Scientific Realism: What It Is, The Contemporary Debate, and New Directions

Darrell P. Rowbottom

Lingnan University

darrellrowbottom@ln.edu.hk

1. Introduction

This special issue was produced in connection with a conference, 'Science: The Real Thing?' The idea behind the conference was to assess the state of the debate on scientific realism, broadly construed, and to investigate or propose new directions – new positions, and new arguments about old positions – in said debate.

In this contribution, I do three things. First, I provide a characterization of scientific realism, with a special focus on the question: 'What's at stake in the realism debate?' This is a surprisingly controversial and tricky matter, which was discussed at length in the closing round table discussion of the conference. Second, I provide an overview of the key developments in this debate over the past decade.[1] Third, I provide a brief overview of the other contributions to this special issue, and explain how they relate to the aforementioned developments.

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[1] It is much easier to reach agreement about what counts as a contribution to the debate than it is to reach agreement about how to characterize the debate. This should be unsurprising; similarly, it is much easier to reach agreement about what counts as a science – or, for that matter, a chair or a table – than it is to reach agreement about a definition, or a significant partial definition, thereof.

2. What Is Scientific Realism?

Given the extent to which scientific realism has been discussed – for a flavour of this, consider that *The Scientific Image* has been cited 6000 times since 1980 and that *Scientific Realism: How Science Tracks Truth* has been cited 1000 times since 1999, by Google scholar's estimations – one might expect there to be considerable agreement on what, precisely, scientific realism involves. But even a perfunctory survey of the literature purporting to be on the topic dashes that hope, for as Chakravartty (2011) pithily notes:

> It is perhaps only a slight exaggeration to say that scientific realism is characterized differently by every author who discusses it …[2]

The two influential monographs mentioned above, for instance, diverge considerably on the meaning of 'scientific realism'. On the one hand, van Fraassen (1980: 8) asserts:

> *Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true.* This is the correct statement of scientific realism.

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[2] Similar sentiments are expressed elsewhere. For example, Hacking (1983: 26) writes 'Definitions of 'scientific realism' merely point the way. It is more an attitude than a clearly stated doctrine… Scientific realism and anti-realism are … movements.' and Leplin (1984: 1) notes that 'Like the Equal Rights Movement, scientific realism is a majority position whose advocates are so divided as to appear a minority.'

On the other hand, Psillos (1999: xix) states:

> What exactly … is scientific realism? I take it to incorporate three theses (or stances)…

> 1   The metaphysical stance asserts that the world has a definite and mind-independent natural-kind structure.

> 2   The semantic thesis takes scientific theories at face-value, seeing them as truth-conditioned descriptions of their intended domain, both observable and unobservable. Hence, they are capable of being true or false. … [I]f scientific theories are true, the unobservable entities they posit populate the world.

> 3   The epistemic stance regards mature and predictively successful scientific theories as well-confirmed and approximately true of the world. So, the entities posited by them, or, at any rate, entities very similar to those posited, do inhabit the world.

To reiterate, neither of these definitions of 'scientific realism' would be endorsed by the majority of experts on scientific realism. However, most experts *would* endorse definitions that bear considerable similarity to one or draw on both.[3] (In the latter case,

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[3] There are some significant exceptions to this general rule. For instance, Mäki (2005: 235) denies that scientific realism should be 'taken to be an epistemological doctrine'. He writes (Mäki 2005: 236):

> I take realism to be primarily an ontological doctrine. Semantics and epistemology are important but not constitutive of the core concern of realism. On this I agree with philosophers

for example, a thesis such as 'science seeks true theories' (Lyons 2005) might be added to Psillos's definition.) It is therefore apposite to consider these definitions as exemplars of two distinct, yet prominent, ways of understanding of scientific realism: one *axiological*, and the other *epistemological*. In the remainder of this section, I'll first discuss each exemplar in turn; in doing so, I will consider how and why other definitions of the same types vary. I will also consider how the two definitional approaches are connected, and whether they can be reconciled. I will argue that they cannot be, and propose a new framework for thinking about the scientific realism debate. My view is that we should be concerned with a cluster of issues that have been discussed under the heading of 'scientific realism' in the past century or so, and which can be easily identified without appeal to any canonical positions. Terms such as 'scientific realism' and 'scientific anti-realism' may then be understood to pick out vague-boundaried resemblance classes of positions on those issues.

2.1 The Axiological View

Van Fraassen's view of scientific realism – and hence his alternative, constructive empiricism – involves two central notions: the aim of science and acceptance of science (and more particularly its content). Let's take each in turn, and then consider how they are connected.

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like Michael Devitt whose formulation of scientific realism is put in more purely ontological terms: "*Scientific Realism*. Tokens of most current unobservable scientific physical types objectively exist independently of the mental" [Devitt 1991: 24] …

The aim of science comes first. A good way to begin to discuss this is with the admission of Popper (1983: 132), writing long before van Fraassen: '[C]learly, different scientists have different aims, and science itself (whatever that may mean) has no aims.'[4] The truth of this partially explains, even if it doesn't fully excuse, the confusion that the talk of 'the aim of science' has caused in some philosophical quarters; confusion which is illustrated, for instance, by Sorensen's (2013: 30) misguided claim that scientific anti-realists are committed to theses such as 'the scientist merely aims at the prediction and control of the phenomena … scientists are indifferent to the truth'.

But if the aim of science isn't a function of the aims of scientists, then what is it (supposed to be)? Answering this question is far from easy, as the literature on the topic – see Rosen (1994), van Fraassen (1994), and Rowbottom (2014a) – illustrates. One might propose to answer, *prima facie*, by suggesting that Sorensen's 'scientists are indifferent to the truth' should be read as 'scientists *should* be indifferent to the truth', or the weaker 'scientists *may reasonably* be indifferent to the truth'. But such an approach is no good either. The characterization of scientific realism offered by van Fraassen is not intended to be epistemological or methodological in character, or to bear on any other area where talk of what's obligated or permitted is appropriate, such as ethics. Rather, in his own words:

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[4] Although it was not published until 1983, *Realism and the Aim of Science* is a part of the postscript to *The Logic of Scientific Discovery* that was written (and read by many in Popper's circle) in the 1950s. Elsewhere, Popper (1972: 290) instead used the notion of a regulative ideal to characterize realism: '[the] regulative ideal of finding theories which correspond to the facts is what makes the scientific tradition a realist tradition.'

> Scientific realism and constructive empiricism are. [sic] as I understand them, not epistemologies but views of what science is. Both views characterize science as an activity with an aim – a point, a criterion of success – and construe (unqualified) acceptance of science as involving the belief that science meets that criterion. (van Fraassen 1998: 213)

Yet if scientific realism were a view of what science is, we would expect, given its popularity, for it to feature prominently in the literature on the demarcation problem. It does not.[5] Moreover, the very idea that scientific realism *centrally* concerns a thesis about the *point* of science, or what counts as success in science, is eccentric. A comparison between science and dowsing (or 'water-witching') illustrates this eccentricity. It's uncontroversial that the point of dowsing is to find water, and that an instance of dowsing is successful if water is found. And we also know that the process often succeeds, so construed. Yet this might be true even if dowsing does not result in a higher probability of finding water than choosing a location at random, and indeed proves less efficient (in so far as considerably more time consuming) than choosing a location at random.

Moreover, it seems that the *attitudes of people towards* dowsing determine the point of the exercise, construed as a process, and also what counts as success in doing it.

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[5] For example, there is not a single mention of realism (or constructive empiricism, for that matter) in the entry on 'Science and Pseudo-Science' in the *Stanford Encyclopedia of Philosophy* (Hansson 2014). There is a section on 'Criteria Based on Scientific Progress'. However, this doesn't engage with any of the recent literature on that topic, discussed in section 3.4. 'Progress' is used in a narrower sense than most participants in the debate on scientific progress intend it.

Views on whether the process is worthwhile *depend on* views about the point of the exercise. For instance, if one thought the point of dowsing were to find *something* of interest to the dowser under the ground, then one might think dowsing worthwhile even were it to transpire that it is not a reliable means of detecting the presence of water.

I contend that something similar is true of science, which is somewhat more complex in so far as it involves many different kinds of practice (and being a scientist doesn't require being involved in, or even competent in, the full range of possible scientific activities).[6] One can learn how to perform various scientific tasks, and perform them well, without any explicit or implicit reference to an ultimate or central 'point' of the exercise – the overarching process – of which they are a part. One may focus instead on the immediate products of these tasks. (As Kuhn (1963) noted, much science education proceeds accordingly. One is judged on whether one can grasp the exemplars, employ the methods, and solve the puzzles, for instance. Whether the puzzle-solving apparatus is fit for some greater purpose is irrelevant.[7]) 'What is science?' can be answered by pointing to those processes, how they interact, and so forth. And what science can achieve may be (largely or wholly) independent of what

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[6] Rowbottom (2014a) says more about the bearing of this variation on talk of 'the aim of science', and Rowbottom (2011a & 2013) treat its significance with respect to scientific method. The existence of such variation is shown by a number of works in recent times, such as Galison (1997), Dupré (2001), Rowbottom (2011c), and Chang (2012).

[7] Kuhn (1963: 368) allows that scientists are (typically), nevertheless, '*taught* to regard themselves as explorers and inventors who know no rules except those dictated by nature itself.' This results in 'an acquired tension … between professional skills on the one hand and professional ideology on the other.' (ibid.: 368 –369)

its practitioners think it can achieve, or any rather mystical 'point' of the exercise. This is a key reason why Psillos (1999: xxi) is on the right track in saying:

> It should be taken to be implicit in the realist thesis that the ampliative-abductive methods employed by scientists to arrive at their theoretical beliefs are reliable: they tend to generate approximately true beliefs and theories.

I would add that this thesis, which I will label 'methodological', should be made explicit in order to avoid confusion, and (perhaps) strengthened so that it doesn't pertain merely to methods of an 'ampliative-inductive' variety.[8] Armed with this methodological thesis, we are at an appropriate juncture to discuss acceptance.

At the heart of the concept of acceptance is a core on which the scientific realist and the anti-realist might agree; namely, that scientists sometimes adopt an attitude towards a theory such that they make:

> [A] commitment to the further confrontation of new phenomena within the framework of that theory, a commitment to a research programme, and a wager that all relevant phenomena can be accounted for without giving up that theory. (van Fraassen 1980: 88)

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[8] There are other methodological theses that realists might commit to as well, such as the thesis that scientists who are scientific realists (or realist in orientation) do better science than those who are not. Theses of this kind tend not to have been discussed much in journals or monographs, but are tackled by Hendry (1996), Rowbottom (2002), and Wray (2015b).

This is just (the pragmatic) part of 'acceptance', however; for van Fraassen, acceptance also involves belief.[9] Scientific realists think, van Fraassen alleges, that acceptance of a theory involves belief in the truth of said theory. But is this correct? I'll argue not. First, it is dubious that the pragmatic part of acceptance taken as a cluster (and hence acceptance) is significant for science. For example, one may be committed to 'a further confrontation of new phenomena within the framework' of a given theory without making any kind of 'wager that all relevant phenomena can be accounted for without giving' it up. One might merely have a 'wager' that most phenomena could be accounted for without giving it up. Or one could have no wager of that kind whatsoever, and merely be in the business of using the only theory that hasn't been refuted so far (as the best guess). In short, that's to say, although the presence of such clusters could be useful for science, this doesn't mean that they're necessary. As I've argued at length elsewhere – see Rowbottom (2011a & 2013) – there are many ways to organize science, construed as a group endeavour, so that the core functions therein are performed satisfactorily despite the psychological facts about its participants varying considerably. This is so much so that flatly irrational individuals may contribute a great deal to the enterprise, if they have the proper roles.

Second, a scientific realist can account for a great deal acceptance-like behaviour without thinking that it is (or should be) typically associated with belief in the truth (or, more feasibly, approximate truth) of a scientific theory. For example, one might be committed 'to further confrontation of new phenomena within the framework' of a

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[9] It seems more natural to call the commitments above 'acceptance' and then discuss what kind of beliefs do, or should, accompany them. But for ease of comparison, I will follow the use that has now, alas, become standard in the literature.

given theory because one is convinced that the theory is false (and empirically inadequate), and wants to show that it is; and one may be similarly committed simply if one wants to discover the theory's resources (and is open-minded about what those are). One may also be committed to a research programme because one wants to see where it goes, because one dreads throwing away all the work done on it already unless absolutely necessary, or because it seems like the best programme available on the basis of its past results. And so on.

Let me make the point more bluntly. Imagine members of an alien species, for whom acceptance – or if you prefer to reserve 'acceptance' for humans, call it 'a-acceptance' – involves belief *neither in (approximate) truth nor empirical adequacy*. (This might be due to psychological constraints.[10] A-acceptance could instead involve belief in significant truth content, high problem-solving power, approximate empirical adequacy, and so on.) Would we want to say that they were incapable of doing science? Or failing that, would we want to insist that they couldn't do anything with the 'character' of science? That would be strange. For they could have institutions similar to our universities, and have theories similar to our scientific theories, arrived at by the use of similar procedures. They could also use these theories for exactly the same purposes for which we use our scientific theories: to explain the origins of the universe, to build spacecraft, and so forth.

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[10] Note that this doesn't present any obstacles to these beings doing research in a way similar to our own *at the level of the group*. For example, different members of the community may pursue different theories simultaneously.

In summary, van Fraassen's ('axiological') characterization of realism is defective on (at least) two counts. First, it is too restrictive; it commits scientific realists to theses that they need not commit. Second, it is incomplete; it does not discuss theses to which scientific realists are typically committed. In short, van Fraassen misrepresents realism in such as way as to make it seem far less plausible than it is. Here is an example of this in action.

Van Fraassen (1998: 213) attributes the following definition to Forrest (1994):

> *scientific agnostic*: someone who believes the science s/he accepts to be empirically adequate but does not believe it to be true, nor believes it to be false.

He then offers a formulation of the opposite:

> *scientific gnostic*: someone who believes the science s/he accepts to be true.
> (van Fraassen 1998: 213)

He continues by declaring that:

> Scientific realists think that the scientific gnostic truly understands the character of the scientific enterprise, and that the scientific agnostic does not. The constructive empiricist thinks that the scientific gnostic may or may not understand the scientific enterprise, but that s/he adopts beliefs going beyond

what science itself involves or requires for its pursuit. (van Fraassen 1998: 213–214)

The claim in the first sentence is false and uncharitable to scientific realists. That is, even if one weakens 'true', as one should, to 'approximately true' (or some near alternative). One who believes the science she accepts to be (approximately) true may do so for a variety of reasons; for example, her default attitude towards testimony from those socially recognized as experts might be to take that testimony at face value.[11] But surely realists are not committed to the claim that such a person somehow understands 'the character of the scientific enterprise'.[12] Van Fraassen's mistake appears to result from accidental inversion of a conditional. 'If you understand the character (or indeed nature) of science, *then* you will believe the scientific theories that you accept to be approximately true' *is* a claim that many scientific realists would endorse.[13] Van Fraassen's claim, on the other hand, involves swapping the antecedent with the consequent.

At the risk of overegging the pudding, here's a final *reductio* of van Fraassen's view of scientific realism. Imagine a (rather naïve) philosopher of science who thinks that:

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[11] 'Scientific gnostics and agnostics need not be philosophers at all.' (van Fraassen 1980: 213)

[12] Note also van Fraassen's slip between 'understanding the character of the enterprise' in the first sentence and 'understanding the enterprise' in the next sentence. These are different. Such imprecision (and hence lack of clarity) is, alas, characteristic of van Fraassen's discussions on this topic. For present purposes I adopt the charitable route of assuming that he means 'understanding the character' throughout.

[13] I still don't think all scientific realists would agree with this claim. For one thing, 'will' should arguably be replaced with 'should'.

(a) scientific theories should be understood literally; (b) there is a scientific method; (c) scientists invariably use this method (else what's going on isn't really science); (d) using this method guarantees that successive scientific theories become closer to the truth (construed in a correspondence sense); (e) highly predictively successful theories are approximately true; and (f) contemporary scientific theories are invariably highly predictively successful. This philosopher also believes, as a result, that (g) what contemporary science says is pretty much right, and that (h) what future science says is guaranteed to be even more right. However, he denies that (i) acceptance should be characterized in any particular way, as he thinks that's a matter for psychological investigations that haven't yet occurred.[14] (He takes psychology to be a science.) On van Fraassen's view, this philosopher is not a realist about science!

I have focused on van Fraassen's characterization of scientific realism because of its influence. However, the idea that 'the aim of science is truth' is also present in work of several self-styled realists, most notably those influenced by Popper (or so called 'critical rationalists'). This should be of little surprise, given the quotation with which I began this section. Popper pre-empted much of what van Fraassen later said about the use of 'aim', although van Fraassen does not refer to this:

> [W]hen we speak of science, we do seem to feel … that there is *something characteristic of scientific activity*, and since scientific activity looks pretty much like a rational activity, and since a rational activity must have some aim,

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[14] Or, if one prefers, he doesn't think there is any such thing as an aim of science, or 'success in science as such'.

the attempt to describe the aim of science may not be entirely futile. [emphasis mine] (Popper 1983: 132)

A notable philosopher working in the critical rationalist tradition is Musgrave (1998: 29), who states that: 'The aim of science, realists tell us, is to have true theories about the world, where 'true' is understood in the classical correspondence sense.' However, he continues (ibid.): 'Obviously, there is more to scientific realism than a statement about the aim of science. Yet what more there is to it is a matter of some dispute among the realists themselves.' Musgrave doesn't think that acceptance has anything to do with what 'more' there is to scientific realism, however: 'If realism could explain facts about science, then it could be refuted by them too. But a philosophy of science is not a description or explanation of facts about science.' (Musgrave 1998: 239) Rather, Musgrave proposes to link the axiological view and the epistemological view, to which we will soon turn our attention. His does so by suggesting that realists are committed to views about the *achievement* of the aim. His own view about this commitment is rather moderate:

Realism … is the view that science aims at true theories, that sometimes it is reasonable tentatively to presume that this aim has been achieved, and that the best reason we have to presume this is novel predictive success. Thus characterised, realism explains nothing about the history of science. In particular, realism does not explain why some scientific theories have had novel predictive success. (Musgrave 1988: 234)

The problem with this view is that it seems far too weak to be of much interest, *except* in so far as it involves the aim component. That's partly because from the fact that it's *sometimes* reasonable to tentatively presume that an aim has been achieved, it doesn't follow that it's *usually* reasonable to so presume. Nor does it follow that it's ever reasonable *only* to presume that the aim has been achieved (as opposed to the contrary). (One might think it's reasonable to presume either way, e.g. if one prefers a voluntarist epistemology.) It doesn't follow even that reasonable presumptions should be based on strong evidence. And on a related note, the best (kind of) reason for thinking something can still be a rather weak (kind of) reason. There may simply be no better (kind of) reason available. (I grant that Musgrave may have been operating with some background assumptions that make the position more interesting. But making those assumptions explicit is important.)

Nonetheless, we can see how Musgrave's approach of introducing views on the achievement of 'the aim of science' is compatible with using the methodological thesis I discussed above. If it is true that the methods of science 'tend to generate approximately true' beliefs and theories then it follows that doing science tends to 'achieve the aim of' generating such theories. Indeed, it's a trivial consequence. It's so trivial that it would be curious to place much emphasis on. The interesting claim is the methodological one from which the claim about achievement evidently follows.[15] To put it rather more bluntly, saying 'Science reliably does X, and achieving X is its aim' adds little of interest to 'Science reliably does X', *when 'aim' doesn't refer to*

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[15] Note that there are plausibly ways to connect the view that doing something is a reliable means by which to get closer to achieving X and the view that X is 'the aim' of doing it. See the discussion of 'the aim of science' in Rowbottom 2010b for more on this.

the aims of the participants in the activity (as it is not supposed to in this context). A mundane analogy may help to see the point. Consider 'Jogging reliably improves one's fitness'. If *my* aim is to improve my fitness, this is a useful thing to know; I know that jogging will help me to achieve *my* aim. It may also be of interest to empirically determine how many people jog with the aim of improving their fitness (and what their other aims in jogging are, and whether those are 'rational' in so far as jogging is a means by which to increase the probability of achieving those aims). But what's at stake in some further dispute about whether improving fitness is *characteristic* of jogging? Perhaps if one were interested in *demarcating* jogging from other activities, one might fret about this. But if one is interested mainly in how jogging works, what jogging can achieve, and how jogging technique can be improved (with reference to specific criteria like efficiency), one needn't worry about this. That is, provided it's possible – as it indeed is – to identify instances of jogging without being able to characterize or define jogging (in a philosophically serious and respectable way).

None of this is to deny that there is a worthwhile debate to be had about what the *value* of science is. Indeed, some of the exchanges concerning scientific progress – discussed in greater depth in section 3.4 – may be understood in this (non-essentialist) vein. Determining what's characteristic doesn't result in determining what's valuable. Nor is it necessary for determining what's valuable.

2.2 The Epistemological View

In section 2, I presented Psillos's (1999: xix) characterization of scientific realism, as an exemplar of the 'epistemological' class of such characterizations. And in my subsequent discussion of the competing 'axiological' class, in section 2.1, I highlighted a methodological thesis that Psillos (1999: xxi) takes to be implicit in scientific realism (and which it is helpful to make explicit). I should now like to consider how other characterizations in the same class vary.

As a starting point – and also as a way to remind you of Psillos's definition without summarizing it or quoting from it again – it is helpful to consider a rather older, but also highly influential, definition. This is from Boyd (1980: 613):

> By "scientific realism" philosophers ordinarily mean the doctrine that non-observational terms in scientific theories should typically be interpreted as putative referring expressions, and that when the semantics of theories is understood that way ("realistically"), scientific theories embody the sorts of propositions whose (approximate) truth can be confirmed by the ordinary experimental methods which scientists employ. There are as many possible versions of scientific realism as there are possible accounts of how "theoretical terms" refer and of how the actual methods of science function to produce knowledge.

This passage bears on what Psillos calls the semantic and epistemic theses. Specifically, it contains one positive claim concerning the literal nature of scientific discourse about unobservable things (like Psillos's semantic thesis), and another

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concerning the correlation between (scientific) confirmation and approximate truth (which is part of Psillos's epistemic thesis).

On the next page, Boyd (1980: 614) also explicitly endorses Psillos's metaphysical thesis (or a near equivalent): 'Reality is prior to thought … with respect to the correctness of theories and the appropriateness of the language in which they are expressed…'[16] (He goes further in so far as he thinks 'Reality is prior to thought … also with respect to the standards by which the rationality of thought is to be assessed.') And as we'll see shortly, he also introduces a methodological thesis.

I delay introducing Boyd's methodological thesis, which is interestingly distinct from Psillos's, because we already have enough material to draw a significant conclusion about how 'epistemological' accounts vary, namely in so far as they involve different *qualifications*. For example, whereas Psillos's (1990: xix) semantic thesis of scientific realism states that 'The theoretical terms featuring in scientific theories have putative factual reference', Boyd's (1980: 613) equivalent only involves the more cautious claim that 'non-observational terms in scientific theories should *typically* be interpreted as putative referring expressions' [emphasis mine].[17] On the other hand, Psillos's (1999: xix) epistemic thesis is more cautious than Boyd's equivalent, in so far as it only concerns 'mature' science.

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[16] The following similar thesis features in Boyd's (1983: 45) later definition of 'scientific realism': 'The reality which scientific theories describe is largely independent of our thoughts or theoretical commitments.'

[17] On a later occasion, however, Boyd (1983: 45) didn't include 'typically' as a qualification: '"Theoretical terms" in scientific theories … should be thought of as putatively referring expressions; scientific theories should be interpreted "realistically".'

Differences in qualifications can be accounted for in a relatively straightforward fashion. Such qualifications are typically introduced in order to narrow the scope of, or render more precise, the position to be articulated and discussed. For realist authors, the aim of introducing qualifications is often to modify existing statements of scientific realism so as to render them more resistant to anti-realist critiques. This comes across nicely in the following passage from Musgrave (1988: 239–240), where successively more plausible views are presented:

> It is fashionable to identify scientific realism with the view that all (or most) scientific theories are true (or nearly so), or with the view that all (or most) *current* scientific theories are true (or nearly so), or with the view that all (or most) *current* theories in the '*mature*' sciences are true (or nearly so).[18]

The view that all scientific theories are nearly true may be easily refuted, by pointing to the considerable changes that have occurred in science over the past century, or even just the deep inconsistencies between competing theories at various times. For example, Thomson's model of the atom – on which it 'consists of a number of corpuscles moving about in a sphere of uniform positive electrification … in a series of concentric shells' (Thomson 1904: 255) – and Nagaoka's contemporary Saturnian model – which 'differs from the Saturnian system considered by Maxwell in having repelling particles instead of attracting satellites' (Nagaoka 1904: 445) – are different

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[18] He continues by noting, quite rightly, that: 'a pessimistic scientific realist might think none of these things without thereby ceasing to be a realist. A slightly more optimistic realist might tentatively accept some particular theory as true.'

enough for it to be clear that even if the content they share is correct, at most one could be nearly true.[19] What's more, the latter model was of an *unstable* system, on the accepted physical laws at the time: in the words of Heilbron (1977: 53), Nagaoka 'blundered'.

Musgrave mentions a variety of ways to avoid such an objection: one might adopt a variant of realism that bears on only current scientific theories, or only on theories in mature science, for example. (It is not necessary to do both, if one wants to argue that atomic theory at the turn of the twentieth-century was not mature.) Or one might take another route mentioned previously, and introduce (a high degree of) confirmation as a requirement. One might then deny that either of the aforementioned models was ever highly confirmed. Naturally, other somewhat more subtle and complicated routes are possible. For instance, one might declare that 'theory' shouldn't be understood to encompass models of the kind mentioned, or that only 'central terms' (such as 'electron') should be taken to refer successfully in the models.

The recognition that qualifications are used in the semantic and epistemic theses suggests that they have the following general form:

(Semantic)   A proper subset of scientific discourse concerning unobservable entities, S, should be taken literally.[20]

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[19] Besides, Thomson did not intend the talk of the sphere of positive charge to be taken literally. Thomson (1899: 565) stated only that: 'the negative effect is balanced by something which causes the space through which the corpuscles are spread *to act as if* it had a charge of positive electricity' [emphasis mine]

[20] 'Entities' includes properties as well as property-bearers.

(Epistemic)   A proper subset of science's content, E, is approximately true (on a

proper subset of theories of truth, T).[21]

Many varieties of scientific realism differ only in so far as they define S, E, or T

differently. We might profitably think of them as involving different sets of sets: $\{S_1,$

$E_1, T_1\}, \{S_2, E_1, T_1\}, \{S_1, E_2, T_1\}, \{S_1, E_1, T_2\}, \{S_2, E_2, T_1\}, \{S_2, E_1, T_2\}, \{S_1, E_2, T_2\},$

$\{S_2, E_2, T_2\}$ and so forth. We can then consider relations between such sets of sets –

and, if desired, their element sets – such as similarity.

Clearly, we need to say something more about the relevant sets to get to what realism

is. That is, even assuming that each variety of realism involves an identical realist

metaphysical thesis like 'reality is prior to thought' (and is otherwise 'filled in' in the

same way, e.g. to include the view that all scientific discourse concerning observable

things should be taken literally). For as it stands, S or E might even be taken to be the

empty set!

So how to move from the above to a (partial) characterization of scientific realism?

To some extent, it's helpful to think in terms of *a spectrum of positions*, ranging from

complete (semantic and epistemic) realism to complete absence of (semantic and

epistemic) realism. That is, in so far as we can use measures on, and assign rankings

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[21] This thesis is expressed imperfectly in so far as 'approximately' might conceivably be deleted;

moreover, one might, in principle, replace 'approximately' with a variable expressing degree. Partly for

reasons of economy and partly due to the current status of the debate, however, I don't include such

factors in my formulation.

on the basis of, the sets. It's fruitful to consider S and E in turn, in the first instance. (Leave T fixed as a single member set, containing the correspondence theory of truth, for the time being.) One might think as follows. First, the size of S, relative to the set of all scientific discourse concerning unobservable entities, is a rough indicator of the strength of realism in the semantic dimension. Second, the size of E, relative to the set of all science's content (including past theories and models), is a rough indicator of the strength of realism in the epistemic dimension. (All such sets are finite.) And although this form of measurement is rather crude (and awkward), comparisons between set pairs will sometimes, at least, give clear rankings: if E_2 is a proper subset of E_1, then a version of (epistemic) employing E_1 is more realist than a version containing E_2, for example. In other cases, say where there is little or no overlap between the sets, comparisons will be fraught with difficulty. Yet this is as it should be. For example, entity realism and epistemic structural realism – discussed further in section 3 – are evidently each less realist than scientific realism (of, say, the form endorsed by Psillos) in the epistemic dimension. Nevertheless, it's unclear which is more realist than the other.

It is also worth noting that the analysis above provides a perspicuous way of characterizing *the core of the scientific realism debate* (or more precisely, when the metaphysical thesis is assumed, the key elements thereof save the methodological one). Said debate involves tackling the following questions: what is S?; what is E?; and (to a lesser extent, in so far as there is more consensus and are fewer options) what is T? Strictly speaking, since absence of realist commitment doesn't imply anti-realist commitment – one might simply be agnostic – one should also consider sets S-

and E-, which feature in two 'mirror' negative versions of (semantic) and (epistemic), as follows:

(Semantic-)   A proper subset of scientific discourse concerning unobservable

                entities, S-, should not be taken literally.

(Epistemic-)   A proper subset of science's content, E-, is not approximately true (on

                a proper subset of theories of truth, T).

So the debate also involves answering: what is S-?; and what is E-? In short, it concerns how to *partition* the space of discourse and the space of content into these sets.

A brief word about T is in order at this juncture. It's uncontroversial that some theories of truth are potential members, and others are not. For example, the correspondence view is, whereas the pragmatic view is not. Whether deflationary views are acceptable, however, is a more controversial matter. Suffice it to say, for present purposes, that truth-makers must be objective and mind-independent entities on an admissible theory of truth; as Psillos (1999: xxi) puts it: 'truth is a non-epistemic concept … assertions have truth-makers … these truth-makers hinge ultimately upon what the world is like.' Thus, on the analysis above, one might fail to be a realist simply by failing to adopt an appropriate theory of truth (irrespective of how one partitions on the space of discourse and content). To make this more explicit, it is possible to remove mention of theories of truth from (epistemic) and (epistemic-), and introduce (alethic) and (alethic-) statements (involving sets T and T-). This would

serve to provide a more precise analysis, but at the expense of greater complexity. The simpler route was preferable, here, partly because disputes on theories of truth are infrequent in the current debate.

We have seen that the foregoing analysis provides a way of characterizing the comparative strengths of at least some forms of realism (and anti-realism), and also that it provides a relatively elegant way to characterize the scientific realism debate. The analysis also avoids – and is of help in illustrating – two reasonably common pitfalls in characterizing scientific realism. The first involves appeal to *arguments*, or key propositions in arguments, that those professing to be scientific realists (tend to) employ. For example, Leplin (1984: 1) includes the following items on his list of 'characteristic claims, no majority of which, even subjected to reasonable qualification, is likely to be endorsed by any avowed realist':

3. The approximate truth of a scientific theory is sufficient explanation of its predictive success.

4. The (approximate) truth of a scientific theory is the only possible explanation of its predictive success.

However, claims such as 3 and 4 are offered to *support* variants of (semantic) and (epistemic), as realists typically acknowledge; they are used in 'no miracles' style arguments *for* – or explanationist defences *of* – scientific realism. It is important to keep them in their proper place. They could be false as a group, despite science reliably producing approximately true theories (in a correspondence sense), for

example. And crucially, *neither is reducible to a statement of a general form that all realists will accept*.

The second pitfall involves characterizing scientific realism in terms of knowledge, or knowledge acquisition. For example, Boyd (1980: 613) writes that scientific realism involves the claim that: 'Scientific knowledge extends to both the observable and the unobservable features of the world' and Psillos (1999: xix) claims that: 'Going for realism is going for a philosophical package which includes a naturalized approach to human knowledge.'[22] Chakravartty (2011) even goes so far as to say that approaches to defining scientific realism: 'have in common … a commitment to the idea that our best theories have a certain epistemic status: they yield knowledge of aspects of the world, including unobservable aspects.' And no doubt such claims inspired Bird (2007) to propose an epistemic view of scientific progress (which has subsequently been criticized from both realist and anti-realist perspectives, as detailed in section 3.4). The temptation to connect claims about truth (or approximate truth) with knowledge is rather natural. After all, a realist is liable to be tempted to think not only that much of science is approximately true, but also that they *know* that much of science is approximately true, and therefore know a good deal about the world in so far as they are familiar with the relevant science.

It is not necessary to succumb to this temptation in order to be a realist, however, and

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[22] Elsewhere, Psillos (2000: 707) writes: 'The… *presumptuous* claim is that, although this world is independent of human cognitive activity, science can nonetheless succeed in arriving at a more or less faithful representation of it, enabling us to know the truth (or at least some truth) about it.' Again, there are two distinct claims here: arriving at a more or less faithful representation of something doesn't entail enabling us to know (some) truth about it.

introducing knowledge into one's characterization of scientific realism (and the debate) is problematic for two reasons. First, it serves to complicate matters unnecessarily, in so far as the extent to which one takes science to generate knowledge will depend on which theory of knowledge – and on which theories of related notions such as justification, warrant, and belief – one prefers. For process reliabilists, for instance, the presence of a reliable means of generating true statements will suffice for science to provide knowledge (perhaps with the addition of some provisos about belief formation, involving, for example, reliability of testimony). For internalists, on the other hand, access to reasons for belief is required for beliefs to be justified (and hence constitute items of knowledge). Even the simplest theories of knowledge introduce complications. Consider Sartwell's (1992), on which knowledge is merely true belief. Might science not isolate true claims, and rely for its successes on truth-like representations, which nobody believes in (or can fully appreciate)? Think, for instance, of the use of computer simulations. No-one can hold in their head all the detail of typical simulations used to forecast weather or to determine chemical reaction pathways, yet their predictive successes might rely on the accuracy of the modeling assumptions (and other data) therein. Note also that in some cases, parts of the data used might never have been believed in. Automated weather stations might provide data directly to the computer conducting the simulation, for example.

This brings us on to the second point, which is that the prior characterizations in terms of knowledge make some positions – including significant and influential positions in the history of philosophy of science – count as 'realist', or as having a 'realist' character, for the wrong reasons. Take Boyd's (1980: 613) claim as an exemplar: 'Scientific knowledge extends to both the observable and the unobservable features of

the world'. Now consider Popper's (1972: 286) view that knowledge may: 'be contained in a book; or stored in a library; or taught in a university'. On this view, knowledge may be false, and not even approximately true (or anywhere close).[23] But clearly it can nevertheless be of 'observable and unobservable features of the world'. So the truth of Boyd's claim is admitted by Popper, but not in a way that is of relevance to the realism debate. Moreover, this oversight would not be fixed by using 'subjective knowledge' in definitions such as Boyd's. For Popper (1972: 111) also thought: 'traditional epistemology, with its concentration on … knowledge in the subjective sense, is irrelevant to the study of scientific knowledge'. Indeed, many a critical rationalist would deny that there is any subjective knowledge, above and beyond belief, in so far as she would deny that justification is possible; see Bartley (1984) and Rowbottom (2010c: ch. 1).

Of course, critical rationalism is now 'old hat'. But an emphasis on knowledge also rules out realist views that have been defended recently. Saatsi (In Press), for example, makes the case that minimal realism involves the view that scientific theories (probably) *latch on to the world* when they're predictively successful. This sets the stage for a discussion of the methodological element of realism, from which Saatsi's minimal realism is distilled.

The methodological component of scientific realism is introduced by Psillos (1999: xxi), recall, as follows: 'the ampliative-abductive methods employed by scientists to arrive at their theoretical beliefs are reliable: they tend to generate approximately true

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[23] The notion of knowledge is close to the contemporary one of 'information', at least if one does not think that information needs to be true. See Allo (2010) and Rowbottom (2014b) for more on this.

beliefs and theories.' A neat way to understand the core of this claim is in terms of the probability calculus, under a world-based interpretation of probability (such as a propensity view).[24] Let t represent a theory, M denote 'was selected by scientific methods', and $\approx$ denote 'is approximately true'. (Selection may involve high confirmation values, as suggested by Psillos's version of the epistemic thesis.) Then, the methodological claim is, at the bare minimum:

$P(\approx t, \text{M}t) > 0.5$

It might plausibly be somewhat stronger, namely:

$P(\approx t, \text{M}t) >> 0.5$

Indeed, it would appear to be reasonable to require Psillos to specify an interval on which he takes $P(\approx t, \text{M}t)$ to lie.

Now let's compare this with the methodological thesis associated with scientific realism by Boyd (1980: 613–614):

> [Progress] is achieved by a process of successive approximation: typically,
> and over time, the operation of the scientific method results in the adoption of

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[24] I assume there is some unintended imprecision in Psillos's statement: he presumably didn't intend to require that the methods *generate* the theories or beliefs, as opposed to confirm them or select them. (Confirmation or selection of generated theories will be a special case. Many generated theories will never be confirmed.)

theories which provide increasingly accurate accounts of the causal structure of the world.[25]

This suggests a rather different thesis (which would hold on the assumption that if T provides a more accurate account of the causal structure of the world than T' provides, then T is more approximately true than T'). Let T+ represent 'is closer to the truth than', and L+ represent 'was selected later than'. Part of Boyd's claim, at the bare minimum, is:

$P(T+(t2, t1), Mt1 \& Mt2 \& L+(t2, t1))>0.5$

As before – in this case and the following – '>' might conceivably be replaced by '>>'. One might also reasonably expect an interval to be specified for the relevant probability, although none is provided.

Boyd's methodological claim also entails, more interestingly, that:

$(Mt1 \& Mt2 \& Mt3 \& L+(t3, t2) \& L+(t2, t1)) \rightarrow P(T+(t3, t1))>P(T+(t2, t1))$

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[25] Interestingly, Boyd (1980) says something similar about scientific language and scientific methods; that's to say, he takes these to improve successively too. Later, Boyd (1983: 45) also offered a weaker methodological claim:

> Scientific theories, interpreted realistically, are confirmable and in fact often confirmed as approximately true by ordinary scientific evidence interpreted in accordance with ordinary methodological standards.

Careful analysis is required to determine which variant of the methodological thesis – Boyd's or Psillos's – is bolder. On the one hand, Psillos's variant doesn't *entail* that future theories will probably be more truth-like than earlier theories; it doesn't entail, that's to say, science's probable convergence on the truth. So at first sight, it avoids the kind of 'convergent realism' that's the target of Laudan (1981), about which I'll say a little more in a moment. On the other hand, Boyd's variant doesn't *entail* that any isolated use of scientific method(s) will probably result in an approximately true theory. It's compatible with thinking that many – or even most – theories arrived at by the use of those methods are (probably) not approximately true.

Consider now, however, what would follow if Psillos's methodological thesis were true and reasonably believed to be true by scientists. Then scientists could legitimately use said thesis to support inferences about theories. Imagine, for example, that they were comparing a new theory (selected with scientific methods) with past theories selected (in the same way). If the new theory diverged considerably from all the past theories, then there would be a very low probability that it was approximately true, in so far as there would be an exceptionally high probability that at least one of the older theories was approximately true. Thus, the scientists would have good grounds to reject the new theory. In essence, their belief in the reliability of their methods would lead them to think (it significantly more probable) that they had failed in one (recent) case, rather than repeatedly.

If approximately true theories all share significant content in common, moreover, then it follows from Psillos's thesis that considerable continuity in scientific theories is much more probable than not, over extended periods time (albeit not continuously).

Thus, it appears plausible that Psillos's thesis is stronger than Boyd's. Both are committed, to 'a [convergent] form of realism' involving 'variants of the following claims', among others:

> [Part of] R1) … more recent theories are closer to the truth than older theories in the same domain.

> R3) Successive theories in any mature science will be such that they 'preserve' the theoretical relations and apparent referents of earlier theories.
>
> (Laudan 1981: 20–21)

To be specific, Boyd and Psillos are committed to weaker variants of R1 and R3 involving the introduction of 'typically' or 'reliably', and hence (world-based) probability claims.

Let's now try to generalize rather more. Realists tend to think scientific methods are reliable means by which to achieve/select truth-like, or to move closer towards achieving/selecting truth-like, theories. But on the reasonable assumption that those methods involve selecting theories on the basis of virtues that they display (perhaps relative to competing theories) – predictive or explanatory power, for example – then the *underlying* claim involves linking said virtues to truth-likeness.

That's to say, there are (at least) two general forms for the methodological theses advocated by realists:

(Methodological)   Scientific methods reliably (or typically) select theories or models that are virtuous.

(Methodological-C)   Scientific methods reliably (or typically) select theories or models that are more virtuous than their predecessors.[26]

And associated with these theses are theses concerning virtues and truth-likeness (or accuracy), such as the following:

(Virtue)   Virtues are (typically) indicative of a degree, *d*, of truth-likeness or representational accuracy.

(Virtue-C)   If *t1* (or *m1*) is more virtuous than *t2* (or *m2*), then *t1* (or *m1*) is (typically) more truth-like (or more representationally accurate) than *t2* (or *m2*).[27]

As suggested above, different realists will also have different views on what the relevant virtues are, how they should be ranked in order of importance (if at all), and so forth. But the details of this need not concern us here. To put it tersely, the need for theses such as (virtue), in addition to (methodological) or (methodological-C), arises because truth-likeness (or representational accuracy) cannot be directly observed, so to speak, rather than detected (or inferred). The oddity of the claim that scientific

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[26] 'C' stands for 'comparative'.

[27] A recent example of a version of realism subscribing to (virtue-C) is the 'relative realism' defended by Mizrahi (2013b).

methods reliably find truth-like theories, but that those theories (typically) have nothing significant in common other than being truth-like, illustrates this. For we devise methods to enable us to select on the basis of *observable* features (whether or not we take said features to be indicative of further features). Note also that many anti-realists endorse (methodological) or (methodological-C), but not (virtue) or (virtue-C).

A significant result is that qualifications concerning the link between scientific methods and theoretical truth-likeness may arise in two distinct ways. First, one can take the methods to typically succeed (or succeed with probability P) in finding virtuous theories. Second, one can take a virtuous theory to typically be (or with probability Q be) truth-like. Lumping instances of theses such as (methodological) and (virtue) together tends to obscure this.

But are such theses necessary for scientific realism, or, failing that, central to characterizing it? One might think not, at first sight, in so far as the role that they play, in combination, is to *support* theses of (epistemic) form. Nonetheless, they cannot convincingly be dispensed with. Consider, for example, a philosopher who accepts that most of the content of science is approximately true – and even that successive generations of scientific theories will be increasingly truth-like – but insists that this is a purely accidental feature of the enterprise (i.e., is a matter of mere luck). She denies that there's any link between predictive power and truth-likeness, or indeed explanatory power and truth-likeness, although she accepts that scientific methods reliably select theories with these properties. (Imagine, if liked, that she has a trusted source who has testified that the theories are, or will continue to be increasingly,

truth-like – an alien who surreptitiously intervenes in our science, or the creator of a simulation in which we dwell, or some such.) Does she count as a scientific realist? It appears not, in so far as one can imagine all the scientific realists I've cited arguing against her view of science rather vehemently. She fails to endorse any of the following aspects of the 'realist stance' that Saatsi (In Press) highlights:

> … trust in the reliability of the scientific method in yielding theories that latch better and better onto the unobservable reality; trust in the corresponding objective theoretical progress of science; trust in the thesis that our best theories that make novel predictions (by and large) do so by virtue of latching onto unobservable reality.

This brings us to Saatsi's 'minimal realism', which posits a correlation between the virtue of (novel) predictive power and 'latching on to the world', or what we might call possessing a *degree* of truth-likeness or representational accuracy (rather than passing a specific threshold, e.g. in the case of 'approximate truth'). In his own words:

> [S]cience can make theoretical progress in the sense of theories latching better and better onto reality in a way that drives theories' increasing empirical adequacy and enables them to make novel predictions. Corresponding to this broader conception of theoretical progress there is a more minimal conception of realism, understood simply as a commitment to this broader kind of theoretical progress. (ibid.)

This is interesting because it *doesn't* require commitment to any E for (epistemic), although it *does* involve commitment to (methodological-C) and (virtue-C):

> [T]his kind of minimal realist commitment provides nothing like a general *recipe* that could be applied to a given current theory—e.g. the standard model of particle physics—to specify what unobservable features of the world we can claim to know… (ibid.)

Indeed, the foregoing analysis shows that there are other positions in the vicinity (and that determining which is minimal is no easy matter). For instance, one might instead couple (methodological) with (virtue-C), and appeal to the same virtue (namely, novel predictive success).

We now come to the thorny question of whether 'minimal realism' and positions in the vicinity, alluded to above, should be counted as forms of scientific realism. I prefer to answer in the negative, in light of the work in the tradition that I've canvassed above, based on the centrality of theses of the form of (semantic) and (epistemic) in historical characterizations of the position. However, I take myself to have argued that positions like minimal realism are *necessary parts* of scientific realism. Hence, I don't think, for example, that one can be a scientific realist and deny (methodological) and (methodological-C) or (virtue) and (virtue-C). It follows that whether such theses hold is a significant bone of contention between realists and anti-realists (and indeed non-realists). It also follows that there are significant forms of semi-realism that involve such theses: and I would call 'minimal realism' such a form of semi-realism. This isn't to denigrate it in any way. It's an interesting proposal for a

modest standalone position on the realism debate, which is worthy of further discussion.

3. Recent Developments

This brings me on to recent developments in the scientific realism debate. These are so many and varied that summarizing them is another Herculean task. As a result, I must make some compromises. Even restricting oneself to work appearing in the last ten years, it is impossible to cover all the relevant developments in the literature. This is partly because methodological issues (e.g. concerning confirmation and models) bear on the realism debate.

I do, however, use a few guiding principles. First, I bias my coverage towards recent work, and especially very recent work that may not yet be widely known. (Other existing resources, such as Chakravartty (2011), can help with older material.) Second, I make a special effort to include work by relatively young scholars that may not yet have attracted as much attention as it deserves. Third, I opt not to explain in detail how the topics covered relate to the discussion in the previous section. Fourth, I settle for rough characterizations of most of the positions I discuss. (And I urge you to look to the cited literature to see more precise characterizations.) Fifth, and finally, I cover how the papers in this special issue contribute to recent developments in the course of the discussion, rather than in a separate section.

Primarily, I hope that this part of the paper will be of use, in combination with the previous part, to those hoping to enter the debate (or even to dip into part of it), by

giving them an orientation and leads to follow up. I hope also it will draw the attention of experienced contributors to the debate to material that they might, understandably, have overlooked. This is likely because the debate is rather fractured. For instance, a specialist in structural realism and philosophy of physics might easily miss what others are writing on arguments concerning old-fashioned scientific realism or on how scientific progress should be construed.

I organize this section with sub-headings at several levels, to make it easier to navigate.

3.1 Alternatives to Scientific Realism

This heading covers a broad array of developments, in one of the most active and exciting areas in general philosophy of science: generating feasible alternatives to older, typically more bold and sweeping, positions in the scientific realism debate. Key, in particular, are views that are more cautious, epistemologically speaking, than full-blooded scientific realism: 'selective' views on which set E is more restricted ('smaller'), and set E- is less restricted ('bigger'). A well-known example of such a view is Hacking's (1983) entity realism, on which, roughly, things that scientists take themselves to be able to manipulate should be thought to exist (although their properties may be quite different from what scientists take them to be and the theories involving them may not be approximately true). Better known still are the alternatives to which I now turn, namely structural realisms.

Before I do, I should emphasize two more things. First, I consider the positions below to be alternatives to scientific realism rather than attempts to revitalize scientific realism. I cover such revitalization attempts subsequently. Second, I don't cover relatively old positions that do not attract so much attention at the cutting edge of the debate, like instrumentalism of a positivistic variety, constructive empiricism, or the aforementioned entity realism. This is a tricky judgement call; but space is limited.

Structural Realisms

It is still fair to say, following Ladyman (1998), that there are two main forms of structural realism. The first, epistemic structural realism, holds roughly that scientific theories (and models) are (or should be expected to be for methodological reasons) approximately true in what they say about the *structure* of the unobservable part of the world, although typically not its inhabitants. The second, ontic structural realism, holds roughly that structure is ontologically basic, primitive, or fundamental (and perhaps that there are good methodological grounds, *inter alia*, for believing this).

Both forms continue to be discussed regularly: notable collections in which both are explored and defended, typically by senior philosophers, are Bokulich and Bokulich (2011) and Landry and Rickles (2012). However, more attention is presently focused on ontic structural realism, which I'll therefore devote more space to.

Ontological Structural Realism

Ainsworth (2010) and Frigg and Votsis (2011: §4) provide excellent overviews and taxonomies of key variants of ontic structural realism, but treat matters differently. Ainsworth discusses three views: (1) (multi-place) relations are ontologically fundamental whereas objects and properties aren't; (2) (multi-place) relations and objects are ontologically fundamental whereas properties aren't; and (3) relations and properties are ontologically fundamental whereas objects aren't. Frigg and Votsis instead distinguish between radical and eliminative ontological structural realism. The former involves the view that there *is* only structure. The latter, which is more plausible, involves the idea that 'relations do not need relata between which they hold… "objects" are only places in a relational structure (in as far as they are something at all) and should not be taken ontologically seriously.' (Frigg and Votsis 2011: 262). However, this appears to rule out Ainsworth's variant (2), which is unintended: instead Frigg and Votsis call this position *attenuated EOSR* (and point out that this is compatible with denying that objects are individuals, as its main advocates, French and Krause, do). So there are disagreements about how best to characterize structural realisms, in line with those about how to characterize scientific realism discussed above, although these are (thankfully) of a less dramatic variety. (It is important to realize, when reading the literature, that even 'eliminative' is not used consistently. For instance, Lam and Wüthrich (2015) use 'eliminative' and 'radical' interchangeably.)

One of the most important recent contributions is French (2014), which articulates and defends ontological structural realism in considerable depth. This monograph is especially notable for examining the extent to which ontological structural realism bears on chemistry and biology (building on French (2011) on the latter), as most

work on the topic focuses on microphysics, and especially areas such as quantum field theory. This has already prompted some more work on structural realism and biology, such as Sterpetti (2016), which focuses on population genetics.

Nevertheless, work on how to understand ontological structural realism in the light of fundamental physics continues unabated. Special focus is currently on making the ontological priority claims more precise. Roberts (2011), for example, examines the plausibility of group structural realism – the view that 'The existing entities described by quantum theory are organized into a hierarchy, in which a particular symmetry group occupies the top, most fundamental position' (ibid.: 50) – and finds it troublesome in so far as defending one metaphysical hierarchy, rather than another, is difficult. A key problem is that 'symmetry groups *are* describable [in] terms of their own symmetry group structure' (ibid.: 57). Wolff (2012) instead examines two different strategies for characterizing priority – in terms of reduction or dependence. She argues that the first is not compatible with ontological structural realism because the *kind* of objects that instantiate a given structure is relevant in a physical context. However, she finds that the dependence-based approach is more promising, in so far as it allows that the identity of an object may depend on the structure(s) of which it is a part. Finally, McKenzie (2014) employs Fine's notion of ontological dependence to show how structural realism might be cashed out in particle physics. She discusses entangled particles and the group-theoretic conception of elementary particles. With regard to the former, she finds that ontological structural realists need to say more about the identity conditions of relations before we can decide if objects and structures are on different ontological footings. With regard to the latter, she finds that 'fundamental particles and the associated group structures are on an ontological par'

(ibid.: 377). She concludes that a new ontological view, which admits both kinds of entity as fundamental, is superior.

Also worthy of mention are Arenhart and Bueno (2015), Nounou (2015), Thébault (2016), and Esfeld et al. (2017). I cannot discuss them all, so will only say something about the two most recent. The first begins to explore whether the process of quantization reveals any general principles for constructing structural frameworks for theories, and uses non-relativistic particle mechanics as a case study. The second presents an ontology based on 'primitive stuff', or stuff with no physical properties, which is consistent with ontic structural realism; it uses Bohmian mechanics for illustrative purposes.

Not all useful contributions proceed with close reference to scientific theories, however. For example, O'Conaill (2014) examines whether structure might be construed as abstract or concrete (on the view that concrete objects depend for their identities on such structure). He also explores whether an ontic structural realist might legitimately deny the very distinction between abstract and concrete. In broad agreement with McKenzie (2014), O'Conaill concludes by emphasizing that ontic structural realists should be explicit about how the identities of concrete objects can be determined by the structures of which they are a part.

Finally, there has also been some interesting work on the radical variant of ontic structural realism. Most notably, Bain (2013) has argued that it is sustainable if one construes fundamental physical theories, such as QFT, in a category-theoretic, rather than a set-theoretic, fashion. Lam and Wüthrich (2015) aim to refute this claim, and

contend moreover that it is difficult to make sense of the notion of relations, or indeed relata, without understanding structure in a set-theoretic way.

Epistemic Structural Realism

In their comprehensive introduction to epistemic structural realism, Frigg and Votsis (2011: §3) denote the two main kinds thereof as *direct* and *indirect*. These differ in so far as they draw the distinction between the observable and the unobservable differently. And hence, they involve different views on the kinds of scientific statements we should expect to be approximately true. (Frigg and Votsis instead set them up as different views on what we can know, but I avoid that for reasons explained in my prior discussion of scientific realism.) Roughly, indirect structural realists take us not to be acquainted (in, e.g., Russell's sense) with physical objects, as opposed to sense data (or something similar), and therefore classify physical objects as unobservable. As such, they hold that we should not expect science to arrive at approximate truths regarding any such objects. Direct structural realists disagree, and take us to be acquainted with many physical things.

Frigg and Votsis (2011: §3.1.1–§3.1.3.) provide a pellucid history of the roots of the indirect version, and draw, in particular, on the work of Russell, (Grover) Maxwell, and Poincaré. They then offer a thorough account of the direct version's history, and some of the key arguments for and against it (ibid.: §3.2.1–§3.2.2). I can do no more than point to this here. However, the remainder of my coverage is made easier by the fact that several of the key arguments against epistemic structural realism bear on both versions thereof.

One important issue is how structure is to be construed. Since this has already been discussed in the coverage of ontic structural realism, however, I will not revisit it here, except in so far as it relates to a key criticism of structural realism, namely the Newman objection. Roughly, the idea behind this is that it is trivially true that the unobservable part of the world exhibits the structures that structural realists say it does. This is typically illustrated with reference to the Ramsey sentence, which several authors, such as Maxwell, Worrall, and Zahar, have used to characterize structure. As Smithson (In Press) puts it:

> Newman's Objection is commonly paraphrased as follows: the Ramsey sentence is "trivially" (i.e., automatically) true so long as:
>
> (1) The observable content of the Ramsey sentence is true and
>
> (2) We quantify over a domain that meets a certain cardinality constraint.

After summarizing the history of the debate on this objection – also recommended is Frigg and Votsis (2011: §3.3–§3.4.2) – Smithson (In Press) claims that a simple response has been overlooked.[28] His idea is that epistemic structural realism is not trivial if it links the predictive success of a theory with the existence of unobservable things exhibiting the structure thereof. He then suggests a way of amending the Ramsey sentence as a result, and considers numerous possible objections to this amendment; however, he argues that his preferred response is effective even if the

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[28] This is implausible, given the history of realist positions discussed previously: the significance of novel predictions on Musgrave's view, theses of the form (virtue) and (virtue-C), and so forth.

Ramsey sentence should be rejected as a means of characterizing structure (as several structural realists, such as French and Ladyman, think).

Also worthy of mention are Davoody (In Press), which argues for Russell's version of epistemic structural realism, and Newman (2010), which argues that structural realism is too narrow, like many other selective forms of realism, in so far as it appeals 'to a *single* principled means by which we ought to interpret our best theories so that we can establish the required historical continuity.' (ibid.: 414).

Other Alternatives

Other alternatives to scientific realism, especially of the selective realist variety, continue to appear. One of the most discussed, in the past decade or so, has been the semirealism proposed by Chakravartty (1998; 2007). Central to semirealism is the distinction between two kinds of properties, namely detection properties and auxiliary properties. In the words of Chakravartty (1998: 394–395):

> We infer entity existence on the basis of perceptions grounded upon certain causal regularities having to do with interactions between objects. Let us thus define *detection properties* as those upon which the causal regularities of our detections depend, or in virtue of which these regularities are manifested. *Auxiliary properties*, then, are those associated with the object under consideration, but not essential (in the sense that we do not appeal to them) in establishing existence claims. Attributions of auxiliary properties function to supplement our descriptions, helping to fill out our conceptual pictures of

objects under investigation. Theories enumerate both detection and auxiliary properties of entities, but only the former are tied to perceptual experience.[29]

Semirealism bears mainly on (epistemic) and (epistemic-); the idea behind it is that we should believe in detection properties (and hence the putative things that bear them), but not in auxiliary properties. Auxiliary properties fulfill heuristic roles, and part of the task of science, as Chakravartty sees it, is to attempt to make auxiliary properties into detection properties. The position has recently been criticized in a number of symposium papers – French (2013), Ghins (2013), and Psillos (2013) – to which Chakravartty (2013) responds.

Several of the contributions to this issue promote other interesting alternatives. First, taking inspiration from the 'second philosophy' of Maddy (2007), Wolff (This Issue) explores the prospects for a quietist, yet naturalistic, stance on the realism debate. She finds that it is difficult to see whether this is a position we can adopt in practice, but suggests that considering the issue from a social epistemological perspective might hold some promise.[30]

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[29] There is some lack of clarity about the definition of 'detection properties', because Chakravartty (1998: 402) later states: 'detection properties [are those] on the basis of which we infer entity existence'. That's to say, the initial definition seems ontological whereas the latter seems methodological. I propose to resolve this tension in the following way. I think Chakravartty's original definition above should have read: 'define *detection properties* as those upon which the causal regularities of our detections *putatively* depend, or in virtue of which these regularities are *putatively* manifested'.

[30] The idea of quietism as an option is older. For example, Suárez (2012) – which is based on a talk originally given in 2006 – argues that Kitcher's 'real realism' reduces to this.

Second, Nanay (This Issue) advances a singularist semirealism, which was first presented in Nanay (2013). This is a position on (semantic) and (semantic-), according to which singular scientific statements should be construed in a realist fashion – i.e., as meaningful – whereas non-singular statements should be construed in an anti-realist fashion: 'there is always a fact of the matter about whether the singular statements science gives us are literally true, but there is no fact of the matter about whether the non-singular statements science gives us are literally true' (Nanay 2013: 371). Nanay (This Issue) endeavours to motivate this position in a new way, namely by comparison with entity realism. Specifically, Nanay argues that entity realists should be singularist semirealists, if they want to avoid their position collapsing into (a form of full-blooded) scientific realism.

Third, and finally, Elgin (This Issue) advocates a position she calls 'constructive nominalism', according to which truth is only defined within theoretical frameworks, in so far as the way in which we classify things depends on our cognitive constructs. She argues that this view is preferable to scientific realism in so far as it better accounts for actual scientific practice.

3.2 Arguments For and Against Realism

We now come to arguments for and against realism. Again, there is much activity in this area. On the one hand, there are discussions of old arguments – new articulations, critiques and defences thereof – that continue unabated. And on the other, there are

entirely new arguments. I'll begin by saying something about the former, which will of practical necessity be incomplete, and then say a little more about the latter.

Old Arguments – No Miracles and the Pessimistic Meta-Induction

The key historical arguments for and against realism are the no miracles argument, which has its roots in the work of Maxwell (1962) and Smart (1963: 39) although it is typically attributed to Putnam, and the pessimistic meta-induction, an early version of which appeared in the work of Poincaré (1902).[31] Discussion on each argument remains remarkably vigorous.

One key recent debate concerns the base rate fallacy, and the extent to which this occurs in either, or both, arguments: Howson (2000) claims that it occurs in the former, Lewis (2001) argues independently that it occurs in the latter, and Magnus and Callender (2003) extend their reasoning somewhat.[32] Consider the no miracles argument, which roughly says that scientific realism is the only view that makes the empirical success of science non-miraculous, for example. This allegedly depends, for its success, on the probability of a considered theory being approximately true, independently of said theory being (predictively or explanatorily) successful, being reasonably high. (Below, I refer to this probability as $P(\approx T)$.) Yet this premise is typically suppressed, and not argued for, in presentations of the no miracles argument.

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[31] On the background to the no miracle argument, see Psillos (1999: 72–77).

[32] For brief introductions to the fallacy, along with similar worked examples, see Rowbottom (2015: 132–133) and Henderson (2017: §3).

However, Henderson (2017) argues that the no miracles argument only succumbs to the base rate fallacy if it is construed in a local rather than global form, i.e. as a thesis concerning individual theories, and if $P(\approx T)$ is evaluated based on a random pick from all theories (compatible with the existing evidence). This point becomes quite obvious – although this is not the way Henderson expresses it – if one recognizes that prior probabilities in real confirmation theoretic contexts are of a conditional form like $P(\approx T, B)$, where *B* represents background knowledge or information.[33] The route open to advocates of the no miracles argument is clear in principle; they might argue that such background-conditional probabilities tend to be reasonably high, and even that they tend to increase as science progresses. (Note that I say this without presuming that the operant interpretation of probability should be subjective, or even degree-of-belief based.)

Along somewhat similar lines, Sprenger (2015) – inspired by Fahrbach (2009), which is discussed below as a new argument against the pessimistic meta-induction – instead develops a new probabilistic model of the local no miracles argument, on which disciplinary context (and hence, one might think, *B*) is relevant. He emphasizes the significance, in particular, of the extent to which there has been prolonged theoretical stability, and the extent to which satisfactory alternative theories are available to scientists in principle.

Finally, Dawid and Hartmann (In Press) provide a formal reconstruction of the global version of the no miracles argument, which they take to be the canonical version.

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[33] For more on the significance of background knowledge or information in confirmation theory, see Rowbottom (2014b).

They argue that it doesn't involve the base rate fallacy, in so far as it rests on a claim about the frequency with which theories have been predictively successful in science (or some proper sub-set thereof, due, for example, to qualifications). In essence, the argument may formally be said to involve $P(T, F)$ – and could be relatively easily modified to involve $P(\approx T, F)$ – where F represents the aforementioned frequency statement. A key challenge for those appealing to the no miracles argument is thus to explain how they arrive at good measures of the frequency of the success of theories.

Other interesting work on the no miracles argument includes Frost-Arnold (2010), which identifies a difficultly for (methodological) naturalists wishing to endorse it. This arises because its explanans arguably doesn't result in any novel predictions or have any unificatory power, as acceptable explanantia do in science.

This brings us to the pessimistic meta-induction, which has to some extent been superseded by the argument from unconceived alternatives, discussed at length below. Very roughly, this argument is that we should expect current predictively successful theories to be false (and not even approximately true), because past theories were false (and not even approximately true) despite being predictively successful. Naturally, however, variations in the argument are possible: for example, Laudan (1981) focuses on the history of false existence claims concerning unobservable entities, in particular, and has only convergent realism, which was covered much earlier, as his target.[34] Vickers (2013) provides an even longer list of examples than that provided by Laudan, and considers what three of these cases tell us about how

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[34] Explorations of different varieties of this argument can be found in Ruhmkorff (2014) and Wray (2015a).

realists might distinguish between working and idle (or active and inactive) theoretical posits.

Frost-Arnold (2014) argues that endorsing the pessimistic induction involves implicitly accepting a considerable degree of *semantic* anti-realism, on standard views in the philosophy of language, although most anti-realists are not semantic anti-realists. The underlying issue is that sentences containing non-denoting terms are (at least *prima facie*) meaningless – i.e., are not truth valued – according to standard views on reference-fixing. (In terms of my earlier terminology, we may say that S- is considerably larger than most anti-realists allow, if the pessimistic meta-induction goes through.) Frost-Arnold also considers the feasibility of many potential anti-realist responses to this problem, pointing out, for instance, that semantic anti-realism need not take the form advocated by the positivists (in line, for instance, with the 'cognitive instrumentalism' of Rowbottom (2011b; Manuscript, Ch. II)).

Fahrbach (2011) instead offers a new realist response to the pessimistic meta-induction, which might be called the argument from the exponential growth of science. His key claims are that over eighty percent of all scientific work has been done since 1950, and that almost all of science's highly successful theories have remained stable thereafter. (His measure of scientific work is based on the number of journal articles produced and the number of scientists.) He concludes that this provides evidence against the view that scientific theories will prove to be as unstable in the future as they were in the more distant past.

Wray (2013) responds to Fahrbach's argument, convincingly, as follows. Past scientists might have inferred, on a similar basis, that the dominant theories of their time were true. However, they would have been wrong to so infer. And so are we, unless some kind of significant differences between contemporary theories and past theories can be identified. This, Wray affirms, is the task that faces the realist in the light of the historical record.[35]

Finally, Mizrahi (2015) challenges several arguments from the history of science against scientific realism, such as those presented by Laudan and Vickers, in so far as they rely on (consciously or unconsciously) 'cherry-picking' historical episodes.[36] In the case of inductive arguments from the history of science, for example, one might doubt that the samples are representative. Thus it is not appropriate to consider them to reveal anything about the probability of a theory being successful while failing to be approximately true (or positing non-existent entities, etc.).

Unconceived Alternatives

The most influential recent argument against scientific realism – and arguably, several other forms of more selective realisms – is the argument from unconceived alternatives presented by Stanford (2001; 2006). The key idea behind this is that unconceived theories may be superior to their conceived counterparts, in several

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[35] See also Müller (2016), which advances a different criticism concerning the burden of proof.

[36] I don't find this charge against Vickers (2013) to be fair, in so far as he claims that 'Divide et impera realism needs to be challenged by, and developed in light of, the full historical record…each example has the potential to bring something new to the debate… it remains possible that we might develop a recipe for identifying idle posits.'

respects.[37] And Stanford (2006) argues also that there have been such unconceived theories repeatedly in the history of science. He then argues that we should expect there to be such theories now, on inductive grounds. (This is sometimes referred to as 'the new induction', by contrast with the old, pessimistic, meta-induction.) As I explain in Rowbottom (In Press), however, the inductive move is not necessary to mount a significant challenge to realism. Rather, the historical presence of such unconceived theories gives grounds for withholding belief in scientific realism, unless scientific realists can give a convincing account of why contemporary science should be thought to be any different from science past (or similarly explain why, as science progresses, we should expect there to be ever fewer unconceived alternative theories).[38]

Most of the responses to the argument from unconceived alternatives (construed broadly) are critical and realist in character. I'll begin by considering some of the most recent pieces of this character.

Inspired by Chakravartty's semi-realism, discussed in the previous section, Egg (2016) argues that the realist can adopt the strategy of appealing only to causal knowledge, in order to develop a form of selective realism that is immune to the argument from unconceived alternatives. In essence, his approach involves offering a more precise characterization of detection properties and auxiliary properties, in terms of causal warrant and theoretical warrant.

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[37] For a formal treatment of the argument, see Rowbottom (In Press; Manuscript, Ch. III).

[38] For more on the burden of proof in underdetermination-style arguments, see Belot (2015).

Dellsén (In Press) instead argues that the probability of there being (serious) unconceived alternatives to a given theory depends on several social and historical factors, such as the size of the relevant scientific community, the extent to which it fosters creativity (or articulation and exploration of rival views), and the amount of time the theory has been considered.[39] As a result, he suggests that one might resist the argument from unconceived alternatives by developing a kind of '*social* scientific realism', involving qualifications based on the aforementioned factors.

Frost-Arnold (This Issue) focuses on the merits of Stanford's own appeal to the history of science to attack scientific realism; specifically, he explores the limitations of the way Stanford puts unconceived alternatives to work towards this end. He argues that Stanford's approach precludes appeal to: (a) conceived theories that were initially rejected as inferior to the dominant theory, but eventually replaced it; and (b) cases where hypotheses made on the basis of *projective* inferences were later rejected (because, for instance, a further variable was recognized to be significant). He appeals to a number of cases from the history of science. These include hypotheses concerning the Earth's motion considered by Ptolemy, in order to illustrate the significance of (a), and concerning velocity addition, to illustrate the significance of (b). Frost-Arnold (This Issue) concludes by also criticizing what Stanford says about theory acceptance.

Not all work on unconceived alternatives is critical, however. Some anti-realists have recently extended the argument from unconceived alternatives – or perhaps, it might

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[39] These might plausibly be simplified into *extent* of imaginative labour and *quality* of imaginative labour factors, in line with the style of treatment in Rowbottom 2011a; 2013.

better be said, devised new arguments from unconceived alternatives. The basic idea involved, which is explored at length by Rowbottom (In Press; Manuscript, Ch. III), is that we need not consider only unconceived theories. Rather, we can consider unconceived models, experiments, observations, predictions, explanations, methods, instruments, experiments, and even values. Wray (2016) discusses one such option, and presents an 'argument from unconceived methods'.[40] He uses this to target an argument for realism offered by Devitt (2011), which is based on the idea that scientific methods improve over time.

Finally, Dawid et al. (2015) explore when, if at all, scientists are justified in believing that there are no alternatives to one of their theories. They focus, in particular, on the issue of whether a kind of non-empirical confirmation is possible: whether failure to find an alternative to a theory might serve to confirm the theory. Their treatment of this question isn't realist in character, however. Rather, they are 'interested only in arriving at empirically adequate theories, and not in the more ambitious goal of finding theories that are true under a given interpretation' (ibid.: 216).

Other New Arguments

Wray (This Issue) argues that scientific theory change is often driven by changes in scientists' research interests, and especially when they opt to investigate phenomena that dominant theories cannot account for. Moreover, he thinks that a lack of appropriate interests explains why some theories are unconceived despite being conceivable in practice. He holds that his findings account not only for the fact that

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[40] Wray and Rowbottom's work on this was independent, occurring at around the same time.

the history of science is a graveyard of failed theories, but also support the view that theory change will continue indefinitely.

3.3 Revitalizing/Reformulating Scientific Realism

Next we come to attempts to revitalize scientific realism. Mäki (2005: 231) helpfully explains that there are two ways to go: on the one hand, one might address 'the unit of science question (realism about which parts of science?)', whereas on the other, one might tackle the 'contents of realism question (which realism about science?)'.

I've already discussed the latter kind of approach, at length, in section 2.2, and engaged with some of the recent contributions in this area, such as Saatsi (In Press), while mentioning others such as Mizrahi (2013b), in the process. Here, then, I will only cover Vickers's pertinent contribution to this special issue.

Vickers (This Issue) targets the success-to-truth inference, and discusses how precisely theses such as (virtue) and (virtue-C), which we both take to be at the heart of realism, should be formulated. He brings together a variety of qualifications from the existing literature in order to present a highly qualified version of scientific realism that is strongly resistant to historical arguments – such as the pessimistic meta-induction – which are effective against other less qualified versions. He also begins to explore the new, non-historical, difficulties that arise for his qualified scientific realism.

Going Local on the 'Unit of Science'

Finally, rather than qualifying one's claims about the whole of science, it's possible to 'go local' – or restrict the scope of scientific realism – in a different way, namely by holding that (such full-blooded) realism only goes for a proper subset of science's content. There are many possible ways to do this; a crude way, for instance, might be to say that scientific realism doesn't go for physics, although it does go for biology. More subtle views are also possible; for instance, one might restrict scientific realism to specific sub-disciplines, or even particular *kinds* of well-confirmed theories. (Indeed, similar moves may be made with several of the alternatives canvassed above.) Asay (This Issue) makes a sustained case that this is the way to go, and advances the methodological thesis that the realism debate should be conducted at a finer-grained level than that of science *tout court*. He also provides a brief overview of related work in this vein in recent years – see Asay (This Issue: §5) – which I will not recapitulate.

3.4 Scientific Progress

As mentioned towards the end of section 2.1, a lively debate concerning how scientific progress should be understood is also underway. I'll begin with a brief overview of this. The resurgence of interest in the topic was prompted by Bird (2007), who argues for an epistemic view of progress, according to which scientific progress invariably consists in increases in scientific knowledge. This claim has been widely criticized. Rowbottom (2008, 2010) argues that Bird's view is too restrictive on several counts; e.g., in so far as discovering new false theories, or believing in true theories without justification, may prove progressive. Bird (2008) responds to some,

but not all, of those charges. Cevolani and Tambolo (2013) and Niiniluoto (2014) offer other criticisms of Bird's proposal, especially from the point of view of defending their preferred (realist) 'semantic' alternative, which involves the notion that increases in theoretical verisimilitude are central to scientific progress.

This semantic view of progress is attacked in Rowbottom (2015a, Manuscript, Ch. I), which advances the view that making scientific progress *more centrally* involves increasing our predictive power and understanding (construed in a non-factive fashion, such that understanding how some phenomena interrelate does not require having an accurate representation of the observable or unobservable systems responsible for generating them). Another important consideration is that predictive power may be understood to involve know how, which is an aspect of progress emphasized more generally by Mizrahi (2013a). This debate continues in Niiniluoto (In Press). Alternative perspectives are also beginning to be presented. Dellsén (2016), for example, advances the view that progress should be characterized solely in terms of understanding (although presumably of a more realist variety than that envisaged by Rowbottom).

Saatsi (This Issue) also argues against Bird's (2007) epistemic view, but from a realist angle. He contends – in connection with the 'minimal realism' presented in Saatsi (In Press), and discussed towards the end of section 2.2 – that theoretical progress can occur merely as a result of theories 'latching better onto unobservable reality'. He focuses on theories that fail to be even approximately true, despite so doing. He also discusses at some length what it means, on his view, for one theory to latch onto reality better than another.

3.5 Other Debates and Developments

Sadly, it hasn't been possible for me to cover every debate of relevance to scientific realism, let alone every contribution of relevance in the past decade, in the above. But I should like to mention a few other areas of significance before I close. First comes the history of philosophy of science, and especially early debates concerning realism in which scientists – many of whom we now think of as scientist-philosophers – were actively involved. For instance, Ivanova (2015a, 2015b) explores the work of Duhem and Poincaré, and Rowbottom (Manuscript: Ch. II, IV) takes a look at some of the anti-realist views of nineteenth century physicists and chemists. Second, there are the ongoing debates about models, and especially about the role of abstractions, idealizations, and approximations therein. Interesting recent work in this area includes Odenbaugh (2011), Psillos (2011), Weisberg (2013), and Reutlinger et al. (In Press). Third, and finally, there are other disputes concerning scientific method – or closely related issues – that bear on the realism debate. These include the extent to which perception is theory-laden, as discussed by Votsis (2015).

4. Conclusion

In this paper, I have done three things. First, I have provided a detailed analysis of scientific realism, with reference to the work of its historical proponents, in order to shed light on its structure and content. I presented a novel characterization thereof (and of the related debate) as a result, which will be of use to existing scholars in the field – in situating their own positions and exploring new options, for example – as

well as to those seeking to understand (or enter) the debate from outside the field. Second, I have provided a summary of some of the most important recent and contemporary work concerning scientific realism, including the papers in this special issue. I trust that this will also be useful to both of the aforementioned groups, in highlighting work that they may not have been aware of, providing access points to the literature on a variety of issues, and so forth.

Acknowledgements

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