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S.I.: SCIENTIFIC REALISM

Going local: a defense of methodological localism about scientific realism

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Abstract Scientific realism and anti-realism are most frequently discussed as *global* theses: theses that apply equally well across the board to all the various sciences. Against this status quo I defend the *localist* alternative, a methodological stance on scientific realism that approaches debates on realism at the level of individual sciences, rather than at science itself. After identifying the localist view, I provide a number of arguments in its defense, drawing on the diversity and disunity found in the sciences, as well as problems with other approaches (such as basing realism debates on the aim of science). I also show how the view is already at work, explicitly or implicitly, in the work of several philosophers of science. After meeting the objections that localism collapses either into globalism or hyperlocalism, I conclude by sketching what sorts of impacts localism can have in the philosophy of science.

Keywords Scientific realism · Anti-realism · Disunity of science · Constructive empiricism

1 Introduction

A notable trend in the philosophy of science has been a growing recognition that in addition to "wholesale" or "global" views regarding scientific realism, there are also "retail" or "local" views. According to these perspectives, we need not develop views that require, say, the approximate truth (or lack thereof) of *all* contemporary, well-confirmed theories, or the existence (or lack thereof) of *all* the unobservable entities postulated by those theories. Rather, subtler and more nuanced positions are avail-

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able, which may turn out to be more defensible. Consider entity realism (Cartwright 1983; Hacking 1983), cognitive instrumentalism (Rowbottom 2011b), semirealism (Chakravartty 1998, 2007; Nanay 2013), and structural realism of the non-ontic variety (Worrall 1989). Perhaps the best view in the philosophy of science is one that doesn't draw such grand, sweeping conclusions about how we should understand the metaphysical and epistemological implications of scientific theories.

My interest in this paper is to take such views as an invitation for pondering whether or not many traditional questions investigated in the name of scientific realism have not been fruitfully posed. This is not to consider the prospects of quietism in the vein of Fine (1984), Maddy (2001), and Blackburn (2002). In past work (Asay 2013) I have argued for the legitimacy of the issues arising from the debate over scientific realism, and for the untenability of the quietistic position; I maintain that perspective. Rather, my project is to ask whether "scientific realism" should ever have been thought of as an easily containable thesis akin to moral realism, mathematical realism, and others. My suggestion is that there is no "domain" of science that is at all comparable to the domains of morality, mathematics, and the like. "Science" involves everything from astronomy and botany and chemistry to zoology. One might think of "science" as not a domain at all, but rather as a way of exploring different domains of thought. Perhaps it was a mistake to find one single realism debate here, rather than indefinitely many.

This "pluralist" perspective has its roots in a long and distinguished history. In some ways, it draws on the difficulties faced by the old "demarcation" problem, of trying to find a principled dividing line between scientific and non-scientific domains (cf. Sankey 2008a, pp. 254–255). The view that there is a tenable distinction was perhaps most forcefully rejected by Feyerabend (1975). If no precise border between science and non-science is available, then the question of what counts as "science" becomes a pragmatic or sociological question, rather than a philosophical question with immediate epistemological and metaphysical significance (cf. Rorty 1988, p. 50; Newton-Smith 2000, p. 2). Yet even if there is broad consensus on this pluralistic, post-demarcation understanding of science, it has not resulted in widespread rejection of global perspectives on scientific realism. Sankey (2008b), for one, defends a methodologically pluralist conception of science alongside a generally globalist commitment to scientific realism. Localized perspectives have, however, begun to flourish, and I shall be incorporating such views into my own. Still, the need remains for a full articulation of what remains of the debate over scientific realism in the wake of a thoroughly pluralist understanding of the nature of science.

My goal, then, is to offer a reconfigured approach to the question of scientific realism, and what will amount to a pluralistic understanding of the many scientific *realisms*. I intend to give a systematic presentation of the localist view, and show how it already informs some recent work in the philosophy of science.[1] The kinds of questions philosophers have raised when discussing scientific theories are all legitimate, as are the parallel questions arising in different corners of philosophy. But when asked so broadly, and explored with necessarily blunt theoretical tools, these questions will not yield the most satisfying answers. My hope is that we can make progress in the

[1] See, for instance, Faust and Meehl (2002), Saatsi (2010, 2012, and 2015b), and Egg (2016).

philosophy of science not by dissolving the standard debate, but rather by breaking it down into a number of smaller debates that more closely resemble the realism debates found elsewhere in philosophy. In so doing, we will respect the important diversity inherent to the sciences while preserving continuity with the metaphysical and epistemological concerns arising elsewhere in philosophy. The philosophy of science that results will doubtless be more challenging, but also liberating.

2 Globalism and the localist alternative

The positive view I shall be defending relies on two crucial distinctions. First is the difference between globalism and localism. Scientific realism, even in its most comprehensive incarnation, is still a kind of local realism. It is a realism concerning the domain of science only; it remains neutral on parallel questions concerning the reality to be found within the domains of morality, aesthetics, mathematics, and others. My concern is thus with globalism about scientific realism in particular. Globalist theses about scientific realism take as their scope the entirety of scientific discourse; they are concerned with the well-confirmed theories that belong to any and all of the sciences. Localist views, naturally, restrict their range to some subset or discipline of scientific discourse. As a result we are left with a continuum between globalist views on the one hand that cover all of science, and hyper-localist views on the other that maximally focus on a single claim of science. Viewed in this way, there are indefinitely many ways of articulating localist positions. My defense of localism is in support of the sorts of views on the localized side of the spectrum.

But this way of drawing the global/local distinction is not yet sufficient for isolating my view. For many of the familiar positions on realism are localist in the sense of isolating their realism to a limited amount of scientific discourse. For example, Bas van Fraassen's constructive empiricism is realist with respect to the observable elements of scientific theories, but anti-realist with respect to their unobservable elements (1980). The entity realist distinguishes the entities posited by scientific theories from what those theories say about them, and embraces realism only about the former. Structural realists distinguish the unobservable structure of the world as expressed by scientific theories from the nature of the entities that stand in those structures. Acceptance of a theory for the structural realist involves belief only in what theories say about those structures. When approaching the same scientific theory that they all accept, constructive empiricists, entity realists, and structural realists all end up with varying commitments concerning the theory, all of which are proper subsets of what scientific realists commit themselves to. In this sense, these familiar not-fully-realist views are all local: they embrace realism only about the observable, the existence of entities, or the world's structure.

While I find the various distinctions drawn from the traditional views illuminating and relevant to the debate over scientific realism, their means of dividing the scientific landscape in two (observable versus unobservable, structure versus nature, etc.) is not what I have in mind by localism. These views are all very much non-localist in that they are still views about *science* as a whole. Constructive empiricism focuses on the distinctions between observables and unobservables, not on the distinctions between

physics and biology. The constructive empiricist is agnostic about *all* unobservables, regardless of whether they feature in the theories of biochemistry or psychology. The localist I seek to isolate is one who approaches scientific realism discipline by discipline. (Or, as the case may require, sub-discipline by sub-discipline.) The localist might adopt structural realism about quantum physics, but realism about biology and constructive empiricism about psychology. In this way, all the tools that have been utilized so far in the debates over scientific realism can continue to feature in localized thinking; what they are stripped of is any pretension that they automatically generalize to all the many corners of science.

Thus far, I have emphasized the distinctions between different branches of science, rather than more general distinctions that can be put to work in any science. But I should stress that localists can and should embrace both sorts of distinctions in articulating their views. The differences between physics and chemistry are relevant to realism, and so too are the differences between structure and nature, or between observables and unobservables. This "all of the above" approach to realism creates a vast terrain of logical space. One might be a traditional realist about some branches of science, an entity realist about others, and constructive empiricist about still others. One might be a structural realist (but no other kind of realist) for several domains, and a constructive empiricist (but no other kind of anti-realist) for the others. The possibilities are endless, and the localist does not prejudge the tenability of these various combinations.

The second distinction at work in defining the localist alternative is between first-order and methodological theses about scientific realism. First-order views about some local domain of science involve what realism-relevant stance (realism, instrumentalism, empiricism, etc.) applies for that domain. One might be a constructive empiricist about physics while being a realist about biology, say. Methodological views, by contrast, are about how scientific realism debates and arguments should be structured. The methodological globalist goes about arguing for global theses about science in one fell swoop. Arguments and distinctions are deployed without restriction. The methodological localist resists such arguments, and approaches the questions of realism individually for individual scientific domains. My interest in this paper is in defending methodological localism. The best way to come to the truth about scientific realism is to explore each branch of science individually on its own metaphysical and epistemological terms. The result of such inquiries could be a first-order globalism: perhaps realism or empiricism turn out to be appropriate for every domain. Even if this turns out to be the case, we will have arrived at that fact more honestly and appropriately if we adopt methodological localism.[2]

Globalist views on scientific realism—on both the first-order and methodological levels—are the status quo. Localist views are, however, gaining momentum. "In recent philosophy of science," writes Samuel Ruhmkorff, "there has been a trend towards the consideration of local rather than global issues" (2013, p. 424; cf. Vickers 2013,

[2] Note that my methodological localism is distinct from the "methodological pluralism" defended by Sankey (2000, p. 211). Sankey's pluralism maintains that there are a variety of rules and methods for evaluating science that apply differentially across scientific disciplines and eras. My pluralism, by contrast, concerns debates over scientific realism, and how they should be conducted on a localized basis. I do see my pluralism as a natural outgrowth of the kind of pluralism that Sankey defends: once we reject global perspectives on the nature of science, we should reject global perspectives on the nature of scientific realism.

pp. 247–248). Several such views will be discussed below.[3] Still, many of the *loci classici* in the philosophy of science are thoroughly globalist in their rhetoric and ambitions. According to Stathis Psillos's entrenched and widely accepted definition (1999, p. xvii), realism is committed to the literal interpretation of all scientific theories, and to the approximate truth of all mature, well-confirmed theories. Van Fraassen's global ambitions are equally manifest: he defines realism and empiricism in terms of the aim of science itself (1980, p. 8). From the outset, then, global views are committed to treating all the sciences uniformly. Science has a single aim—be it truth or empirical adequacy—and all aspects of all scientific theories are to be given the same sort of interpretation. All unobservables, all entities, all structures are created equal, regardless of the branch of science in which they appear.

Arguments for and against scientific realism frequently have a similarly global character, which reveals the discipline's implicit preference for methodological globalism. This is certainly evident, again, in many of the classic presentations of the arguments for and against realism. The argument from cosmic coincidence (Smart 1963, p. 39) and the no miracles argument (Putnam 1975, p. 73) take as their explanandum the success of science in general. The pessimistic induction (e.g., Laudan 1981) looks to the entire history of science for its collection of false but empirically successful theories. Underdetermination arguments (e.g., Stanford 2006) apply equally well to different scientific theories from various domains.[4] As realism is argued for and argued against, particular examples from particular domains of science occur quite frequently; however, such examples are used in the service of demonstrating global positions about science in general.

My main interest is in challenging this globalist perspective, and adding further support to the localist camp. The philosophy of science need not operate with the assumption that what goes for one science goes for the others; claims to the effect that all the sciences share a unifying aim need, at the least, compelling arguments to support them. As I shall argue, scientific realism suffers from an inability to take advantage of insights from other domains of philosophy precisely because its domain is not properly analogous to the domains of the other realism debates. "Science" need not be understood as a domain of discourse in the way that morality, aesthetics, and mathematics are. In response to such concerns, I'll be offering a defense of a more localized approach to various kinds of scientific *realisms*.

3 Motivating localism

As we have noticed already, the philosophy of science has long made room for views that settle somewhere between the full-bore realism currently defended by Psillos and

[3] See also Miller (1987), which defends a kind of localism that is more partial toward realism than my methodological view.

[4] Stanford actually presents a difficult case *vis-à-vis* localism and globalism. At one point, he suggests that underdetermination arguments might affect some disciplines more strongly than others (2001, p. S6). Magnus detects a tension in Stanford's presentation of his underdetermination argument, arguing that although he presents it "as a retail argument when he is being careful, he has wholesale aspirations" (2010, p. 809). Magnus argues that Stanford's argument is most effective when deployed locally. See also Egg (2016).

the radical instrumentalism at times defended by positivists and other thoroughgoing empiricists (though not usually in the way that I am advocating). The motivations behind some of these views are instructive, and if properly followed through with lead to the more radical localized approach to scientific realism that I defend.

Commenting on van Fraassen's empiricist conception of science as "saving the phenomena", Ian Hacking writes:

> Van Fraassen is fundamentally in error when he holds that all science is a matter of empirical adequacy and saving the phenomena. He holds this erroneous view because, like almost all philosophers, he is totally theory-oriented, and thereby blind to experiment. Natural (experimental) science is a matter not of saving phenomena but of creating phenomena […]. But in astrophysics we cannot create phenomena, we can only save them. (1989, p. 578)

Hacking's point is that while the idea of saving the phenomena might go some way toward explaining the aim of astrophysics, it doesn't apply equally well to other branches of science where we are more in charge of which phenomena come to exist. Hence, while the history of science provides us with rich, powerful case studies from astronomy, we should be cautious in deriving global conclusions about science itself from them. Hacking's interest in making this observation is, naturally, to support his own view, which takes the sciences as providing sufficient justification for believing in certain kinds of unobservable entities such as electrons. But he captures the basic localist sentiment: what goes for one science need not go for others.

In the remainder of this section, I consider a number of arguments in favor of methodological localism. Taken together, they provide a strong defense for reconfiguring the standard debates in scientific realism.

3.1 Diversity

The first, and I think most powerful argument for localism comes from the rampant diversity of the sciences. Immersing oneself in the literature on scientific realism, one finds myriad examples drawn from astronomy, physics, and biology. There's already an enormous amount of ontological diversity between stars, subatomic particles, and species. But the sciences enjoy a still broader range of topics, including everything from anthropology to meteorology to zoology. The concerns of science range from the smallest quark to the largest galaxy, and from simple ideal hunks of matter to massively complex ecosystems. Because of this ontological diversity, there is substantial reason for being particularly cautious when it comes to the metaphysical consequences of scientific theories. The ontological and epistemological issues concerning the existence of fields, quarks, and forces are distinct from those concerning mitochondria and bacteria, not to mention beliefs and desires. Yet all of these entities figure into mature scientific theories.

As a result, there appears to be far less unity of subject matter in all the fields covered by scientific realism than by other forms of realism in philosophy. Mathematical realism, for instance, concerns the central metaphysical and epistemological concerns about numbers and other mathematical entities. Ontological unity between different

kinds of numbers (natural, rational, real) is quite plausible.[5] If so, there is positive reason for identifying mathematical realism as an informative and useful thesis. Moral realism concerns the metaphysical and epistemological implications of our commitments regarding moral, evaluative, and normative patterns of behavior and thought. While perhaps not as uniform as mathematics, ethics is still a relatively homogenous enterprise. Moral realists, for example, hope for a relatively uniform theory of the ontology of moral facts and properties. Scientific realists, by contrast, are faced from the outset with a most eclectic menagerie of entities.

Simply put, "science" is nowhere near as unified a domain of our thought as are morality and mathematics (assuming even that those domains are sufficiently unified). In fact, thinking of science as a domain of thought at all, alongside morality, mathematics, aesthetics, and others, might just be misguided. At best, physics, chemistry, biology, and the others are decent candidates for being unified domains of thought. Science itself is too broad a category to qualify. Perhaps, then, rather than thinking of science as a whole as a particular domain of thought, apt for its own unique realism debate, we should conceive of it more as a methodology, a way of approaching different domains of thought. One can approach the subject matter of physics scientifically (though one can approach it other ways as well), but it's ultimately physics, not science, that is the target of issues concerning realism and anti-realism.

Compare, for example, someone who lumped together morality and aesthetics as branches of "normativity", and then argued for global realism or anti-realism about normativity. Such a person might take considerations drawn from aesthetics ("beauty is in the eye of the beholder"), and use them to argue for anti-realism about all of normativity. Such a perspective would obscure the kind of account that might, for instance, offer a more realist interpretation of moral values alongside a more anti-realist perspective on aesthetic values. The unsubtle global view would miss the fact that perfectly valid observations concerning one aspect of normativity are irrelevant to other aspects. The broader and more global our realist theses are, the less nuance we will be able to bring to them.

Hence, the sheer diversity of the concerns of science suggests that smuggling them all under the single heading of "scientific realism" is bound to ignore and erase the very real differences between the different scientific disciplines. Because those differences are relevant to the metaphysical and epistemological concerns of philosophers of science, the globalist is bound to end up with an inadequate, or at least needlessly hindered, philosophy of science.[6]

One unfortunate consequence of ignoring this diversity is that by lumping all the sciences together into a single realism debate, we lose the ability to draw analogies with other forms of realism in philosophy. Finding common ground with the other realism debates in philosophy is potentially quite illuminating; the closer these other

[5] At least nowadays. But each kind of number has had to earn its ontological keep, and some mathematicians (such as Leopold Kronecker) have been skeptical about only some kinds of numbers (Ferreirós 2007). So perhaps localism can identify a useful precedent in mathematics. Thanks to Johanna Wolff on this point.

[6] Cf. this admirably clear statement of localism from Magnus: "the question of scientific realism is not one that can be decided for science *simpliciter*. Perhaps we should believe in some of the unobservable entities posited by our best scientific theories but not in others" (2013, p. 50).

realism concerns come to the realism concerns of science, the more the parties to the different debates can learn from one another. Richard Boyd's work (e.g., 1988) finding commonality between scientific and moral realism, for instance, has been especially influential. Of course, the worth of such comparisons depends on just how strong the similarity is between the different domains to which realist ideas are being assigned; topics of particularly strong relevance in one realism debate may be of little importance elsewhere. My contention is simply that localists are in a better position to utilize insights and arguments from other realism debates, given that their theses are more analogous to other realism theses than are globalists'.[7]

3.2 Disunity

Above I argued that science is a disparate field, one whose concerns are sufficiently heterogeneous so as to motivate not sweeping them all under a single rug for the purposes of realism and anti-realism. This is a sort of "disunity" thesis about science. A related claim concerning the disunity of science is found in the discussion over scientific reduction. In Oppenheim and Putnam's seminal discussion of the unity of science (1958), they defend the view that the special sciences are appropriately reducible to fundamental physics. Others have argued against this reductionist picture, thereby defending instead a kind of autonomy for the special sciences (e.g., Fodor 1974). While theses concerning the reducibility of certain sciences into others is strictly orthogonal to concerns about realism or anti-realism in science, the arguments in favor of disunity also point in favor of localism about realism. For example, anti-reductionism about the sciences is sometimes motivated by looking to the differences in methodology in the different sciences [see, e.g., Keller (2002), Rowbottom (2009), and Rowbottom (2011a)]. If different branches of science employ vastly different methodological tools, then it is reasonable to adopt localism. For the strengths or weaknesses of realist and anti-realist perspectives sometimes depend on the methodological principles active in the different sciences. If different sciences use different epistemological methods, then we need to evaluate those methods on a case-by-case basis in determining what the best attitude is to take with respect to them *vis-à-vis* realism.

One example of disunity at work in science concerns not the differences between different branches of science, but rather the intra-domain distinction between historical and experimental science. The methods involved in doing historical research (collecting fossils, observing geological structures, examining records, etc.) are very different from those employed in designing and running new experiments. One sort of inquiry collects existing phenomena which nature has spared from destruction, while the other creates new phenomena to specifically test the tenability of the relevant hypotheses. Given these very different sets of methods, it would be premature to assume that both forms of inquiry are of equal epistemological standing. The methodological localist sees this state of affairs as presenting a strong case for localism: whether the same attitude about realism is appropriate for both the historical and experimental dimensions

[7] One salient place for localists to start might be particularism in metaethics (e.g., Dancy 2004), which is its own kind of (ethical) localism.

of a particular science can be decided only after investigating the merits of the case; matters may turn out differently in different domains, depending on what evidence is available. This sort of debate can be witnessed in the exchange between Cleland (2002) and Turner (2005) over the competing merits of historical and experimental science; Cleland argues that they are epistemically on a par, whereas Turner argues that historical science is epistemically inferior in at least some respects. Regardless of how this first-order matter turns out, that it needs exploration at all is an argument favoring localism.

One major proponent of the disunity view of science is John Dupré (e.g., 1983, 1993). Dupré argues that science is best thought of "as consisting of a loose and heterogeneous collection of more or less successful investigative practices" (1993, p. 238). Dupré rejects reductionism on the grounds that the requisite "bridge principles" that connect higher-level sciences with lower-level sciences are not forthcoming. In particular, the natural kinds of certain sciences do not appear to be plausibly reducible to the kinds of other sciences. Furthermore, the natural kinds at work in many sciences often do not exhibit any *essential* features—features that would be necessary in order to ground the biconditionals that would compose the needed bridge principles.

If non-reductionism about the special sciences is true, then one possible avenue to globalism about science is immediately shut down. Suppose that all the special sciences can be reduced down to fundamental physics. Then, presumably, whatever view *vis-à-vis* realism one decides on for fundamental physics would carry over to all other branches of science. If one is a realist about physics, and all the other sciences reduce down to physics, then presumably one would be a realist about all of science.[8] But on the non-reductionist alternative, there is no fast track to globalism. Non-reducible sciences enjoy a degree of autonomy from the sciences to which they're not reducible. As a result, no metaphysical and epistemological results from the different sciences can be immediately applied, without sufficient demonstration, to the non-reducible science.

Some related grist to the localist mill can be found in John Norton's argument for the thesis that "inductive inference schemas will only ever be licensed locally" (2003, p. 652). Norton is interested in familiar puzzles about induction, including Goodman's famous puzzle to the effect of why inductions on 'green', say, succeed whereas inductions on 'grue' are problematic (1954). Norton argues that proffered universal rules of induction are such that "if they are general enough to be universal and still true, the axioms or principles become vague, vacuous, or circular" (2003, p. 651). Induction requires particular uniformities in nature, and those uniformities must be brought into the inferences in question. In this way, induction must always be understood locally, taking into account the relevant uniformities at stake. Norton's view, if correct, has localist implications for any view about scientific realism that relies on inductive argument, most notably, of course, the pessimistic (meta-)induction and no miracles arguments.

[8] Though not necessarily. One might be pushed to the view, say, that realism was appropriate *only* for fundamental physics, since other sciences are derivative and therefore not fully realist. How the realism/anti-realism divide connects to the fundamental/derivative divide is an independent matter not to be decided here.

Obviously, the unity of science and Norton's material theory of induction are themselves controversial issues in the philosophy of science, and this paper is not the place to fully evaluate their merits or faults. I appeal to them mainly in order to highlight the commonalities between methodological localism and disunity views. Challenges to the unity of science (and induction) can be harnessed in support of the localist point of view. Those who defend disunity should adopt localism, and localists can turn to disunity for support of their own view.

3.3 Against aims

Another pathway to globalism about realism is by way of the aim of science. If realism and anti-realism are presented as views about the aim of science (as in van Fraassen 1980), then the assumption of a single aim of science will entail a single position about realism and anti-realism. I grow less and less convinced that there's any fact of the matter as to whether or not there is an aim to science, and what it would be if there is. Furthermore, even if there is such an aim, I am no longer convinced that our understanding of it will provide an answer to the questions that scientific realism and its opposing doctrines are traditionally trying to solve. Let me now defend both of these contentions.[9]

A familiar challenge to van Fraassen's aim-first approach to realism is that it might appear to identify the aim of *science* with the various aims of *scientists* (e.g., Rosen 1994). Van Fraassen rightly rejects this overtly sociological presentation of the realism debate. Instead, the aim of science is to be understood in terms of what its "criterion of success in actual practice" turns out to be (van Fraassen 1994, p. 182). While individual scientists may have distinct, personal goals behind their work, they "participate in a common enterprise, defined by its own internal criteria of success, and this success is their common aim 'inside' this cluster of diverging personal aim" (ibid.). A common analogy here is the game of chess (e.g., van Fraassen 1980, pp. 8–9). Individual players may have their own individual ambitions and reasons for playing, but the game itself is defined by the pursuit of checkmate. The aim of chess is to checkmate one's opponent; to succeed in chess is to realize that aim (even if one doesn't achieve one's personal goal of, say, international fame). Determining the aim of chess is simple enough. Deciphering a common criterion of success behind the various practices of scientists, however, is no easy task, as van Fraassen recognizes: "We should presumably grant that there is a strong disanalogy between chess whose rules and criteria for success are uncontroversially defined by official rule books and such large and vaguely circumscribed cultural phenomena as 'the game of science"' (1994, p. 186).

The problem with van Fraassen's response to the disanalogy, however, is that it ought to suggest that the aim of science, if such a thing even exists, is likewise rather messy and vaguely circumscribed. But van Fraassen's proffered answers to the aim question—constructing true theories, or constructing empirically adequate theories—are, to the contrary, quite clean and simple. Van Fraassen's ideas on what the aim of science might be are too polished to be capturing what counts as success for what is,

[9] See also Rowbottom (2014) for further critiques of the "aim" approach to scientific realism.

by his own admission, an extremely unpolished set of disparate, loosely connected practices. At best, we should hope for some wildly disjunctive account of the aim of science. In fact, the large and vaguely circumscribed nature of that cultural phenomenon we call 'science' strongly suggests that there just is no fact of the matter as to what counts as success in that enterprise. What would provide the grounds for such a fact? Van Fraassen contends that the conscious intentions of scientists and their own views *vis-à-vis* the aim of science are not what constitute the aim of science. Still, the aim of science must somehow be born from the scientists themselves: "There is nothing *outside* the conscious understanding of the participants to define the activity" (*ibid.*). Van Fraassen admonishes us not to construe this idea naively, yet it is not easy to see what the sophisticated view is supposed to be, and how it ultimately leads to a precise aim for science.

Suppose somehow that this ontological worry about there being a fact of the matter as to what constitutes the aim of the messy business of science can be solved. It strikes me that an answer to the question of aim still misses the point of the debate over scientific realism. To see why, consider two hypothetical communities. The first community engages in practice A, whose constitutive aim is in fact to construct empirically adequate theories. The second community engages in practice B, whose constitutive aim is to construct true theories. Van Fraassen's question is: is *our* practice that we call 'science' A or B? He thinks it's A; realists think it's B. My question is: who cares? What I care about, given my interest in realism, is whether I should believe that the things that A and B produce are true, approximately true, empirically adequate, or bear other such properties. I care about what sorts of ontological commitments I should take on when I accept the theories coming out of A and B. If A and B produce the same theory, I'm interested in what my cognitive attitude should be toward that theory. That A aimed to produce it as empirically adequate, and B aimed to produce it as true, is of little relevance to my assessment of the thing itself. (Unless, of course, I somehow *already* know which enterprise has been successful.) To put the point bluntly, determining the aim of science will enable you, at best, to figure out who is doing science and who isn't. But it won't enable you to figure out what you're justified in believing when you do science.

One might object at this point that I have overstated the irrelevance of the aims of science. For consider a third community that engages in practice C, whose constitutive aim is to develop uplifting and edifying theories.[10] Doesn't knowing that science is either A or B, and not C, shed some light on whether or not we should be realists? If science turned out to be C, wouldn't that be a strike against its realist credentials? It's certainly true that A or B bear a relevance to realism that C doesn't, given that truth and empirical adequacy are related to the metaphysical questions surrounding realism, whereas being uplifting and edifying are, presumably, not. (The matter depends on whether those properties are truth-conducive or not.) Still, what ultimately matters is whether or not C's theories are empirically adequate or true; if C's methods turn out to reliably produce true theories, then there is a strong case to be made for being a realist about the products of C. And even in those cases where a practice's achieving

[10] Thanks go to Johanna Wolff for this sort of example.

its aim would be indicative (or not) of realism, the matter remains whether we who are interpreting the results of that practice think that the practice's methods are reliable methods for producing its aim. Indeed, we might think that a practice whose aim is the truth is more likely to arrive at the truth than a practice aiming for something else. But in that case, what matters most is our evidence for what the practice in fact accomplishes: it's the practice's ability to discover the truth that's relevant, not the fact that it aims at the truth.

It's an empirical question (I'm assuming) whether or not what we call science is practice A. Suppose, alongside van Fraassen, that it is. Does that license me to now believe that its theories are empirically adequate? Only if I believe that science has accomplished its aim. Am I licensed to believe that its theories are true? Not if I believe that science has accomplished no more than its aim. The point is that getting clear on the aim of science doesn't serve to in any way inform me about what metaphysical and epistemological attitudes I should take toward the products of science, unless I already bring to the table views about whether or not science is achieving its aims. I could think that the aim of science turned out to be empirical adequacy, but nevertheless that science offers me plenty of evidence to believe that its theories are approximately true. Or I could think that science is actually practice B, but think, on traditional empiricist grounds, that its products do not achieve their aim, and thus license me to believe only in their empirical adequacy.

Long story short, it's not clear that there is a fact of the matter as to what the aim of science is. I'm unsure what the grounds would be for such a fact, and how we could discover them. Finally, even if we could determine what the aim of science is, this would appear to offer us little more than a conceptual analysis of what the activity of science is, and how it differs from other enterprises. It wouldn't answer any of the core epistemological and metaphysical queries that fuel the debate over realism.

To return to the main thread of argument, if science is indeed a large and vaguely circumscribed cultural phenomenon, as even van Fraassen accepts, it's unlikely to be unified by any particular aim. Rather, science will be characterized by a number of aims, none of which are guaranteed to be operant in all the various sciences, let alone in all aspects of all those various sciences. If realism debates are to be construed in terms of aims, then we should expect a highly disjunctive and localized account of realism for different aspects of different sciences. If realism is best thought of not in terms of aims (as I suspect—see my 2013), then even if there is one single aim of science, there is no argument for globalism to be found by attending to that aim. Either way, considerations surrounding the aim of science are best accounted for by localism.

3.4 Hasty generalizations

The final consideration I offer in favor of localism draws on the fact that some arguments that have been used to motivate global views are in fact better arguments for local views. The most salient case is John Worrall's defense of structural realism (1989). Worrall's key example is the case of light, and in particular the shift from Fresnel's theory of light as a kind of wave transmitted by an elastic, all-pervading ether to

Maxwell's theory of light as displacement in the electromagnetic field. On Worrall's accounting, the structure of Fresnel's theory of light is preserved in Maxwell's by way of Fresnel's mathematical equations governing the intensity of light, which still figure into Maxwell's view. What the two disagree about is the nature of light itself, of what sort of ontological character light actually has. Worrall takes away from this example the idea that we should be realists about the structure of unobservable entities when such structure is preserved across theory change, but not about the nature of unobservable entities, which seems too tenuously attached to the particular theories of the day, and are bound to be discarded as new theories come along.

My major concern about Worrall's approach is that it attempts to motivate a global view about the metaphysics and epistemology of science (namely, that we're justified in believing in the structure of unobservables, but not their nature) on the basis of a single historical example. It might thereby successfully undermine certain global views (such as global entity realism), but it does not by itself support a global structuralism. To see why, imagine a case where, over a substantial period of time, certain theories preserve a certain view about both the structure of unobservables and the nature of unobservables as well. Consider the contemporary view of genes as sequences of DNA and RNA molecules that function to enable regeneration of cells and inheritance of traits. Should this account of the nature of genes remain unchallenged in the future development of genetics, it would appear to tell as equally against global structural realism as the case of light tells against global entity realism.

Another example of history telling against structural realism is the germ theory of medicine. Due to scientific discoveries by Semmelweis, Pasteur, and Koch in the nineteenth century, the scientific community came to the view that the predominant causes of human disease were microscopic organisms such as bacteria (and, discovered later, viruses). Acceptance of the germ theory came at the expense of miasmatic theories, which maintained that poisonous air was the main cause of disease. The transition from miasmatic theories to germ theories does not fit the structural realist template well. We don't have one thing (light) governed by a certain preserved structure whose nature switches from theory to theory. The key to the proposed mechanism (air, microscopic organisms) isn't even the same in the two cases. Nor is it clear how to fit the structure/nature distinction onto the transition. All told, there is little obvious support for a structuralist reading of this crucial episode in epidemiology.

The better perspective, according to the methodological localist, evaluates each case on its own (cf. Peters 2014, p. 382). If the history of the theory of light is best understood along the lines that Worrall tells it, then perhaps a structuralist view about *light* is appropriate; but it's no argument at all that we should be structuralists about *genes* or epidemiology or any other area of science. Whether a realist attitude is appropriate or not for genes would seem to depend on the best account of genetics, taking account of its history and likely future development; similar remarks apply to medical science and the others.

Another example of hasty generalization comes from a different corner of the structural realist camp, the ontic structural realism of Ladyman and Ross (2007). Their structuralism is largely motivated by considerations drawn from quantum physics, and the problems inherent to accepting a scientifically informed worldview that treats individual entities as fundamental. Now, one might be convinced that when it comes

to discussing quantum particles (and the problems associated with quantum entanglement), treating them as discrete individuals is a far inferior perspective as compared to treating quantum structure as fundamental. On this view, the particles themselves are derivative entities, or perhaps merely useful fictions that enable us to talk more easily about a metaphysical reality deeply incongruous with our everyday worldview of individual objects. But at most this is an argument for ontic structuralism about quantum mechanics, not about science itself. The problem of quantum entanglement is no argument for thinking that the individuals of the other sciences (Venus, Mars, you, and I, among countless others) do not exist. In any event, the localist can channel Ladyman and Ross's arguments toward a localized structuralism, and need not thereby admit any general conclusions about scientific realism.

As it turns out, Ladyman and Ross avoid committing an obvious hasty generalization by way of principles that extend their structuralist views about fundamental physics to ontic structuralism about everything.[11] For one thing, they are strongly committed to a unity view of science, doubts for which I have canvassed briefly already. More perniciously, they subscribe to certain principles that elevate fundamental physics above all other forms of inquiry. Of most importance is their "principle of naturalistic closure": "Any new metaphysical claim that is to be taken seriously at time t should be motivated by, and only by, the service it would perform, if true, in showing how two or more specific scientific hypotheses, at least one of which is drawn from fundamental physics, jointly explain more than the sum of what is explained by the two hypotheses taken separately" (2007, p. 37). This principle likely falls only on deaf ears. Imagine an anthropologist discovering a heretofore unknown native tribe, or a lepidopterologist who discovers a new species of butterfly. They will not cease taking their findings seriously simply because they have not shown how their ontological discoveries make any impact unifying fundamental physics with any other science. Even if their findings could be put to such work in fundamental physics, this is of no concern to special scientists, who do not justify their enterprises by way of making themselves relevant to physicists. Yet it's the principle of naturalistic closure that Ladyman and Ross need in order to generalize (their interpretation of) the findings of quantum physics for all of reality. The motivation for denying the existence of planets, plants, and people (just a few of the kinds of individuals posited by the special sciences) is thinking that the categories of fundamental physics are the only categories there are, and so an interpretation of physics that has no room for individuals has no room for individuals anywhere in reality. The localist will rightly see this move as question-begging, as it assumes from the outset a strong unity of science and primacy of physics. But such physics-chauvinism has no appeal to the localist. Without it, there is no way of extending ontic structuralism about quantum physics to ontic structuralism about everything.[12]

[11] Literally everything: "objects are pragmatic devices used by agents to orient themselves in regions of spacetime, and to construct approximate representations of the world" (2007, p. 130).

[12] A subtler take on structuralism can be found in French (2011), which considers the independent merits of a structuralist treatment of biology. French's approach is implicitly localist, as it does not assume from the outset that structuralism must hold across all domains of scientific inquiry.

4 Objections to localism

I have now presented my basic case for why I believe the realism debate in the philosophy of science is best approached at a local level, and that a globalist approach does not serve the interests of the inquiries that motivate the enterprise. In this section I shall consider two potential objections to the localist perspective. In particular, I respond to the concerns that localism collapses either into globalism, or into some kind of hyperlocalism. Either way, so go the objections, localism as I have defined it is not a stable resting point.

4.1 Collapse into globalism

One possible objection to the localist view is that it quickly collapses into the standard forms of globalism already quite familiar in the philosophy of science. After all, one might think that if the various arguments for and against scientific realism are generally successful, then they will be successful in the various localized domains. The localist will then end up with local realisms across the board, or local anti-realisms across the board. Either way, the resulting position won't be importantly distinct from the standard realist and anti-realist options on the table.

A first sort of response to this objection is that it, in effect, begs the question against the localist. The localist is concerned to show that one's commitments *vis-à-vis* the interpretation of one domain of science need not force parallel commitments concerning a different domain. What fuels the localist suspicion is that the standard arguments for and against realism have been too crudely wielded to generate general conclusions about science. For example, the no miracles argument suggests that the best explanation for the empirical success of well-developed and confirmed scientific theories is that they are approximately true. But does the success of every scientific theory merit the same explanation? Which theories are well-developed and confirmed? To what degree are theories approximately true? Without answers to these questions, the global realist is left with a frustratingly vague view. Science in general, the realist claims, has produced approximately true theories. But the no miracles argument is of no use in determining just which ones are approximately true, and to what extent. To actually determine what one's commitments should be—how confident should I be in theory X, or in theory Y?—one must attend to the specifics of individual theories, which is exactly what the localist suggests. Perhaps, at the end of the day, the no miracles argument can be applied to every last domain of science. At that point, *and only at that point*, will the distinction between globalism and localism even potentially fail to be relevant. If realists concede that not all theories are well-developed and confirmed, and not all equally approximately true, then their central thesis lacks the requisite content. To fill in the missing information, they need to travel down the localist's path.

Even if it turns out that all domains of science should be treated the same way *vis-à-vis* realism and anti-realism, the localist path to the global conclusion at least represents the more honest means of arriving there. The globalist, if relying on familiar arguments, must assume without argument that the best way of interpreting success in astronomy, say, is the same as how best to interpret success in physics, psychol-

ogy, and meteorology. Perhaps that is correct, but it serves as an unargued premise in the globalist's argument. Plus, the global realist/anti-realist must take a stand on just which theories in which corners of science are indeed the successful ones. Realists or anti-realists who don't commit to which theories are successful (or to just how successful they are) have yet to shed light on what their metaphysical and epistemological commitments are when it comes to scientific theories.

A final important response to this objection is that it misses the fact that localism is essentially a methodological or metaphilosophical position on the realism debate.[13] The localist who adopts realism in every domain is still distinct from the globalist who, by default, adopts realism in every domain. Methodological localism is consistent with all manner of first-order combinations of realism and anti-realism about various domains of science, including the limit cases where all domains are treated the same. This methodological distinction remains important, even if particular localists end up sharing the same first-order views as globalists. For even in such a case, it's a contingent fact that the localist and globalist ended up agreeing. Had the course of science run in a different direction, they may well have ended up with starkly different first-order views.

4.2 Collapse into hyperlocalism

According to localism, rather than striving to be realists or anti-realists with respect to "science" (as one might be with respect to mathematics and ethics), we should strive to at most be realists about physics, biology, chemistry, and others. But if the localist's idea is correct, why not take matters further? Rather than arguing about realism about chemistry, shouldn't we be arguing about realism about organic chemistry, physical chemistry, and biochemistry? We might take matters even further: perhaps localism should be concerned with realism about compounds, or even realism about individual molecules or atoms. If 'science' is too broad a label to fuel an informative realism debate, as the localist argues, perhaps 'chemistry' is similarly too broad. After all, one might have very different attitudes toward the different branches of chemistry. Hence, even the more localized debates I am advocating are still not local enough. Localism thus leads to hyperlocalism, realism debates about the most narrow domains of science possible. In the limit, hyperlocalism reduces scientific realism to indefinitely many forms of realism about single, individual theses.

My response to this objection is essentially conciliatory. The localist need not have any principled problem with hyperlocalism. One way to think about the localist position is that while it's not pragmatist when it comes to debates about realism, it is pragmatist about the label 'realism'. Certain metaphysical and epistemological views have been grouped together as 'realist' over the years, while others have been deemed 'anti-realist'. Getting clear on what those labels are tracking is a project concerned with how *we* as philosophers understand these views. 'Realism' is a label we use to

[13] Fine's criticisms of "piecemeal realism" (1991) appear to apply only to the piecemeal (i.e., localized) approach gone globally realist, and then only to Miller's (1987) particular view of that sort. Fine offers no specific objection to localism as a methodological view, aside from referring to his preferred non-realist stance (e.g., Fine 1984), which I criticize in Asay (2013).

classify a variety of positions that share a similar character, not some sort of natural kind term of deep metaphysical significance. The domains to which we apply 'realism' and 'anti-realism' should be as broad or as narrow as is useful to us when it comes to the metaphysical distinctions we wish to draw. The localist argues that the domain of science is too broad a category for a realism debate worth having: it covers too much territory, and doesn't allow us to draw as many metaphysical distinctions as we should like. Earlier I suggested that 'normativity' is likewise too broad a category; to target one's realism concerns at normativity without attending to the distinctions to be drawn between ethical and aesthetic values, for instance, is to deplete one's stock of argumentative resources. The most globally realist position of all applies realism to *all* domains—one could just be a full-bore metaphysical realist about the truths of all domains, and find the distinctions between ethical, mathematical, and scientific truths unenlightening.

By contrast, I think there is much to be learned by studying the differences between ethical thoughts, mathematical thoughts, and scientific thoughts. The localist thinks that studying the differences between physical, chemical, biological, and astronomical truths can likewise be enlightening. Might it be enlightening to distinguish further still, as between organic and physical chemistry? Perhaps. We should attend, as localists do, to the details of the individual cases. If certain corners of some domains of science seem more ontologically suspicious than others, there is no reason why such suspicions should not be investigated. At some point, the differences within certain domains might not matter any longer. The distinction between even and odd numbers, for instance, is probably of no ontological significance, so "realism about even numbers" is most likely not a particularly enlightening focus of study. Still, should there be motivation for such a specific view, the localist has no objection to engaging realism debates at that or any other level of specificity.

5 Next steps

So far I have attempted to articulate and defend the localist's methodological perspective on realism debates in the philosophy of science. I'd like to conclude by considering what should happen next in those debates, as a result of taking on the localist approach. I'll also consider some of the ways in which localism can make a difference in some of the ongoing conversations in the philosophy of science, and how localism is already playing a role in the arguments of several philosophers of science.

First, I have argued elsewhere (Asay 2013) that realism is best understood as a distinctly metaphysical thesis, thereby avoiding the problems with thinking about realism in terms of aims or theories of truth and/or reference. I define realism about some set of claims as a commitment to (i) the truth of some claims from that set, (ii) a realism-relevant ontology in virtue of which those truths are true, and (iii) the relationship between those truths and their ontological grounds being realism-relevant. This account is instantly accommodating to the localist, as realism can be defined for any set of claims, no matter how large or small. On this account, anti-realism about unobservable entities, for example, can take several forms. Constructive empiricists avoid (i) by way of their agnosticism; instrumentalists and idealists who accept

(i) might avoid (ii) by refusing to countenance that truths supposedly about unobservables are actually made true by observable things. What counts as a "realism relevant" ontology depends on the domain, and how *we* think about realism for that domain. Oftentimes "realism relevance" will amount to something like mind-independence. Scientific theories being made true by a mind-independent external world is a standard presentation of the scientific realist's worldview—but the notion of mind-independence is an obvious non-starter when it comes to realism about minds and mental states. To determine what sorts of entities realism about some domain involves, we need to look to our own judgments about what a realistic interpretation of some domain should require.[14]

With methodological localism in mind, the next step is to consider the many first-order debates to be had over the various scientific realisms. One positive benefit of the localist program is that it allows consideration of all the various ideas, distinctions, and arguments that have been wielded in the philosophy of science. One can make use of the observable/unobservable distinction, or that between structure and nature, without fearing that employing the notion will result in unwarranted global results. In this way the localist can learn from and take advantage of the existing debates and ideas in the philosophy of science while also advancing them.

For example, one can productively make limited use of certain distinctions in the philosophy of science without developing an entire view around it. Consider Rowbottom's recent cognitive instrumentalism (2011b). The cognitive instrumentalist's key insight is that not all unobservable posits of scientific theories need to be treated alike. On the one hand there are unobservable entities that possess the same properties that are observable when possessed by observable entities (such as color), and unobservable entities whose properties can be understood by way of analogy with observable entities. On the other hand there are unobservable entities (such as electrons and quarks) whose properties are in many cases not possessed by observable entities, and whose properties (such as spin and (the other kind of) color) are not understandable by way of analogy with observable things. The cognitive instrumentalist is happy to commit to the former sort of unobservables, but not the latter. The cognitive instrumentalist's idea here is a useful one, and might appeal to those who find some unobservables more ontologically suspicious than others. Localists can harness this distinction and its implications, but they don't need to develop a globalist view out of them. In other words, localists can put together an argument for agnosticism about electrons and quarks here, without committing to cognitive instrumentalism as their overarching philosophy of science. After all, there might be other compelling distinctions and principles in the philosophy of science that could influence our judgments about realism for this or that corner of the scientific enterprise.

Another benefit of localism is that one can more freely look to the history of science for guidance on whether a realist understanding of some scientific phenomenon is appropriate. In so doing, we might arrive at the view, say, that while ontic structural realism is appropriate for quantum physics, epistemic structural realism is best

[14] Commitment (iii) is in place to accommodate projectivist and expressivist views, which are common in metaethics but not the philosophy of science. The idea is that realists take the relationship to be one we discover, whereas expressivists take it to be one that we project onto the world.

for optics, and traditional realism makes the most sense out of microbiology. From a globalist point of view, the history of science can be a frustrating set of successes and failures that confusedly points sometimes towards realism, and sometimes towards its opposition. As a result, it's unclear that the history of science can be a clear source of arguments for or against realism, globally construed. But from a localist perspective, the history can be quite informative. Consistent success and continuity in some domains can lead to a successful but localized no miracles argument, while consistent scientific revolutions in others can lead to a successful but localized pessimistic induction.

This localized approach to the history of science has been advanced by a number of philosophers; their work demonstrates the utility in pursuing a localized methodology. Magnus and Callender (2004), for instance, argue for the failure of the no miracles and pessimistic induction arguments when understood globally, and urge us to consider localized arguments in their place. Ruhmkorff (2013) argues that the pessimistic induction, globally construed, is self-undermining. Nevertheless, he believes, localized pessimism may be appropriate for certain cases. (He explores contemporary medical research, and its tendency to produce soon-to-be-refuted theories.) Saatsi and Vickers point out that various globally-inclined realists [e.g., Psillos (1999) and Ladyman (2002)] have often overreacted to the pessimistic induction, and taken realism to be underminable by a single case of a successful but false theory (2011, p. 32). To illustrate, they provide such a counterexample: Kirchhoff's diffraction theory. They argue that "the realist should try to resist this line of thought by showing how the field of theorizing in question is idiosyncratic in relevant respects, so that Kirchhoff's curious case remains isolated and doesn't provide the anti-realist with grounds for projectable pessimism" (2011, p. 44). This approach is a clear example of localism at work.

Localism also permits a more stable theoretical resting ground for theories retreating from full-bore realism. In response to challenges from the history of science, some of the champions of traditional realism have offered more subtle views, but still intend to stay on the globalist side of the spectrum. Kitcher (1993), for instance, distinguishes between the *presuppositional* and *working* posits of theories, and advocates realism only about the latter. Psillos introduces a "divide and conquer" response to the pessimistic induction, which identifies "the theoretical constituents that were responsible for the empirical success of otherwise abandoned theories", and adopts "a substantive version of scientific realism" with respect to only those constituents that have been retained in our contemporary scientific theories (1999, p. 103).

"Selective realist" views of this sort face a battery of objections. (See, e.g., Stanford 2003 and 2006, Chap. 7, Elsamahi 2005, and Peters 2014.) The overall dialectic that emerges is one in which realists move further and further away from a straightforward (first-order) global realism, but continue to fly the realist banner. The result is a "watered-down" realism that relies on a number of tenuous distinctions (cf. Saatsi 2009 and Peters 2014). Matthias Egg summarizes this attitude when he writes: "different versions of selective realism, in their attempt to show that certain parts or aspects of scientific theories are immune to [Stanford's objections], have not succeeded in reliably characterizing these parts, or they managed to do so only with the benefit of hindsight" (2016, p. 138). Egg himself goes on to offer his own "selective realist" response to Stanford, and my interest here is not to refute the tenability of any particu-

lar attempt at selective realism. The lesson I draw from the exchange between selective realists and their historically driven opponents is that it's fundamentally unproductive to engage these episodes in the history of science with the aim of defending a (more or less) globalist perspective on scientific realism (cf. Saatsi 2015a). Psillos's divide and conquer strategy, for instance, requires delving into the details of particular corners of science, and finding ways of interpreting it that respects the general attitude behind scientific realism. This undertaking must be applicable to any episode in the history of science, if the strategy is to produce its intended globally selective conclusions. The result can often be, to borrow Stanford's expression, a "lingering whiff of ad-hoc-ery or special pleading" (2006, p. 10). I applaud Psillos's digging into the details, but suggest it be divorced from any pretensions of defending scientific realism on the global scale. If realism is the best interpretation of some episode or corner of science, then that case should speak for itself. Realist-minded philosophers of science are better off going on the offensive, and showing which areas of science admit of a realistic interpretation, rather than going on the defensive, and adopting various theoretical contortions in order to avoid those episodes in the history of science that challenge the global realist thesis.

At the end of the day, the localist looks to individual areas of science, and deploys an arsenal of metaphysical and epistemological tools in efforts to determine what the most justifiable commitments for that particular domain are, given the strength of the evidence for the theories in that area.[15] My localist might therefore appear to be a kind of "second philosopher", to employ Maddy's (2007) meta-philosophical perspective. But there are salient differences between the localist and the second philosopher. Second philosophers immerse themselves in the sciences, and their contribution to ontology is "to reveal and explicate the rationality of existence claims based on each of these particular varieties of evidence" (2007, p. 407). A prominent example of Maddy's is the evidence for the existence of atoms accumulated by Perrin and Einstein. It is *that* evidence that is sufficient to establish the existence of atoms, says the second philosopher, and not, say, the avoidance of a cosmic coincidence that would otherwise accompany the success of scientific theories that presuppose the existence of atoms. Where Maddy's second philosopher diverges from my localist is that her second philosopher comes equipped with a variety of epistemic stances that I do not frontload onto the localist. For example, the second philosopher, apparently by definition, does not accept the epistemological scruples of the constructive empiricist. In light of the Perrin/Einstein evidence, constructive empiricists maintain agnosticism (driven by their demanding threshold for what counts as compelling evidence for unobservables), and second philosophers embrace a realist attitude toward atoms (since their threshold is considerably lower) (e.g., Maddy 2007, p. 306). The methodological localist is not from the start committed to either of these views' epistemic stances, and is free to deploy whatever epistemic principles are most appropriate for

[15] Given this commitment, it might be thought that the localist is just "doing science", not philosophy (cf. Magnus 2013, p. 50). I suspect that such a response depends on either assuming that "doing science" is possible in a metaphysically and epistemologically neutral way (with no immediate implications for realism), or that views about scientific realism are somehow independent from the first-order evidence provided by the sciences. The remainder of this paragraph should demonstrate why I reject both suggestions. Thanks to Kyle Stanford for raising this issue.

the case at hand. The localist is free to be skeptical of the constructive empiricist's fully general commitment to the unknowability of unobservables, but also of the generally realist attitudes that appear to characterize the second philosopher.

By not committing localists to any overarching, general epistemological and metaphysical views, it may appear that I have given them absolutely no ammunition with which to conduct their investigations. But in fact the opposite is true: localists can draw on the various arguments used by philosophers of science of any stripe, without thereby taking on those partisan philosophies of science. One might agree with Hacking, for instance, that "If you can spray them, then they are real" (1983, p. 22). As an ontological principle, this slogan is beyond reproach. In adopting it, however, one need not become an entity realist; nor—more importantly—need one deploy it without scruples. That a group of practitioners all claim that a device sprays electrons is of disputable evidence as to whether or not it does; a similar group of more empiricist-minded practitioners might withhold from making such a claim. The former group's commitment might be better evidence that they are naturally inclined realists than that electrons exist. In any event, the localist accepts that the claims of science are in as much need of epistemological and metaphysical investigation as any other, and that we can bring to bear any theoretical resources available to the task. Whether these investigations lead to more global or more local *first-order* theories is something we can only determine afterwards. Regardless of what that outcome turns out to be, what the methodological localist will provide us, I suggest, is a philosophy of science that is more nuanced, more respectful toward individual sciences, and ultimately more honest.

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