

# MIRACLES, TRUST, AND ENNUI IN BARNES' *PREDICTIVISM*

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ABSTRACT: Eric Barnes' *The Paradox of Predictivism* is concerned primarily with two facts: predictivism (the fact that novel predictions play an important part in scientific confirmation) and pluralism (the fact that scientific development is not just a matter of isolated individuals judging the truth, but at least partly a matter of trusting legitimate experts). In the middle part of the book, he peers through these two lenses at the tired realist scarecrow of the no-miracles argument. He attempts to reanimate this weather-worn realist argument, contra suggestions by people like me that it should be abandoned. In this paper, I want to get clear on Barnes' contribution to the debate. He focuses on what he calls the miraculous endorsement argument, which explains not the success of a specific theory but instead the history of successes for an entire research program. The history of successes is explained by reliable and improving methods, which are the flipside of approximately true background theories. Yet, as Barnes notes, the whole story must begin with methods that are at least minimally reliable. Barnes demands that the realist explain the origin of the minimally reliable take-off point, and he suggests a way that the realist might do so. I contend that his explanation still relies on contingent developments and so fails to completely explain the development of take-off theories. However, this line of argument digs into familiar details of the no-miracles argument and overlooks what's new in Barnes' approach. By calling attention to pluralism, he reminds us that we need an account of scientific expertise. This is important, I suggest, because expertise is not indefinite. We do not trust specific experts for everything, but only for things within the bounds of their expertise. Drawing these boundaries relies on our own background theories and is only likely to be reliable if our background theories are approximately true. I argue, then, that pluralism gives us reason to be realists (about some things).

KEYWORDS: scientific realism, prediction, predictivism,  
expertise, Eric Barnes, no-miracles argument

## 1 The no-miracles argument

To put it crudely, the familiar no-miracles argument (NMA) is this: Science is remarkably successful. If our theories were not true, then this success would be an

inexplicable miracle. Contrarwise, the success could be explained by the truth of our theories. So our theories are (probably, approximately) true.

Barnes notes that the no-miracles argument, in casual presentations like this one, is ambiguous. One interpretation, which he dubs the ‘miraculous theory’ argument, explains the success of a particular theory *T* in terms of the truth of *T*. I am sympathetic with Barnes’ reasons for rejecting the miraculous theory argument,<sup>1</sup> and the arguments I have elsewhere given against the NMA most readily apply to the miraculous theory argument.<sup>2</sup> I will not rehearse these reasons here. Rather, I will set the miraculous theory argument aside.

The other interpretation, which Barnes dubs the ‘miraculous endorsement’ argument, is concerned with the success of scientific practice in general. It roughly takes this form: Scientists make predictions which are verified. If these scientists were merely guessing or accommodating prior evidence, then this success would be an inexplicable miracle. Contrarwise, this success could be explained by scientists’ general reliability. So scientists are (probably, mostly) reliable. The reliability of their methods only makes sense if scientists’ background theories are true. So those background theories are (probably, approximately) true.<sup>3</sup>

The miraculous endorsement argument is not about a particular theory *T*. Rather, it is about the ability of scientists to develop theories like *T*. It explains their ability in terms of the general reliability of their methods and the truth of their background theories. This strategy shifts attention away from specific, isolated theories and towards the background theories which underwrite scientific methods.

Moreover, it leads first to the conclusion that scientists are reliable experts and only from there to the truth of theories. As Barnes explains, “novel success is not direct evidence of theory truth — it is rather evidence for the credibility of the endorser. More specifically, it is evidence for the truth or empirical adequacy

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<sup>1</sup> Eric Christian Barnes, *The Paradox of Predictivism* (Cambridge: Cambridge University Press, 2008), esp. 132–136.

<sup>2</sup> P.D. Magnus, Craig Callender, “Realist ennui and the base rate fallacy,” *Philosophy of Science* 71, 3 (2004): 320–338.

<sup>3</sup> As Barnes acknowledges, Boyd has long championed the miraculous endorsement argument; see e.g. Richard Boyd, “Scientific realism and naturalistic epistemology,” *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association (1980) Volume 2*, 1982, 613–662. Lipton also draws the connection between reliable methods and true background theories; see e.g. Peter Lipton, “Is the best good enough?,” in *The Philosophy of Science*, ed. David Papineau (Oxford: Oxford University Press, 1996), 93–106.

of the endorser's background beliefs."<sup>4</sup> Note that, in this passage, he is still allowing for the possibility that predictive success might just be explained by the empirical adequacy of background theories. He thinks that making it an argument for realism requires solving two problems — what dubs 'the problem of take-off theories' and 'the anti-realist challenge.'<sup>5</sup> In the next section, I discuss the problem of take-off theories. In the subsequent section, I discuss the anti-realist challenge.

Barnes' miraculous endorsement argument, situated as it is in a book about predictivism, ultimately yields a conclusion about predictivism — viz., that an anti-realist cannot be a predictivist. In the final section, I evaluate this claim.

## 2 The problem of take-off theories

The miraculous endorsement argument offers us a picture of science which begins with modestly reliable methods, uses them to generate approximately true theories, embodies the theories in more reliable methods, generates better theories... and so on. With each cycle, the methods become more reliable and the theories closer to the truth. Looking at the story in explanatory rather than historical order: The success of present theories can be explained by the reliability of present methods, which in turn is explained the approximate truth of previous theories, which is explained by the more modest reliability of previous methods... and so on.

This story presumes that, in the primordial days of urscience, there were some methods that were at least a little bit reliable that embodied theories which were at least in the neighborhood of truth. Boyd calls this a 'take-off point', "a point in the development of the relevant scientific discipline at which the accepted background theories are sufficiently approximately true and comprehensive."<sup>6</sup> For Boyd, it is a contingent matter that past science reached a take-off point. Barnes insists, however, that the realist owes us an explanation of how this could happen. Without such an explanation, the realist story about increasing reliability involves a miracle at the very beginning. The worry is that Boyd "provides no account of the emergence of take-off theories that is clearly miracle-free [and so he] cannot claim to win the miraculous endorsement argument for realism: he has simply buried the miracle in the emergence of take-off theories."<sup>7</sup>

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<sup>4</sup> Barnes, *The Paradox of Predictivism*, 140.

<sup>5</sup> Barnes, *The Paradox of Predictivism*, 145–147.

<sup>6</sup> Boyd, "Scientific realism and naturalistic epistemology," 627.

<sup>7</sup> Barnes, *The Paradox of Predictivism*, 146.

The realist can answer this worry, Barnes suggests, only by showing that take-off theories are motivated by standards to which everyone — realist or not — is committed. These pre-theoretic standards show how initial methods were at least a little bit reliable. In the metaphor, they provide the launching pad for the take-off theories. So, Barnes insists, “the realist must argue that take-off theories were assessed as plausible on some basis that was not itself theory laden — but was nonetheless truth conducive.”<sup>8</sup> Barnes suggests that simplicity judgements, suitably understood, serve as such a standard.

Barnes discusses the example of William Bateson, important promulgator of Mendelian genetics. Bateson believed Mendelism because he was committed to using artificial breeding as a method for studying heredity, to specific techniques of numerical analysis, and because Mendelian genetics made the most sense of these data. Barnes maintains that these commitments were “not... based on Bateson’s acceptance of any scientific theory of his day.”<sup>9</sup> Barnes continues, “Neither the experiments that were designed, nor the observations that these biologists made, nor the inference to the Mendelian explanation itself were critically dependent on the acceptance of anything that deserves to be called a scientific theory.”<sup>10</sup> According to Barnes, these experimental and methodological commitments provided an independent ground for the take-off theory.

I want to consider two objections to Barnes’ argument.

First, he relies too much on the distinction between what “deserves to be called a theory” and what does not. Admittedly, the use of artificial breeding to discover the nature of heredity did not depend on Bateson’s acceptance of Mendelian genetics. If it was shared by Bateson and all his interlocutors, then indeed it was not based on any *specific* scientific theory of his day. Nevertheless, the reliability of the method is contingent. We can easily imagine a world in which it fails. Imagine a world of disjunctive heredity, for example, in which creatures in the wild reproduce by entirely different means than creatures in cages. If we demand an explanation for the modest reliability of methods used in crafting take-off theories — lest it be a miracle — then we must demand an explanation for the reliability of Bateson’s method.

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<sup>8</sup> Barnes, *The Paradox of Predictivism*, 148. Of course, showing that we are all committed to some pre-theoretic standard is a step short of showing that the standard is truth conducive. Barnes recognizes this. He argues that anyone willing to apply the standard at the observable level should be willing to use them in inferences about unobservables; see the next section.

<sup>9</sup> Barnes, *The Paradox of Predictivism*, 150.

<sup>10</sup> Barnes, *The Paradox of Predictivism*, 150-1.

This regress is inevitable. Principles of scientific inference are ampliative, so the reliability of scientific methods is always contingent. For any method, the world could (logically) have made the method unreliable. So we can ask what it is about the actual world that makes the method reliable. One might reply by reconstructing scientific inferences without ampliative principles, as *deductions from the phenomena*. However, the reconstruction can only be accomplished by hiding the inductive risk in contingent premises. We can ask what it is that explains why those premises are true.

Second, set the first objection aside for a moment and allow that some inference principles are so basic that their reliability does not require an explanation. It is still a contingent fact that *scientists were committed* to these rather than other standards. We can ask why Bateson and his contemporaries accepted methods of artificial breeding as a clue to heredity.

Suppose that one did explain why Bateson accepted it, why another scientist accepted it, why a third did, and so on. This would explain, for each scientist, why that scientist accepted the primordial method. If some of them came to accept it for different reasons, then there would still be a puzzle. The *explanandum* is their agreement, why these diverse causal processes should lead them all to accept this same method.<sup>11</sup>

This worry turns on a familiar point about explanation: Any explanans can become an explanandum. For any fact, we can ask — why that? The requirement Barnes puts on realist is that, for any take-off theory, we explain the take-off point. Barnes explains Bateson's commitment to Mendelian genetics in terms of Bateson's extra-Mendelian commitments, and he is satisfied to stop there. We can ask — why are these commitments reliable? why was Bateson committed to them?

Any specific explanatory story that the realist offers must stop somewhere. Where it stops, it relies on a contingent and unexplained *explanans*. If these still count as miracles in a way that undoes the no-miracles argument, then the no-miracles argument necessarily fails. Yet talk of *miracles* can be misleading here. The realist begins with a the present success of science taken altogether (a seeming miracle that requires explanation), explains it in terms of prior science (less miraculous), which in turn is explain in terms of science before that, and so on. The realist story will still end in some contingent fact, but something more like a

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<sup>11</sup> This is like the well-worn example of explaining the hair colour of the members of the Red Hair Society. The individual explanation for each red-haired member is different than the group explanation of synchromaticity.

coincidence than a miracle. Some coincidences neither require nor admit of explanation.

Arguably, common sense involves commitment to some basic rules of evidence. For example, what visually appears to be the case under good viewing conditions should be taken to be actually so. Refusal to accept basic commitments like these is tantamount to embracing scepticism. However, the commitments that Barnes appeals to as take-off points are not primordial rules of this kind. One might insist that artificial breeding is no clue to heredity in nature without thereby sliding into scepticism. This is not just a shortcoming of Barnes' example, either. No scientific advance could possibly be explained just in terms of universal, common sense rules, because an *advance* is something that previous thinkers had not hit upon yet. The contingencies which make the advance possible are themselves potential *explananda*.

So I think the realist should refuse Barnes' challenge to explain the take-off point of take-off theories. Rather, the realist should insist that science began in contingency. It involved a little bit of luck.

### 3 The anti-realist challenge

If I am right, then the problem of take-off theories is actually a non-problem. The realist story begins, in the days of primordial science, with methods that are at least a little bit reliable and theories that are at least in the neighborhood of truth. The realist may take that minimal starting point as a happy thing that does not itself require explanation in terms of earlier reliability or earlier truth. Even granting such a starting point, however, an anti-realist might insist that the early methods were *empirical* and the early truths were *observable*. The anti-realist may further insist that long climb of science has not been about the refinement of reliable methods and more precise truths, but about the refinement of empirical methods and empirically-adequate theories. This insistence is what Barnes calls the *anti-realist challenge*.

Barnes' reply begins by noting that, for claims about observables, there is no difference between truth and empirical adequacy. To believe that 'I am eating a sandwich' *is true* is no different than believing that observable things like a sandwich are such *as if it were true*. This means that the anti-realist and the realist form beliefs in the same way throughout the observable domain. The anti-realist employs judgements of simplicity (for example) to conclude from scat and chattering noises that there is an unseen mouse in the wainscoting. If we continue to employ such judgements, they lead beyond beliefs in observable things to beliefs in the unobservable. Just as we believe in unseen mice on the basis of

observed mouse evidence, we should believe in unseen electrons on the basis of observed electron evidence.

Barnes characterizes this as the move from horizontal inference (inferring from observables to other observables) to vertical inference (inferring from observables to unobservables). It is, in Barnes' words, "the oldest argument for realism in the book"<sup>12</sup> and a "venerable realist tradition."<sup>13</sup> It is what Philip Kitcher calls the 'Galilean Strategy.'<sup>14</sup>

I have responded elsewhere to Kitcher's version of the argument.<sup>15</sup> The gist of my objection is this: Suppose the anti-realist has no good reason to *deny* that judgements of simplicity which are legitimate for horizontal inference are also legitimate for vertical inference. This does not show that the anti-realist *must* or even *should* accept this extension. The realist claims that these principles of judgement start out applying both to observables and unobservables, and so the realist sees the anti-realist as unjustifiably stifling them. Yet the anti-realist claims that these principles start out applying to observables, and so the anti-realist sees the realist as unjustifiably exploding them.

When we are talking about judgements of simplicity, inference to the best explanation, or success-to-truth inferences, this debate amounts to nothing more than the realist and anti-realist starting from different places. This is not so much an argument *for realism* as it is a hymn which is comforting *to realists*. The anti-realist is free to think that simple explanations or successful theories do not form homogenous classes, and so insist that there is no positive reason to extrapolate from the ones that do not invoke unobservable entities to those that do.

The problem is not that the horizontal-to-vertical move is necessarily a bad inference pattern. It just requires, as a premise, that the horizontal and vertical cases are relevantly similar. An anti-realist refuses to accept the premise by refusing to presumptively see simplicity and explanation as stable mechanisms.

Suppose instead we consider a specific instrument, like a microscope. A weak optical microscope can be used to look at things that sharp-eyed people can see without lenses, and the same instrument can be used to look at things that no one can discern unaided. The continuity of the instrument, the fact that it is the very same material object, gives us a positive reason to think that the two cases are

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<sup>12</sup> Barnes, *The Paradox of Predictivism*, 153.

<sup>13</sup> Barnes, *The Paradox of Predictivism*, 156.

<sup>14</sup> Philip Kitcher, "Real realism: The Galilean Strategy," *The Philosophical Review* 110, 2 (2001): 151–197.

<sup>15</sup> P. D. Magnus, "Success, truth, and the Galilean Strategy," *The British Journal for the Philosophy of Science* 54, 3 (2003): 465–474.

similar. So we should rely on the instrument even for the things we could not check with our naked eyes. Once we have established a weak optical microscope, we can use the overlap with higher power microscopes to argue for beliefs about smaller and smaller things. We are not simply noting the absence of a specific reason to stop at the boundary of the observable, but providing a positive reason to move beyond it. This is an answer to the anti-realist for the specific unobservable things that are detectable under microscopes.

The crucial factor here is the material continuity of the instrument. The instrument is the very same thing when pointed at observables or at unobservables, and so there is a positive reason to think that its reliability with the former extends to the latter. Plausibly, breeding as a clue to heredity exhibits the same kind of material continuity as microscopy as a clue to the very small. So perhaps we can give positive reasons — without begging the question in favor of realism — to believe Mendelian genetics. Obviously, spelling this out would require more work.

#### 4 The limits of expertise

Taking a step back from the miraculous endorsement argument, we should recognize Barnes' concern with predictivism; i.e., with the claim that novel predictive success provides a special reason to accept a theory. Barnes argues that the anti-realist cannot be a predictivist. His argument has two parts and goes roughly like this:

Begin by considering whether novel predictive success is *ipso facto* probative.<sup>16</sup> The realist can say that it is. By way of the miraculous endorsement argument, the realist sees novel predictive success as a reason to believe that the predictors are experts — that is, that predictors have approximately true background theories. The anti-realist, however, has no reason to think of novel predictive success as anything more than just a theory's being empirically adequate in this instance. This particular success is no special reason to think that the theory is empirically adequate *tout court*.

Yet novel predictive success might still be a reliable guide to other theoretical virtues.<sup>17</sup> The view here is that “accommodators are prone to endorse theories that are built to fit data in some disreputable way — either because such theories violate some extra-empirical criteria or because they incorporate ad hoc hypotheses that are insufficiently supported either by extra-empirical criteria or

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<sup>16</sup> Barnes calls this ‘virtuous predictivism.’

<sup>17</sup> Barnes calls this ‘unvirtuous predictivism.’



empirical data.”<sup>18</sup> The realist, seeing extra-empirical criteria as truth indicative, will condemn such vicious accommodation as leading away from the truth. The anti-realist, seeing the extra-empirical criteria as merely pragmatic, can only condemn it as awkward.

The first part of the argument seems right to me. The anti-realist should say that, although novel predictive success *qua* success is indicative of empirical adequacy, it has no special value *qua* novel. If this is counter-intuitive, it is no more counter-intuitive than many other aspects of anti-realism.

The second part also seems right for an anti-realist who insists that criteria like simplicity can only be non-empirical and purely pragmatic. However, an anti-realist can deny that such virtues are utterly extra-empirical. It is common<sup>19</sup> to argue that complex theories over-fit by describing idiosyncrasies of a particular data set rather than describing the broader pattern. If a theory is simpler, then it is less likely to just describe the history of previously observed phenomena — and more likely to be adequate to unobserved phenomena. Similarly, a theory which makes successful novel predictions does not succeed just by being antecedently fit to those predictions. So predictive success similarly suggests that the theory is really empirically adequate rather than being fit just to the specific data set. This suggests a way that an anti-realist can be a predictivist. This means that an anti-realists may accept a *theory* based on its novel predictive success.

Still, this is different than the kind of predictivism Barnes advocates: that we should trust an *expert* based on her novel predictive success. Barnes suggests that an anti-realist cannot accept that kind of predictivism.

His own case for this, as we have seen, goes by way of the miraculous endorsement argument. My criticisms of the argument threaten to sever this connection: In §2, I argued that the realist must resist the urge to explain the origins of every precondition for present science — even for the realist, the story must be looser than that. In §3, I argued that this amount of slack leaves the realist no way of overthrowing the anti-realist at the general level which does not illicitly rely on realist intuitions. These criticisms leave the anti-realist room to

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<sup>18</sup> Barnes, *The Paradox of Predictivism*, 161.

<sup>19</sup> e.g. P.M. Churchland, *A Neurocomputational Perspective: The Nature of Mind and the Structure of Science* (Cambridge: The MIT Press, 1989), 179–181; Malcolm R. Forster, Elliott Sober, “How to tell when simpler, more unified, or less Ad Hoc theories will provide more accurate predictions,” *The British Journal for the Philosophy of Science* 45 (1994): 1–35; Christopher Hitchcock, Elliott Sober, “Prediction versus accommodation and the risk of overfitting,” *The British Journal for the Philosophy of Science* 55, 1 (2004), 1–34.

maneuver. I want to offer a somewhat different route to Barnes' conclusion, making use of the looser realist story.

At the end of the last section, I observed that material instruments which are reliable when applied to observable things continue to be the same material thing when applied to unobservables. This continuity provides a positive reason to accept realist conclusions about the entities detected *with these instruments*, even if the general issue of realism versus anti-realism is a standoff. My further suggestion is that *trusting an expert* is like using an instrument. If we trust an expert on matters which we can check directly, then our trust in that expert means that we should respect their opinion about other matters as well. The point is not that anti-realists cannot antecedently accept expert-trusting predictivism, but that it would subsequently require them to abandon their anti-realism at least regarding the things about which experts can testify.

The anti-realist may respond that trusting an expert is never a matter of unlimited trust. Even if I trust my doctor's medical opinion, I may not trust her opinion about cars or distant galaxies. So the anti-realist might try to trust experts only about observables.

The anti-realist reply is correct insofar as expertise always has its limits. This is an aspect of expertise that is missing from Barnes' argument. The miraculous endorsement argument, as he presents it, lends the glow of truth to the predicting scientist. Yet it is a mistake to think that a scientific expert is a general purpose truth machine. When an elder physicist pontificates about vitamins or the recent election, we do not trust them to the same degree that we do when they talk about physics.

To make this discrimination, we must be able to recognize the legitimate domain of a scientist's expertise — to separate physics from biomedicine and politics. The anti-realist may be able to do that much, because the phenomena of physics, medicine, and politics are very different. Yet we must also be able to parse expertise in more detail. An expert on viruses may be able to tell us quite a lot about viral infection, but should be less authoritative on bacterial infection, even though the difference between a viral and bacterial infection is not an observable or phenomenal difference. The difference is the kind of unobservable organisms that cause the trouble. As such, the anti-realist (*qua* anti-realist) will have trouble properly distinguishing the two domains of expertise.

Work by Frank Keil and others<sup>20</sup> can be seen as supporting my argument. Even preschoolers see groups of experts as having authority that is defined and constrained by causal patterns in the world. In order to mark the boundaries of expertise, children and adults alike need to have some sense of how the world is causally structured into different domains. Keil *et al.* write: "There are domain-specific patterns in the world that experts know and use to understand a wide range of phenomena that arise from those patterns."<sup>21</sup> The studies suggest that these patterns are often a matter of unobservable, internal features rather than observable, superficial features. So the assignment of expertise is most naturally understood in a realist way.

Here the anti-realist might insist that following the advice of appropriate experts is a matter of *doing* rather than *believing*. We can distinguish bacterial from viral infections on the basis of some observable indicators, for example, and so one might try to exploit those indicators without believing anything about the unobservable difference that they reflect. Constructive empiricists are prepared to *accept* all the claims that realists are prepared to believe, where 'acceptance' means acting as if it were so. That is, constructive empiricists will not believe but will act just as if they believed. If this distinction between belief and acceptance is tenable — if it is possible to act in every respect that matters as if you do believe something while nonetheless not believing it — then anti-realists can treat experts in just the way realists would. Yet the distinction begins to fray, both in general and specifically in this case. In general, it is not clear how non-belief that makes no practical difference actually differs from belief; it looks as if anti-realists really do believe, but are in denial about it. Moreover, the posture of mere acceptance runs afoul of how expertise actually works. The trust we place in experts is epistemic as well as prudential. What one *does* when trusting an expert is *believe*.

The argument I am suggesting can be summarized in this way: Suppose that we trust experts who can make successful novel predictions. This trust is limited to their proper domain of expertise. In some cases, that domain can only be recognized by considering the unobservable objects of enquiry about which they are experts. So the trust cannot be merely a matter of accepting their

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<sup>20</sup> Donna J. Lutz, Frank C. Keil, "Early understanding of the division of cognitive labor," *Child Development* 73, 4 (2002): 1073–1084, Frank C. Keil, "Categorisation, causation, and the limits of understanding," *Language and Cognitive Processes* 18, 5/6 (2003): 663–692, Frank C. Keil, Courtney Stein, Lisa Webb, Van D. Billings, and Leonid Rozenblit, "Discerning the division of cognitive labor: An emerging understanding of how knowledge is clustered in other minds," *Cognitive Science* 32, 2 (2008): 259–300.

<sup>21</sup> Keil et al., "Discerning the division of cognitive labor," 298.

proclamations as empirically adequate; that is, we end up committing ourselves to these unobservable objects. Therefore, an anti-realist must either give in to realism (with regard to specific unobservable entities) or deny the supposition of predictivism (as a general matter).

Barnes uses the word ‘pluralism’ to indicate the fact that scientific development is not just a matter of isolated individuals judging the truth, but at least partly a matter of trusting legitimate experts. In that terminology, the upshot of my argument is that thoroughgoing anti-realism is incompatible with pluralism.

## 5 Conclusion

Elsewhere, I’ve drawn the distinction between wholesale arguments (which attempt to establish realism for all or most science) and retail arguments (which attempt to establish realism about specific kinds of things).<sup>22</sup>

Barnes argues, on the basis of the miraculous endorsement argument, that we should think of the broad picture of the world offered by science as roughly correct. Because of the objections I raised earlier (§§2&3), I think it fails if we think of it as a wholesale argument. It could succeed as the strategy for a retail argument, but it is missing details. More needs to be said about how far expertise reaches and about how confident we ought to be in particular parts of scientists’ background theories.

Nevertheless, the miraculous endorsement argument is enlightening because it makes the connection between predictivism and what Barnes’ calls ‘pluralism’ — the fact that scientific practice involves trusting experts. I have suggested that limiting trust to an experts’ domain of competence, as a pluralist must do, relies in part on claims about the unobservable world. So a pluralist and predictivist must be a realist *with regard to those claims*. Since I have only sketched the argument, however, I have not been able to say which claims those are. The answer will require a more detailed discussion of how expertise functions. The conclusion of it will be a limited realism, just about some specific things — so it will only be a retail argument.

Reflecting on scientific practice, predictivism and pluralism almost seem like data with which we as philosophers of science must reckon. Barnes is right, I

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<sup>22</sup> Magnus, Callender, “Realist ennui and the base rate fallacy,” P. D. Magnus, “Inductions, red herrings, and the best explanation for the mixed record of science,” *The British Journal for the Philosophy of Science* 61, 4 (2010): 803–819. Barnes responds directly to issues raised by Craig Callender and me, specifically over whether base rate information is required to formulate the No Miracles Argument. Nothing I have said in this paper turns on base rates.

think, in arguing that reckoning with them will demolish monolithic anti-realism. Yet I think we ought to demolish monolithic positions generally. If we give up looking for a wholesale argument by which questions can be settled all together, we start the hard work of crafting retail arguments that can settle matters here and there. Predictivism and pluralism are promising clues to how we might construct some of them.<sup>23</sup>

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<sup>23</sup> An earlier version of this paper was presented as part of a book symposium at the Philosophy of Science Association 2010 Biennial Meeting in Montreal.