences. Where the conditions are right, realism is defensible, and this leaves plenty of room for engagement between realists and antirealists of various sorts who judge these conditions differently. I have argued that some attempts to render realism defensible in the course of such engagement go too far, diluting the view to the extent that it becomes difficult to see how it can amount to anything more than a hollow shell–realism in name only. If this is really where the debate has led, irresistibly, I submit that realists should throw in the towel. But I do not believe that this is where we are. The spirit of a more substantive realism is alive and well, even if there is still a lot of work and new thinking to do in articulating what forms it can take, and when, and where.

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