Is the Scientific Realism Debate Worth Having?

Ioannis Votsis

1. Introduction

Several philosophers have questioned the value of the scientific realism debate. The accusations are varied in content but they have been trickling in at a constant rate. The aim of this chapter is to take part in the debate over whether the scientific realism debate is worth having. I begin with a short introduction of the debate, distinguishing between broad and narrow construals as well as outlining the main positions and arguments. I then canvass three charges that have been launched against it. I argue that although all three, and indeed the whole meta-debate, should be taken seriously, their proponents are rushing in their attempt to seal the object debate's fate.

2. The Scientific Realism Debate: Broadly Construed

Scientific realists, henceforth just 'realists', hold that there exists a mind-independent world and that, under some circumstances, theories reveal true or at least truthful things about it. Under what circumstances? We are presumably warranted to assert that a theory or, more accurately, a theory-part is true or at least has some true content when it enjoys a certain measure of success. Success here is conceived in terms of empirical power – the agreement of a theory's empirical consequences with observations – and explanatory power. On this view, success implicates truth content. Moreover, increased success implicates convergence to the truth. The anti-realists argue that it is not the case that, or we cannot know whether, theories or theory parts are true or have some true content, at least when that content concerns posits that go beyond the observational realm. In other words, they tend to deny one or more of the above claims by endorsing claims that contradict them.

It is useful to think of realism and anti-realism as separate clans. Like a clan of folk, a clan of realist (or anti-realist) views has several distinct but, in important ways, inter-related members. The members of the realist clan include entity realism, epistemic structural realism, ontic structural realism, semi-realism and traditional scientific realism. The members of the anti-realist clan include conventionalism, constructivism, perspectivism, instrumentalism and constructive empiricism. Not all members of each clan object to members of the other in equal measure. Traditional scientific realists and constructive empiricists, for instance, agree that there is a mind-independent world and that it is there for the observational taking. By contrast, some types of constructivists do not even concede the existence of a mind-independent world, let alone the claim that we have any observational grasp of it. Aside from the varied nature of the disagreements across clans, there is also some mouth-watering infighting. Ontic structural realists, for example, exchange blows with other realists on whether or not we need to introduce any substantial notion of object-hood into our ontology.

Given the varied membership of each clan, it only makes sense to talk about there being one realism debate when such a debate is described in a very general and somewhat disjointed way, as was done in the first paragraph of this section. But, as a matter of fact, the debate is typically more narrowly circumscribed. In the last few decades, for example, the anti-realist view that evoked a sustained reaction from the realist camp is constructive empiricism. Starting with the next section and for the remainder of this essay, our focus will be on the more narrowly circumscribed version of the debate. The reason is quite simple: to faithfully reflect the situation on the ground. After all, the

accusations against the debate we are considering here, use this narrow rendering as a prime example.[1]

3. The Scientific Realism Debate: Narrowly Construed

To better fathom this version of the debate, we need to take a closer look at the points at which there is some sort of contact, whether smooth or frictional, between the two parties. Both take *empirical evidence* as the bedrock of good belief. Alas, what each means by empirical evidence differs to some extent. Constructive empiricists articulate this notion solely in terms of claims that can be verified with unaided senses.[2] Thus, data acquired through instruments count as empirical evidence so long as the same information can be gleaned through the unaided sensory organs of humans. Realists are more inclusive. Instrument-acquired data may also qualify as empirical evidence, even if the same information cannot be sensorially gleaned. Despite this incongruity, the two sides clearly agree on something: When thinly-conceived, i.e. in terms of sensorial verifiability, empirical evidence forms at least part of the bedrock of good belief.

But what exactly do we mean by this expression? We can think of the bedrock as composed solely by those premises that are epistemically permissible. It is upon these premises that a set of epistemically permissible inferential rules act so as to yield our epistemic commitments to claims and posits. Other things being equal, the slimmer the bedrock the more conservative our epistemic commitments. That is, even if the set of epistemically permissible inferential rules were shared by the two parties, their epistemic commitments would still differ on account of the disparities in their respective bedrocks. As it turns out, not all inferential rules are shared by the two parties. Both endorse deductive inference rules but the realists also endorse a number of non-deductive ones that the constructive empiricists either expressly forbid or admit only restricted versions thereof, e.g. basic enumerative induction.[3] Thus, just as the constructive empiricist bedrock is strictly slimmer than the realist one, so is their set of inferential rules. The upshot is that commitment, according to the constructive empiricists, reaches only as far as observable posits. After all, no amount of evidence, construed as it is above, and no inferential rule of the kind they endorse, seem capable of professing anything about the unobservable world. By contrast, realists, with their more inclusive conception of empirical evidence and their richer repertoire of inference rules, commit themselves also to unobservable posits.

The most prominent inference rule endorsed by the realists, but not by the constructive empiricists, is inference to the best explanation. This is an ampliative form of inference, i.e. the truth of the premises does not guarantee but nonetheless is meant to lend credence to the conclusion. Not just any ampliative inference is an inference to the best explanation. The latter earns its name by imposing an additional constraint, namely that the conclusion offers the best explanation of the premises. What does all of this have to do with the observables vs. unobservables distinction? This type of inference is intended as a bridge between observable and unobservable posits. A hypothesis about some

---

[1] Having said this, the reader should always keep in mind that the same or similar accusations (and replies) are likely to be applicable to other renditions of the debate and even to the debate when it is broadly construed.

[2] Strictly speaking, constructive empiricists talk about models, not claims.

[3] Van Fraassen appears to deny any legitimate role for induction. He asserts, for example, that he does "not think that there is such a thing as Induction, in any form" (2007, p. 343). Even so, and as various scholars – e.g. Brown (1985) – have pointed out, it is difficult, if not impossible, to do away with induction altogether.

unobservable posits can be secured as a conclusion so long as it offers the best explanation of the empirical evidence stated in the premises. What makes for a best explanation? Opinions diverge, but they all seem to gesture in the general direction of offering simpler, more unified and/or better understood accounts of the empirical evidence. Ultimately then, the rule of inference to the best explanation relies on concepts like simplicity, unifying power and understanding. These are oftentimes called 'extra-empirical' considerations on account of the fact that they are seen as going beyond the empirical evidence.

The main argument for realism, the so-called 'no miracles argument', is an instance of inference to the best explanation, thereby leaning heavily upon the aforesaid concepts. The argument holds that the best, and indeed sometimes even the only, explanation for the success of science is the truth or approximate truth of its theories, including those parts that deal with unobservable posits. Not surprising then that the constructive empiricists take better explanations like simpler and more unified accounts of the phenomena to be at best pragmatic and at worst merely aesthetic considerations in matters of theory choice.

The main argument for constructive empiricism may be said to be the argument from the underdetermination of theory by empirical evidence. The claim here is that no matter how much empirical evidence we amass, it is always possible to construct more than one, and potentially infinitely many, conflicting theoretical stories about unobservable posits around it. Thus, empirical evidence is deemed insufficient to fix the one true story. Unsurprisingly, the realists repudiate this argument, claiming that homing in on the one true theoretical requires recourse to the aforementioned, 'extra-empirical', considerations. The two parties to this debate thus fundamentally disagree about what are legitimate epistemic considerations in theory choice.

4. Three Charges

In this section, I identify three charges that have been launched against the scientific realism debate. The first is that the debate is at an irremediable impasse that cannot be resolved through rational means. The second is that science can be done perfectly well without having to worry about the debate. The third is that the distinctions the debate thrives on are not well drawn. In what follows, we explore each of these in turn.

Before we consider these charges, we must first clarify their scope. What is common to all three is an attempt to utterly devalue the debate to the extent that the only way forward is to ditch it altogether. To achieve this, it is not enough to claim that the debate suffers from some transient issues, as issues of this kind are arguably present in any debate, whether it be in philosophy or beyond. Rather, the claim must be that there is some systemic failure from which recovery is unlikely or even impossible. To make this point concrete, consider the first charge for a moment. That there is, at least in some respects, an impasse is not a point about which many disagree. The real issue is whether the debate cannot but be at an impasse or is unlikely to be in any other state. For, clearly, if overcoming the impasse were a live option, we would not be forced to give up the debate.

Without further ado, let us turn our attention to the first charge. This one needs a little stage setting. Recall that the two sides disagree on fundamental things like how thinly or thickly empirical evidence must be conceived and on whether rules of inference like

inference to the best explanation are epistemically permissible. If the only route to an unobservable ontology is through a thickly conceived conception of empirical evidence and an ampliatively rich repertoire of inferential rules and associated concepts, then the realists cannot claim victory so long as those very concepts and rules are contested. Conversely, if the only route to a purely observable ontology is through a strict adherence to a contracted notion of empirical evidence and to an ampliatively poor repertoire of inferential rules and associated concepts, then the constructive empiricists cannot claim victory since the severity of that strictness is also disputed. Without a clear verdict on the status of such concepts and rules, the deadlock seems to remain firm.

There are those who suggest that no progress should be expected. The presumption here is that the disagreement is so basic that even rational considerations are powerless to break the deadlock. Anjan Chakravartty (2011a) captures this sentiment, though he falls short of explicitly sanctioning it, when he offers the following diagnosis: "[t]he issues contested range so broadly and elicit so many competing intuitions (about which, arguably, reasonable people may disagree), that some question whether a resolution is even possible" (p. 15). In another publication, he seems to sympathise with this pessimistic point of view when he asserts that "it is questionable what grounds there could be for thinking that there is, in fact, some ultimately compelling reasoning, simply waiting to be uncovered or formulated, with which to break this deadlock once and for all" (2011b, pp. 42-43). Otherwise worded, resolving the debate may be out of reach precisely because even reasonable people may find it hard, perhaps impossibly so, to be compelled to agree on the basics. The implication here being that the discussion may have reached a point where it is totally unresponsive to reason.

Chakravartty (ibid.) takes his cue from the voluntarism of Bas van Fraassen (2002), a position that is clearly a climb-down from the latter author's earlier views. According to voluntarism, two genuinely rival stances may be such that there can be no rational obligation to choose one over the other.[4] To be clear, such stances cannot be jointly endorsed on pain of inconsistency but choosing either at the expense of the other is, rationally speaking, legitimate. Van Fraassen just happens to voluntarily choose constructive empiricism but he could just as well have chosen a realist stance. It should be obvious that this approach, if it is indeed a bitter pill we are forced to swallow, leads to the debate's total demolition. For to truly debate is to debate rationally and choosing sides on whim plainly leaves reason out of the equation.

The second charge against the debate banks on lessons that are presumably inherent in the attitude of scientists. This charge originates with Arthur Fine (1984; 1986), who has famously pronounced realism, and the realism debate more generally, 'well and truly dead'. He puts forth what he considers to be an alternative view, the 'natural ontological attitude' (NOA), and classifies it as form of non-realism. Unlike realist and anti-realist positions, NOA is not meant to impose grand metaphysical or epistemological theses, e.g. aims like truth or empirical adequacy, on the activities of science. Such theses he brands 'unnatural'. Instead, NOA is meant to be a neutral and deflationary attitude that focuses on what both realists and anti-realists share. It consists of

> ... the certified results of science as on par with the more homely and familiarly supported claims [i.e. presumably those of common-sense]... Let us say, then, that both realist and

[4] An epistemic stance, holds Chakravartty (2011b), is "a collection of attitudes and policies governing the generation of factual beliefs" (p. 37).

> antirealist accept the results of scientific investigations as 'true', on par with more homely truths. (I realize that some antirealists would rather use a different word, but no matter.) And call this acceptance of scientific truths the 'core position'. What distinguishes realists from antirealists, then, is what they add onto this core position (1984, p. 96).

Why adopt Fine's non-realist point of view? Because scientists have "turned their backs on realism [and anti-realism] and have managed, nevertheless, to do science successfully without it" (p. 83). That, in a nutshell, is the second charge.

The final charge to be considered here is sourced from Simon Blackburn (2002). Before we examine this charge in detail, it is worth supplementing our account of constructive empiricism with a few more relevant details. Of particular importance in this context is van Fraassen's distinction between belief and acceptance. Constructive empiricism recommends that we accept, not believe, scientific theories. Simply put, to believe a theory is to commit to its truth, i.e. to what it says about both the observable and unobservable posits. By contrast, to accept a theory is to merely commit to its empirical adequacy, i.e. only to what it says about observable posits. The gap between the two commitments isn't as wide as one might at first imagine. Truth still plays a role in constructive empiricism as the very notion of empirical adequacy rests upon it. It's just that this role is restricted. As van Fraassen explains "a theory is empirically adequate exactly if what it says about the observable things and events in the world is true" (1980, p. 12). Thus, to accept a theory is to restrict commitment to a theory's empirical consequences, i.e. those formulated purely in terms of observable posits and relations.[5] On this view, we are meant to be agnostic towards all other consequences. To complicate matters further, the gap between belief and acceptance is even tighter. Acceptance, like belief, involves an attitude of total immersion into a theory. This means full use of the theoretical language, including references to unobservables, but, crucially, without commitment to the corresponding posits.

Blackburn calls into question the scientific realism debate largely by criticising the belief vs. acceptance distinction. Not only is the distinction vague, in his view, but also resistant to attempts at reducing it to other more transparent distinctions. More damningly, he holds that there is no way to discover whether a given person believes or merely accepts a theory. In his own words:

> The problem is that there is simply no difference between, for example, on the one hand being animated [this is his choice of term for immersion] by the kinetic theory of gases, confidently expecting events to fall out in the light of its predictions, using it as a point of reference in predicting and controlling the future, and on the other hand believing that gases are composed of moving molecules. There is no difference between being animated by a theory according to which there once existed living trilobites and believing that there once existed living trilobites (p. 127).

But if the distinction cannot be drawn well, the argument goes, then there is no reason to enter the debate in the first place. Better to stay silent on such matters. As Blackburn puts it, "a surprising 'quietism' or pessimism about a metatheoretical position begins to seem attractive" (p. 111).

---

[5] Again, let me remind the reader that, strictly speaking, this is not correct as Van Fraassen opts for a model-theoretic instead of a syntactic characterisation of theories, evidence and their relationship. One can ignore this detail here for expedience.

The abovementioned charges need to be assessed with the utmost seriousness. In what follows, I will show that the extreme pessimism the advocates of these charges counsel is no more than an exercise in alarmism. We begin with Blackburn.

5. Against Alarmism: Non-collapse and Multi-polarity

The discontent Blackburn expresses towards the belief vs. acceptance distinction is nothing new to the realists. Early on in their inculpation of constructive empiricism, the said distinction was singled out for criticism. Indeed, one objection directed at it – see Melchert (1985) – is strikingly similar to Blackburn's own, as it argues that endorsement of the distinction results in constructive empiricism's collapse to scientific realism. Another objection calls into question the distinction's coherence – see, for example, Horwich (1991). If these and other such objections are *raised by participants* in the debate, does it even make sense to doubt the debate's value? Blackburn undoubtedly thinks so, presumably because he considers them to be self-undermining. Something like, the realist unwittingly kills off the debate by disposing of their main opponent. In what follows, I deflate this threat by questioning the robustness of collapse accusations.

Collapse accusations are common in philosophy. Indeed, in the debate at hand, it's not only constructive empiricism which has been accused of collapsing to realism but also, inversely, various versions of realism have been accused of collapsing to constructive empiricism – see, for example, Ketland (2004).[6] It is tempting to suggest that the ubiquity of such accusations all but guarantees that the respective debates are worth having. I refrain from this temptation. Having said this, their ubiquity does raise the question whether we should scurry away from a debate at the mere sight of a collapse accusation. Notice also that, from a logical point of view, two oppositely directed collapse accusations could not be true together but they could be false together. Now, although I personally think that some of the collapse accusations against versions of realism are true and hence the corresponding accusations against the collapse of constructive empiricism to versions of realism are false, I will not try to convince the reader of this. Instead, I will do something much simpler. I will give a general, though not unerring, recipe of how one can avoid the potentially detrimental consequences of collapse accusations.

The success of many objections is highly sensitive towards framing assumptions. That is, modification of those framing assumptions usually leads to the nullification of the threat. Collapse accusations are no different. In the case at hand, Blackburn helps himself to various framing assumptions. One of them is that the attitudes of being immersed in a theory and believing that theory are indistinguishable. It is far from clear that they are. If, other things being equal, the immersed scientist is much more likely than the true believer to give up that theory in light of an alternative, then there is a genuine difference between the two scientists. Why would the immersed scientist be more willing to turn their back on the default theory? Because in theory choice, you may recall, they are unfettered by constraints like simplicity, unification and other such theoretical virtues.

My critique of Blackburn's accusation is so far quite specific, trading on details about the particularities of the views discussed. But I promised the reader a general recipe. Before I disseminate it, a little proviso is needed. The recipe is meant to save debates, not

[6] In the cited paper, the Ramsey version of structural realism is accused of collapsing to constructive empiricism.

individual positions. In fact, this kind of defence is normally activated when the position being accused of collapse does indeed collapse. Moreover, it is activated when the debate is bi-polar and hence the collapse of one of the poles precipitates the collapse of the conflict in its entirety; we return to the issue of bi-polarity below. For now, suppose that Blackburn's collapse accusation is successful. Does this spell the end of the debate? It does not! Any view that collapses to, what at first appears to be, a diametrically opposite view can be modified so as to escape this fate. Of course, the resulting view is not the original one. But it is a relative, perhaps even a view very close to it. The view is dead, long live the view, sort-of-thing! Whether the resulting view is plausible is another matter – and surely there can be no guarantee of something like that. Ask yourselves this. Has Blackburn demonstrated, or even made a convincing case, that no modification of constructive empiricism can lead to a plausible alternative to realism? He clearly hasn't. Then he has no right to call for the abandonment of the debate and no right to try to impose upon us his quietist viewpoint.

To make the above thought concrete, consider a modified version of constructive empiricism. This modification strips the view of the (supposedly) aggrieving acceptance vs. belief distinction. Call the resulting view 'reconstructive empiricism'. It does not aim to do justice to the actual practice of science, like constructive empiricism, but instead offers a rational reconstruction of what scientists are, and more importantly what they ought to be, doing. According to this view, there is no reason to immerse oneself in the theories of science. All a scientist needs to do is believe the observable parts of those theories. They can then just be agnostic with respect to the unobservable parts. Why? Because they presumably cannot be confirmed through our inborn instruments, i.e. our sensory organs. Is this view plausible? That's something for the reader to decide but it does appear to me that it at least enjoys a certain amount of prima facie plausibility.

Note that reconstructive empiricism preserves the one core element of van Fraassen's view, namely the observable vs. unobservable distinction and the epistemological constraints the distinction engenders. Albeit seeming to appreciate the distinction's import, Blackburn intentionally overlooks it (and its cousin the observation vs. theory distinction).

> Naturally much attention has focussed on van Fraassen's right to invest the observation/theory distinction with this significance (which is not just to query his right to draw the distinction). In this paper I put that aside, saying nothing about the proper reach of 'experience' or 'observation'. I wish instead to explore a different angle: the epistemological conservatism that van Fraassen recommends… (p. 116).

Ironically, the distinction constitutes the *raison d'être* for van Fraassen's epistemological conservatism. Qua an empiricist, van Fraassen thinks that knowability is delimited by unaided sensorial verdict. Observables are things that we can confirm with our unaided senses and hence are knowable. Any posit that does not meet this condition then counts as unobservable and, by van Fraassen's lights, unknowable. Realists urge a broader conception of observability. As we saw earlier, the realists and the constructive empiricists diverge in their exact take of the bedrock of good belief. This very disagreement places the realists and the empiricists on firmly distinct sides of the debate and so neither, thus construed, can be accused of collapse.

Back to the issue of bi-polarity, notice that if the debate is multi-polar, i.e. possessing three or more poles, then eliminating one pole does not result in the dissolution of the debate. Recall the discussion in section two. There the debate was broadly construed with several realist and anti-realist views (and hence several poles) fighting for supremacy. It is only when the debate was more narrowly circumscribed, as in section three, that its characterisation as bi-polar made more sense. But even that characterisation is somewhat foolhardy. That's because the business of circumscription is more of a continuum than an all-or-nothing affair. This is evidenced by the fact that views evolve and branch over time in reaction to new evidence and arguments; more on this in section 7 below. Thus, strictly speaking, there are more than two views within the 'narrowly' circumscribed debate. This includes different interpretations of both views, including the version of constructive empiricism presented above. Moreover, a significant part of the debate, as it is currently conducted, concerns the delectable infighting. So, strictly speaking, bi-polarity in the realism debate(s) is an illusion. But, if that's the case, Blackburn's hope of dismissing the entire debate by dismissing one of its poles is just as illusory.

6. Against Alarmism: Practical Relevance

It is now time to turn to the second charge, namely Fine's accusation that the debate should be jettisoned as scientists get on splendidly without it. Now this accusation could effectively be vanquished through denying that the debate needs to be practically relevant. Some philosophers argue that that their work is intellectually rewarding without having to impinge on practical matters. I am not one of them. Moreover, tempting though it may be, I am not going to rehearse familiar misgivings about NOA.[7] Instead, in what follows, I will try to convince the reader that practitioners of science already display an active engagement with issues that (if not in name, then in spirit) are central to the realism debate. Moreover, I will try to convince the reader that it is far from obvious whether scientists get on splendidly without engaging with those issues.

Let us begin with two theses that even Fine would have trouble denying. First of all, it should be clear that one of science's central characteristics is the appeal to observation as a way to resolve questions about the world. The *Oxford Dictionary's* definition of science as "[t]he intellectual and practical activity encompassing the systematic study of the structure and behavior of the physical and natural world through observation and experiment" is one of many that highlight this central role of observation.[8] Now, scientists may understand the notion of observation in a number of ways but they all pretty much agree that, whatever it means, sensory observation is included. Given what was already said about the philosophers' notion of empirical evidence as the bedrock of good belief, it is not surprising to find significant overlap between the philosophical and the scientific attitudes towards observation. After all, this bedrock contains sensory observation as the only (or at least an important) part.

It should also be clear that there is a difference between what is: (i) immediately available to observation, (ii) only mediately available to observation and (iii) not

[7] Musgrave, for example, (1989) has criticised NOA for its failure to adequately distinguish itself from realism. In his view, NOA is just realism in disguise, for it accepts something that anti-realists like van Fraassen reject, namely the truth of scientific claims about the unobservable world. That is, against Fine's claims, Musgrave thinks that NOA cannot be equated with the common core.

[8] http://www.oxforddictionaries.com/us/definition/american_english/science

available to observation at all.[9] An obvious example of (i) is an object, event or process observed with the naked-eye, e.g. the transit of the moon across the night sky. An obvious example of (ii) is an object, event or process observed only with the help of instruments and calculations, e.g. the transit of an exoplanet across its parent star. An obvious example of (iii) is an object, event or process so remote and inaccessible that it cannot possibly be observed, e.g. the exact distribution of matter in the first few moments after the big bang. Moreover, it should be clear, and this is our second thesis, that these differences involve varying degrees of risk vis-à-vis the veracity of the resulting observation judgments.[10] Even judgments based on what is immediately available may turn out to be false or inaccurate. That is, what is immediately available is not without risk. But that risk is, other things being equal, compounded with additional ones in the case of what is only mediately available and taken to the extreme in the case of what is not available to observation altogether. To give an example, observations of the moon's orbit from planet Earth are pretty routine and low risk. Observations of that orbit from an exoplanet, a few light years away, would be much more difficult, the risk of error increasing. Finally, observations of that same orbit from some spatio-temporal parts of the universe would, presumably, be impossible.

Scientists are not blind to these differences and have even come up with classification systems to capture them. In the social sciences, for example, a distinction is made between manifest and latent variables.[11] The latter play an uncannily similar role to categories (ii) and (iii). Perhaps even more uncannily, latent variables are sometimes described in terms that participants in the scientific realism debate would find very familiar. To adduce both of these claims, let us consider two rather long quotations from Kenneth A. Bollen, whose critical survey of what social scientists mean by latent variables is highly illuminating.

> The idea that *observable phenomena* are influenced by underlying and *unobserved causes* is at least as old as religion, where *unseen* forces affect *real world events*. In the more secular sphere of everyday living, latent variables find wide application. From the response to 'how are you feeling today?' to the description of a worker as "efficient" or a student as 'bright,' such abstract concepts elude *direct measurement*. What these examples illustrate is the common practice among humans to explain, to understand, and to sometimes predict events based on the role of concepts that are not *directly observable* (2002, pp. 606) [emphasis added].[12]

Bollen singles out a number of different approaches to latent variables, of which three of the non-formal ones are worth considering.

> One common set of definitions of latent variables considers them as 'hypothetical variables.'… as 'hypothetical constructs'... a property such as self-esteem is *not real*... but there are *real phenomena* (or traits) to which researchers apply this

[9] The difference between the first two may be one of degree but it is still there.
[10] The constructive empiricists claim that they involve too much risk to be worth endorsing while the realists think that the risk is manageable.
[11] That such notions or cognates can be found widely across the sciences is a claim whose case is made compellingly by Glymour et al. (1987).
[12] In fact, as this quotation makes abundantly clear, Bollen argues that latent variables are deeply embedded into human thinking.

> term... This perspective contrasts with the Platonic view of latent variables in which the latent variables are seen as *real* (Sutcliffe 1965)… Another common definition type treats latent variables as *impossible to measure*, as *unobservable* or *unmeasurable* (pp. 607-8) [emphasis added].

Of particular note to us are the unmistakeable references to divisions between observed and unobserved, directly and indirectly observable, observable and unobservable, as well as real and unreal phenomena, events, things, etc. Such distinctions, far from being the exclusive and idiosyncratic pre-occupation of philosophers in the realism debate, form part of the very fabric of the practice of science.

Why do scientists favour such distinctions? Because, as I just tried to explain a few paragraphs ago, they are well aware of the additional risks indirect observation, the unobserved and the unobservable invite. They thus see the need for distinct concepts that track risk differentials. As Bollen indicates, one of the roles played by latent variables is to denote non-existing or merely hypothetical constructs. Indeed, in a manner reminiscent of empiricist philosophy, some scientists are highly sceptical of the reality of latent variable posits. The father of behaviourism, B.F. Skinner, thought that psychology should steer clear of latent variables, particularly those that denote mental states or events. Others recognise the risk but emphasise the gains. Thus, when Urbain Le Verrier and John Couch Adams independently postulated the existence and orbital characteristics of the planet Neptune on the basis of calculations and prior to any direct observation, they were in effect putting their trust in latent variable posits.

These diverging views suggest that scientists are as engrossed as philosophers in the debate over the credible reach of observation and inference rules. Actual and direct observation can only take science so far. The rest of the voyage must be must be made on the backs of inference rules. Which rules and under what conditions of deployment are issues at the heart of both the philosophical and the scientific discussions. Granted, philosophers in the scientific realism debate have tended to discuss very general rules, e.g. inference to the best explanation, whereas scientists have focused on more specific rules, e.g. the five-sigma rule used in fundamental physics. Although there are indeed some differences, the similarities should not be overlooked. Philosophers have more specific things to say about 'best explanations' and these are often informed by scientific concepts and practice. Thus, Bayesian articulations of inference to the best explanation attempt to do just that – see, for example, Niiniluoto (2004) and Henderson (2014). Moreover, these articulations do not always originate with philosophers but are sometimes pursued by scientists themselves – see, for example, Glass (2007) and Kwisthout (2015).[13] In other words, the traffic is two-way. Philosophers consult scientific concepts and practice and scientists consult philosophical concepts and practice.

It must be admitted, of course, that the traffic going in each direction is not likely to have the same volume. Philosophers of science are much more likely to consult scientific work than the other way around. In addition, it must be admitted that when scientists consult work in the philosophy of science they do not always consult the realism debate. But even that happens sometimes. Borsboom et al. (2003), for example, explicitly argue for the need to realistically interpret latent variables appearing in a certain class of

[13] Indeed, these two references as well as various others demonstrate that some interest exists on the part of scientists to engage with the philosophical literature.

psychological models. When it does not happen, scientists still tend to discuss methodological and epistemological issues that play a crucial role in the realism debate. Thus, as Steele and Wendl (2013) report, climate scientists worry about so-called 'double-counting', i.e. using the same data to calibrate a model but also to confirm it. This issue also plays a central role in the debate over the correct interpretation of the no miracles argument. As some have argued – see, for example, Worrall (2006) –we can infer the truth, approximate truth or partial truth of theories only if those theories enjoy predictive success that is not double-counted. Other, rather widely discussed, issues both in the philosophical literature (on scientific realism) and the scientific literature include ad hoc-ness, simplicity and confirmation measures.[14]

There is no claim here that the views advocated in each context, i.e. philosophical and scientific, are, or indeed need be, exactly same. For example, I am not aware of any scientists, though there may very well be some, advocating constructive empiricism. But there are certainly scientists who lean more towards anti-realist, or ontologically nimble, views. Famous examples include those who advocate instrumentalist interpretations of quantum mechanics and the aforementioned behaviourists. And there are definitely scientists who lean more towards realist, or ontologically bulky, views. Famous examples include those who advocate hidden variable interpretations of quantum mechanics and various enemies of behaviourism, most notably Chomsky-inspired psychologists and linguists. [15]

Finally, we can ask the question: Do these views and related considerations play a positive role in the development of science? This is a question that we cannot easily answer.[16] To adequately answer it would require painstakingly collecting all sorts of information about, among other things, cases where such views and related considerations did indeed play an active role in the scientific enterprise and the results were positive and cases where they played an active role but the results were negative. One could then determine whether such views and related considerations offer, on the whole, a benefit, make no difference or even impede the development of science. Although this and other relevant information is not readily available to us, it is not available to Fine either. So it is premature of him to conclude that the realism debate should be junked. Our rebuttal of Fine's conclusion can be enhanced by one last consideration. Suppose some scientists get on splendidly well without having to concern themselves with the realism debate or related considerations. We can, and indeed should, still ask: Could they have done even better had they concerned themselves with it? This is a highly non-trivial question that requires a highly non-trivial answer.

7. Against Alarmism: Progress

Recall that the first charge is that the debate is at an irremediable impasse. To be more precise, this impasse is the result of the existing arguments not being able to provide a

[14] To give some references: ad hoc-ness (philosophy: Votsis 2016; science: Shavlik and Dietterich 1990), simplicity (philosophy: Sober 1988; science: Akaike 1973) and confirmation measures (philosophy: Fitelson 2007; science: Rips 2001).

[15] Noam Chomsky argued, against Skinner, that language acquisition cannot be explained through behaviour alone but requires the postulation of innate faculties. The latter are obviously not observed (hence they qualify as latent variable posits) but their structure is meant to be discoverable through indirect means.

[16] If we could, neither philosophers nor scientists would presumably be bothered to discuss all of the aforementioned issues. But perhaps a cynic will recoil and argue that those philosophers and scientists who do indeed discuss them are misguided.

clear edge to one or the other side of the debate. The debate is thus accused of being totally unresponsive to reason with no breakthrough in sight. In this section, it will be argued that this claim is also the result of an exaggerated reaction.

Over the last few decades, a number of novel views and arguments or at least adjustments to existing ones have become pivots in the debate. To illustrate this point, I present a list, which, in my view, includes some of the most influential arguments that have graced the pages of journals and books since the 1960s.[17]

- the argument from incommensurability (Feyerabend 1962 and Kuhn 1962)
- the no miracles argument (J.J.C. Smart 1963 and Putnam 1975)
- the pessimistic meta-induction (Laudan 1977)
- the argument from the grid (Hacking 1985)
- the experimenter's regress (Collins 1985)
- the argument from novel predictions (Worrall 1985)
- the inference to the loveliest explanation (Lipton 1991)
- the presuppositional vs. working posits distinction (Kitcher 1993)
- the problem of unconceived alternatives (Stanford 2006)

For each of these arguments numerous discussions – too many to list here – have ensued. Some of them seek to support the arguments. Others seek to undermine them. Several seek to adjust them. There is no insinuation here that the listed arguments are more plausible than the counters that followed. Beyond arguments and in fact sometimes because of them one can also note the emergence of several new or modified views within the debate. I have already mentioned some of the main ones in the preceding section so I will restrict myself to an example here. The structural realism of John Worrall – see his 1989 – is meant to be a compromise between, and hence to draw motivation from, the no miracles argument and the pessimistic meta-induction. It was, in fact, advertised as such under the title 'The Best of Both Worlds'.

Needless to say, the mere emergence of new or modified concepts, views and arguments does not entail progress.[18] That's why I want to sketch in what ways progress has indeed been made and, moreover, in what ways this progress can be amplified or accelerated. The focus of my discussion will be on the historical record of science. I will argue that this record helps in two ways. First, the adequacy of positions in the realism debate can be measured against how well they account for the present historical record, i.e. what has already taken place. Second, their adequacy can be measured against how well they account for the future historical record, i.e. what will take place. In either case, the debate's fairly newfound sensitivity towards the historical record has forced an emergence, elaboration and modification of positions. It has, among other things, made them more accountable than ever before. Let us consider, in turn, each of these ways the historical record is meant to help.

The 1960s were an important turning point for the development of today's debate. That's when a systematic and widespread attempt to account for the historical record of science begun in earnest. Philosophers with largely anti-realist messages, like

[17] Novel or adjusted views are discussed a few paragraphs below.

[18] It may even be argued that deadlocked debates are equally, if not more, likely to lead their participants into the production of new or modified concepts, views and arguments in an effort to break the deadlock. I don't think this is correct, especially when it comes to long-running debates.

Feyerabend and Kuhn, drew attention to the fact that the history of science is littered with theories and, more generally, theoretical frameworks or paradigms that once enjoyed the endorsement of the scientific community but were ultimately supplanted by what appeared to be radically distinct and perhaps even incommensurable rivals. To name but a few, the oxygen theory of combustion replaced the phlogiston theory, the kinetic theory of heat replaced the caloric theory and relativistic as well as quantum physics replaced classical physics. This anti-realist view was propped up further with the development of the pessimistic meta-induction argument in the 1970s, an argument that, as we have seen, employs the historical record to call into question the legitimacy of inferences from success to truth.

The first casualty in this phase of the historical battle was the naive version of the convergence thesis; roughly, the thesis that successful successive theories uphold the theoretical posits and relations of their predecessors. That this is a real victory for the debate is evidenced by the fact that, by and large, not only anti-realists but also realists acknowledged the incredulity of this thesis. If realism is to be a serious contender, then whatever form it takes, it cannot incorporate the convergence thesis, at least not in its foregoing coarse-grained form. Thus, far from talking past each other, the two sides are united in their stance against this thesis. Moreover, not only do the two sides speak with one voice on this issue but they also came to that conclusion by rationally deliberation and appeal to evidence, namely the historical record. That is the unmistakeable mark of progress.

The story does not end there. One lesson taken to heart by the realists is the need for more refined views about the nature of the relationship between success and truth. A direct consequence of this is the realisation that not all parts of successful theories need survive in the wake of a scientific revolution but only those that made genuine contributions to their success. Recall that the realist wants to demonstrate the strength of inferences from success to truth. Parts that play no role in the success of their respective theories are thus irrelevant in such a demonstration. Note that although this approach, dubbed by Psillos (1999) 'divide-et-impera', provides an ingenious fix to the convergence thesis, it doesn't yet decide the matter one way or the other. The ultimate arbiter, once more, is the historical record of science.

Can the more subtle realist positions, i.e. those that have reworked the convergence thesis to reflect the divide-et-impera fix, fend off their anti-realist rivals? To answer this question several historical case studies are at present being carried out. Although this is (on the whole) a positive development, much still needs to be ironed out. Many of these studies suffer from inaccuracies, exaggerations, lack of relevant details, the inclusion of irrelevant details and, above all, an irresistible urge to interpret the facts in a self-serving way. Unsurprisingly, there is no consensus even amongst the realists whether one particular version of the view fares better than the others. Indeed, there is sometimes fundamental disagreement over individual cases. For example, some take the phlogiston theory to be empirically and explanatorily unsuccessful and hence undeserving of the divide-et-impera treatment while others are more charitable.

In spite of these very real problems, I am optimistic that the quality of such studies will steadily improve and that the crucial concepts employed in their evaluation, e.g. genuine success, will become better motivated and perhaps even largely standardised. Moreover, if we are to get a good lay of the historical landscape, the quantity of case studies will

also need to be increased. But that's only part of the solution. The other part is that the positions in the debate must themselves be articulated in more detail. So long as the claims made by positions are interpretationally rather malleable, it will be easier to fit the case studies to a multitude of positions. To give the reader an example, the Fresnel-Maxwell case has been claimed to offer exclusive backing to the following positions, among others, traditional scientific realism (Psillos 1999), structural realism (Worrall 1989), semi-realism (Chakravartty 2007) and constructive empiricism (van Fraassen 2006).

A major spanner in the works for the divide-et-impera approach has been thrown by Stanford (2003) in what may be called as the 'argument from retrospection'. If those parts of theories that yield its empirical success are only identifiable in retrospect (or post-hoc), namely as those that have survived, then the realist fix is trivialised. Stanford's counter-argument to the divide-et-impera approach is useful in that it forces realists to tidy up their back yard. One direct reaction to this challenge, see for example Votsis (2011), is to offer criteria whose satisfaction seek to tell us in advance of any scientific theory change – but, of course, not in advance of testing – which parts of theories are responsible for its empirical success.[19] Regardless of whether or not a given set of criteria is warranted, notice that this type of move allows the realists to stick their neck out for refutation through the making of predictions about the future course of science. That's what I meant earlier when I asserted that the adequacy of positions in the realism debate can be measured against how well they account for the future historical record. Note that the ability to predict which parts of theories are likely to be abandoned in the next scientific revolution puts additional pressure on the debate's participants to articulate and elaborate their views. In other words, if one wants to predict the future form of scientific theories, they need to make their philosophical standpoint in the debate interpretationally rather rigid or at least much more rigid than it is currently. Otherwise any predictions emanating from a given standpoint will be so vague so as to render the standpoint virtually unfalsifiable and therefore empty.

To recapitulate the main points of these last few paragraphs, it is clear that there are problems with the divide-et-impera approach but these are not by any stretch of the imagination insurmountable. Stanford's objection, despite its pessimistic outlook, is in fact a driving force for good in the debate. As before, far from talking past each other, the two sides are largely united in their stance, this time against the view that it is acceptable to identify those parts of theories that yield its empirical success only in retrospect, namely as those that have survived. Also, as before, this conclusion was reached by rational deliberation and an appeal to historical evidence. That surely earns this facet of the debate the characterisation 'progressive'.

Beyond these internal benefits there are also some external ones. Demanding that views in the realism debate or related considerations are sufficiently articulated to facilitate predictions is potentially useful for science. After all, a view or consideration may be used as a heuristic tool for the postulation or the modification of future theories. If, for example, structural realism contains an ounce of truth, then the fate of currently successful theories is destined to be such that some of their structure gets preserved. Under propitious circumstances, the realism debate and related considerations may

[19] The central criterion proposed is that of minimally interpreted mathematical parts. For details and an example see Votsis (2011, §4).

even have a hand in scientific progress being made.[20] If that would not allay the concerns voiced by the debate's critics, nothing would.

6. Conclusion:

It was not my intention here to argue that there is absolutely no way that the scientific realism debate may be misguided or even a dead end. My intention rather was to critically evaluate some reasons people have entertained in dismissing it. These, I have argued, are not compelling, at least not as they are presently formulated. Thus, the debate's worth is, at worst, to be decided and, at best, undaunted. I have also endeavoured to show that the meta-debate, i.e. the debate over the legitimacy of the scientific realism debate, is a worthwhile endeavour. Indeed, since other object-level debates in philosophy and beyond come up against, or are likely to come up against, similar objections to the ones explored above, it is my hope that this essay may serve as a blueprint for their judicious and unbiased evaluation.

References:

Akaike, H. (1973) 'Theory and an Extension of the Maximum Likelihood Principle' in B. N. Petrov and F. Csaki (Eds.), *Second International Symposium on Information Theory*, Budapest: Akademiai Kiado, pp. 267‑81.

Blackburn, S. (2002) 'Realism: Deconstructing the Debate', *Ratio* (New Series), vol. XV: 111-133.

Bollen, K. (2002) 'Latent Variables in Psychology and the Social Sciences', *Annual Review of Psychology*, vol. 53: 605–634.

Borsboom, D. et al. (2003) 'The Theoretical Status of Latent Variables', *Psychological Review*, vol. 110(2): 203–219.

Brown, J. R. (1985) 'Explaining the Success of Science', *Ratio*, vol. 21(1): 49-66.

Chakravartty, A. (2007) *A Metaphysics for Scientific Realism: Knowing the Unobservable*, Cambridge: Cambridge University Press.

Chakravartty, A. (2011a) 'Scientific Realism', *The Stanford Encyclopedia of Philosophy* (Fall 2015 Edition), Edward N. Zalta (ed.), URL = <http://plato.stanford.edu/archives/fall2015/entries/scientific-realism/>.

Chakravartty, A. (2011b) 'A Puzzle about Voluntarism about Rational Epistemic Stances', *Synthese*, vol. 178: 37-48.

Collins, H. (1985) *Changing Order: Replication and Induction in Scientific Practice*, London: Sage Publications.

Feyerabend, P.K. (1962) 'Explanation, Reduction and Empiricism', in H. Feigl and G. Maxwell (eds.) Scientific Explanation, Space, and Time, vol. 3, Minnesota Studies in the Philosophy of Science, Minneapolis: University of Minnesota Press, 28–97.

Fine, A. (1984) 'The Natural Ontological Attitude', in J. Leplin (ed.), Scientific Realism, Berkeley: University of California Press, pp. 83-107.

Fine, A. (1986) 'Unnatural Attitudes', *Mind*, vol. 95(378): 149-179.

Fitelson, B. (2007) 'Likelihoodism, Bayesianism, and Relational Confirmation', *Synthese*, vol. 156: 473-489.

Glass, D. H. (2007) 'Coherence Measures and Inference to the Best Explanation', *Synthese*, vol. 157:275–296.

Glymour, C. et al. (1987) *Discovering Causal Structure: Artificial Intelligence, Philosophy of Science, and Statistical Modeling*, Orlando (FL): Academic Press.

[20] Arguably, something like that happened when Niels Bohr applied the correspondence principle as a heuristic for the formulation of quantum mechanics.

Hacking, I. (1985) 'Do we See through a Microscope?' in P.M. Churchland and C.A. Hooker (eds.), *Images of Science*: *Essays on Realism and Empiricism, with a Reply from B.C. van Fraassen*, Chicago: University of Chicago Press, pp. 132-152.
Henderson, L. (2014) 'Bayesianism and Inference to the Best Explanation', *British Journal for the Philosophy of Science*, vol. 65: 687-715.
Holton, G. (1969) 'Einstein, Michelson, and the "Crucial" Experiment', *Isis*, vol. 60(2): 133–197.
Horwich, P. (1991) 'On the Nature and Norms of Theoretical Commitment', *Philosophy of Science*, vol. 58(1): 1–14.
Ketland, J. J. (2004) 'Empirical Adequacy and Ramsification', *British Journal for the Philosophy of Science*, vol. 55(2), 287–300.
Kitcher, P. (1993) *The Advancement of Science*, Oxford: Oxford University Press.
Kuhn, T. ([1962]1996) *The Structure of Scientific Revolutions*, third edition, Chicago: University of Chicago Press.
Kwisthout, J. (2015) 'Most Frugal Explanations in Bayesian Networks', *Artificial Intelligence*, vol. 218: 56–73.
Laudan, L. (1977) Progress and its Problems: Toward a Theory of Scientific Growth, Berkeley: University of California Press.
Lipton, P. (1991) *Inference to the Best Explanation*, London: Routledge.
Melchert, N. (1985) 'Why Constructive Empiricism Collapses into Scientific Realism', *Australasian Journal of Philosophy*, vol. 63(2): 213-215.
Musgrave, A. (1989) 'Noa's Ark – Fine for Realism', *The Philosophical Quarterly*, vol. 39: 383-398.
Niiniluoto, I. (2004) 'Truth-Seeking by Abduction', in F. Stadler (ed.), *Induction and Deduction in the Sciences*, vol. 11, Vienna Circle Institute Yearbook, Netherlands: Springer, pp. 57-82.
Psillos, S. (1999) *Scientific Realism: How Science Tracks Truth*, London: Routledge.
Putnam, H. (1975) *Mathematics, Matter and Method*, vol. 1, Cambridge: Cambridge University Press.
Rips, L. J. (2001) 'Two Kinds of Reasoning', *Psychological Science*, vol. 12(2): 129-134.
Shavlik, J. W. and T. G. Dietterich (eds.) (1990) *Readings in Machine Learning*, San Mateo (CA): Morgan Kaufmann Publishers.
Smart, J.J.C. (1963) *Philosophy and Scientific Realism*, New York: Humanities Press.
Sober, E. (1988) *Reconstructing the Past. Parsimony, Evolution, and Inference*, Cambridge(MA): MIT Press.
Stanford, K. (2006) *Exceeding our Grasp: Science, History, and the Problem of Unconceived Alternatives*, Oxford: Oxford University Press.
Steele, K. and C. Wendl (2013) 'Climate Models, Calibration and Confirmation', *British Journal for the Philosophy of Science*, vol. 64: 609-635.
Van Fraassen, B.C. (2002) *The Empirical Stance*, New Haven: Yale University Press.
Van Fraassen, B.C. (2006) 'Structure: its shadow and substance', *British Journal for the Philosophy of Science*, vol. 57(2): 275–307.
Van Fraassen, B.C. (2007) 'From a View of Science to a New Empiricism', in B. Monton (ed.), *Images of Empiricism: Essays on Science and Stances, with a Reply from Bas C. van Fraassen*, Oxford: Oxford University Press.
Votsis, I. (2011) 'The Prospective Stance in Realism', *Philosophy of Science*, vol. 78(5): 1223-1234.
Votsis, I. (2016) 'Ad Hoc Hypotheses and the Monsters within' in V. C. Müller (ed.), *Fundamental Issues of Artificial Intelligence* (Synthese Library), Berlin: Springer.

Worrall, J. (1985) 'Scientific Discovery and Theory-Confirmation' in J. C. Pitt (ed.), *Change and Progress in Modern Science*, Dordrecht, Netherlands: D. Riedel.
Worrall, J. (1989) 'Structural realism: The best of both worlds?', in D. Papineau (ed.), *The Philosophy of Science*, Oxford: Oxford University Press, 1996.
Worrall, J. (2006) 'Theory-confirmation and History', in C. Cheyne & J. Worrall (eds.), *Rationality and Reality*: *Conversations with Alan Musgrave*, Dordrecht: Kluwer, pp. 31–62.