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Against Restricted Methodological Naturalism

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Restricted methodological naturalism (RMN) is the stipulation that, within the epistemic domain of scientific inquiry, if x explains y, then x is natural *(that is,* not supernatural*). In this article, I argue that RMN cannot do its main job as a stipulation, namely, the job of guarding against present and future "dead-end" explanatory paradigms and encouraging consideration of promising novel explanations. RMN cannot do this job because of two well-known problems: what I call the* intelligibility *and* truth-seeking *problems. My argument proceeds in three basic stages: (1) a brief rundown of RMN, in the interest of clarifying the sort of position I have in mind; (2) a recasting of the intelligibility and truth-seeking problems for RMN in case studies from historical and present science; and finally (3) concluding remarks about the staying power of these problems in light of recent work on the intellectual history of naturalism.*

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There is a special prestige afforded to explanations that are reasonably regarded as scientific, and for good reason. The progress of scientific institutions, practices, methodologies, and theories are among the finest achievements of our species' history. We therefore have a special interest in clarifying the conditions under which this progress was achieved, and indeed those under which it might continue in present and future science. The development of methodological naturalism (MN) is one particularly well-regarded attempt to contribute to this venerable, wide-ranging project of delineating the past, present, and future boundaries of science, specifically with respect to the task of *explanation*. But this regard is misplaced—or so I will argue here.

Broadly construed, methodological naturalism (MN) is "the stipulation within a domain of study to offer explanations only in terms of natural phenomena."[1] That is, within MN, for any phenomena *x* and *y* in a given epistemic domain, if *x* explains *y*, then *x* is natural. The doctrine can be stated negatively as well, with reference to its contrary: for any phenomena *x* and *y* in a given epistemic domain, if *x* explains *y*, then *x* is *not* supernatural.

There is a large and growing literature in the philosophy of science on the nature and viability of MN as a stipulation. The distinction between MN and *metaphysical* naturalism is significant, for example, since metaphysical naturalism is (roughly) the view that only natural things *exist*. There is no straightforward logical connection between MN and metaphysical naturalism, given (among other things) the possibility of anti-realist accounts of explanation, although there is considerable debate about the relationship of the former to the latter.[2]

Variations of MN are "strong" or "unrestricted" in direct proportion to the size of

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the explanatory domain covered by the stipulation. So, for example, a purely unrestricted methodological naturalism (UMN) is a stipulation governing *all* forms of explanation, whereas "weak" or "restricted" versions of MN govern only certain *sorts* of explanatory enterprises (e.g., *scientific* explanations, in particular).

This article concerns one version of restricted methodological naturalism (RMN): the stipulation that, within the epistemic domain of *scientific inquiry*, if *x* explains *y*, then *x* is natural (that is, not supernatural). I argue that RMN cannot do its main job as a heuristic, namely, the job of (a) guarding against present and future "dead-end" explanatory paradigms, and (b) encouraging consideration of promising novel explanations. The reason RMN cannot do this job is because of two well-known problems: what I call the (1) intelligibility and (2) truth-seeking problems, respectively.[3] While these problems for all forms of naturalism are well known, I sharpen them by reframing them in light of the above-mentioned task of RMN as a heuristic for scientific inquiry. Specifically, I argue that RMN has an intelligibility problem if it is only as heuristically informative as an obviously fatuous alternative—what I call good theory-ism (GT), the stipulation within the domain of scientific inquiry to offer only "good" explanations. Correlatively, I argue that RMN has a truth-seeking problem if there is reason to think that it encourages dead-end explanatory paradigms or serves as an obstacle in the way of promising novel explanations.

My argument proceeds in three basic stages: (1) a brief rundown of different versions of MN, in the interest of clarifying the sort of position I have in mind; (2) a recasting of the intelligibility and truth-seeking problems for RMN in light of pertinent examples in historical and present science; and (3) some final remarks about why the staying power of these problems shouldn't surprise us in light of recent work on the intellectual history of naturalism.

Ultimately, I argue that RMN faces a dilemma: either it is just as uninformative as good theory-ism, or it is a potential hindrance to scientific progress. And if this is right, of course, then RMN should be rejected.

What Is Restricted Methodological Naturalism?

The literature on MN is large and unwieldy, and so it is not easy to say exactly what the view amounts to. While adherents all subscribe to the preference for natural rather than supernatural explanations as previously stated, it is not at all clear what the precise claims are, how they are justified, or what is implied epistemically. Miles Donahue has done a great service to this debate by identifying two relevant categorical distinctions: *intrinsic* (vs. *provisional*) MN;[4] and *restricted* (vs. *unrestricted)* MN.[5] The former distinction features answers to the question of whether MN is an *essential* feature of scientific explanation—past, present, and future (intrinsic)—or instead whether MN simply *happens to have been* a useful stipulation in the past, even if defeasible (provisional). But it is the latter distinction (restricted vs. unrestricted) that interests me here, since it is the particular, restricted domain of *scientific* explanation that I want to consider.

My concern here is to present a renewed case against all forms of RMN. To the extent that the present argument is successful, I will have identified some core reasons to be suspicious of RMN as a heuristic for past, present, and future scientific explanation.

The Intelligibility Problem

As nearly everyone contributing to this literature realizes, RMN as a stipulation requires at least some accounting for the distinction between natural and supernatural explanations. Obviously, if we are going to limit our consideration to natural explanatory posits when doing science, we need to know what the terms "natural" and "supernatural" mean in the relevant contexts. But just how intelligible do these terms have to be, exactly?

Responses to the intelligibility problem

Some have tried to offer definitional accounts. For example, Peter van Inwagen and David Armstrong offer "object-based" definitional accounts of the term "natural" in terms of that which is contained in or constituted by space and time in some way.[6] Others offer "theory-based" definitional accounts in terms of what is postulated by standardized theories in well-regarded scientific enterprises such as contemporary physics.[7]

But there is a well-known problem with definitional accounts that is captured in "Hempel's dilemma"—a notorious one for naturalism and its close relatives, physicalism or materialism:[8]

> If [the natural] is defined via reference to contemporary physics, then it is false—after all, who thinks that contemporary physics is complete?—but if [the natural] is defined via reference to a future or ideal physics, then it is trivial—after all, who can predict what a future physics contains?[9]

This formulation of Hempel's dilemma seems to apply both to object- and theory-based definitions of "natural." In the case of object-based definitions, any account of a unified spatiotemporal domain will require specific content to be meaningful.[10] But the principal sources of this content—that is, contemporary physical theories that are surely at least partially false[11]—countenance explanatory phenomena that are either mischaracterized or fail to exist altogether. In the case of theory-based definitions, the problem is even more straightforward, since (again) what counts as a well-regarded physical theory today could be consigned to the dustbin of scientific history tomorrow.[12] If what we seek is a meaning of the term "natural" that could be useful for the purposes of MN as a stipulation, then these accounts leave much to be desired.

Indeed, the problems posed by Hempel's dilemma for purported accounts of the natural-supernatural distinction have driven many, if not most, recent contributors to this literature to abandon a definitional approach entirely.[13] This is the reason that others adopt a non-definitional approach to making the distinction intelligible. In the non-definitional approach, positive definitions of "natural" are elided in favor of informative contrasts with certain paradigmatically "supernatural" phenomena, such as irreducible or otherwise fundamental mental phenomena,[14] entities of popular mythology, or God.[15] In this non-definitional strategy, natural phenomena are precisely those that are *not* these paradigmatically supernatural entities. Graham Oppy offers one such approach:

> Suppose I give you the following list: angels, animals, atoms, cells, demons, fairies, galaxies, ghosts, goblins, gods, mermaids, molecules, planets, plants, protons, quarks, stars, vampires, werewolves, and zombies. If I ask you to sort these into the natural and the non-natural, …, you will have no trouble producing exactly the same list that other competent ordinary language users produce. This shared competence is all that we need to appeal to in order to justify the claim that the notion of natural reality has substantive content.[16]

For Oppy and other non-definitional strategists, naturalism need not demand more intelligibility from the term "natural" than such exercises of intuitive competence evidence. Even if we can't spell out the differences in a precise and general way, we know that animals, atoms, and the like are natural, since they are obviously *not* like supernatural things such as goblins and mermaids in the relevant respects. And if we can reliably distinguish these things in this intuitive way, then we have all the intelligibility we need to fund RMN as a heuristic for epistemic ventures such as scientific theorizing. But is this enough?[17]

Good theory-ism and RMN

Unlike metaphysical naturalism, MN does not need to posit that only natural phenomena *exist*; it simply needs to stipulate a limit on what sorts of *explanatory posits* are licit in a given epistemic venture. And, unlike UMN, RMN does not need to stipulate any limitations on non-scientific explanatory posits. What RMN *does* need to do is provide adequate heuristic guidance as a stipulation for properly *scientific* explanatory enterprises.[18] If it cannot even do that, then it is not clear what work it would do as a stipulation. And if this is right, then we have good reason to demand as much intelligibility from the term "natural" as this task requires. My claim, then, is that RMN cannot deliver on this particular task. Here is one way to think about it.

Consider good theory-ism (GT). Like RMN, GT is a stipulation about what kinds of explanatory posits are licit in any given scientific enterprise. GT is the stipulation that, for any x and y in the domain of scientific theorizing, if x explains y, then x is a good explanation. Now, you might ask, what makes an explanation "good"? It is a simple enough matter for apologists of GT: although we can't give necessary and sufficient conditions in a general way, in GT good explanations are made intelligible by contrast with bad explanations, and there are certain paradigmatic cases of each that we can all recognize. Suppose you are given the following list of explanatory posits: phlogiston, kinetic energy, quarks, luminiferous aether, and others. If *I* ask you to sort these into good and bad explanations, you will have no trouble producing exactly the same list as others who happen to be familiar with the relevant terms. This shared competence—reason our GT apologists—is all that we need to justify the claim that the notion of "good explanation" has substantive content.[19]

Obviously, this shouldn't satisfy those who are interested in the task of developing a unifying heuristic for scientific inquiry. GT is fatuous, since it provides almost no such heuristic value. And while this result is not surprising given that GT is a joke rather than a first-order theory with apologists, what I want to suggest is that the above parallel provides a clear and reasonable standard by which we can operationalize the intelligibility problem for RMN. That is, we should demand significantly more content from the terms "natural" and "supernatural" in RMN than are provided by "good" and "bad" in GT. Put another way, if a stipulation is

going to provide heuristic guidance for what sorts of explanatory posits are licit in scientific theorizing, then it should do a better job than GT.

But can RMN meet this standard? The above parallel suggests that it does not, *prima facie*. And this is true even if, at some very trivial level, we should all be advocates of GT. Neither (non-definitional) RMN nor GT says anything about cases that are not explicitly mentioned as paradigmatic, and thus it is unclear why the undefined intuition behind the natural–supernatural distinction in RMN would somehow generalize better than its good–bad counterpart in GT. That is, it is not clear that the RMN stipulation would outperform GT when it comes to guarding against future pseudoscientific explanatory posits or encouraging consideration of promising novel explanations.[20] But, this is precisely the work that such a stipulation is supposed to do.

Now a defender of RMN might reply as follows:[21] even if non-definitional accounts like Oppy's can appeal only to paradigm cases when it comes to the term "natural," this is no problem for RMN as a stipulation, since we understand the meaning of certain terms via paradigm cases all the time—even in stipulations. After all, nobody bats an eye when it comes to stipulations such as "do the right thing" or "believe the best theory," even when it would be difficult or impossible to come up with necessary and sufficient conditions for terms such as "right" and "best." Indeed, comparatively respectable stipulations such as "inference to the best explanation" (IBE) are closer to GT than it might seem at first glance. After all, what is IBE other than a stipulative preference for the *best* (and, therefore, for good) explanations available?

In response, we should note, of course, that this is true: we use terms we can't define all the time, and we do so reasonably well. When someone says that we should "do the right thing," we obviously don't think that this is a *totally* unintelligible thing to say. But it seems equally obvious that such stipulations are not going to offer much in the way of informative, heuristic value for our moral decision-making. The same seems to be true for Oppy's non-definitional RMN account—it is not *totally* unintelligible, but if it purports to be a unifying heuristic for present and future scientific explanation, we are going to need more than just a handful of paradigm cases to do the relevant job, namely, the aforementioned job of (a) guarding against present and future "dead-end" explanatory paradigms, and (b) encouraging consideration of promising novel explanations.

Further, it is important to note that comparatively well-regarded IBE accounts of scientific explanation are not similar to RMN (or even GT!) in this respect: serious developments of IBE appeal to *appropriately general* virtues of scientific explanation such as empirical adequacy, consistency, unification, fertility, and so forth when it comes to lending intelligibility to terms such as "better" and "worse."[22] Such general features are precisely what is missing in non-definitional versions of RMN and precisely what is needed for the relevant heuristic task of the stipulation.

It might also be argued that even if the respective intuitions behind the laundry lists of RMN and GT do not *immediately* seem to differ notably in terms of their heuristic value, RMN enjoys an advantage over GT at least in the sense that the extension of its critical term ("natural") is likely *narrower* than that of GT ("good"). After all, at least in terms of logical possibility, the set of "good" explanatory posits is surely much, much larger than the set of "natural" explanatory posits. And, *ceteris paribus*, a narrower range of logically possible candidate explanations is more useful for heuristic purposes than a broader one. Therefore, at least in this sense, RMN might outperform GT in terms of intelligibility.

But it is difficult to know how to offer a rejoinder to this point in the absence of something *like* a definition or explication[23] of the term "natural." While it might be true that "natural" has a narrower extension than "good," it is not obvious why this narrowness would be especially relevant to the heuristic value of RMN over GT. Indeed, even if we agree for the sake of argument that it *is* relevant, nothing seems to foreclose the possibility that RMN has *barely* more heuristic value than GT. This would hardly be a ringing endorsement for RMN, given that GT is more obviously fatuous.

The intelligibility problem might be enough to call into question the viability of RMN as a stipulation for scientific inquiry, but the case against it builds ever more ominously when it is considered together with another well-known problem for RMN: the truth-seeking problem.

The Truth-Seeking Problem

The truth-seeking problem for RMN pertains not to the intelligibility of its critical terms, but rather its ability to be an aid to scientific progress. Truth-seeking is a *problem* for MN to the extent that the stipulative limiting to natural explanatory posits in the sciences either (a) fails to ward off pseudoscientific or otherwise unscientific

explanatory posits, or (b) closes off consideration of promising novel explanatory posits. As Bradley Monton puts it,

> If science really is permanently committed to methodological naturalism, it follows that the aim of science is not generating true theories. Instead, the aim of science would be something like: generating the best theories that can be formulated subject to the restriction that the theories are naturalistic … I maintain that science is better off without being shackled by methodological naturalism.[24]

In other words, if RMN hurts scientific progress more than it helps,[25] then it is not clear why we should affirm it. Science should be more interested in understanding the world than being acceptably "naturalist."

Responses to the truth-seeking problem

Donahue argues that RMN avoids the truth-seeking problem, since its specified epistemic domain is that of scientific investigation, and "one can only determine the *range* of scientific investigation on the basis of *a posteriori* considerations."[26] The idea is that when we appropriately delimit the epistemic domain of scientific inquiry, it becomes much easier to defend RMN as a stipulation. For example, cosmic fine-tuning is a traditional area of inquiry that regularly features "supernatural" explanatory posits that many take to deserve serious consideration, if not outright adoption.[27] While advocates of UMN have trouble accommodating such a situation without significant revisionist redescription,[28] RMN offers a plausible, accommodative account by acknowledging that such areas of inquiry feature (legitimate) questions that simply go beyond what science can deal with. In RMN, the stipulative limit to natural explanatory posits remains on such a view, but only within the appropriately humble remit of science. Indeed, as John Perry and Sarah Lane Ritchie assure us,

> a scientist will not … invent a naturalistic explanation in order to preserve an overarching metaphysical [or methodological, presumably] naturalist worldview. A good scientist understands that "I don't know" or "I don't know yet" is always a valid answer.[29]

At least one problem with this approach is that it requires some accounting for the remit of scientific inquiry—a task that is difficult (if not impossible) to accomplish in advance of the progress of science.[30] Indeed, a significant part of the success story of the modern sciences is precisely the (unanticipated) *expansion* of its remit, and so any sort of stipulation that requires knowledge of preset boundaries is likely to run up against a different version of the truth-seeking problem—one that arises because of some over- (or under-) confidence about the remit of the sciences.[31]

But there is a more pressing problem with RMN: we have clear examples from the history of science to suggest that relevantly similar stipulations fail to overcome the truth-seeking problem.

Newtonian gravitation and Quinean behaviorism: Case studies of the truth-seeking problem

Perhaps the most obvious problem—and the one that is often cited in the literature on naturalism[32]—concerns the stipulations of the so-called "mechanical philosophy" of the 17th century with respect to the development of Newton's theory of gravitation. According to influential authors representing this prevailing mechanical philosophy,[33] it is impossible for two spatially separated bodies to act causally on one another "at a distance"—that is, without some sort of mediating matter. Yet, the most natural interpretation of Newtonian gravity is that it is a "force" that enables precisely this sort of action at a distance. For example, it is very difficult to make sense of Newton's law of universal gravitation—that every massive object attracts every other massive object in the universe with a force that is proportional to the product of their masses and inversely proportional to the square of the distance between their centers of mass—without the possibility of action at a distance.

As it happens, this very discrepancy between the "mechanistic" stipulation on causal explanation proved to be an important objection to Newton's theory. Andrew Janiak summarizes the situation as follows:

> The mechanist criticisms of the *Principia* are legion and involve numerous philosophical issues. One especially subtle and deep criticism, raised by Leibniz among others, highlights the question of whether forces such as gravity exist. It does so by suggesting that Newton's treatment of gravity saddles him with a dilemma. If Newton contends that gravity exists, he must admit that material bodies act on one another at a distance, thereby violating a crucial norm of the mechanical philosophy (in all its guises). However, if Newton seeks to avoid action at a distance, as all of his contemporaries do, he must admit that gravity does not exist, and that he has therefore failed to discover the cause of the previously disparate celestial and terrestrial phenomena unified by his gravitational theory.[34]

It seems difficult to avoid the implication that this mechanist objection to Newton's theory is precisely an example of a stipulation that is guilty of closing off consideration of a promising novel explanatory posit (in this case, gravity as a fundamental force). Indeed, Newton *himself* rejects action at a distance many times throughout his writing career, creating a rather awkward relationship with his own theory.[35]

But even if we agree that this *a priori* stipulation on causal explanation in the mechanical philosophy turned out to get in the way of scientific progress, its connection to contemporary versions of naturalism might not be obvious. After all, just because one stipulation on explanatory posits (that is, the mechanical philosophy) had a truth-seeking problem at a critical moment in history of scientific progress, it doesn't mean that naturalism must also have a truth-seeking problem.

The point is well taken, as it is clearly true that naturalism and the mechanical philosophy are not identical stipulations. But identity is surely too high a bar when it comes to looking to the history of science for such lessons. After all, contemporary naturalism is *contemporary*, not historical. What we should be looking for instead are *relevant similarities* between past stipulations and contemporary naturalism. If the mechanical philosophy's rejection of the possibility of action at a distance is relevantly similar to contemporary naturalism, then we are in a better position to make judgments about the quality of the latter as a heuristic for scientific inquiry.

And it certainly seems that the mechanical philosophy's rejection of action at a distance is, in fact, relevantly similar to contemporary versions of naturalism. After all, as others have pointed out, the rejection of action at a distance was understood to be a rejection of what we today would call "spooky" physical forces and other explanatory posits—one of the key motivations of naturalism in all of its varieties.[36] Nowhere is the point clearer than in a now-famous letter from Leibniz to Samuel Clarke, which is worth quoting at length:

> But then what does [Newton] mean when he will have the sun attract the globe of the earth through an empty space? Is it God himself that performs it? But this would be a miracle if ever there was any. This would surely exceed the powers of creatures … That means of communication (he says) is invisible, intangible, not mechanical. He might as well have added inexplicable, unintelligible, precarious, groundless, and unprecedented. But it is regular (the author says), it is constant, and consequently natural. I answer that it cannot be regular without being reasonable, nor natural unless it can be explained by the natures of creatures. If the means which causes an attraction properly so called are constant and at the same time inexplicable by the powers of creatures, and yet are true, it must be a perpetual miracle, and if it is not miraculous, it is false.[37]

Now admittedly this is just one letter, but surely it captures something like the spirit of the discrepancy between the mechanical philosophy's account of causal explanation and Newtonian gravitation. Because it is not "mechanical" (that is, it offers no account of local causation), Leibniz reasons, Newtonian gravity is a "perpetual miracle" beyond the scope of intelligible causes. Indeed, remarkably for our purposes, it is *supernatural* in the sense that it lies beyond all possible creaturely natures to explain.

If Leibniz's objection to Newton's theory is not an example of a stipulation that is relevantly similar to contemporary naturalism in the history of science, then it is not clear what would count as such. Had the mechanical philosophy prevailed in its rejection of "spooky" explanatory posits, the progress of science would have suffered. In short, there was a truth-seeking problem with the mechanical philosophy—and for reasons that are suspiciously similar to contemporary naturalism.

But refusing promising novel explanations is only one way to be a bad heuristic for scientific progress. It is also possible that stipulations on explanatory posits could encourage a more positive bias toward flawed theoretical paradigms. Are there examples from the history of science that are guilty on this count and relevantly similar to RMN? Once again, the affirmative position seems inviting.

Although perhaps less striking than the case of the mechanical philosophy and Newtonian gravitation, the late nineteenth and early to mid-twentieth century paradigm of behaviorism in psychology is arguably another cautionary tale when it comes to the truth-seeking problem. Indeed, the motivations behind behaviorism are arguably even closer to those of RMN than the mechanical philosophy with respect to Newton, given the critical influence of the Darwinian revolution on naturalism as a program in the philosophy of science.[38] Indeed, as Gary Hatfield points out in a condensed history of behaviorism, most of the paradigm-making theorists understood Darwinian functionalism as specifying a condition on explanatory posits in psychology: "all forswore introspection and made learned changes in behaviour the primary subject matter and

explanatory domain."[39] Put another way, the "logic of naturalism" stipulates that psychological terms are explanatory or "theoretically valid" to the extent that they have a "behaviorally grounded analysis."[40]

Now it should be noted that at least some important behaviorists did countenance *explanantia* that we might deem supernatural today—for example, irreducibly mental or otherwise non-mechanistic phenomena.[41] Indeed, it suggests the difficulties presented by the aforementioned intelligibility problem. But suffice it to say that the broader history of behaviorism as a paradigm certainly evidences its close kinship with naturalism. To venture a historical generalization, we might say that behaviorism was behaviorism *just to the extent* that it was naturalistic in spirit—with increasingly austere positivist and eventually Quinean banishments of the "non-physical" (often a euphemism for the supernatural) from the explanatory domain entirely.[42]

But, of course, few working psychologists today would fail to recognize that behaviorism—especially in its explicitly naturalistic expressions in positivism and Quine—proved to be a dead end:

> After being the dominant paradigm in American psychology for some decades, behaviorism was overtaken by a variety of research results that yielded anomalies revealing its limitations as an overall account of psychological functioning. These evidential failures led to the "cognitive revolution" and to a resurgence of biological approaches. These anomalies included evidence of complex planful cognitive behavior that is teleologically ordered even in rats let alone humans, the reality and importance of unconscious semantic processing and conscious imagery, and startlingly strong evidence of biological preparedness to learn and to maintain certain kinds of learned behavior in ways not in accord with classic learning theory. The net result was that, rather than being a dominant default view, behavioral accounts had to be demonstrated to be useful in each domain to which they were applied.[43]

To the extent that behaviorism sought to explain cognitive or otherwise mental phenomena armed solely with posits from the Darwinian functionalist domain—and to the extent that it was a theory moved by the heuristic exigencies of naturalism—it is at least plausible to think that we have another relevantly similar example from the history of science. In other words, it sounds a lot like naturalism misleading scientific inquiry.

In response, it might be argued that the eclipse of behaviorism in psychology is evidence not so much of a "wrong path" for science but of a paradigm shift. After all, the history of scientific progress is in many ways a story of such paradigm shifts. Even Newtonian gravitation was eclipsed by general relativity.

I certainly grant that the eclipse of a theoretical paradigm is not evidence of that theory's being a "dead end" for scientific inquiry. $F = ma$ is perhaps the most productive and brilliant generalization in the history of science. Ultimately, the question of whether behaviorism was more like a dead end than a productive theoretical paradigm that was ultimately overcome is not a question I can credibly answer here.[44] But the case for the former seems compelling *prima facie*, especially when compared to its Newtonian counterpart. There is nothing in the history of behaviorism that compares when it comes to explanatory posits that are both novel and productive. Indeed, if anything, the hallmark commitments of behaviorism were precisely to *shut down* theoretically productive lines of inquiry—for example, introspection as a method of data collection, irreducibly mental causation, and others—in the process of extending the Darwinian functionalist explanatory program.[45]

But, again, the point is not to get bogged down in the details here. If these examples show that it is even *plausible* to think that something like RMN as a stipulation could do more harm than good as a heuristic for scientific inquiry, then there is a truth-seeking problem. And it seems quite clear that this is plausible indeed, given these prominent examples.[46]

Heuristic for Science or Reactionary Disposition?

So RMN comes with some deep and abiding problems as a stipulation. It is not clear whether it is sufficiently intelligible as a thesis; even if it is, there are reasons to think that it does more harm than good when it comes to at least certain key episodes in the history of scientific progress. In short, RMN doesn't do its job.

But there is more that we can say here to drive home the point. In his magisterial treatment of the intellectual history of naturalism, Peter Harrison points out that founding figures of the naturalist turn in the philosophy of science such as Thomas Henry Huxley popularized the (vacuous or false) claim that the history of science features a perennial struggle between "naturalism" and "supernaturalism" as competing epistemic dispositions.[47] Riding the Darwinian revolution in biology and, critically, a renewed effort to wrest control of English institutions of knowledge production from the Church

of England,[48] Huxley's "founding myth" of naturalism as an epistemic heuristic gained traction. It is in this fundamentally reactionary context that we have to understand the invention of naturalism as a movement in philosophy and the sciences.[49]

While it is beyond my purposes to rehearse this complicated intellectual history, Harrison's story of naturalism as a fundamentally *reactionary* disposition with respect to contingent historical circumstances is profoundly relevant to contemporary philosophical accounts of RMN. It is remarkable to consider, for example, how prominently the American intelligent design debates feature in this literature.[50] Indeed, some even outright adopt solutions to the intelligibility problem by defining naturalism as a view in which explanatory appeals to God, specifically, are ruled out within the respective domains of special sciences.[51]

Indeed, against the background of Harrison's story, even ostensibly more fair-minded approaches such as Oppy's abovementioned non-definitional strategy for solving the intelligibility problem also seem implicated as reactionary. To see this, consider the possibility that the reason we can all agree that quarks are natural and ghosts supernatural has less to do with some sort of intuitive sense of the scientifically respectable and more to do with contingent historical circumstances given to us by a specific culture's folklore. That folklore could have been different, obviously, and if Oppy's non-definitional strategy is all we have to go on, so would the corresponding heuristic for scientific inquiry.

But this is a bad result. If RMN is what it purports to be—a viable heuristic for scientific inquiry—surely it cannot simply amount to a rejection of highly contingent historical circumstances, such as the Church of England's power in the 19th-century British academy or controversies in American fundamentalist circles in the 20th century. A reactionary disposition toward these very particular episodes in the history of science does not make a viable, unifying heuristic for scientific explanation *in general*. We don't want our understanding of science to depend on such things.

If Harrison is right, it is no surprise that naturalism (and therefore RMN) suffers from the intelligibility and truth-seeking problems. The problems of one episode of scientific inquiry can be quite different from those of others, after all, and so, if a heuristic for science is what is sought, then there is no substitute for something more positive. It is not my business here to give such an account, but if the preceding argument is correct, RMN is unlikely to look much like naturalism at all—unless, of course, broad appeals to the aforementioned explanatory virtues such as empirical adequacy, explanatory breadth, simplicity, and others, and strategies pertaining to inference to the best explanation (IBE), as, for example, Steve Petersen, count as naturalistic.[52] In other words, RMN might escape the two abovementioned problems, but only to the extent that it abandons its specifically and recognizably naturalistic content. The resulting conclusion is surely something like the following one from Timothy Williamson published in a popular *New York Times* article:

> Naturalism tries to condense the scientific spirit into a philosophical theory. But no theory can replace that spirit, for any theory can be applied in an unscientific spirit, as a polemical device to reinforce prejudice. Naturalism as dogma is one more enemy of the scientific spirit.[53]

For those interested in the project of delineating the boundaries of scientific explanation, RMN ought to be rejected.

Conclusion

I have argued that if RMN as a stipulation purports to offer any heuristic value to scientific inquiry, it cannot avoid two classic problems—what I call the intelligibility and truth-seeking problems. Illustrative examples of relevantly similar stipulations suggest as much, as does the broader history of naturalism as a paradigm in the history of the philosophy of science. We should reject RMN for many of the same well-known reasons that we reject other, less "restricted" versions of naturalism.

Notes

[1]Miles K. Donahue, "Methodological Naturalism, Analyzed," *Erkenntnis* 90, no. 5 (2025): 1981–2002, https://doi.org/10.1007/s10670-024-00790-y.

[2]For an overview of this debate, see Peter Forrest, "Methodological Naturalism Undercuts Ontological Naturalism," *American Philosophical Quarterly* 60, no. 1 (2023): 99–110, https://doi.org/10.5406/21521123.60.1.08.

[3]The intelligibility and truth-seeking problems, respectively, are more or less identical with what Julia de Menezes calls the "interpretation" and "truth" questions, respectively. Julia Telles de Menezes, "On Understanding Physicalism," *Kriterion: Journal of Philosophy* 59, no. 140 (2018): 511–31, https://doi.org/10.1590/0100-512x2018n14009jtm.

[4]For defenses of provisional versions of MN, see Gregory W. Dawes, "Understanding Naturalism," *Australasian Journal of Philosophy* 88, no. 4 (2010): 757–58, https://doi.org/10.1080/00048400903077093; and Yonatan I. Fishman and Maarten Boudry, "Does Science Presuppose Naturalism (or Anything at All)?," *Science & Education* 22, no. 5 (2013): 921–49, https://doi.org/10.1007/s11191-012-9574-1.

[5]Alexander Rosenberg defends a version of "scientism" that would count as unrestricted in this sense. See Alexander Rosenberg, "Scientism Versus the Theory of Mind," *Think* 19, no. 56 (2020): 59–73, https://doi.org/10.1017/S1477175620000214. Mario De Caro and David Macarthur's "liberal naturalism" is an example of restricted MN. See Mario De Caro and David Macarthur, eds., *Naturalism in Question* (Harvard University Press, 2008).

[6]See Peter van Inwagen, *Material Beings* (Cornell University Press, 1990) and David Malet Armstrong, *Sketch for a Systematic Metaphysics* (Oxford University Press, 2010). As others have noted, these sorts of appeals to an all-embracing "spatiotemporal system" as the proper domain of natural phenomena do not seem to help all that much. Indeed, it is hard to see how they escape any sufficiently rigorous rendering of Hempel's dilemma when it comes to the meaning of spatiotemporality. On this point, see Aisling Crean, "Dispositionalism and Modality," *Annals of the Japan Association for Philosophy of Science* 12, no. 2 (2004): 75–88, https://doi.org/10.4288/jafpos1956.12.75.

[7]Examples of this strategy include Robin Brown and James Ladyman, *Materialism: A Historical and Philosophical Inquiry* (Routledge, 2019); Alexander Rosenberg, "Disenchanted Naturalism" in *Contemporary Philosophical Naturalism and Its Implications*, ed. Bana Bashour and Hans D. Muller (Routledge, 2013), 13–17; and Janice L. Dowell, "The Physical: Empirical, Not Metaphysical," *Philosophical Studies* 131, no. 1 (2006): 25–60, https://doi.org/10.1007/s11098-005-5983-1.

[8]In his *Stanford Encyclopedia of Philosophy* article on the subject, David Papineau makes the point that "many ontological naturalists thus adopt a physicalist attitude to mental, biological, social and other such "special" subject matters." David Papineau, "Naturalism" in the *Stanford Encyclopedia of Philosophy*, ed. Edward N. Zalta and Uri Nodelman (2023), https://plato.stanford.edu/archives/fall2023/entries/naturalism/.

[9]Daniel Stoljar, "Physicalism," *The Stanford Encyclopedia of Philosophy* (Spring 2024 Edition), ed. Edward N. Zalta and Uri Nodelman, https://plato.stanford.edu/archives/spr2024/entries/physicalism/. In this quotation, I have put "natural" in place of "physical," but the core point about the intelligibility problem is unchanged.

[10]On this point, see Janice Dowell, "From Metaphysical to Substantive Naturalism: A Case Study," *Synthese* 138, no. 2 (2004): 149–73, https://doi.org/10.1023/B:SYNT.0000013235.88476.e3.

[11]An important point of contention in debates about scientific realism is that of the meaning and plausibility of "approximate truth." According to proponents of approximate truth, scientific explanations can be *partially* or *approximately* true, i.e., without being the whole truth about a given phenomenon. One might think therefore that current, approximately true theories could serve as positive indicators of the content of a future, completed theory that is exactly true. Without further argument, however, this appeal to approximate truth seems unpromising, since even proponents of approximate truth recognize that future theories that are anticipated by present theories will almost certainly countenance explanatory phenomena that are absent—or even outright rejected—by present theories. Indeed, the attractiveness of approximate truth lies with its ability to preserve continuity between theories that are otherwise radically different in their actual content. See Seungbae Park, *Embracing Scientific Realism* (Springer International, 2022).

[12]For an overview of the problems posed by Hempel's dilemma and similar problems to different variations of MN, see Ian Anthony B. Davatos, "The 'Natural' in Methodological Naturalism," *Philosophia: International Journal of Philosophy* 25, no. 1 (2024): 98–111, https://doi.org/10.46992/pijp.25.1.a.6.

[13]See, e.g., Alyssa Ney, "Physicalism as an Attitude," *Philosophical Studies: An International Journal for Philosophy in the Analytic Tradition* 138, no. 1 (2008): 1–15, http://www.jstor.org/stable/40208856; and Bas C. van Fraassen, *The Empirical Stance* (Yale University Press, 2002). For a recent criticism of these "attitude" or "stance" versions of naturalism, see Thomas J. Spiegel, "Why Naturalism Cannot (Merely) Be an Attitude," *Topoi* 42, no. 3 (2022): 745–52, https://doi.org/10.1007/s11245-022-09846-6.

[14]See Jessica Wilson, "On Characterizing the Physical," *Philosophical Studies: An International Journal for Philosophy in the Analytic Tradition* 131, no. 1 (2006): 61–99, https://doi.org/10.1007/s11098-006-5984-8.

[15]See, e.g., Daniel C. Dennett, "The Bright Stuff," *The New York Times*, July 12, 2003, https://www.nytimes.com/2003/07/12/opinion/the-bright-stuff.html.

[16]Graham Oppy, "Naturalism," chap. 1 in *Naturalism and Its Challenges*, ed. Gary N. Kemp, Ali Hossein Khani, Hossein Sheykh Rezaee, and Hassan Amiriara (Routledge, 2025). This passage is drawn from his broader defense of *metaphysical* naturalism, but the point about his non-definitional strategy for addressing the intelligibility problem applies to variations of MN (including RMN) as well.

[17]Sandy Boucher, in "Methodological Naturalism in the Sciences," *International Journal for Philosophy of Religion* 88, no. 1 (2020): 57–80, https://doi.org/10.1007/s11153-019-09728-9, considers this exact question and argues for a negative answer, drawing an analogy with Quine's famous critique of the analytic-synthetic distinction:

> When Quine offered his radical critique of the analytic/synthetic distinction, it was not an acceptable response to insist (as some did) that we have an intuitive grasp of which sentences are analytic and which synthetic, since if the critique was cogent, it would cast doubt on the validity of those very intuitions. (p. 67)

I agree with Boucher here, and indeed the problem only deepens when we consider that the sort of intelligibility we need is one that is up to the relevant *heuristic task* for scientific inquiry.

[18]Following E.V.R. Kojonen in "Methodological Naturalism and the Truth Seeking Objection," *International Journal for Philosophy of Religion* 81, no. 3 (2017): 335–55, https://doi.org/10.1007/s11153-016-9575-0, Donahue argues that science should be committed to RMN, but also that "one can only find out which domains of inquiry science is applicable to by going out into the world and seeing where naturalistic methodology works" (Donahue, "Methodological Naturalism, Analyzed," p. 1996). But presumably the whole point of a heuristic is that it provides anticipatory guidance *in advance* of "going out into the world." After all, no scientist is in the business of testing RMN as a stipulation.

[19]Fishman and Boudry, in "Does Science Presuppose Naturalism?," make a similar point when they claim that

> the problems plaguing some supernatural hypotheses are not unique to "supernatural" hypotheses, but may apply also to (now discredited) 'natural' hypotheses, such as the Ptolemaic model of the solar system and the theory of Phlogiston. (p. 942)

[20]Indeed, there is even a sense in which GT actually *outperforms* MN in this respect, since presumably we can generate a larger list of paradigmatically "bad" explanations than we can "supernatural" explanations, and thus we have more to go on when generalizing to future cases.

[21]Thanks to an anonymous reviewer for identifying these objections.

[22]On these points, see Igor Douven, "Abduction," in the *Stanford Encyclopedia of Philosophy*, ed. Edward N. Zalta and Uri Nodelman (Stanford University, 2025), https://plato.stanford.edu/archives/sum2025/entries/abduction/; and Samuel Schindler, "Theoretical Virtues in Science," in *Oxford Bibliographies Online*, Philosophy (2020), https://doi.org/10.1093/obo/9780195396577-0409.

[23]We should note that terms can be "rationally reconstructed" or otherwise made intelligible without clear and precise necessary and sufficient conditions. For an overview of classical accounts of explication as such a strategy for making theoretical terms intelligible, see Jonas Raab, "Quine on Explication," *Inquiry: An Interdisciplinary Journal of Philosophy* 67, no. 6 (2024): 2043–72, https://www.researchgate.net/publication/356014826_Quine_on_explication. Non-definitional accounts of the natural-supernatural distinction such as Oppy's do not attempt such an explication.

[24]Bradley Monton, *Seeking God in Science: An Atheist Defends Intelligent Design* (Broadview Press, 2009), 58.

[25]While Monton seems to assume a realist account of scientific theories here, the point applies equally to anti-realist accounts that take empirical adequacy (rather than truth) to be the aim of science.

[26]Donahue, "Methodological Naturalism, Analyzed."

[27]As in, e.g., Robin Collins, "The Teleological Argument: An Exploration of the Fine-Tuning of the Universe," chap. 4 in *The Blackwell Companion to Natural Theology*, ed. William Lane Craig and J. P. Moreland (Blackwell Publishing, 2009).

[28]By "revisionist redescription," I mean polemical redescriptions of the motives of, e.g., those who defend fine-tuning arguments for design as irrational or even dishonest inquirers with an "agenda." See, e.g., Gregory W. Dawes and Tiddy Smith, "The Naturalism of the Sciences," *Studies in History and Philosophy of Science* 67 (2018): 22–31, https://doi.org/10.1016/j.shpsa.2017.11.012.

[29]John Perry and Sarah Lane Ritchie, "Magnets, Magic, and Other Anomalies: In Defense of Methodological Naturalism," *Zygon* 53, no. 4 (2018): 1064–93, https://doi.org/10.1111/zygo.12473.

[30]Paul Feyerabend's classic criticism of the unity of scientific methodology bears on this point about the remit of science as well. See Paul Feyerabend, *Science in a Free Society* (Verso, 1978).

[31]Ironically, a restricted MN might be a bad heuristic because it is *not confident enough* about the potential remit of natural science. Swiss biologist and prominent critic of the Darwinian revolution Louis Agassiz echoed a common indictment of *The Origin of Species* when he contended that the existence of distinct species posed a problem in principle for any "transmutation" (read: evolutionary) theory, and that it was a "scientific mistake, untrue in its facts, unscientific in its method, and mischievous in its tendency." See Jean Louis Rodolphe Agassiz, "[Review of] *On the Origin of Species*," *American Journal of Science and Arts* 2nd ser., vol. 30 (July 1860): 142–54 in John van Wyhe, ed., *The Complete Work of Charles Darwin Online*, https://darwin-online.org.uk/converted/Ancillary/reviews/1860_Agassiz_A45.html. This indictment resulted from the fact that, among other things, the existence of distinct species was a phenomenon that was beyond the remit of biology to explain. It was reasoning inspired by something like RMN.

[32]See, e.g., Thomas Raleigh, "The Emptiness of Naturalism," *Philosophy* 99, no. 4 (2024): 597–623, https://doi.org/10.1017/S0031819124000172. Mario De Caro and David Macarthur's "liberal naturalism" is an example of restricted MN. See Mario De Caro and David Macarthur, eds., *Naturalism in Question* (Harvard University Press, 2008).

[33]For an alternative, slightly more expansive account of the core commitments of the mechanical philosophy (i.e., without specific commitments to corpuscular matter and local causation) and its continuity with Newton, see Peter Machamer, J. E. McGuire, and Hylarie Kochiras, "Newton and the Mechanical Philosophy: Gravitation as the Balance of the Heavens," *Southern Journal of Philosophy* 50, no. 3 (2012): 370–88, https://doi.org/10.1111/j.2041-6962.2012.00128.x.

[34]Andrew Janiak, *Newton as Philosopher* (Cambridge University Press, 2008).

[35]Janiak expands on this point:

> Newton's theory of gravity in the *Principia*, of course, is perfectly consistent with the contention that material bodies, such as the planets, act on one another directly across empty space—this might even be the most natural interpretation of the theory, and it was prevalent in the eighteenth century. For Newtonian gravity acts instantaneously, and the *Principia* posits no medium for gravitational interactions. And yet Newton himself appears to reject action at a distance. (Janiak, *Newton as Philosopher*, p. 33)

[36]See, e.g., Raleigh, "The Emptiness of Naturalism." This is not to say, of course, that the rejection of action at a distance by Newton and his critics was motivated by any sort of predisposition to *atheism*. Indeed, as Alexandre Koyré points out, debates about the mechanical philosophy were often debates about which "book of nature" God had decided to write. See Alexandre Koyré, *Newtonian Studies* (Chapman and Hall, 1965). Finally, Steve Clarke also cites Newtonian gravitation as a plausible instance in which supernatural (i.e., divine) activity figured into plausibly scientific explanatory hypotheses. See Steve Clarke, "Naturalism, Science and the Supernatural," *Sophia* 48, no. 2 (2009): 127–42, https://doi.org/10.1007/s11841-009-0099-2.

[37]Gottfried Wilhelm Leibniz and Samuel Clarke, *Correspondence*, ed. Roger Ariew (Hackett, 2000), 64.

[38]Peter Harrison identifies the Darwinian biologist Thomas Henry Huxley as the author of something like a "founding myth" of naturalism (i.e., anti-supernaturalism) as an essential commitment in the history of scientific inquiry. See Peter Harrison, *Some New World: Myths of Supernatural Belief in a Secular Age* (Cambridge University Press, 2024). Dawes and Smith seem to want to affirm something like Huxley's founding myth, but even they acknowledge that various traditions of natural *theology* formed the vast majority of epistemic contexts for the history of the sciences. See Dawes and Smith, "The Naturalism of the Sciences."

[39]Gary Hatfield, "Behaviourism and Psychology," in *The Cambridge History of Philosophy 1870–1945*, ed. Thomas Baldwin (Cambridge University Press, 2003), 640–48.

[40]See N. K. Denzin, "The Logic of Naturalistic Inquiry," *Social Forces* 50, no. 2 (1971): 166–82, https://psycnet.apa.org/doi/10.2307/2576935.

[41]In fact, as Noam Chomsky notes in Language and Mind (Cambridge University Press, 2006), the naturalist rejection of irreducibly mental phenomena also happens to be traceable to the mechanical philosophy: "The reasons for the dissatisfaction of Newton, Leibniz, and the orthodox Cartesians with the new physics are strikingly similar to the grounds on which a dualistic rationalist psychology was soon to be rejected," p. 7.

[42]Hatfield, "Behaviourism and Psychology."

[43]Jerome C. Wakefield, "Is Behaviorism Becoming a Pseudoscience? Replies to Drs. Wyatt, Midkiff and Wong," *Behavior and Social Issues* 16, no. 2 (2007): 170–89, https://psycnet.apa.org/doi/10.5210/bsi.v16i2.919.

[44]For an overview of various ways that behaviorism can be characterized as an important paradigm in the history of the psychological sciences, see Henry L. Roediger III, "What Happened to Behaviorism," *APS Observer* 17, no. 3 (2024): 40–42, https://www.psychologicalscience.org/observer/what-happened-to-behaviorism.

[45]This is not to deny the benefits of asking questions a committed behaviorist might ask about mental life, since, of course, no serious school of psychological research denies that such questions are good and an aid to scientific progress. Indeed, the point is that the good part of behaviorism is not uniquely behaviorist.

[46]Peter Harrison mentions several other examples of (plausibly) super- or otherwise non-naturalistic heuristics or explanatory posits in the history of science that seem to serve the purposes of scientific progress: (1) the thesis of "divine creative activity" as an explanation of classical laws of nature in, e.g., James Clerk Maxwell and Lord Kelvin; (2) divine omnipotence as a condition of possibility for the distinction between empirical adequacy and truth in scientific theories; and even (3) the hypothesis of intelligent design in William Harvey's discovery of blood circulation. See Peter Harrison, "Naturalism and the Success of Science," *Religious Studies* 56, no. 2 (2020): 274–91, https://doi.org/10.1017/S0034412518000574.

[47]For the relevant primary textual evidence here, see Thomas Henry Huxley, *Essays upon Some Controverted Questions* (Cambridge University Press, 2009), https://www.gutenberg.org/files/34698/34698-h/34698-h.htm.

[48]Indeed, for this reason among others, considering naturalism as *ideological* in something like the Marxian sense is an inviting hypothesis. For an explanation of just this hypothesis, see Joshua Lee Harris, "Is Naturalism Ideology?," *Renovatio: The Journal of Zaytuna College* (forthcoming).

[49]It is sometimes claimed that MN has a longer track record in the history of the philosophy and theology of science, extending into the Middle Ages and even antiquity. But this is not quite right—at least not if the abovementioned definition of MN is what we have in mind. As Harrison demonstrates, any rigorous division of the world into categorically "natural" and "supernatural" phenomena—a division that is critical to contemporary naturalism debates—is simply unknown to ancient and medieval authors. To illustrate the point, consider that nothing could be more "natural" to pre-Socratics such as Anaxagoras or medievals such as Aquinas than, e.g., God's intelligent and creative causality in the material world. Harrison, *Some New World*, 232.

[50]To cite just a handful of examples, Davatos, "The 'Natural' in Methodological Naturalism"; Donahue, "Methodological Naturalism, Analyzed"; Kojonen, "Methodological Naturalism and the Truth Seeking Objection"; and Maarten Boudry, Stefaan Blancke and Johan Braeckman, "How Not to Attack Intelligent Design Creationism: Philosophical Misconceptions About Methodological Naturalism," *Found Sci* 15 (2010): 227–44, https://doi.org/10.1007/s10699-010-9178-7.

[51]Gregory Dawes and Tiddy Smith defend an account of RMN according to which individual sciences consider explanatory posits relevant to phenomena characteristic domains. The sciences are naturalistic, then, because God is not found among the various explanatory posits relevant to any of these domains—even if God is a licit explanation in traditional natural theology. Unfortunately, Dawes and Smith do not make much an effort to respond to the intelligibility and truth-seeking problems. Again, if RMN is an interesting stipulation, then it provides heuristic value for scientific inquiry *as a whole*—not just for specific sciences. If RMN is only about excluding God, then another stipulation that simply excludes uniquely astrological explanatory posits would be just as interesting. It is hard to imagine either thesis without precisely the sort of reactionary disposition described above. See Dawes and Smith, "The Naturalism of the Sciences."

[52]Steve Petersen defends a version of "naturalism" centered on the primacy of IBE as an epistemic commitment that constrains all explanatory posits. But if Petersen's account is naturalistic, it is only in the weakest possible sense, as he admits that even distinctively theistic explanatory posits could be licit in principle (i.e., if they satisfy conditions specified by IBE criteria). One wonders whether this is naturalism at all. See Steve Petersen, "A Normative Yet Coherent Naturalism," *Philo* 17, no. 1 (2014): 77–91, https://philarchive.org/archive/PETANY.

[53]Timothy Williamson, "What Is Naturalism?," *The New York Times*, September 4, 2011, https://archive.nytimes.com/opinionator.blogs.nytimes.com/2011/09/04/what-is-naturalism/.