

NATURALISM AND ONTOLOGY: A REPLY TO DALE JACQUETTE†

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In *World Without Design: The Ontological Consequences of Naturalism*, I argued that there is an important sense in which naturalism's current status as methodological orthodoxy is without rational foundation, and I argued that naturalists must give up two views that many of them are inclined to hold dear—realism about material objects and materialism. In a review recently published in *Faith and Philosophy*, Dale Jacquette alleges (among other things) that my arguments in *World Without Design* are directed mainly against strawmen and that I have neglected to discuss at least one formulation of naturalism that straightforwardly addresses my main objections. In this reply, I show that these and other objections raised by Jacquette are unsound and, in fact, rest on egregious misrepresentations of the book.

For many years now, philosophical naturalism has enjoyed the status of orthodoxy in the Western academy; and it is widely recognized among theists as one of the most important sources of opposition to belief in God. In *World Without Design: The Ontological Consequences of Naturalism*, I raised two main objections against naturalism. First, I argued that there is an important sense in which naturalism's status as orthodoxy is without rational foundation. Second, I argued that naturalists must give up two views that many of them are inclined to hold dear—realism about material objects (RMO) and materialism. I also argued that naturalists will have a difficult time avoiding solipsism. If I am right, naturalism comes with some very unattractive commitments.

In a review recently published in *Faith and Philosophy*, Dale Jacquette raises several objections against my book, ultimately concluding that its arguments are "myopic[ally] focus[ed] on easily, if not succinctly, defeatable strawmen." Moreover, as Jacquette sees it, once I have conquered my army of strawmen (2004: 127), I "still [leave] the strongest contrary and sometimes most obvious opposing views untouched." (2004: 127) Indeed, he alleges that I overlook at least one formulation of naturalism that straightforwardly "answers [my] main objections to naturalism." (2004: 127) These are serious charges. If true, they make it difficult to resist his final verdict—that the book is "hard to recommend on its philosophical merits." (2004: 130)

The question, however, is whether any of these charges are backed up by anything even approaching decent argument. In what follows, I will show that they are not. Given the serious nature of the charges, one would



expect Jacquette's review to contain powerful arguments against at least one of the premises of the book's main argument. Upon closer inspection, though, it turns out that Jacquette's objections are supported by nothing more than confusion and misrepresentation. Moreover, his misrepresentations of the book are egregious, and the resulting portrait is a caricature. For this reason, a reply is in order.

This reply is divided into four sections. Section 1 briefly summarizes the argument of *World Without Design*, and identifies the main objections that Jacquette brings against it. Sections 2 – 4 consider these objections in detail, showing that none of them comes even close to hitting its mark.

The Argument of World Without Design

The main argument of *World Without Design* can be summed up as follows:

- (1) Naturalism is not a philosophical thesis, but a research program. The program consists of a disposition (or set of dispositions) to treat the methods of science and those methods alone as basic sources of evidence.
- (2) Research programs cannot be adopted on the basis of evidence; and what counts as evidence from the point of view of one research program might not count as evidence from the point of view of another.
- (C1) Therefore: There is no rational basis for declaring categorically that one particular research program is rationally to be preferred over every other—i.e., naturalism's status as orthodoxy is without rational foundation.
- (3) Furthermore, the methods of science alone provide no justification for accepting *realism about material objects* (RMO)—the thesis that there exist material objects with intrinsic modal properties.
- (4) If premises (1) and (3) are true, then naturalists cannot rationally accept RMO.
- (5) If naturalists cannot rationally accept RMO, then they are committed to mind-body dualism and will have a hard time avoiding solipsism.
- (C2) Therefore: Naturalists cannot rationally accept RMO, they are committed to mind-body dualism, and they will have a hard time avoiding solipsism.

The first conclusion, C1, helps us to locate naturalism's proper place on the philosophical landscape—as just one research program among many that embodies (so far as we can tell) just one set of legitimate methodological preferences among many. The point here is not that naturalists cannot sensibly believe that it is rational to be a naturalist. Nor is the point that naturalists cannot sensibly dismiss some research programs as irrational. (One could do this by showing a research program to be self-defeating.) Rather, the point is just that there is no evidence on which to base a claim that *all non-naturalists ought to become naturalists* (and likewise for other research

programs). The second conclusion, C2, highlights some of the more unpalatable consequences of embracing naturalism.¹

The book is divided into three parts. Part 1 defends premises (1) and (2) of the argument, as well as the inference from those premises to the conclusion that naturalism's status as orthodoxy is without rational foundation. Part 2 defends premises (3) – (5). In Part 3, I consider two alternatives to naturalism—intuitionism and supernaturalism. I argue that intuitionism is self-defeating unless it somehow supports belief in something like the God of traditional theism, and then I go on to offer programmatic suggestions to the effect that “some form of supernaturalism *might* succeed where naturalism has failed at saving RMO and [realism about other minds].” (2002: 214)

Jacquette raises objections to all three parts of the book. His main objections are to my characterization of naturalism (premise 1), my claim that research programs cannot be adopted on the basis of evidence (premise 2), and my claim that naturalism conflicts with realism about material objects (premise 3). But he also claims that my treatment of supernaturalism is superficial and lacks cogency. In what follows, I will address each of these objections in turn.

2. *Naturalism as a Research Program*

As the previous section makes clear, the starting point of my argument in *World Without Design* is the claim that naturalism must be understood not as a philosophical thesis but as a research program. A research program is a set of methodological dispositions—dispositions to treat certain kinds of arguments or belief-sources as basic sources of evidence. Naturalism, I said, is a *shared* research program—a subset of a maximal set of methodological dispositions²—that treats the methods of science and those methods alone as basic sources of evidence. Relevant rivals include intuitionism and supernaturalism, which differ from naturalism only by treating certain *additional* belief-sources (intuition in the case of intuitionism and religious experience in the case of supernaturalism) as basic sources of evidence. A source of evidence is treated as basic, I said, just in case it is trusted without reliance on independent evidence in favor of its reliability. (2002: 2 – 3)

I also said that research programs cannot possibly be adopted on the basis of evidence.³ The reason, in short, is as follows. Evidence can only be recognized as such from within a research program. In order to recognize E as evidence for a claim C, one must already be disposed to treat items like E as evidence—which is to say that one must already have some methodological disposition or other. Research programs might generate evidence that prescribe their own rejection, and they might also generate evidence that some other research program ought to be accepted. Still, as I say in the book, it won't be—it *can't* be—on the basis of such evidence that one accepts a rival research program. For in order to accept the new program, one must first reject the old one; but once the old one is rejected, one can recognize evidence as such only after the new program is already in place. (2002: 4 – 6)

Here, two illustrations will be helpful. First, suppose I'm a supernaturalist and I somehow come to see that the outputs of religious experience are

incoherent, or that scientific evidence compellingly supports the claim that religious experience is an unreliable source of belief. Here, a natural move for me would be to convert to naturalism. But, despite the fact that this move is natural, it is hard to see how it is evidentially supported. After all, I do have other options: I could move to a research program that includes no methodological preference for coherence; or, if the trouble is not incoherence but conflict with science, I could convert to a research program that treats religious experience alone as a basic source of evidence, giving up my reliance on science. I could become an intuitionist rather than a naturalist, or I could adopt some other research program altogether. The question of which of these alternatives to adopt is not decided by the fact that religious experience produces incoherent outputs, nor by the fact that it produces outputs inconsistent with the deliverances of science. Indeed, it is not even decided by these facts together with (say) a methodological preference for science or a methodological disposition to favor scientific evidence over evidence from religious experience in cases of conflict. For once the incoherence or the conflict is recognized, these methodological preferences are as much up-for-grabs as any others. We may be built in such a way that it would be a lot harder for us to give up (say) the preference for coherence rather than the reliance on religious experience, but that fact goes no distance toward showing that our decision to give up one rather than the other was evidentially guided. And I see nothing else in the description of this sort of case that suggests that the decision to convert to naturalism would be evidentially guided either.

On the other hand, suppose I'm a naturalist, and suppose that, after undertaking some sort of investigation to find out whether religious experience is a reliable source of evidence, I discover that it is and then come to rely on it as a source of evidence. In the book, I made a point of saying that one could come to trust religious experience in such a way. But doing so would not involve any sort of conversion from naturalism to supernaturalism. The reason is that the naturalist who comes to trust religious experience via this sort of route has not come to treat religious experience as a basic source of evidence—a source that is trusted even in the absence of evidence of its reliability. If science vindicates religious experience as a reliable source of evidence, then naturalists and supernaturalists will build their theories from a commonly shared evidential base. But naturalists will not thereby have become supernaturalists. For a naturalist to become a supernaturalist, she would have to come to trust religious experience in the absence of evidence in support of its reliability. But, of course, it is just trivial that this is not the sort of thing that could be done on the basis of evidence.

In light of what has just been said, we can see why the following remarks of Jacquette's reflect misunderstanding:

If an inquirer ... believes that there are only two possible research programs and comes to reject one on the basis of evidence, which Rea admits can happen, why would it not be rational to adopt or at least gravitate toward the only remaining alternative? To consider the case directly relevant to Rea's study, if we think that the origin of the universe can only be explained by naturalism or supernaturalism,

and we come to reject supernaturalism, say, on the basis of the problem of evil, why would it not then be rational to adopt naturalism as constituting the only viable research program of choice? (2004: 126)

For one thing, research programs, being collections of *dispositions*, aren't the sorts of things that could *explain* anything. But never mind that for now. Again, I do not deny that it could be rational for someone to "adopt or at least gravitate toward the only remaining alternative." In fact, as I say in the book (2002: 17), for all I know it might be that every right-thinking person ought to embrace (say) supernaturalism, or some other specific research program. Nor do I deny that research programs can be discarded on the basis of evidence. What I deny is that research programs can be adopted on the basis of evidence. And, I say, it is partly for this reason (and partly because what counts as evidence differs from one research program to the next) that one cannot have rational grounds for declaring categorically that everyone rationally ought to adopt naturalism or any other specific research program.

So much, then, for my characterization of naturalism and for my reasons for thinking that research programs cannot be adopted on the basis of evidence. The passage just quoted, as well as the remark discussed in note 3, express Jacquette's main objections against my claim that research programs cannot be adopted on the basis of evidence; and we have now seen why those objections miss their mark. Against my characterization of naturalism, Jacquette raises two main objections: (a) that it is ill-defined, and (b) that it is inadequately supported. I'll take each in turn.

In support of the first objection, Jacquette says this:

Unfortunately, Rea nowhere explains or tries to define what he means by a thesis as contrasted with a research program. He is satisfied to use these terms and draw upon what he takes to be their methodologically charged implications without saying precisely what differences are supposed to hold between them and how they are supposed to be interrelated.

All we learn from Rea's book about the distinction between a thesis and a research program is that Rea regards naturalism as a research program rather than a thesis, and that as a research program naturalism necessarily functions theoretically without rational foundation. ... Nor does Rea provide examples of genuine philosophical theses for the sake of comparison so that the reader can try to determine whether a thesis is or is not open to the same limitations as research programs. (2004: 126)

But these claims are simply false. As indicated above, the term 'research program' is defined in detail in the book, along with supporting terms like 'methodological disposition' (see pages 2 – 4). Admittedly, I did not give a definition of the term 'philosophical thesis'. But one would think that professional philosophers would not have to be told what a philosophical thesis is.⁴ Even so, there are in the book, contrary to Jacquette's assertion, no less than *sixteen* numbered examples of genuine philosophical theses

(explicitly identified as such) that might be, and have been, regarded as characterizations of naturalism. (2002: 55 – 65) It is hard to see, then, what Jacquette could have been thinking in making the remarks quoted above.

Continuing to press the objection that my characterization of naturalism is ill-defined, Jacquette goes on to chide me for treating a research program as something that can have consequences. He writes:

Oddly, and in a way that we are never in position to assess in Rea's exposition, naturalism ... despite being a research program, is nevertheless understood as having consequences like those of any thesis in terms of which at least its unacceptability can be judged. (2004: 126)

But here too we have outright misrepresentation. In a section of the book labeled 'On the Consequences of a Research Program', I explicitly raise this issue:

So naturalism is a research program. But now how will this book proceed? How do we draw out the consequences of naturalism if naturalism is not a thesis that can have consequences? (2002: 73)

And having raised the issue, my response is just as explicit:

The consequences of a research program are just the views to which one is committed by virtue of adopting it. (2002: 73)

Perhaps one might doubt that there ever will be any views to which one is committed simply by virtue of adopting a research program. But that is a different objection, and to drive it home one would have to offer an argument against either premise (4) or premise (5) of the main argument laid out above.

In support of the second objection—that my characterization of naturalism is inadequately supported—Jacquette offers two remarks. Here is the first:

There is no obvious system to the alternatives Rea considers ... so we do not know whether he has eliminated all of the possibilities or overlooked what might otherwise turn out to be the most promising answers. (2004: 126)

Jacquette is surely right that I haven't surveyed an exhaustive list of characterizations of naturalism and discovered all but mine to be wanting. But I never proposed to do this, and my argument doesn't require it. To see why, consider the following summary of my reasons for thinking that naturalism is not a thesis:

In the last section [section 1 of Chapter 3] I examined sixteen different theses that have been offered in the literature as characterizations of naturalism or of some version thereof. All suffered from crippling defects qua characterization of naturalism. Furthermore, I argued

that the problem was quite general: *any* substantive thesis would suffer from the same sorts of crippling defects *qua* characterization of naturalism. Thus, it seems clear that (insofar as we want to be charitable) naturalism should not be understood as a definite philosophical thesis at all. Instead, it should be understood as a research program. (2002: 66)

Perhaps the objection is simply against the last move in this passage: the move from 'is not a thesis' to 'is a research program'. But the justification for that move should be obvious: 'thesis' and 'research program' are, broadly speaking, the only viable alternatives in the literature. Of course, as noted in the book (2002: 66), Roy Wood Sellars (1922) characterizes naturalism as an *admission of a direction* (whatever exactly that means); and Peter Forrest (1996) characterizes it as a *program* (minus the word 'research'). Moreover, Bas van Fraassen (1998) characterizes materialism as an *attitude*, and so one might think that naturalism could be similarly characterized. But all of these characterizations are clearly at least in the direction of, if not just alternative versions of, my own characterization. Jacquette suggests other characterizations: "naturalism might instead be an ideology, theory, framework for a thesis, or something else yet again". (126) But ideologies and theories sound a lot like theses; and I doubt that even Jacquette knows exactly what he means by 'framework for a thesis'. Given this, and given the absence of clearly different alternatives, the disjunction 'thesis or research program' seems at least warranted, even if it is (admittedly) not logically exhaustive.

The more serious charge comes in Jacquette's second remark in defense of the objection that my characterization of naturalism is poorly supported. He writes:

A useful way of thinking about naturalism that Rea does not try to refute formulates naturalism as the thesis that all and only those putative entities actually exist that are hypothesized, entailed, or presupposed by a correct natural science. This definition avoids the need to describe naturalism as a research program...and answers Rea's main objections to naturalism.... (126–7)

The charge, in other words, is that there is a viable characterization of naturalism that I ignored and that avoids all of the consequences I say attend commitment to naturalism. But look again at the characterization he offers: Naturalism is the thesis that all and only those putative entities actually exist that are hypothesized, entailed, or presupposed by a correct natural science. This bears striking resemblance to Philip Pettit's characterization of naturalism, a thesis which, together with several similar theses, receives explicit and detailed treatment in the book:

(3.7) Naturalism imposes a constraint on what there can be, stipulating that there are no nonnatural or unnatural, praeternatural or supernatural entities. ... Nature comprises those entities and constructs made of those entities that the ideal physics, realisti-

cally interpreted, posits. (Pettit 1992: 245, 247; quoted in Rea 2002: 57)

My assessment of this thesis was as follows:

...3.7 is either an obviously false metaphysical thesis or else a disguised epistemological thesis. As we all know, there is no such thing as the ideal physics or the best physics. The physics we now have is not the best ... and it is less than ideal. But if there is no ideal physics, then there is no *ontology of the ideal physics*. Thus, taken at face value, 3.7 implies that *no* ontology is correct. But that is obviously false. Of course, the idea probably is that the correct ontology is whatever ontology *would be* (or more optimistically, *will be*) implied by an ideal physical theory if (or when) such a one were to exist. But if that is right, then 3.7 is not so much a metaphysical thesis as an affirmation of the ability of physics to tell us the whole truth about the world. In other words, it is a disguised epistemological thesis. (2002: 57 – 58)

The main substantive difference between Jacquette's proffered characterization and Pettit's characterization is just this: Jacquette's replaces 'ideal' with 'correct' and 'physics' with 'natural science'. Do these replacements help? No. But whether it suffers from exactly the problems Pettit's characterization suffers from depends on how widely one is willing to construe the category 'natural science'.

It would appear that, on Jacquette's view, natural science is (as most people, myself included, think) just a particular sort of empirically based theoretical activity. In his own words:

Science is best construed not merely as a systematization of observations, but as a structure of hypotheses that support one another in a mutually justificatory coherence network that is empirically justified in complicated ways. (127)

Contrary to what he suggests, this characterization of science and of scientific justification is almost identical to my own. (See, e.g., 2002: 70 – 71, 104 – 105, and 128 – 30.) But if this is what science is, then Jacquette's characterization, like the most plausible reading of Pettit's, is simply a disguised epistemological thesis affirming the ability of a particular sort of empirically based theoretical activity—natural science in general this time, rather than physics in particular—to tell us the whole truth about the world. As such, it falls prey to objections I raise against characterizations commonly referred to by the label 'epistemological naturalism' or 'methodological naturalism'. (2002: 59 – 65).

But suppose Jacquette meant to apply the term 'natural science' more broadly. Suppose (contrary to appearances) that, on his view, 'natural science' is a placeholder for whatever sort of inquiry can in fact reveal the whole truth about the world. A *correct* natural science would then be whatever theory in fact tells the whole truth about the world. Thus, his character-

ization of naturalism would boil down to something like this: Naturalism is the view that the correct ontology is the ontology of whatever theory tells the whole truth about the world. It's hard to argue with that; but, then again, that's precisely the problem. On this reading, naturalism is trivial. Everybody is a naturalist. But, of course, that claim is just ridiculous.

In sum, then, Jacquette's objections against my characterization of naturalism fall flat. Most importantly, his assertion that I have failed to consider genuine examples of philosophical theses that might serve as candidate expressions of naturalism is false, as is his assertion that I have overlooked a characterization that provides the resources to answer all of my objections. In the next section, I turn to objections against my defense of premises (3) – (5) of the main argument.

3. Realism about Material Objects

Why think that naturalists cannot be realists about material objects? Before answering this question, let me first clarify it. As indicated above, realism about material objects is defined in the book (2002: 8 – 15) as the thesis that there exist material objects with intrinsic modal properties. Modal properties are characterized as follows:

Modal properties are properties involving necessities or possibilities for the objects that have them. They are properties like *being necessarily human*, *being possibly spherical*, *being essentially such as to undergo mental activity*, and so on. (2002: 77)

Intrinsic properties are “properties that can be had by something regardless of whether it is accompanied or unaccompanied by other contingent beings.” (2002: 11) To be a realist about material objects, then, is to affirm at least three theses:

- (T1) Material objects, if there are any, have modal properties.
- (T2) The modal properties of material objects, if there are any, are intrinsic.
- (T3) There are material objects.

T1 I take to be a conceptual truth: naturalists and anti-naturalists alike ought to accept it. I defend this claim in the book (2002: 81ff), but won't do so here. What is at stake in the debate over realism about material objects, then, is just T2 and T3. This is important to notice since Jacquette, throughout his review, seems to take realism about material objects to be equivalent to the affirmation of T3 alone. Thus, for example, he says that I conclude that “naturalism is incompatible with belief in the existence of material objects” (127), and then later remarks that “it is hard to see how can there [sic] possibly be any objection to philosophical naturalism as including ... material objects, including their intrinsic modal properties, if any” (127). But, of course, I do not deny that naturalists can believe in material objects. Indeed, my argument throughout Chapters 4 – 6 proceeds on the assumption that naturalists will accept this claim—i.e., T3—and thus is

aimed only at showing (a) that commitment to naturalism precludes acceptance of T2, and (b) that those who reject T2 (and who are naturalists) are committed to mind-body dualism and will have a hard time avoiding solipsism.

Now for the argument. Let us say that IMP-beliefs are beliefs expressible by sentences satisfying schema IMP (where 'x' is a placeholder for kind-terms like 'human', 'frog', 'table', etc.):

- (IMP) In any region containing matter arranged *x*-wise, there exists a material object that has *intrinsically* the nontrivial modal property *p*.

In the book, I argue that one can justifiably believe that there are intrinsic modal properties only if one has at least one justified IMP-belief—e.g., the belief that, wherever one finds matter arranged human-wise one finds something that is essentially such as to be able to undergo mental activity. (2002: 81 – 98) The problem for naturalists, I argue, is that (a) they can have justified IMP-beliefs only if such beliefs can be justified via the methods of science, but (b) IMP-beliefs seem not to be justifiable via those methods.⁵

Why should we think that IMP-beliefs cannot be justified by the methods of science? Broadly speaking, in order to have scientific justification for any sort of belief, the belief must either be a perceptual belief or play some explanatory or simplifying role in a theory grounded at least in part in perceptual beliefs. But IMP-beliefs are clearly not perceptual beliefs. Moreover, it is hard to see how IMP-beliefs could play any sort of explanatory role in our theories; and, though they might well play a simplifying role, that fact is not sufficient to confer *epistemic* justification upon them. Or, at any rate, it is not sufficient to confer epistemic justification upon them in the absence of either theistic or anti-realist assumptions that explain the epistemic value of simplicity. (2002: Ch. 6) But theism is, for most naturalists, worse than the alternatives (anti-realism and dualism) that I am urging; and to embrace anti-realism is to give up T2. Thus, I conclude, naturalists must give up T2.⁶

As we have seen, Jacques's general assessment of this argument (and, indeed, of the whole book) is two-pronged: first, he thinks that it is myopically focused on easily defeatable strawmen; second, he thinks that it leaves untouched the strongest and most obvious opposing views. The first charge is yet another misrepresentation. It simply ignores my attacks on sixteen different characterizations of naturalism (in Chapter 3), my arguments against specific views about the epistemology of modal properties offered by Michael Burke and David Lewis (in Chapter 4), my attacks on naturalistic accounts of proper function offered by Ruth Millikan, Karen Neander, and Robert Koons (in Chapter 5), my extended treatment of Crawford Elder's views about the epistemology of modal properties (in Chapter 6), my objections against defenses of the evidential value of intuition by George Bealer, Jerrold Katz, Alvin Goldman, and Joel Pust (in Chapter 8), as well as a variety of additional arguments against other explicitly identified targets throughout the book. The most charitable interpretation of Jacques's remark is that, by his lights, the philosophical landscape is virtu-

ally littered with strawmen. But if that is his view, it is hard to see why he would fault someone for trying to clear away the debris.

As to the second and more interesting charge—that I leave untouched the most obvious and powerful opposing views—here again we find more bluster than substance. The sentence immediately following the objection reads as follows:

Thus, not only does Rea not try to explain what he means by the key concept of science, but from what he does say, it appears that he has a narrow excessively skewed [sic] and to that extent implausible view of what scientists actually do and what science actually involves. ... Natural science, however, is ontically committed to the existence of many kinds of putative entities that do not make their presence known on any Geiger counter. (2004: 127)

Jacquette then goes on to offer the characterization of science that I quoted earlier and remarks that, once we accept that view of science (which, as indicated earlier, I do accept), it is hard to see what objection there could be to belief in material objects with intrinsic modal properties.

Several remarks are in order here. First, it is (again) just false to say that I offer no explanation of what I mean by ‘science’ (see, e.g., 2002: 68). Second, as I have already noted, my own view of science is pretty much identical to Jacquette’s.⁷ Third, it is important to note exactly what Jacquette is doing here. He lays out a view of science that I explicitly endorse, declares it to be an “obvious opposing view” that I leave untouched, and then reports his inability to see why, in light of the view that I endorse, one should think that naturalists cannot endorse the conjunction of T2 and T3. Well, one reason for leaving the view “untouched” is that I endorse it. As to Jacquette’s inability to see why, given that view of science, one might think that T2 in particular cannot be justified on the basis of science, let me just say that here even a casual reading of the book—especially Chapters 4 and 6—might have been helpful.

So at least this one “obvious opposing view” is neither opposing nor ignored by me. But there is only one other view that Jacquette offers up as potentially both “opposing” and “untouched”. He introduces the view by saying that “Rea overlooks an important distinction between intrinsic logical and intrinsic causal modal properties.” (128) He does not actually say what he means by ‘logical modal properties’, but we may speculate that they are just the sorts of modal properties I have been talking about: i.e., properties involving what some might refer to as ‘broadly logical’ necessities or possibilities for the objects that have them. Causal modal properties he defines as follows:

Intrinsic causal modal properties might be understood as those a material entity has by virtue of falling under a particularly empirically established causal law, by which some strength of causal rather than logical necessity is implied. (2004: 128)

The objection, so far as I understand it, is that “material objects do not and

cannot even be imagined to have [nontrivial] intrinsic logical modal properties”, but they “might nevertheless be said to have and even in some sense to be defined in terms of their intrinsic causal modal properties.” (2004: 128) The intrinsic causal modal properties, in turn, will be empirically detectable (being just the properties that a thing has by virtue of falling under an empirically established causal law); and, somehow, the attribution to objects of such properties is supposed to suffice for saving RMO.

Thus stated, this alleged “obvious opposing alternative” looks rather sketchy. But it is not hard to flesh it out into a view that is well worth taking seriously. Objects, we might say, are sorted into natural kinds on the basis of various empirically detectable properties: causal powers and dispositional properties are clearly the ones Jacquette has in mind, but we might also suppose that microstructural features and causal histories (perhaps even Millikan-style proper functions) form the basis for such sorting as well. Of course, saying this doesn’t by itself get us to the conclusion (needed for RMO as I have defined it) that any of the properties had by a thing (even its defining properties) are in any sense essential to it; but here too there is at least a superficially plausible story to tell. One might think (and some have thought) that the empirical indicator of the fact that a property *P* is essential to members of a natural kind *K* is just the fact that objects of kinds closely related to *K* uniformly have some competitor of *P*. Thus, in this vein, Crawford Elder writes:

What suggests that gold essentially has a melting point of 1063°C is that each of the other metals itself uniformly possesses just one competing melting point; what suggests the scientific claim that water is essentially H₂O is that various acids and bases which science treats as substances generically similar to water, uniformly possess their own alternative chemical structures. (Elder 1992: 325)

In short, then, one might think that the uniform clustering of explanatorily rich properties—itsself an empirically detectable fact—not only provides a basis for sorting objects into kinds, but also provides reasons for thinking that the properties by which the objects are sorted are essential to the things that have them. And, we might add (as Jacquette apparently wants to add), the modality involved is no stronger or weaker than good old-fashioned *nomological* necessity. Indeed, we might think (and, again, many have thought) that there simply is no brand of necessity stronger than *nomological* necessity—which, again, one might well regard as perfectly empirically detectable.

Perhaps Jacquette would not wish to see the view that he is proposing fleshed out in exactly this way; but so far as it is intelligible at all, it is clearly in the neighborhood of this sort of view. And one must give credit here: this is surely an interesting and worthwhile objection to raise against the main argument of my book. But, alas, like just about every other objection that Jacquette raises in his review, this one is discussed at length and rebutted in the book. For, after all, the view just described is none other than Crawford Elder’s theory of the epistemology of modal properties.⁸ Against the view, I raised three objections. For the sake of brevity, I’ll only

reproduce the most important of the three:

On Elder's view, the fact that [certain kinds of explanatorily rich] properties cluster in the ways that they do is explainable (and, presumably, best explained) by supposing that the matter exemplifying the properties in question composes an object which has those properties essentially. If this view is right, then it is easy to see how we might have empirical grounds for particular [beliefs attributing modal properties to objects]. We observe that a certain way of arranging matter uniformly exemplifies certain explanatorily rich properties, and so we infer ... that wherever we find matter arranged in the way in question, we find an object that is essentially such as to exemplify the relevant explanatory properties. Nothing so far provides a basis for thinking that the essential properties of the objects we identify are intrinsic; but this lack is made up for by the pragmatic considerations outlined at the beginning [of the chapter].

This last point bears emphasizing. Elder's view [and Jacquette's too] *presupposes* rather than justifies our belief that the objects of scientific investigation are material objects with intrinsic modal properties. This is not necessarily a defect; but it is important for us to observe in the present context lest anyone think that Elder's [or Jacquette's] view might be able to stand alone, apart from the pragmatic considerations mentioned earlier [in the chapter] as a solution to the [problem of explaining how the methods of science could justify our IMP-beliefs]. (2002: 133)

Of course, nothing I have said so far precludes the possibility that the views of Elder or Jacquette in conjunction with a certain kind of pragmatic argument for belief in intrinsic modal properties might explain how IMP-beliefs could be justified by the methods of science. But, then again, the bulk of Chapter 6 of the book is devoted precisely to addressing this concern. And nothing in Jacquette's review even attempts to rebut the argument of that chapter.

4. Supernaturalism

I turn now to Jacquette's objections against the final part of the book—the part that suggests that only supernaturalism, or a theistic-friendly version of intuitionism, will be able overcome the sorts of objections I have leveled against naturalism. The discussion here will be brief.

Jacquette has nothing really to say about the chapter on intuitionism. His main complaint about Part 3 of the book is that

[t]he endorsement of theism and of the existence of an intelligent designer lacks depth in Rea's concluding chapter, because he does not consider theism as thesis or research program in light of the same kinds of objections to which he subjects naturalism. (129)

The reason I regard this objection as deserving a reply is just that it reflects

a fundamental misunderstanding of the goal of the book. In a later passage, Jacqueline says that I 'clearly [have] a theistic axe to grind in metaphysics' and that I want 'to discredit naturalism because it stands in the way of belief in God'. This, together with the passage just quoted, might convey the impression that the book bills itself as, but fails to succeed as, a defense of theism. But this impression is mistaken. Indeed, not only is the book not a defense of theism, it is not even really a defense of *supernaturalism* (a distinction Jacqueline seems not to have appreciated). I made it clear in the chapter on supernaturalism that my remarks there would be programmatic and that a proper assessment of supernaturalism's prospects for avoiding the problems besetting naturalism would require another book. Though I think (and hope) that the argument of the book might somehow contribute to a full-blown defense of both supernaturalism and theism, it is not intended to stand alone as such a defense. It is, as the subtitle indicates, and as I explicitly stated, simply an attempt to explore some of the more interesting ontological consequences of a particular research program—one that dominates the Western academy and is at least widely perceived to be intrinsically hostile to belief in God. Perhaps in Jacqueline's mind the bare affirmation of theism, or the mere production of an anti-naturalistic argument counts all by itself as grinding a theistic axe. If so, I plead guilty; but, by that standard, just about every article and book in the philosophical literature is grinding an axe.

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NOTES

† Thanks to Michael Bergmann and Jeff Brower for very helpful comments on an earlier draft.

1. I should note that I do not myself take dualism to be an unpalatable view; but most naturalists do. Moreover, I have argued that there are still further unpalatable consequences. See Rea 2005.

2. A set of methodological dispositions is maximal just in case it is possible to have all of the dispositions in the set but it is not possible to have all of them and to have other methodological dispositions as well.

3. Jacquette colorfully characterizes this view, together with the very distinction between thesis and research program, as a 'postmodernist wolf dressed up in analytic sheep's clothing'. He doesn't explain this remark, or even really try to defend it; but the remark would at least seem to suggest that (a) my view is pernicious; (b) I have tried to mask its perniciousness; and (c) it is somehow connected with postmodernism. As noted below, however, I do not deny (in fact, I explicitly affirm) that for all we know, there might be some one research program that is, objectively speaking, the research program that every right thinking person objectively ought to adopt. That by itself should suffice to cast doubt on, if not acquit me of, the charge of being a postmodernist. As to whether my view is pernicious: here we have bare assertion on Jacquette's part. Nowhere does he offer *argument* for that charge. Likewise with the claim that I have somehow dressed up a pernicious view in such a way as to make it appear innocent.

4. But, just in case, we can define a *philosophical thesis* as a thesis with philosophical content. As for the definition of 'thesis', the 4th definition listed in the OED will do: "a proposition laid down or stated, esp. as a theme to be discussed and proved, or to be maintained against attack; a statement, assertion, tenet."

5. Here and in what follows I construe justification as *epistemic* justification unless otherwise indicated.

6. And from the denial of T2, it is a relatively short step to dualism and (perhaps) solipsism. Naturalists ought to reject T2, but they ought to accept T1 and T3 (since scientific theories quantify over material objects, and T1 is a conceptual truth). So far as I can tell, however, the only available views about modal properties that are consistent with the denial of T2 are those that make modal properties mind dependent. But if we accept some such story, it follows that if minds are material objects, their modal properties are mind dependent, which (I argue) is impossible. Thus minds are not material objects: dualism is true. But, I argue, if dualism is true, standard anti-dualist arguments which many naturalists endorse (e.g., explanatory impotence arguments) then turn their teeth against belief in other minds.

7. The remarks about scientific justification that I offered earlier in this section (which basically duplicate remarks I made in the book) should make that clear; but the strongest evidence, I think, comes from passages in which I explain in some detail that the coherence-based method of wide reflective equilibrium is compatible with naturalism so long as its reliance on 'considered judgments' is not construed as treating rational intuitions as evidence. (2002: 70 – 71)

8. Elder has since developed his view in much fuller detail and at book length in his 2004; but it is beyond the scope of this paper to explore the question of whether and to what extent the objections I raise in *World Without Design* apply to the newly developed view.