THE "NO-MIRACLES ARGUMENT" DOES NOT COMMIT THE BASE-RATE FALLACY

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Abstract
For the last forty years the most popular argument for scientific realism has been the "No Miracles Argument" ("NMA"): realism offers the best explanation of the observational success of scientific theories. Howson and others have objected that NMA "neglects the base rate" and so is fallacious. The paper argues that whatever failures NMA may have, it is not guilty of this neglect. The base-rate objection overlooks that NMA is not defending a realism about current theories in general but a realism about a quite restricted group of current theories, the "well-established" ones. The objection inappropriately foists a fallacious argument on the realist that is in fact irrelevant to NMA.

Key Words
No-Miracles Argument, base-rate fallacy, scientific realism, Howson, Magnus and Callender

1. Introduction

For the last forty years the most popular argument for scientific realism has been that it offers the best explanation of the observational success of scientific theories.[1] Sometimes the argument goes further: it would be "a miracle" that theories were so successful if they weren't approximately true (Putnam 1975, 73; 1978, 18-19). This has yielded the name "no miracles argument" ("NMA").[2] Realism does not just have the best explanation of success, no other good explanation is *possible*.

NMA has recently been subjected to an interesting objection. Colin Howson (2000, 52-9; 2013) uses probability theory to argue that NMA "as it is usually presented…is fallacious, thoroughly fallacious" (2000, 59). Its problem is that it commits the fallacy of "neglecting the base rate". P. D. Magnus and Craig Callender (2004) argue likewise. Peter Lipton finds it "difficult not to suspect that the original plausibility of the miracle argument is just an instance of philosophers falling for the ubiquitous fallacy of ignoring base rates" (2004, 196). Anjan Chakravarrty thinks that NMA is committed to a claim that "is arguably…an instance of the base rate fallacy" (2014). Stathis Psillos, a devout realist, struggles with the base-rate objection over many pages (2006, 143-53) without, it seems to me, striking a fatal blow. I aim to deliver that blow, arguing that NMA is not from guilty of the base-rate fallacy. The objection inappropriately foists a fallacious argument on the realist that is in fact irrelevant to NMA.

[1] For evidence of this popularity, see Kukla1998, Psillos1999, Harker 2010.
[2] Other names include, "the success argument" and "the ultimate argument".

In arguing this I do not mean to imply that NMA is a good argument for scientific realism. Quite the contrary: I argue elsewhere (forthcoming) that NMA is a dubious abduction.[3] But, if that is right, NMA's failure is a rather subtle matter and very different from an obviously fallacious neglect of a base rate.

We need to start defending NMA from the base-rate objection by saying some more about NMA. In particular, what is scientific realism and what is the success that realism is supposed to explain?

2. NMA

What is scientific realism? The literature provides a bewildering variety of answers but I take it that the core doctrine that NMA is supposed to justify can be described along the following lines.

Science appears to be committed to the existence of a variety of unobservable entities - to atoms, viruses, photons, and the like - and to these entities having certain properties. The central idea of scientific realism is that science really is committed and is, for the most part, right in its commitments. So, for the most part, those scientific entities exist and have those properties.

This could do with some clarification. First, what is meant by the commitments of "science"? Given the concerns of NMA, the answer is the commitments of *current scientific theories*. Second, the qualification "for the most part" is necessary because current scientific theories are surely making some mistakes. Third, and very important, we need a further qualification in light of the fact that scientists themselves have many epistemic attitudes to the theories that they deal with. Sometimes a theory is rejected by all but a few mavericks. Sometimes scientists know a theory is false but still find it useful for predictions. Often scientists are agnostic because a theory is an exciting speculation at the frontiers, without a firm evidential basis. And often scientists have a strong commitment to a theory that has been thoroughly tested by the scientific method (whatever that may be) and has become part of the scientific consensus. The realist is not less skeptical than the scientist: she is committed only to the claims of the latter theories, which I shall briefly call "well-established".[4] *This qualification turns out to be crucial in assessing the base-rate objection.* In brief, realism is a cautious generalization of the commitments of current theories that are well-established, in the sense described.

In light of this we can define the doctrine that concerns NMA well enough as follows:

Scientific Realism (SR): Most well-established current scientific theories positing unobservables are approximately true.

[3] If NMA fails, is scientific realism doomed? I think not because there is another argument for it in the literature (Smart 1979, Devitt 1984; Glymour 1984; McMullin 1984; Salmon 1984) that has been almost entirely ignored: realism best explains *the observed phenomena*.

[4] An example might help. The nineteenth century theory of atoms was controversial at the time. Many scientists took what was, in effect, an instrumentalist view of the theory. Then, famously, the controversy ended early in the twentieth century with the work of Einstein and Perrin. In my terms, a theory that was not "well-established" had then become so; see Gardner 1979.

Now, according to NMA, the observational success of most well-established current scientific theories is best explained by their approximate truth. So NMA is an "abduction" to SR. What is it for a theory to be observationally successful? It is for its observational predictions to tend to come out true. According to NMA, that tendency in well-established theories is mostly best explained by their approximate truth. For example, why are all the observations that we make just the sort we would make if there were atoms? Answer: because there *are* atoms. Sometimes, as noted, the realist goes further. Not only does SR provide the best explanation of this success, no other good explanation is *possible*. The success of science would be "a miracle" if those theories were not mostly approximately true.[5]

I turn now to the objection that NMA neglects the base rate.

3. The Base-Rate Objection

The base-rate fallacy is often illustrated by a vivid example, "the Harvard Medical School Test", once administered to staff and students in that School. The Harvard Test supposes that there is a test for some disease and that:

D1. If a person has the disease, then she is 100% likely to test positive.

D2. If a person does not have the disease, then she is 5% likely to test positive.

The latter is the "false-positive" rate. Question: If a person tests positive, what is the likelihood that she has the disease? One is tempted to say quickly, as most in the Medical School did, "95%". But we are actually in no position to answer this question *until we know "the base rate"*. Suppose, for example, that only 1 person in a 1000 in the population tested has the disease. So, if we test 1000 people we should expect among those who test positive just 1 who has the disease *but 50 people who do not* (given the 5% false-positive rate). So the chance of a person who tests positive actually having the disease is far from 95%: it is approximately 2%!

The base-rate fallacy is famously seductive. I suggest something else that is seductive: assuming that NMA is analogous to the fallacious argument in the Harvard Test. Let us call this seductive assumption, "the Medical Analogy".[6] It represents NMA along the following lines. NMA takes success to be a test for a current theory's approximate truth. (For ease of exposition, I shall henceforth drop the qualification "approximate" in discussing the Analogy.) NMA is then alleged to continue like the medical argument, for example:

T1. If a current theory is true, then it is 100% likely to be successful.

T2. If a current theory is not true, then it is 5% likely to be successful.

[5] According to Alan Musgrave (1988), NMA has been around a long time, at least since Clavius.

[6] See, e.g. Howson (2000, 53-4; 2013, 205-6), Magnus and Callender (2004, 324-6), and Lipton (2004, 196-7). I was certainly seduced. As a result, I included the Medical Analogy in my case against NMA in papers delivered in eight places from August 2014 until May 2015, when I changed my mind about the Analogy. My presentation of the Analogy on those occasions received no objection, thus providing further evidence of the seductive power of the Analogy.

Question: If a current theory is successful, what is the likelihood that it is true? NMA implicitly answers, "Very high". Once again, the base rate is being neglected. Suppose that only 1 current theory in 1000 is true. Then the chance of a successful theory being true, just like the chance of person who tests positive having the disease, is not very high but very low, approximately 2%! So NMA is fallacious.

I shall now argue that the Medical Analogy is mistaken and so the base-rate objection fails.

4. What is Wrong with the Base-Rate Objection?

Note first that *friends* of NMA do not typically present NMA in probabilistic terms but rather in explanationist terms; see, for example, my presentation in section 2. According to the friends, scientific realism is the conclusion of an abductive inference: briefly, SR best explains success; more fully, the observational success of well-established current theories is mostly best explained by their truth. We can say confidently that the friends *typically* present NMA in an explanationist way but has *any* friend *ever* presented NMA in anything like the probabilistic way of the Medical Analogy? Perhaps Cornelis Menke has. He thinks that Howson has given "a reasonable reconstruction of the miracle argument" (2014: 104) and sets out to save NMA from fallacy by changing its explanandum. But in the absence of more evidence that NMA's friends regard this reconstruction as reasonable, we should at least wonder whether the Medical Analogy is appropriate.

But doesn't the "no-miracles" rhetoric *imply* that NMA is probabilistic? Howson thinks so:

> An explicitly probabilistic rendering of the NMA is not only natural: it is arguably implicit in the use of the word 'miracle', with its connotation of an event whose occurrence, if not due to the contextually specified agency, would be remotely improbable. (2013, 205)

Dictionaries certainly list this connotation as *one* meaning of 'miracle'. But they also list, and tend to list first, a different meaning: a miracle is *an event that is not explicable by natural or scientific laws and is therefore attributed to a divine agent*. And that, I suggest, is what the friends of NMA have in mind. The point of their "no-miracles" rhetoric is that realism is the only possible *natural* or *scientific* explanation of success.[7]

So, *prima facie*, NMA does not have the probabilistic form of the Medical Analogy. Still, it surely has the following *probabilistic consequence*:

(P). The likelihood of a successful well-established current theory being true is very high.

Now, one might well doubt that presentations of NMA give (P) the support it needs. This amounts to doubting that NMA, as presented, is a *good* abduction. And I do doubt it, as indicated

[7] I emphasize that I am not endorsing this claim; see section 1.

(section 1). And it is reasonable to wonder whether NMA rests (P) on some probabilistic assumptions and so is, implicitly, open to a probabilistic interpretation. I shall remain neutral on that issue. But NMA's *enemies* should not suppose, apparently without evidence, that NMA's friends have in mind to support (P) with the fallacious argument captured by the Medical Analogy. For, as we shall now see, NMA is crucially *dis*analogous to the medical argument.

To see what is wrong with the Medical Analogy let us start with what Magnus and Callender point out: in answering the Harvard Test question we must consider the population[8] from which this patient was drawn (2004, 325). Now that population is implicitly the current population of *people in general*. If the population in NMA was the current population of *theories in general*, the Medical Analogy might hold. *But that is not the population.* NMA is an argument for a doctrine like SR. As I emphasized in section 2, SR is committed to the truth of most theories in a quite restricted set of current theories that I used the expression "well-established" to briefly identify. The realist is not committed to the likely truth of maverick theories, of theories which scientific practice itself treats instrumentally, of speculative theories at the frontiers of science, or of other theories about which science has not yet made up its mind. This shows that the Medical Analogy is quite mistaken.

First, the Analogy's question about the likelihood of a successful current theory being true is not the concern of NMA. The likelihood that matters to NMA is (P), the very high likelihood of a successful *well-established* (in that sense) current theory being true. And, being well-established is a deal more demanding than just being successful.[9]

Second, according to a crucial premise of the Analogy's argument, T2, the likelihood that a false current theory will be successful is only 5%. In Menke's words, "it is exceedingly unlikely that a false theory makes successful predictions" (2014: 103). Yet NMA need have no commitment to this low rate of false positives. NMA is committed to (P) and hence to it being exceedingly unlikely that a successful well-established current theory is false. So it is committed to one version of "if successful, then probably not false". It is not committed to *any* version of "the reverse", "if false, then probably not successful", whether about theories in general, T2, or about some restricted class of theories like the well-established ones. (We shall consider NMA's attitude to the false-positive rate of the well-established ones in a moment.) Even if, contrary to T2, the false-positive rate of theories in general was high, and the proportion of theories that are false was also high, that false-positive rate could not be brought to bear against NMA.

[8] They actually say "sample" not "population" but I assume that this is a slip.

[9] In my view (1984, 2005, forthcoming), being well-established (in that sense) requires that the theory be the conclusion of a good scientific abduction.

Third, NMA is not concerned with the base rate of truth among current theories in general, allegedly neglected by NMA in answering the Analogy's question. That base rate is irrelevant to NMA.[10] As it stands, the Medical Analogy fails.[11]

Could the Analogy be saved by revising its argument to apply to the more restricted set of well-established theories? Let us try. We could replace T1 and T2 in the Analogy's representation of NMA with:

T1(r). If a *well-established* current theory is true, then it is 100% likely to be successful.
T2(r). If a *well-established* current theory is not true, then it is 5% likely to be successful.

And we could replace the Analogy's original question with: If a *well-established* current theory is successful, what is the likelihood that it is true? This revised representation of NMA cannot be right either and the Medical Analogy still fails.

First, we have already noted that NMA is not committed to any version of "if false, then probably not successful" and T2(r) is a version. Indeed, everyone should accept, and realists surely do, that *any* well-established theory, *whether true or false*, is successful. For, any theory that wasn't successful wouldn't *be* well-established (in the sense specified)![12] Note that Larry Laudan, a famous critic of NMA, doesn't just accept this, he *emphasizes* it in pointing out that every false theory on his famous list, "which could be extended *ad nauseam*,…was once successful" (1981:

[10] So, contrary to what Lipton suggests, it is appropriate that "we ignore…the base rate that…the vast majority of theories are false" (2004, 197). NMA is not about the general population of theories.

[11] My attention has recently been drawn to a paper by Leah Henderson (2015) which makes much the same point. She argues that "the base rate fallacy allegation relies on an assumption of random sampling of individuals from the population which cannot be made in the case of the no miracles argument" (p. 1). She supports the point persuasively by saying this about the person who tested positive in the Harvard Test:

> we have assumed that the individual in question was randomly drawn from the population as a whole. But consider what would happen if this were not true. Suppose instead that the individual were sent for the medical test by the doctor after presenting various symptoms of the disease. If the sampling process is such that those being sent for test are more likely to have the disease, then it is no longer appropriate to take the prior to be the base rate in the overall population. And in this case, it need not be so unlikely that the person does have the disease, if he tests positive. (p. 5)

The difference between sampling from symptomatic people rather than from people in general is analogous to the difference I am emphasizing between sampling from well-established theories rather than from theories in general.

[12] NMA does not, of course, aim to explain why well-established theories are successful but why any particular one of them tends to be successful; see Lipton 2004, 193-5.

33).[13] So *T2(r)'s false-positive rate of 5% should be replaced by one of 100%*! And because of this, importantly, that rate is *obviously irrelevant* to the assessment of NMA.

Second, the Medical Analogy's revised question about successful well-established current theories, unlike the original question about successful current theories in general, certainly concerns NMA. But the base rate needed to answer the revised question - the rate of true theories among well-established current theories - *is certainly not neglected*. Given that all those theories, whether true or false, are successful (see above), that rate *simply is* the very base rate that the Analogy's revised question is about and that NMA answers with (P)! Far from being neglected by NMA, that base rate is the center of attention. And there is no other base rate that is both neglected and relevant. In brief, NMA does not *neglect* this base-rate in *arguing for* its thesis, the base-rate *is* the thesis.

I emphasize that I have said nothing to assuage the concern, which I share, that the key probability claim of NMA, (P), needs support that it has not been given. What I do hope to have shown is that *it is not appropriate to construe NMA as supporting (P) with an argument that commits the base-rate fallacy*. The original Medical Analogy deals with the wrong population and is simply irrelevant to (P). I think that our attempt to revise the Analogy shows that there is no argument for (P) that is both plausibly a version of NMA and analogous to the medical argument. The friends of NMA should not be seen as embracing such an argument but rather as having simply failed to appreciate adequately the weakness of NMA and hence (P)'s need of further support. That weakness is a rather subtle matter, it seems to me (and that is my excuse, as a former friend, for having overlooked it in earlier writings; for example, in 1984, 2005). The base-rate objection mislocates NMA's problem. The false Medical Analogy is a distraction from whatever weakness NMA may have.

In brief, it is one thing to charge the friends of NMA with failing to support (P), it is another to charge them with (implicitly) supporting (P) by the fallacious argument of the Medical analogy. I think, but have not argued, that the first charge is right. I have argued that the second is quite wrong.

Magnus and Callender's own discussion gives their game away (and partly inspired my discussion). They contemplate that the realist might respond to the base-rate objection by putting a restriction with some similarity to mine on the relevant set of theories: the realist might restrict "the set to theories *actually professed* by our mature sciences". They claim that T1 would then "hold trivially" and T2 "would simply be false, since almost [all theories in the set] would be successful, whether true or not." So Magnus and Callender have assembled all the elements for a realist refutation of the base-rate objection. But they see this realist response as more a matter of cheating

[13] So Magnus and Callender have gotten Laudan wrong. As they point out, Laudan thinks that many successful theories "proved in the fullness of time not to be true". But Magnus and Callender continue: "in effect, Laudan is chipping away at" the low false-positive rate (as in T2(r)), "recommending a higher value" (2004, 323). A low value for this rate is obviously mistaken and doesn't need to be chipped away at. Laudan is actually chipping away at something very different: the high probability that NMA assigns to a successful theory being approximately true.

than refuting: "The realist would thus avoid base rate neglect, but at the cost of sample selection bias" (2004, 326). There is no bias in restricting the sample to the well-established theories. For, NMA's commitment is to the high probability of *those* successful theories, not any other successful theories, being true.[14]

We can demonstrate the failure of the Medical Analogy by making a better one, "the Culinary Analogy". Consider the chefs at Michelin 3-star restaurants. Obviously they are all successful. The thesis is proposed that most of these 3-star chefs have been trained at premier cooking schools on a designated list, *L*. An argument for this thesis is presented: the *success* of most of these 3-star chefs (though not necessarily that of any other chefs) is best explained by their being *L*-trained. This argument is analogous to NMA's argument that the success of most well-established current theories (though not necessarily that of any other theories) is best explained by their being true.

Now consider the Culinary Analogy's argument. If the Medical Analogy were right and NMA had, implicitly at least, the form of the fallacious argument in the Harvard Test, then so too should the Culinary argument. Yet the Culinary argument manifestly does not have that form.

First, the Culinary argument would not be accompanied by a claim about a false positive rate. As with NMA, the false-positive rate is 100% - all those chefs are successful, whatever their training – and so that rate is obviously irrelevant to the argument. And so too one might add, mimicking the earlier discussion of T2, is the false-positive rate of success among not-*L*-trained chefs in general. For, the argument is not concerned with the success of chefs *in general* but with that of *3-star* chefs.

Second, the Culinary argument obviously does not arise from neglecting the base rate of *L*-trained chefs in the set of chefs. That rate is as irrelevant to the argument as is the rate of true theories in the set of current theories to NMA. Just as NMA is concerned with *well-established* theories not theories in general, the Culinary argument is, to repeat, concerned with *3-star* chefs not chefs in general. So, what about the base rate of *L*-trained chefs in the set of 3-star chefs? That rate is relevant to the argument but it is no more neglected than is the base rate of truth among NMA's well-established current theories: it is central to what is at issue.

Now there is, of course, a disanalogy between the *L*-training of 3-star chefs and the truth of well-established current theories. Whereas we have a simple test for the *L*-training of 3-star chefs – just check their resumes – and so have no need for the likes of the Culinary Analogy's argument to support the thesis that most of those chefs are *L*-trained, we do not have a simple test for the truth of well-established current theories and so do need the likes of NMA to support the view that most of those theories are true. But this difference is beside the point. Even though

[14] Magnus and Callender, following Lewis 2001, also represent the pessimistic meta-induction against realism as committing the base-rate fallacy. Perhaps this is one plausible representation. In any case, in my view, the main realist response to the meta-induction should be that the indubitable improvement in our scientific methodology over recent centuries (partly as a result of improved technology) provides a good empirical reason for resisting the meta-induction's inference from the past to the present (1984, 162-5; 2005, 89-90; 2010, 96-8).

the Culinary argument is not needed it *is*, nonetheless, an argument for its thesis about 3-star chefs that is analogous to NMA's argument for its thesis about well-established theories: the Culinary Analogy is good. And the point is this. We have just seen that it would be a serious misrepresentation of the Culinary Analogy's argument to take it to have the form of the fallacious argument of the Harvard Test. So too is it a serious misrepresentation of NMA to take it to have that form. And that misrepresentation of NMA *is* the Medical Analogy. Perhaps both these explanationist arguments should be given probabilistic reconstructions – I take no stand on this – but the form of probabilistic argument in the medical case is simply irrelevant to both the Culinary argument and NMA.

My rejection of the Medical Analogy and embrace of the Culinary Analogy includes claims about the irrelevance of many probabilities. It may help to demonstrate these irrelevancies using a diagram (Figure 1). (It certainly helps me!) The diagram has boxes for five different categories of current theories, each box labeled in the right-hand top corner with a letter from **A** to **E**. The abbreviations for the contents of the boxes are obvious: '~' for 'not'; 'T' for 'true'; 'S' for 'successful'; 'WE' for 'well-established'. On the Culinary Analogy, the diagram would capture probabilities for chefs if we were to substitute '*L*-trained chef' for 'true' and '3-star' for 'well-established'.

Now it matters to NMA (the Culinary argument) that the probability of a successful well-established current theory being true (of a successful 3-star chef being *L*-trained) is high; that's (P). This probability is captured in the diagram: D >> C. The diagram also demonstrates that none of the following probabilities matters to NMA (the Culinary argument):

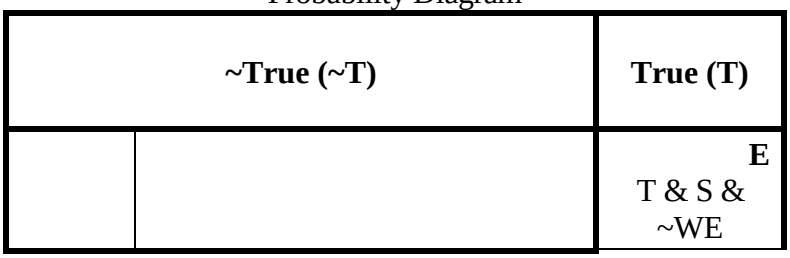
(i) The 100% (false-positive) rate of success among not-true well-established current theories (not-*L*-trained 3-star chefs): C is the box for those theories (chefs) and there is no box for unsuccessful well-established theories (unsuccessful 3-star chefs) because there are none.

(ii) The high (false-positive) rate of success among not-true theories (not-*L*-trained chefs): B+C >> D+E.

(iii) The low base rate of truth (*L*-training) among theories (chefs): D+E << A+B+C.

(i) to (iii) are simply irrelevant to NMA (the Culinary argument).

Probability Diagram



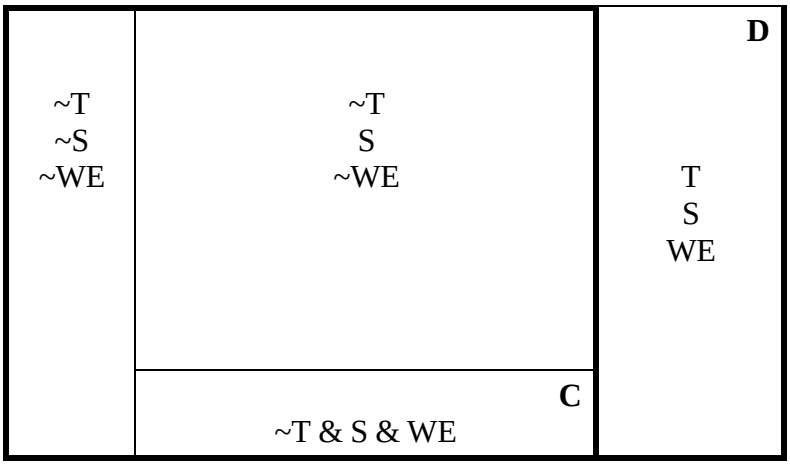


Figure 1

5. Conclusion

The "No Miracles Argument" (NMA) for scientific realism has been charged with "neglecting the base rate" and hence with being fallacious. I have argued that NMA is not guilty of this neglect. The base-rate objection overlooks that NMA is not defending a realism about current theories in general but a realism about a quite restricted group of current theories, the "well-established" ones. The objection inappropriately foists a fallacious argument on the realist that is in fact irrelevant to NMA. NMA may not be a good argument for realism - indeed I think that it is not. And perhaps it is open to *some* probabilistic interpretation. But it is not open to the interpretation of the Medical Analogy. So it is not obviously fallacious in the way assumed by the base-rate objection. That objection is a distraction from any real failings that NMA may have.[15]

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REFERENCES

Chakravarrty, Anyan (2014), "Scientific Realism", in Edward N. Zalta (ed.), *The Stanford Encyclopedia of Philosophy* (Spring 2014 edition) URL = <http://plato.stanford.edu/archives/spr2014/entries/scientific-realism/>

Devitt, Michael (1984), *Realism and Truth*, Oxford: Basil Blackwell. 2nd rev. (1991). Repr. with new Afterword, Princeton, NJ: Princeton University Press (1997). (Page references are to 1997.)

___ (2005), "Scientific Realism", in Frank Jackson and Michael Smith, (eds.), *The Oxford Handbook of Contemporary Philosophy*. Oxford: Oxford University Press: 767-91. Reprinted with Postscript in Devitt 2010, 67-98. (Page references are to 2010.)

___ (2010), *Putting Metaphysics First: Essays on Metaphysics and Epistemology*. Oxford: Oxford University Press.

___ (Forthcoming), "An Ignored Argument for Scientific Realism".

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Gardner, Michael R (1979), "Realism and Instrumentalism in 19th-Century Atomism". *Philosophy of Science* 46: 1-34.
Glymour, Clark (1984), "Explanation and Realism", in Leplin 1984, 173-92.
Harker, David (2010), "Two Arguments for Scientific Realism Unified", *Studies in History and Philosophy of Science* 4: 192–202.
Henderson, Leah (2015), "The No Miracles Argument and the Base Rate Fallacy". *Synthese* DOI 10.1007/s11229-015-0995-7
Howson, Colin (2000), *Hume's Problem: Induction and the Justification of Belief*. Oxford: Oxford University Press.
___ (2013), "Exhuming the No-Miracles Argument", *Analysis* 73: 205–211.
Kukla, Andre (1998), *Studies in Scientific Realism*. New York: Oxford University Press.
Laudan, Larry (1981), "A Confutation of Convergent Realism", *Philosophy of Science* 48, 19-49.
Leplin, Jarrett (ed.) (1984), *Scientific Realism*. Berkeley: University of California Press.
Lewis, Peter (2001), ''Why the Pessimistic Induction is a Fallacy'', *Synthese* 129: 371–380.
Lipton, Peter (2004), *Inference to the Best Explanation*, 2nd edition. London: Routledge.
McMullin, Ernan (1984), "A Case for Scientific Realism", in Leplin 1984, 8-40.
Magnus, P.D., and Craig Callender (2004), "Realist Ennui and the Base Rate Fallacy", *Philosophy of Science* 71: 320–338.
Menke, Cornelis. (2014), "Does the miracle argument embody a base rate fallacy?", *Studies in History and Philosophy of Science* 45: 103-08.
Musgrave, Alan (1988), "The Ultimate Argument for Scientific Realism", in Robert Nola (ed.), *Relativism and Realism in Sciences*. Dordrecht: Kluwer Academic Press, 229–252.
Psillos, S. (1999), *Scientific Realism: How Science Tracks Truth*. London: Routledge.
___. (2006), "Thinking about the Ultimate Argument for Realism", in C. Cheyne and J. Worrall (eds.), *Rationality and Reality: Conversations with Alan Musgrave*. Dordrecht: Springer 133–156.
Putnam, Hilary (1975), *Mind, Language and Reality: Philosophical Papers, vol. 2*. Cambridge: Cambridge University Press.
___ (1978), *Meaning and the Moral Sciences*. London: Routledge & Kegan Paul.
Salmon, Wesley C. (1984), *Scientific Explanation and the Causal Structure of the World*. Princeton: Princeton University Press.
Smart, J. J. C. (1979), "Difficulties for Realism in the Philosophy of Science". In J. J. Cohen, J. Los, H. Pfeiffer, and K. P. Podewsky (eds.), *Logic, Methodology and Philosophy of Science VI*. Amsterdam: North-Holland Publishing Company, 363-75.