ORIGINAL RESEARCH

The pragmatic turn in the scientific realism debate

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Abstract

In recent years there has been a noticeable yet largely unacknowledged 'pragmatic turn' in the scientific realism debate, inspired in part by van Fraassen's work on 'epistemic stances'. Features of this new approach include: an ascent to the meta-level (the focus is not so much on whether scientific realism is true, but on the prior questions of the nature of the positions in this debate, how to decide whether to be a scientific realist, etc.); a reinterpretation of scientific realism and anti-realism as (or as closely associated with) stances or frameworks, rather than theories or beliefs; a move away from the previously dominant empirical-explanatory (i.e. quasi-scientific or naturalistic) conception of scientific realism, anti-realism, and their justification; and a stress on the pragmatic and values-based elements in the debate. The traditional scientific realism debate is concerned with determining which position is true, or most epistemically justified. The new approach by contrast is concerned with determining which position best serves certain values, e.g. is most useful, fruitful, or otherwise prudentially preferable. In this paper we try to bring together the various strands in this new orientation, summarise its key features, contrast it with superficially similar but opposing views, and explore the similarities and differences among some of its adherents. Given we are advocates of the turn, we also offer a defence of the value and fruitfulness of this reconceptualization of the debate.

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[1] More precisely, Scientific Realism is generally understood as a metaphysical, an epistemological, a semantic, or an axiological claim, or some combination of these. For our purposes here the best construal of Scientific Realism is the epistemological view that we know, or ought to believe that, our best scientific theories are true or approximately true, and that we know, or ought to believe that, unobservable entities postulated by science (e.g. electrons, germs, black holes, etc.) exist objectively and mind-independently. Scientific Anti-Realism, then, is the view that we know, or ought to believe, at most that our best scientific theories are something less than true or approximately true, e.g. merely "empirically adequate". (A theory is empirically adequate exactly if what it says about observable things and events is true, regardless of whether what it says about unobservable things and events is true.)

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Simply put, scientific realism (SR) is the view that our current best scientific theories are true, at least approximately so.[1] The philosophical debate about the viability of this position that has taken place over the last 40 years has been dominated by two master arguments, one for realism—the No Miracles Argument (NMA), and one for anti-realism—the Pessimistic Induction (PI). Both arguments represent a kind of naturalistic approach when debating the SR question, where quasi-scientific arguments are put forward by SR's defenders or detractors to support or reject it on an empirical basis as the true, most rational, or *epistemically* best-justified picture of science. But in recent years the debate over SR has taken a distinctly pragmatic turn, where philosophers of science have turned their focus towards investigating and understanding the costs and benefits of accepting or rejecting SR. Our purpose in this paper is to identify, characterize, and advocate for this "pragmatic turn" in the scientific realism debate. We begin by discussing the traditional scientific realism debate and its two main arguments before discussing what constitutes the pragmatic turn, its different varieties, and some of its most salient contrasts.

1 The traditional debate over scientific realism

The No-Miracles Argument (NMA) is based on the following idea: if our best scientific theories were not true or approximately true, then the success of science would be a miracle; but since any scientist in good standing knows that no phenomena can be explained away as a miracle, we can conclude that the only scientifically-acceptable explanation for the patent success of our best scientific theories is their (approximate) truth (i.e. SR is true). In putting forward the NMA as an argument for their position, the would-be scientific realist demands that we *explain* the fact that modern scientific theories reliably make very accurate predictions, just as a physicist might demand an explanation for the value of the gravitational constant, or a biologist for the persistence of some apparent maladaptation in a population. The success of our current best scientific theories, the realist suggest, is simply a phenomenon that needs to be explained, just like any other empirical regularity the sciences strive to explain. One adequate explanation for the predictive, instrumental, and technological success of a scientific theory would be that it was true, of course, at least approximately. The NMA proposes that there really is no other reasonable explanation besides some "cosmic coincidence" whereby a deeply false theory is miraculously able to consistently issue in true predictions.[2] The truth of our currently accepted scientific theories and the existence of the entities they posit is thereby offered as the empirically best-supported, quasi-scientific explanation for the predictive, instrumental, and technological success of science.

As the Pessimistic Induction (PI) stresses, however, countless successful scientific theories once widely accepted as true by the scientific community have been rejected by

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[2] Smart (1963) and Putnam (1975) serve as the modern origin for these types of arguments, with all the talk of miracles and cosmic coincidence, but these types of arguments can be traced much further back and through multiple traditions (see Psillos, 1999, 2018).

their successors as false. And so, the anti-realist argues that based on a meta-induction on the history of empirical science we should infer that our best current theories will also eventually be rejected as false, just like their once-successful predecessors. History clearly shows that many mature, predictively successful scientific theories are eventually rejected, so inductively speaking we should not expect our current best theories to fare any better. Like the NMA, the PI employs a quasi-scientific mode of argument and inference (induction, rather than abduction), but marshals it against SR through the suggestion that the empirical evidence—the past failure of scientific theories—actually supports the truth of some form of anti-realism.[3]

Debates around and developments of these positions and arguments have become quite sophisticated (see e.g. Chakravartty[4] (2007), Stanford (2006), Ladyman et al. (2007), Giere (2006), and subsequent discussion), but in every iteration the NMA and PI reflect a naturalist conception of how SR and its alternatives should be evaluated, according to which the positions in question are to be construed as quasi-scientific claims and subjected to broadly empirical confirmation or disconfirmation using the established methods and standards of scientific theory evaluation. This approach, though dominant for the past 50 years, has not always gone unquestioned, and in recent years we can discern a growing discontent in some philosophical quarters where this approach to the SR debate has been variously criticized. Indeed, as far back as 1989, Worrall (1989, pp. 140–142) noted that the NMA fails to satisfy many of the key demands placed on theories and explanations within science, in particular the demand that the claims being supported by the IBE be independently testable.[5] He also pointed out what has often been noted, that given many anti-realists reject inference to the best explanation, the realist would seem to be begging the question in couching her argument for realism in that form (see also Wylie, 1986, Psillos, 1999)[6]. PI is equally problematic, and inconclusive, when considered as a quasi-scientific inference from the history of science. It suggests that a highly conjectural, philosophical induction over the whole history of science should trump the particular inductive and abductive inferences within science that seem to support the truth of theories and the existence

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[3] Wolff (2019) suggests that Stanford's New PI (2006) is a naturalist, quasi-scientific argument, proposing to inductively infer from the detailed history of actual episodes of science to anti-realism, in quite a scientific way. The fact that Stanford infers from a very small number of cases leads to the worry that his new PI also fails to satisfy basic demands of scientific inference.

[4] In his (2007) Chakravartty suggests that a dogmatic defence of standard SR (or standard anti-realism) in the traditional mode is no longer tenable, and we would do well to try to arrive at a position that takes into account and accommodates the best views, arguments and insights (on both sides) that have emerged from several decades of debate. This he tries to do with his 'semi-realism'. Although in some ways transcending the old dichotomies, we interpret this is a sophisticated view within the traditional NMA-PI framework. (Much the same could be said for structural realism (Ladyman, 1998)). In this work however, and to a greater extent in later work such as his (2017), he moves in the direction of the PT.

[5] See also Chalmers (1999, p. 241), Ghins (2002), and Gutting (1982, p. 126).

[6] The point has also been made (see Jaksland, 2022, p. 5, Frost-Arnold, 2010) that at least some of those who endorse versions of the NMA, such as Ladyman (2012), tend to also insist on the difference between IBE as used in science and as appealed to in traditional metaphysics: IBE in science, they suggest, always works in concert with, and is in a sense subordinate to, empirical success (and is thus legitimate), whereas in traditional metaphysics IBE is completely divorced from empirical success (and is thus illegitimate). Yet the NMA, by this criterion, appears to be much closer to (illegitimate) traditional metaphysics than it is to science, since it too seemingly has no relation to empirical success.

of entities. Note that such critiques of the NMA and PI do not involve merely rejecting the arguments themselves, but involve casting doubt on the quasi-scientific form that such arguments take, i.e. on the very idea that SR could, should, or must be justified or discredited using the inferential methods of empirical science.

The pragmatic approach to the SR debate that we will discuss and recommend shares these critics' concerns with naturalistic approaches to the SR debate, and offers a diagnosis of where they go wrong. The presupposition more traditional approaches share is that it is possible to construct an effective, convincing global argument that establishes on the basis of the empirical evidence, that realism, or anti-realism, is true, or is the best-justified, in the sense of *epistemically* justified, picture of science. The pragmatic approach we discuss in this paper rejects this presupposition, but strives nevertheless to find edifying ways to evaluate SR and the anti-realist positions that oppose it.

The feeling that the traditional SR debate has been, as it were, argued out to a stalemate has been voiced by a number of philosophers in recent years (cf. Fine, 1986, Monton, 2007, 3). Many (but not all) of them have suggested new approaches to the SR debate to help progress it in one way or another, but not all of these suggestions are examples of what we are calling the pragmatic turn. In the next section we offer a characterization of the pragmatic turn, then review and contrast several examples. To help illustrate what the pragmatic turn is by making clear what it is not, we then consider some similar but distinct critiques of the traditional approach to the SR debate in Sect. 4.

2 The pragmatic turn

The traditional scientific realism debate is concerned with determining which position is *true, warranted, correct, or most rational.* The pragmatic scientific realism debate, by contrast, can be thought of as being concerned with determining which position best serves certain values, i.e. is *useful, preferable, prudent, or most practical.* Philosophers of science have been increasingly addressing questions of scientific realism through the latter approach rather than the former in recent years, but this shift in the discourse lacks some self- and communal-awareness. Our goal in this section is to better characterize this pragmatic turn (PT) in the scientific realism debate by drawing out some central threads in much of the recent literature on SR.

The main features of the pragmatic turn (PT) include: a shift to the meta-level (the focus is not on whether scientific realism is true, but on the prior questions of nature of the positions in this debate, how to decide whether to be a scientific realist, etc.); a focus on stances or frameworks, rather than theories or beliefs; a move away from the previously dominant empirical-explanatory, i.e. quasi-scientific or naturalist conception of scientific realism and its justification; a stress on the pragmatic and values-based elements in the debate; sensitivity to doxastic and practical context; a permissive view of rationality where rationality permits rather than forbids; and pluralism, on many levels.

The PT is thus based more on a cluster of claims and approaches than on a single doctrine, but we can discern two central themes:

- *Stance*: Realism and anti-realism should be construed as, or as closely related to-perhaps a function, or outgrowth of-stances or frameworks, and thus their justifications are heavily value-laden.
- *Pragmatic Justification*: Realism and anti-realism-or the stances or frameworks with which they are closely associated-are to be assessed in large part in terms of the pragmatic benefits of adopting them given agent-relative goals, values, and practical contexts.

These views are logically independent of one another—the former being largely descriptive and the latter being normative-methodological—but both involve rejecting the quasi-scientific, naturalistic meta-thesis behind the NMA and PI. Some, such as Forbes (2017), clearly stress both *stance* and *pragmatic Justification*. Others, such as Psillos (2011) and Chakravartty (2017), seem to stress *stance* without explicitly committing to *pragmatic justification*; still others, such as Hendry (1995, 2001) and Arabatzis (2006, 2018) seem to stress *pragmatic justification* without first committing to *stance*. We understand the PT broadly as involving commitment to *at least one* of these claims, with the full-fledged PT involving commitment to both. We conclude this paper by discussing what advantages come from adopting both rather than only one or the other.

In the next subsection we consider those philosophers who explicitly accept *stance* but do not explicitly accept *pragmatic justification.* In the subsection following that, we consider those who explicitly accept *pragmatic justification* without explicitly accepting *stance*.

2.1 Stances, frameworks and values[7]

In this section, we discuss *stance*, the view that realism and anti-realism should be construed as, or as closely related to, value-laden stances or frameworks: we focus on van Fraassen's classic account of epistemic stances; and Psillos' work on the Realist Framework.

2.1.1 Van Fraassen on stances

Famously, van Fraassen introduced the stance idea as a solution to what he took to be a self-refutation worry for empiricism (1995, 2002). According to the empiricist's own position, all factual beliefs are contingent and a posteriori. It follows that if empiricism is a factual belief, it must itself be contingent and a posteriori, and thus that, by the empiricist's own lights, it may turn out to be false. But since this conclusion was reached by presupposing the truth of empiricism, empiricism threatens to be self-undermining, if construed as a factual belief. For the empiricist, empiricism seemingly must be both unquestioned presupposition, and vulnerable empirical hypothesis. This is a hopeless situation. As Mohler (2007, p. 210) expresses it, '[Empiricism] must not be subject to the challenge it itself grounds'.

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[7] It is notable that the pragmatic turn as we understand it here has been taken by theorists from across the spectrum of the traditional SR debate: van Fraassen: anti-realist empiricism; Chakravartty: semi-realism; Psillos: scientific realism.

The alternative is to construe empiricism is an epistemic stance:

'…a philosophical position can consist in something other than a belief in what the world is like. The alternative is a stance (attitude, commitment, approach) … What empiricists have shared over the centuries… has not most obviously been a set of beliefs… [empiricism is] an attitude, or rather a cluster of attitudes, a philosophical stance.' (van Frassen 1995, p. 83, p. 86).

Stances are not true or false, like propositions, and are not believed or disbelieved. They are adopted, like an approach or policy, and are heavily value-laden. The main rival of the empiricist stance is the metaphysical stance. Scientific Realism is closely related to, perhaps a function of, the metaphysical stance. The metaphysical stance accepts, while the empiricist stance rejects, demands for explanations of the observable in terms of unobservable entities and processes.[8] This is a disagreement at the level of epistemic policy, rather than factual belief (Chakravartty, 2007, p. 18-19). Disagreement is ultimately the result of divergent goals, commitments, and values. The empiricist simply 'disdains' the metaphysical stance as an epistemic policy, given its failure to uphold her characteristic epistemic values (van Fraassen, 2004, p. 179).[9]

While epistemic stances are not factual claims, beliefs, or propositional attitudes, they do act as guides, policies, or strategies during the formation and evaluation of factual claims, beliefs, or propositional attitudes. Some of the claims traditionally associated with Scientific Realism are certainly truth-apt (e.g. "electrons exist"), but on van Fraassen's analysis people typically believe such claims because of their *prior adoption of a particular epistemic stance*, i.e. the metaphysical stance. Similarly Constructive Empiricism may be a truth-apt claim about the aim of science, but people typically believe that claim as a result of their prior adoption of the empiricist stance. Epistemic stances are themselves adopted as a result of one's values, e.g. whether one values explanation in terms of unobservable entities and structure (metaphysical stance), or values minimizing epistemic risk associated with such explanations (empiricist stance).[10]

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[8] Before van Fraassen introduced the notion of a stance, Wright (1987) argued that realism (by which he meant metaphysical realism, but this includes SR) is less a thesis that a characteristic set of attitudes. In particular, it involves a balance between the apparently conflicting attitudes of modesty/deference (the world exists independently of us) and presumption/self-assurance (we can come to know this world). The realist-metaphysics stance clearly embodies these two attitudes. The empiricist stance can seemingly accept the modesty attitude, i.e. the world exists independently of us. But it doesn't have as much epistemic presumption as the realist-metaphysics stance, as it does not presume we have or can obtain knowledge of the unobservable world. The empiricist typically accuses the realist of being overly presumptuous, while the realist accuses the empiricist of being overly sceptical (Wright suggests that modesty without presumption leads to scepticism, while presumption without modesty leads to idealism).

[9] Alspector-Kelly (2001, p. 421) notes that 'Empiricism is appropriately characterised by the attitudes of respect for science, suspicion of the comfort that explanation-by-postulate brings, and a call to experience as a safeguard against theoretical flights of fancy…'.

[10] In a different context Sober (1999, p. 550) notes that explanations can be *broad* (i.e. general, applying to a large range of cases) or *deep* (i.e. detailed, giving specific and detailed information about the explanans and its relation to the explanandum), and suggests that whether one prefers explanations with depth or with breadth is just a question of values and taste: neither criterion in objectively superior to the other. This suggests that within the metaphysical stance, there are as it were sub-stances, corresponding to the (epistemic) values different people attach to different explanatory virtues.

Van Fraassen defends voluntarism about stances (see also Chakravartty, 2017). In this context, voluntarism is the view that the rational constraints on stance selection are highly permissive and indeterminate. A stance counts as rational so long as it is not irrational, where to be irrational is to violate logical or probabilistic rules of consistency or be somehow pragmatically incoherent, i.e. to inevitably lead one to sabotage oneself by one's own lights. This account of rationality sets quite a low bar for any given stance to count as rational, and countenances a plurality of stances as rational as a result (including some stances that more stringent accounts of rationality would likely declare irrational). Voluntarism is traditionally associated with the idea that one can believe at will, but the kind of stance-voluntarism we are considering is weaker than that. One may adopt a stance, and this may be a matter of choice, constrained by one's antecendently accepted values. This stance then carves out the space of which types of beliefs are considered admissible, and, perhaps, which inadmissible, but those beliefs can still be sensitive to evidence in the standard way, and need not be believed merely on the basis of the will, or one's values.

2.1.2 Psillos on theories and frameworks

Psillos, prior to 2011, had been one the most prominent scientific realists within the traditional, epistemic, SR debate. His (1999) is *the* classic contemporary defence of SR within the NMA-PI tradition. But in his (2011), he decisively took the pragmatic turn. He notes in that paper that he had previously defended a version of NMA, and had accepted the quasi-scientific construal of SR and its justification:

> Putnam (and following him Boyd, and following him myself and others) took realism to be a theory and in particular an empirical theory that gets supported by the success of science because it best explains this success… the overarching common thought was that realism (as a theory) gets supported by the relevant evidence (the success of science) in the very same way in which first order scientific theories get supported by the relevant evidence (2011, p. 312).

He has come to reject this however:

> The problem lies in the thought that scientific realism can be supported by the same type of argument that scientific theories are supported (*sic*.). This is a tempting thought. But it is flawed, I now think. The reason for this claim is that the very idea of counting empirical success as being in favour of the truth of a scientific theory—the very idea of evidence making a theory probable, or the very idea that a theory is the best explanation of the evidence, and the like—presupposes that theories are already placed within the realist framework… Hence, the no-miracles argument works within the realist framework; it's not an argument for it (ibid., p. 312). (S)cientific realism is not a theory; it's a framework which makes possible certain ways of viewing the world. Scientific realism lacks all the important features of a scientific theory (ibid., p. 311).

Frameworks in Psillos's conception are not identifiable with beliefs, theories or propositions[11], and thus are not 'objects of belief or doubt', the way that theories or beliefs formed from *within* the framework are (ibid., p. 312). The framework essentially involves non-belief elements such as goals, and the realist framework as he sees it embodies goals such as 'the explanation of observable phenomena and … achieving maximum causal and nomological coherence in our image of the world' (ibid., p. 313).

If we accept the realist framework, Psillos argues, we can construct an argument internal to that framework for believing in unobservables: they are indispensable for achieving the realist's goals. But such an argument presupposes, and will be compelling only for those who already accept, the realist framework. This is the same point about the question-begging character of arguments for realism that others have made, as discussed above. If someone doesn't share the relevant aims, Psillos notes, that SR helps to achieve those aims will not move them. Thus, he concludes, there can be no framework-independent, 'ultimate', argument for SR that would be compelling to any rational agent per se. Adoption of the framework itself is not a matter of evidence, reasoning, or empirical facts (ibid., p. 311); it is an 'unforced' matter of choice. He thus says that his argument has a 'pragmatic ring to it' (ibid., p. 303).

Although he doesn't talk in precisely the same terms, Psillos' analysis maps quite closely on to van Fraassen's. His realist framework seems very similar to what we above called the metaphysics stance (defined in part in terms of goals or values that prioritise explanation and causal unity), with the thesis that certain theoretical entities exist being a belief generated on the basis of the realist framework's distinctive aims, values, and attitudes. This distinction between the higher-level frameworks people choose and the lower-level beliefs they form as a result is not always clear in his discussion however. At times he claims to be offering an indispensability argument *for* the adoption of the realist framework, which appears to be inconsistent with his claim that such arguments are internal to, or presuppose, the realist framework, and there can be no such argument for the framework itself. This is perhaps because he holds that the claim that there are theoretical entities is *constitutive* of the realist framework (ibid., p. 312). If this is the case, it is easy to see why he might be misled into thinking that arguments for this claim count as arguments for the framework itself, and be led to blur the distinction between characteristic realist theses and the metaphysics/realist stance/framework. It is a mistake however, we suggest, to see beliefs or theses as definitive, or constitutive, of frameworks/stances, at least as the latter are typically understood in the literature on van Fraassen's notion of a stance (see Boucher, 2018b). Certain theses that may be the object of belief may fit naturally within the metaphysics stance, and may typically be generated as a result of the prior acceptance of that stance, but they do not fully define or constitute the stance.[12] Acceptance of a stance or framework along with rejection of the characteristic beliefs or theses associated with it may involve one in a kind of pragmatic incoherence (Boucher, 2018a)—and thereby count as irrational even on van Fraassen's permissive account of rationality—but one does not necessarily thereby contradict oneself, as one would do if one adopted a framework while rejecting its defining policies, attitudes, and goals.

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[11] He is not always clear on this; see below.

[12] The same could be said for the relation between the empiricist stance and Constructive Empiricism.

In this section we have looked at several theorists who seem to adopt *stance* but do not (at least not explicitly) adopt *pragmatic justification*. In the following section we shall look at theorists who adopt *pragmatic justification* without also (explicitly) adopting *stance.*

2.2 The pragmatic evaluation of the scientific realism debate

2.2.1 Should working scientists be scientific realists, or anti-realists (or does it even matter)?

Whether they defend scientific realism or anti-realism, most philosophers of science are "pro-science," epistemologically speaking. Their theories of scientific knowledge are not developed to question the value, actuality, or primacy of scientific knowledge, but rather only to understand its character, nature, extent, and the logic of its methods and development. Several philosophers have nevertheless touted scientific realism or anti-realism as an essential assumption or foundation of the scientific method, the implication being that working scientists should (or even must) adopt one position over the other lest their philosophical outlook hamper their efforts towards scientific progress. This is a distinctly pragmatic standard for evaluating the preferability of SR vs. anti-realism, from the perspective of working scientists, and is certainly the oldest and most widely-employed pragmatic approach to the evaluation of SR that philosophers have taken.

Hasok Chang, for instance, a historian as much as a philosopher of science, has expressed his conviction that the traditional scientific realism debate has gone (and is going) nowhere. He writes that realists need to: 'face the fact that we cannot know whether we have got the objective Truth about the World (even if such a formulation is meaningful). Realists go astray by persisting in trying to find a way around this fact, as do anti-realists in engaging with that obsession' (2018, p. 31). The worth of realism, as a philosophical position, is to be measured (on his view) by whether its adoption is "useful for scientists and others who are actually engaged in empirical inquiries" (ibid., see also 2012, Ch. 5 and 2022). Developments and elaborations of scientific realism should therefore be less focused on navigating between the Scilla of the PMI and the Charybdis of the NMA, and instead focus on elaborating an attitude that is useful to working scientists. His prescription for the progression of the scientific realism debate, from the perspective of a would-be realist, is therefore to work not on defending realism as the correct philosophical account of the aims, methods, and epistemic accomplishments of modern science, but rather on developing and elaborating a kind of "activist realism," i.e. a realist interpretation of science capable of conceptually supporting scientific research aimed at helping improve our knowledge of reality.

Chang's approach to questions of scientific realism is novel and nuanced, but like many others taking the PT he suggests that any justification that could be offered for SR through his approach would only be some kind of cost-benefit analysis, considered from the perspective of working scientists. Seungbae Park similarly starts from the

position of the working scientist, and employs *pragmatic justification* when defending scientific realism over anti-realism in that context:

> … suppose that scientists have the goal of making scientific progress, i.e. that they have the goal to be closer to truths and empirical adequacy than before. Should they be realists or antirealists? To answer this question, they should conduct cost–benefit analyses of being realists and of being antirealists… (Park, 2019, p. 148).

Cost-benefit analyses of realism vs. anti-realism from the perspective of the working scientist can be roughly divided into two groups: theoretical arguments and empirical arguments. Theoretical arguments typically begin with an idealized conception of the scientific method, infer the practical and cognitive demands upholding this method must place on working scientists, then argue that realists or anti-realists can be expected to be more competent and effective working scientists than the other group because their philosophical outlook provides some sort of mental or motivational advantage. Empirical arguments, by contrast, need not assume any conception of the scientific method, as they simply argue that some pool of *evidence* shows realists or anti-realists are more effective at achieving scientific progress. In the following two subsections we review how various philosophers have proffered either theoretical or empirical arguments for the pragmatic benefits of working scientists adopting or rejecting SR, followed by a discussion of some recent attempts to pragmatically evaluate SR in other contexts (e.g. when studying the history of science). In each instance, the offered arguments aim to show that specific people in specific practical contexts should adopt a realist or an anti-realist outlook for pragmatic reasons—i.e. because it makes them more effective scientists—and are thus instances of what we are calling *pragmatic justification*.

Theoretical arguments: Planck, Mach   Discussions of scientific methodology amongst working scientists in the early 20th century display a great preoccupation with philosophical matters, especially with the metaphysical and epistemological issues arising from recent advances in fundamental physics. In this context there was little division between philosophers of science and working scientists, and considering philosophical issues related to the new empirical sciences was often treated (quite rightly in many cases) as intimately bound up with achieving scientific advancement. In this context, Ernst Mach explicitly argued against anyone adopting a realist outlook because, he believed, the realist "knows only one view or one form of a view and does not believe that another has ever stood in its place, or that another will ever succeed it; he neither doubts nor tests" (Mach, 1911; as cited in Park, 2019, p. 153). Max Planck, responding directly to Mach, countered that "the physicist, if he wants to promote science, has to be a realist, not an economizer" (Planck, 1910/1992; as cited in Park, 2019, p. 154). Mach maintained that realists can do anything to maintain their preferred scientific theory, and will not be sufficiently motivated to question or empirically probe it; Planck, by contrast, maintained that anti-realists will not be sufficiently motivated to develop a complete theory of reality, capable of guiding future inquiry into surprising new domains, and will instead simply revise their preferred scientific theory to accommodate new empirical results. Both of them, effectively, accused the other's position of

encouraging (or at least allowing) physical theorists to accommodate whatever novel phenomena might crop up in order to save their preferred theoretical framework, each claiming the other's philosophical outlook insufficiently motivates them to constantly question established theories and search out novel empirical results. If true, permitting the proliferation of such a complacent philosophy of science would threaten to stall further development of physical theory and experimentation. In the end, neither Mach nor Planck nor anyone else convinced the scientific community that realism or anti-realism posed such a danger, and in the following decades working scientists became less concerned with resolving many philosophical debates once intimately bound up with the practice of science, including the realism question.

The problem with theoretical arguments for scientific realism or anti-realism is that they tend to bottom out in axiological debates about the true aim of science or the accuracy of highly abstract and idealized conceptions of scientific methodology. Philosophers of science and philosophically-minded scientists have long disagreed about what proper scientific method looks like, and the post-Feyerabendian move towards a pluralistic and fundamentally incompletable conception of scientific methodology suggests that (normatively speaking) we should probably not try to *enforce* some monistic conception of good scientific practice, lest scientific progress be hampered by methodological dogma. Thus, it is unlikely that the adoption of scientific realism or anti-realism can be globally justified for the working scientist on the pragmatic basis that it is likely to better promote proper scientific practice, as the consensus amongst philosophers of science now seems to be that there is no way to definitively characterize proper scientific methodology that accounts and allows for the many diverse and fruitful forms of scientific practice seen throughout history (especially not one that fundamentally involves adopting some position along the realist/anti-realist spectrum).

Fine (1986) argued persuasively against theoretical attempts to pragmatically justify or reject SR. Fine pointed out that both realist and anti-realist philosophies of science can always justify any scientific practice on purely pragmatic grounds, even if that practice does not immediately "make sense" given that philosophy's conception of proper scientific method. All plausible philosophies of science are flexible enough that they would never be inconsistent with any type of scientific activity. Thus, the debate over scientific realism vs. anti-realism has reached a stalemate when conceived as a debate over which position validates the practices of working scientists: if pushed, either position is able to justify any effective or fruitful form of scientific activity, at least on pragmatic grounds. Theoretical arguments in favour of or against SR based on the adoption of a specific philosophy being a requirement for operating effectively as a working scientist seem inevitably ineffective these days, but were important to include here because they do represent an early attempt to apply *pragmatic justification* to the evaluation of SR.

Empirical arguments: Hendry, Forbes   More recently, philosophers of science have begun considering whether we might be able to pragmatically decide the debate over scientific realism on distinctly *empirical* grounds, without committing ourselves to theoretical conceptions of proper or productive scientific practice. Hendry (1995, 2001), for example, has suggested that studies of the history of science might reveal that

working scientists who were realists or anti-realists were significantly more effective at achieving scientific progress than the other group. Hendry believes Fine is correct that any type of scientific activity can, in principle, be (pragmatically) motivated by either realism or anti-realism. Nevertheless, he argues, historical study might reveal that realists tend to better perform (or are better motivated to perform) specific types of scientific research, while anti-realists tend to better perform in other contexts. Hendry's point is that history might show a significant correlation between adopting certain philosophical views and succeeded as a working scientist in particular fields. Or it might not, but it can't hurt to ask these kinds of questions.

Building on Hendry's suggestions, and working explicitly within the Stance framework developed by van Fraassen, Forbes (2017) looked to develop a methodological framework through which such questions could be investigated. The idea was to facilitate historical research aimed specifically at discovering edifying information about the practical benefits for working scientists of adopting scientific realism vs. anti-realism in different research contexts. He demonstrated this proposal through a comparative case study of the way different philosophical attitudes impacted scientific practice within the three main traditions of late 19th century European electrodymanics, but his proposed approach to the debate has not been carried through any further than that to date.[13]

Neither Hendry nor Forbes begins or grounds their approach by assuming a substantial conception of proper scientific method, instead simply suggesting we look to the way science has been practiced by committed realists and anti-realists to determine whether making philosophical commitments along the realist spectrum influences the way scientists practice their craft. The hope, broadly speaking, is that by examining whether realists and anti-realists (statistically speaking) tend to conduct scientific research differently, and noting the types of successes they each have as a result, the history of science might reveal to us that realists tend to be more successful scientists when it comes to specific scientific activities, while anti-realists tend to be more successful when it comes to others. In that case, while we may not be able to provide the kind of global, pragmatic argument for adopting realism or anti-realism that most theoretical arguments for these positions have tried to offer, nevertheless, we might be able to provide a more limited, but empirically grounded, "best practices" guide linking realism and anti-realism to success in particular research contexts.

2.2.2 Should historians of science be scientific realists, or anti-realists (or does it even matter)?

While most discussions of the acceptability of scientific realism vs. anti-realism have focused on the practical context of working scientists, there are many other contexts

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[13] McArthur, in arguing against the "anti-philosophical" approach to the scientific realism debate which contends it is stalemated, also reviews the history of science to illustrate "examples of instances where philosophical stances to the realism question have affected scientific practice" (2006, p. 370). As such, he presents an example of someone who might also accept both *stance* and *pragmatic justification*, like Forbes and Boucher, though he is more immediately concerned with showing only that it matters whether working scientists are realist or anti-realists, not with justifying realism or anti-realism on pragmatic grounds.

in which one might ask whether there is a pragmatic justification for being a realist or an anti-realist about science. Admittedly not out of concern for answering the realism question per se, historians of science have treated the adoption of a realist or an anti-realist attitude as a pragmatic question for several decades, and overwhelmingly concluded that an anti-realist perspective is methodologically preferable for the historian of science.

The so-called "Strong Program" in the sociology of scientific knowledge expressly prescribed a non-realist outlook for working historians and sociologists of science. It insisted, on prudential grounds, that the sociological study of science should proceed under a radical form of relativism by refusing to use "truth" or "falsity" as an explanatory resource when accounting for why certain theories were rejected by the scientific community while others were accepted. The methodological demand that sociologists and historians of science explain the development, adoption, and rejection of scientific theories "symmetrically" by appeal to things such as social processes, is tantamount to the view that an anti-realist outlook is a methodological requirement for the humanistic and scientific study of science, and has been extremely influential and productive in these fields over the past half century. Jed Buchwald's introduction to his deep and unprecedented account of Hertz's experimental work, for instance, begins by taking it as given that historians of science "must at least act as professional agnostics … without relying unduly on statements about what must, or must not, have been going on [in reality]" (1994, p. 1). Harry Collins, a progenitor of the Strong Program, affirmed his commitment to it several decades after first formulating it:

> The question asked by the social analyst of science is: "Why do scientists believe 'p' rather than 'not-p' and how do they come to this belief?" If the trump of reality is always up the sleeve of the analyst there is little chance that the question will be pushed to the limit because social inquiry can be trumped anytime the analyst fancies: "Scientists came to believe this because it is true, rational, or whatever." Thus, since 1981 my position has been "methodological relativism" in which reality-trumps are not allowed (2018, p. 39).

Theodore Arabatzis ultimately agrees with Collins, Buchwald, and the vast majority of other sociologists and historians of science that 'for historiographical purposes an agnostic attitude with respect to scientific theories and unobservable entities is the most appropriate' (2000, p. S531). Nevertheless, he has also explored the potential historiographical benefits of relaxing the total prohibition on allowing anything like 'reality-trumps' into our histories of science. His 2006 book *Representing Electrons* develops a 'biographical history' of the representation of the electron that (provisionally, at least) assumes the term "electron" had a real, existing type of unobservable particle as its stable referent throughout several tumultuous periods of scientific change. This assumption allows him to explain many of the radical revisions physicists made to their theoretical models of the electron as the entity 'pushed back' against various inadequate attempts to theoretically characterize its many odd properties. He acknowledges how this realist approach to the history of physics departs from decades of historiographical dogma, but justifies it in terms of its practical utility for revealing important aspects of the history of physics that more traditional, non-realist approaches

struggle to make salient. Arabatzis allows elements of a realist outlook into his historiography of science so that he can treat the electron as an agent in its own right: not merely as a construction from theory but rather as an autonomous participant in the practice of science that is able to resist changes to its theoretical characterization that some scientists would impose on it, and thereby ensure its own survival within physics despite radical shifts in scientific theory. "The main historiographical advantage of this approach," he writes, "is that theoretical entities become explanatory resources for the historian. To explain the outcome of an episode in which a theoretical entity participated, one has to take into account the entity's contribution (both positive and negative) to the outcome of that episode. If, on the other hand, one neglects the entity's active participation, the understanding of the episode will be in some respects flawed" (2006, 44).

What is most relevant for our purposes about this methodological conversation amongst sociologists and historians of science is that it is fundamentally pragmatic.[14] They ask whether a realist or an anti-realist attitude better serves their practical needs, as professional sociologists and historians of scientific knowledge, and ground their adoption of one over the other in those terms alone; they display little if any concern with determining whether SR is "true, correct, justified, or rationally preferable" per se. Neither do they do try to justify or reject SR on some kind of quasi-scientific basis. Instead, they focus exclusively on its methodological potential in their specific practical contexts as sociologists, anthropologists, or historians of science. Like Chang does vis-a-vis working scientists, Arabatzis is concerned with developing and justifying a form of realism that serves the purposes of working historians of science, given their specific aims, values, and goals *qua historians*.[15]

In Sect. 3 we have discussed various approaches to the realism debate that embody either *stance*, *pragmatic justification*, or both. Most theorists who have abandoned the traditional approach to the SR debate and taken the PT have tended to focus on articulating and applying one of these principles but not necessarily or explicitly the other. In the final section we offer some reasons why philosophers of science sympathetic to PT would likely benefit from embracing both principles together when addressing the SR question. Before doing so, however, it will be helpful to contrast

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[14] Arabatzis is not alone in thinking that the question of scientific realism has a bearing on various practical matters outside of the sciences proper, with some even suggesting it has bearing on the administration and operation of our modern societies. Park (2016) argues, for example, that scientific realism is more appropriate than anti-realist empiricism in science education, and recommends its adoption for science educators and textbook authors on that basis; Godfrey and Hill (1995) argue that scientific realism is preferable to positivism in strategic management research; and Roy Bhaskar marshals realism as the preferred framework for emancipation-oriented sociologists, given their activist ambitions (e.g. 1975, 1986). All such arguments, which we are only able to mention here in passing, would count, on our reckoning, as instances of *pragmatic justification*.

[15] Speaking more broadly about the realism debate in philosophy of science, Arabatzis suggests that an approach based on *pragmatic justification* will be the most likely way to make progress there as well, writing: 'If we narrow our focus to issues about whether we are justified in believing in successful scientific theories (and the ontologies they sanction), then the prospects of overcoming the current standoff in the realism debate … are rather slim. If, on the other hand, we evaluate realism and anti-realism according to their capacity to make sense of scientific practice, both past and contemporary, hopefully we'll be able to move beyond the present stalemate' (2018, pp. 36−37).

the PT with somewhat similar but importantly different approaches that have recently been taken to the scientific realism debate.

3 What the pragmatic turn is not

In this section we contrast the pragmatic turn with two superficially similar views: Hacking's entity realism, and Fine's and Maddy's naturalistic quietism. This will clarify what the pragmatic view is and is not committed to.

3.1 Hacking's entity-realism

One of the best-known "pragmatist" approaches to the scientific realism question was developed and defended by Hacking (1982, 1983). Hacking famously bemoaned the excessive focus in the debate on questions about *representation*, i.e. relations of reference, truth, and so on. It is this which makes anti-realism seem a live option, as it is the picture of science as involved primarily in representation that makes possible the classic arguments against realism (such as the PI), with their challenges to the realist notions of truth, reference, and empirical success, and the claim that science "gets it right." A decisive victory for realism on such representational terms shall always, Hacking suggests, be elusive.

The alternative is to defend realism on the ground not of representation, but of *intervention*. It is what scientists *do*, not what they believe or how they and their theories represent the world, that suggests the strongest argument for realism about the many unobservable entities posited by modern science. It is scientists' ability to manipulate theoretical entities such as electrons, protons, quarks, germs, etc. to use them as instruments in experiments on other entities, as medical interventions on disease, and so on, that makes realism seem inescapable for anyone but the most intransigent skeptic.

> Most of today's debate about scientific realism is couched in terms of theory, representation, and truth. The discussions are illuminating but not decisive. This is partly because they are so infected with intractable metaphysics. I suspect there can be no final argument for or against realism at the level of representation. When we turn from representation to intervention, to spraying niobium balls with positrons, anti-realism has less of a grip (1983, p. 31).

The type of realism that is supported by such considerations is *entity* realism, not *theory* realism. The latter is the type of realism associated with the representational-ist paradigm that Hacking urges us to reject. His pragmatic argument forces us, he suggests, to believe that particular entities such as electrons exist, while supposedly allowing us (in light of PI) to remain agnostic about the truth of all particular theories concerning the nature of such entities.[16] We 'have good reason to suppose that

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[16] Chakravartty (2007) notes, this is not so much entity realism rather than theory realism, but *selective* theory-realism, i.e. realism only about parts of theories, i.e. those parts that tell us that certain entities exist. '…entity realists appeal to the epistemic significance of our causal connections to particular entities.'

electrons exist, although no full-fledged description of electrons has any likelihood of being true. Our theories are constantly revised; for different purposes we use different and incompatible models of electrons which one does not think are literally true, but there are electrons, nonetheles.'(Ibid, p. 27).

There is much that can be (and has been) said about Hacking's pragmatic entity realism, both with respect to the claim that scientists can patently "do things" with theoretical entities, and the claim that one may be a realist about entities without being a realist about theories.[17] But here we wish to point out only that it is a quite different type of pragmatism from that associated with PT. Hacking is, as we have noted, arguing that entity realism is forced upon us by the realization that scientists do things to and with theoretical entities. That is, realism is strongly supported by the facts about the practical activity of scientists. The pragmatism is located in these practical activities, but the argument for realism is not a pragmatic one. It is not being suggested that adopting realism *itself* is justified in terms of the pragmatic benefits of adopting it, either for scientists or philosophers, whereas PT is specifically a move towards justifying (for scientists, philosophers, or anyone else) the adoption of scientific realism or anti-realism in terms of the pragmatic benefits of doing so.

It is true that Hacking suggests at one point that realism 'is more an attitude than a clearly stated doctrine. It is a way to think about the content of natural science' (1983, p. 26). This is in keeping with PT. However he makes it clear that he thinks his intervention and manipulation argument conclusively establishes that *we should believe that realism is true, or epistemically justified*, with respect to the existence of many specific unobservable entities. This places him outside the PT.

3.2 Naturalistic quietism and localism

Another position in the SR debate that bears some similarities to PT is what Wolff (2019) calls 'naturalistic quietism', associated with philosophers such as Arthur Fine and Penelope Maddy. Fine (1984) famously defended the Natural Ontological Attitude (NOA), according to which science may be accepted at face-value as providing us with truths about the world, including the unobservable world. But so long as the notion of truth is not given a realist interpretation (e.g. as correspondence to the facts), this falls short of being a realist conception of science, in Fine's view. But neither is it an anti-realist view, if the notion of truth is not given an anti-realist interpretation. The view is rather 'non-realist,' and its formulation is meant to allow philosophers of science to avoid the scientific realism debate, treating any further attention to it as a waste of time. Even the approach recommended by Forbes and Hendry of empirical study

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[17] A natural objection to the former claim is that it begs the question against the anti-realist, since 'scientists do things with electrons' presupposes that electrons exist. The anti-realist denies this premise. On the latter claim, some have questioned the coherence of believing in the existence of certain entities while not accepting the truth of any particular theories concerning them; see Musgrave (1996, p. 20), Chakravartty (2007). Hacking in fact does not make the implausible claim that we should not be committed to the truth of *any* claims about the entities in question; there are, he notes, well-established low-level causal generalisations regarding the behaviour of the entities that scientists accept and that are presupposed in their experimental practices. We may accept the truth of those while remaining agnostic about more abstract, high-level theoretical descriptions of the nature of the entities.

into the potential practical benefits of adopting or rejecting SR in specific contexts, Fine argues, is doomed to failure (2018). Fine's view of the SR debate in any form, pointedly put, is that it is "dead" (1986, p. 112). *Stance* and *pragmatic justification* both admit the possibility of someone deciding on rational grounds whether to adopt or reject SR; NOA dismisses this possibility.[18]

Maddy defends a more emphatically naturalistic version of quietism she labels 'second philosophy' (2007). On her view "(t)here simply is no room for philosophical debate outside of science to settle questions about whether a particular theory is true, whether certain entities exist, or what we have good reason to believe." (Wolff, 2019) There is no place, on this view, for a distinctively philosophical perspective, external to science, from which to debate epistemic or metaphysical questions about science, using distinctively philosophical concepts and forms of reasoning. The specific, local evidence adduced within science for the truth of certain theories and the existence of certain entities is the only evidence that could matter; further purely philosophical, global, arguments for or against realism, such as the no-miracles IBE, are superfluous. Quine famously opined, 'philosophy of science is philosophy enough'. Maddy may be interpreted as going even further: science itself is philosophy enough.

As Wolff suggests, (and as both Fine and Maddy accept) naturalistic quietism is a distinctive *attitude* one may take towards science, not primarily a theory or factual claim.[19] As such it can, Wolff notes, be understood as an epistemic stance in van Fraassen's sense. It thus belongs alongside the metaphysical and empiricist stances, perhaps representing something of a compromise between the two (in Fine's case) or a radical rejection of both (in Maddy's case). The claim *that* quietism is a stance or attitude clearly chimes with PT.[20] In terms of the content of quietism and its implications for the SR debate, there is some agreement between Fine, Maddy, and the enactors of the PT we've reviewed above in that they all reject the possibility of resolving the traditional SR debate. All agree that no global epistemic argument (such as NMA or PI) can be offered to conclusively establish that realism (or anti-realism) is the only tenable philosophy of science, so all agree we should stop arguing about *this* issue. But pragmatists are not on the whole quietists. For the quietist, the SR debate in all its forms is simply a waste of time and should be discontinued. For the pragmatist, the SR debate should be reconceptualized, but it is by no means fundamentally useless, as SR

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[18] In anything but the most trivial senses involving bribery, duress, or other extrinsic incentives, e.g. "I will give you $10 to adopt SR."

[19] Wolff (2019) suggests that a motivation for this move is that if formulated as a thesis, quietism would look too much like scientific realism, as both agree that unobservable entities exist, etc. At the level of stance or attitude it is easier to distinguish quietism from realism, since the former involves a determinedly non-philosophical acceptance of science on face-value (indeed, a deference to whatever the modern scientific consensus is), while the latter offers the possibility of a philosophical/metaphysical interpretation of contemporary science from outside of science, even if the resultant beliefs—'atoms really exist' etc.—are the same in specific instances. As we discuss below, localist defences of SR complicate this picture, since localists agree with quietists that the evidence adduced within science is all that we need to appeal to (with philosophical arguments being redundant), but sees this as a way of defending SR, not dismissing the debate.

[20] Fine and Maddy, in presenting quietism as a stance or attitude rather than a thesis, do not thereby qualify as advocates of the PT, unless they wish to construe the other views in the debate, i.e. realism and anti-realism, as stances or attitudes also.

and its rivals are potentially meaningful, valuable positions which present a plethora of practical consequences to consider when deciding which position to adopt.

Maddy's claim that the local, first-order evidence that scientists themselves appeal to in support of certain theories is the only evidence that counts is echoed by others, such as Achinstein (2002, 2010)[21], who suggest that such evidence provides conclusive support for SR in local cases. This view, which we can call 'localism', differs from quietism, in that it does not reject the SR debate as pointless. Rather, it suggests we can resolve the traditional (epistemic) SR debate, at least for specific cases, on the basis of local, purely scientific (rather than philosophical) arguments and evidence. Thus, Achinstein argues that the experimental scientific evidence that Jean Perrin presented in the early 20th Century in favour of the atomic hypothesis conclusively established that atoms really exist, rendering further extra-scientific arguments for the same conclusion, such as NMA, superfluous.[22]

So, we can distinguish three responses that have been made by those who reject the value or effectiveness of global arguments such as NMA, or PI. Quietists dismiss the entire SR debate; localists still engage questions about whether one should be a realist about science, but seek to recast such questions as a series of local, specific debates to which only first-order scientific evidence is relevant[23]; PTers also engage such questions, but seek to recast them in pragmatic, values-based, stance/framework terms. PTers reject the anti-philosophical attitude shared by quietism and localism, though most especially the quietists' dismissal of the SR debate in all its forms. PTers also do not concern themselves with the localists' claim that SR is conclusively supported in particular cases by the first-order scientific evidence, without needing to appeal to distinctively philosophical reasoning.[24] PT focuses, exclusively and specifically, on the pragmatic evaluation of adopting or rejecting SR: will it serve my aims, goals, and values to be a realist about X in context Y, regardless of the truth of the matter?

4 Conclusion: the full version of PT

In Sect. 3 we looked at several different instances where philosophers have turned towards a more pragmatic approach to addressing questions concerning scientific realism. We showed how most of these theorists have tended to adopt only one of the two central principles associated with this pragmatic turn: *stance* or *pragmatic justification*. As we shall now argue, however, they are best thought of as two halves of a single approach, each less useful on its own than when combined with each other.

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[21] See also Magnus and Callender (2004), who recommend 'retail' rather than 'wholesale' arguments for realism.

[22] Perrin's work is often appealed to by realists; see van Fraassen (2009) for an empiricist reply.

[23] Or, in its weaker version (Fitzpatrick, 2013), to which first-order scientific evidence is crucial, but not sufficient.

[24] Fitzpatrick (2013) notes that despite claiming to appeal only to evidence offered within science, localist arguments such as Achinstein's, on closer inspection, can be seen to make use of distinctively philosophical concepts and arguments. Fitzpatrick's own view is a less radical form of localism, which denies that SR can be established though simply 'reciting first-order evidence', and accords a central role to 'philosophical argumentation that goes over and above this evidence', but still sees this evidence as indispensable in defending realism on a case-by-case basis.

Previous articulations of the PT in terms of either *stance* or *pragmatic justification* have tended to be one-sided, focusing almost exclusively on either describing the nature of the positions in question (*stance*) or their mode of justification (*pragmatic justification*). Those stressing *stance* (Sect. 3.1) have done much to clarify the relation between SR and anti-realism and the broader, value-laden stances or frameworks with which they are associated, but have not had much to say about the methods by which a stance can or should be evaluated against its alternatives. In particular, the efforts to develop a program for studying the potential pragmatic payoff of adopting the (anti)realist stance for scientists, historians and science decision-makers (as outlined in Sect. 3.2), has been largely absent from their analyses.

Chakravartty (2017), for instance, claims that the only grounds we can have for adopting a particular epistemic stance have to do with (1) whether the stance is rational, where this is understood in the minimal voluntarist sense of internal consistency and coherence, and the absence of self-sabotage; and (b) whether the stance embodies the values we endorse. As he makes clear in the *Coda* to his book, where he compares the situation we face when confronted with rival rational stances to that faced by Pyrrhonian sceptics confronted with questions for which the arguments and evidence are equally strong on both sides, his conception of stance-debate is ultimately quietist. Equally rational stances are equally good, and that's pretty much the end of the matter. If your values incline you to favour a particular stance you can adopt it on that basis, but no rational stance is in any sense superior to any other, and a detached, tranquil, 'Pyrrhonian' attitude of non-commitment and suspension of judgment may often be appropriate. We have similarly seen that Psillos (2011), in advocating a version of *stance*, also has little to say about the grounds on which a stance such as the realist framework can or should be evaluated by someone not already committed to it, beyond noting that it seems to be an 'unforced' matter of free choice.

On the other side, without assuming a framework like *Stance* to understand how values factor into any justification of SR, theorists who adopt only *pragmatic justification* might find they lack the kind of conceptual framework required to avoid traditional questions about realism's "truth" while working to conduct edifying, fine-grained, context-sensitive, empirical investigations of the pragmatics of adopting or rejecting SR. If it proceeds under the standard conception of SR as a straightforward belief or thesis, any investigation of the pragmatics of SR's adoption will be at risk of becoming embroiled in the unanswerable questions and intractable problems that have characterized the traditional SR debate, which *stance* theorists have highlighted. Moreover, if SR is conceived as a belief or thesis, *pragmatic justification* faces the difficult task confronting any pragmatist view of belief-justification of answering the question how it can be rational to decide whether to believe P on entirely pragmatic grounds, when what we really want to know is whether P is *epistemically* justified, whether there is *evidence* to think that it is *true* (not just useful). If SR is a stance this question doesn't arise, as stances are not true or false, epistemically justified or unjustified.

Stance and *pragmatic justification* have sometimes been combined, as we've seen. Forbes (2017), for example, focuses on trying to understand both (a) what it means for a stance to best serve someone's values (*stance*) and (b) how someone can ground or

inform their stance choice by investigating which stance best serves their values (*pragmatic justification*). Different people can have irreducibly different values informing their stance choices, he notes, and different stances could thus be rational for different people, or even the same people in different practical contexts; furthermore, it may not be immediately obvious to someone which stance best serves their idiosyncratic set of values. Thus, Forbes develops a general conceptual framework through which we might empirically investigate which stance will be most pragmatically effective, for ourselves (or for other agents), in specific contexts, given our agent-relative aims, goals, and values. Forbes suggests that the way forward for the realism debate is to try "to show that someone is more likely to achieve specific ends in a specific context if they adopt one [stance] rather than another" (2017, p. 216). This overt and general statement of his "pragmatic, existentialist approach to the scientific realism debate"—which looks to help people choose between equally rational epistemic stances by determining the likely practical consequences of different stance choices—is only possible because he accepts both *stance* and *pragmatic justification.* Recall how Chakravartty (2017) sees stance selection as a somewhat mysterious and ungrounded process without any clear prospects for generating edifying guidance regarding stance selection because stances are grounded in values, but values are themselves ultimately ungrounded. By focusing on *pragmatic justification* as well as *stance*, Forbes builds his case that edifying guidance might be generated regarding stance selection by investigating which stance in fact best serves specific values in specific practical contexts. Boucher (2014, 2015, 2018a, 2018b, 2019) also combines *stance* and *pragmatic justification*, applying them to a range of issues beyond the scientific realism debate.

Stance and *pragmatic justification* on their own embody at best partial, incomplete moves away from the traditional conception of the scientific realism debate. *Stance* embodies the acceptance that all justifications for the adoption or rejection of SR will need to ultimately find their grounding in some already held set of values, and that we will therefore never be able to conclusively establish what the rational choice is per se. *Pragmatic justification* embodies the commitment to investigating whether, and if so when, there are any practical advantages to adopting or rejecting scientific realism, so that even if we cannot establish what the rational choice is per se, we might still manage to establish what the (pragmatically) rational choice is for some people, in some contexts. *Stance* provides the framework for understanding why the adoption of SR or its alternatives cannot be grounded objectively for all rational agents by quasi-scientific appeals to "the facts," but only for individual rational agents by appealing to their antecedently held values; *pragmatic justification* provides the drive to investigate the details of that grounding relationship, to help people determine whether SR is right for them (even if it's not right for everyone).

The prospects of success for those taking the PT are modest compared to traditional efforts to establish that one amongst several competing philosophical doctrines in the SR debate is true, correct, or epistemically rational. The pragmatic tradition recognizes that such philosophical ambitions are, in most if not all instances, impossible to achieve. Fine, Maddy, and other Quietists or Localists recommend we just give up comparing and weighing the available options in the realism debate. In taking the PT many others are embracing a potentially progressive alternative, trying to establish that one amongst several competing doctrines is the best for her, or me, or you, or someone

else, given their values and practical context. Success in such efforts will not generate an easy, total, final, silver bullet solution to the problem of whether to be a realist or anti-realist about science. But those kinds of philosophical aspirations are rarely if ever achieved, and a more pragmatic approach to the SR debate does have prospects for limited success in providing edifying, fruitful guidance to the perplexed. It is thus no wonder that taking the PT is a growing tendency within the literature on SR in the Philosophy of Science and beyond. We hope this paper has at least established that this trend exists, and that characterizing and identifying it as we have might provide some basis for furthering efforts to take a pragmatic approach to the SR debate.

Declarations

Conflict of interest There are no conflicts of interest to declare.

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