ONTOLOGICAL NATURALISM*

BY

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Abstract: Ontological naturalism is the view that our best construal of what there is, is what science says there is. This paper argues that while such a doctrine is very appealing, unfortunately, determining what there is, is neither as simple, nor as straightforward, as ontological naturalism would have it seem. Determining what there is, it is claimed, involves three steps. First, one must decide which part of scientific discourse should be taken as true. One must then regiment that part of scientific discourse, and finally, one has to apply a criterion for ontological commitment to that regimented discourse. This paper will concentrate on the first and third of these tasks. It is argued that resolving the tasks these steps put before us requires the employment of *philosophical* maneuvers, that is, maneuvers that neither originate within, nor are ratified by, the practice of science. But these are maneuvers that the ontological naturalist is not entitled to. Thus, ontological naturalism is not a feasible doctrine.

I. Introduction

On one very simple reading, naturalism is the view that our best construal of what there is, is what science says there is. And so, to find out which entities we should take to exist, all we need to do is look toward scientific practice. We should take protons and electrons to be real if physics tells us they're real, and we should take numbers and sets as real if mathematical practice says so.[1] Witches, elves, and phlogiston, on the other hand, are not to be taken as real; and this is simply because they do not play a role in science.

To distinguish this way of understanding naturalism from other possible versions, I'm going to call it "ontological naturalism". Ontological naturalism is in many ways an appealing doctrine, and much can be said in its favor. For instance, Ellis (1990) reminds us that concerning science,

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there is no other body of knowledge which is as well supported or attested, as thoroughly checked, as precise and detailed in its predictions, as comprehensive and systematic in its explanations, or as satisfying intellectually.[2]

In addition, and more importantly, science is supposed to give us "objective knowledge of the world".[3] And shouldn't it then follow, that if science treats an entity as real, this provides the best reasons possible for us to also take it as real? As Ellis puts it: "One would have to have very good reasons indeed, or be very arrogant, not to accept the scientific viewpoint on questions of ontology as the best there is".[4]

Furthermore, ontological naturalism seems to make the enterprise of ontology both simple and tidy: in order to find out what there is, we needn't get entangled in any awkward metaphysics. Rather, there are only three basic steps necessary in order to read a set of ontological commitments off of science:

(1) Determine which part of scientific discourse should be taken as true.
(2) Regiment that part of scientific discourse.[5]
(3) Apply a criterion for ontological commitment to the result of (2).

In this paper, I will focus on steps (1) and (3). A closer examination of these will show that reading one's ontological commitments off of science is, unfortunately, neither as simple, nor as straightforward a matter as ontological naturalism would have it seem. Resolving the tasks these steps put before us requires the employment of *philosophical* maneuvers, that is, maneuvers that neither originate within, nor are ratified by, the practice of science.

But alas, while such philosophical maneuvers appear to be crucial to accomplishing those tasks, they are maneuvers that by his own admission, the ontological naturalist is not entitled to. And that is precisely what makes ontological naturalism much too restrictive; it makes it a doctrine so restrictive, in fact, that philosophers end up deserting it despite themselves (we will look at a number of examples in the next two sections).

These considerations, moreover, raise the deeper question whether there really is a scientific viewpoint on ontology, that is, whether scientific discourse actually impels a unique set of ontological commitments. However the answer to that question may turn out, the observations made here indicate that, sad as it may be, the simple and appealing version of ontological naturalism described in the opening paragraph will have to go.

II. Truth

Let us begin by concentrating on the first of the three steps mentioned above and ask ourselves which part of scientific discourse must actually be taken as true. This question is well motivated, for if we *don't* take a

theory to be true, it doesn't matter *what* sorts of entities that theory commits us to. Thus, figuring out *which* scientific theories must be accepted as true is clearly the first task to be accomplished.

One would think that this task is easy, at least for the ontological naturalist: just take what scientists say at face value. If scientific practice says that theory X is true, then theory X must be accepted as true by philosophers as well. End of story. But, as philosophers well know, the story does *not* end here, because a number of philosophers believe that some scientific theories, even indispensable ones, are not true. These philosophers argue that their reasons for not taking these scientific theories to be true arise from scientific practice *even though* scientists themselves do not actually treat such theories as false (and might even object to doing so).

Let me give an example: Nancy Cartwright, in her 1983, argues that only phenomenological laws (which may contain theoretical terms, but only ones that *describe* phenomena) are candidates for being true, whereas fundamental laws (which try to *explain* phenomena by invoking the theories we have about the entities these theoretical terms denote) are not.[6] That is, for Cartwright, the fundamental laws of physics, such as the law of gravitation, are, strictly speaking, false. She claims that when it is stated correctly, the law of gravitation contains a ceteris paribus clause, since it applies to phenomena only when no other forces are at work. But since, in nature, other forces are always at work, the law of gravitation can only explain in ideal, and not in actual, circumstances. And while such "high level explanations of theoretical science . . .", "organize . . . the unwieldy, and perhaps unlearnable, mass of highly detailed knowledge we have of phenomena . . .", "organizing power has nothing to do with truth".[7]

This view is *not* shared by physicists. In the introduction to his *Lectures on Physics* (1963),[8] Feynman makes a distinction between laws that are only *approximately* true and laws that *are* true. For instance, the "constant-mass" law is only approximately true. Other laws, such as those of relativity theory and four-dimensional space-time, are not approximately true, but simply *true*.[9]

On Cartwright's picture, Feynman's way of distinguishing between true and approximately true laws, and more importantly, his sorting of which laws fall into which category, is lost. For example, for Cartwright, relativity theory would certainly not be true, because it involves fundamental laws. Thus, with regard to truth, relativity theory is indistinguishable from a theory about phlogiston. And while Feynman does not explicitly distinguish the truth content of phlogiston theory from that of relativity theory, it seems obvious that he would.

While the reasons Cartwright gives for treating certain scientific theories as true derive from her understanding of confirmation (in particular, whether the fundamental laws of physics are actually confirmed), such is not the case with other philosophers. Often, they are driven to reject

scientific theories by (a priori) worries over ontology, which oddly reverses the original direction of this undertaking. These philosophers claim that parts of scientific discourse must be treated as false because in their view, the entities referred to in that discourse could not possibly exist.[10] What this shows is that step (1) – determining what part of scientific discourse should be taken as true, and step (3) – applying a criterion for ontological commitment to that discourse, are not actually independent from one another. Instead, what happens in step (3) determines the answers philosophers give to step (1).

Most of the reasons philosophers give for not taking certain entities to exist are quite familiar, and they *are* philosophically substantial enough to put in doubt the claim that adherence to such reasons is compatible with ontological naturalism. Scientific theories refer to numbers and sets for example, which, on most views, are abstract objects. Hence – again on most views – they are not spatial, not temporal, and they don't have causal powers. It follows, so the argument continues, (1) that it is unclear how these entities could ever have any impact on us, (2) that it is unclear how we could ever know about such entities, and/or (3) that it is unclear what we could *possibly* take as evidence that the properties we attribute to those entities are also properties these entities indeed have. The conclusion: if we can get away without positing such entities, then that is certainly what we must do. Therefore, we cannot treat the theories these entities play a role in as true.

Here's an example: Famously, Field (1989) considers himself a fictionalist about mathematics. He defines a fictionalist as someone who "does not regard . . . [mathematical sentences], taken at face value, as literally true".[11] Thus, for a fictionalist, mathematics is true only within the story of mathematics in the same way that a statement of fiction, such as "Watson is Sherlock Holmes' best friend" is true only within the story of fiction, but otherwise false (since Watson and Sherlock Holmes, at least by most philosophers, are thought to be nice examples of non-existing entities). Motivated by epistemic scruples of roughly the sort I alluded to above, together with the view that statements that are indispensable to science are ontologically committing, Field sees himself as forced to abjure "all appeal to mathematical entities in explanations when the chips are down: it must be possible, to develop theoretical physics without any appeal to mathematical entities".[12]

In the actual practice of mathematics, however, we certainly do find that some statements are taken as simply true (and not merely as true within the story of mathematics). Fermat's last theorem is now considered true (while its negation is not). Alternatively, "there are at least three prime numbers greater that nine" is said to be true (whereas "there are only three prime numbers greater than nine" is not). And no textbook in mathematics features the distinction between true, simpliciter, and true

within the story of mathematics that Field speaks of. This distinction, therefore, is an imposition on scientific practice (and one mathematicians would be entirely unfamiliar with).

Here's another example: Ellis (1990) distinguishes between theory that "describes the processes which may be supposed to underlie physical phenomena" and theories that refer to "ideal types [of entities]". The first type of theory provides what he calls "causal process explanations", and these are candidates for being true. Further, if these theories are true, then the entities referred to in those types of explanations exist. The second type of theory provides model-theoretic explanations, and since the ideal types of entities these explanations refer to would have to have properties that "no ordinary physical system does, or even could, possess",[13] such theories are not to be taken as literally true and the entities referred to in these theories do not exist.[14] Examples of model theoretic explanations, according to Ellis, are explanations that make use of the laws of mechanics, the laws of Euclidean Geometry, and the laws of set-theory.[15]

Although the picture that emerges from Ellis' project is very similar to the one Cartwright paints, his motivations are different. While Cartwright is concerned with whether or not fundamental laws are actually confirmed, Ellis clearly worries that we would be committed to a number of unwanted entities if we took model-theoretic explanations to be literally true. What is important, however, is that the charge that can be leveled against Cartwright – that her views about the truth of scientific laws are not consistent with scientific practice – applies to Ellis as well, and for much the same reason: the distinctions he makes cannot be found in scientific practice.

This further illustrates that Field's and Ellis' views on what scientific claims are true cannot really originate from scientific practice. Rather, the distinctions these philosophers make are *imposed onto* the discipline, because of worries over the ontology we would be committed to if we took what scientific practice says at face value: one that includes, for instance, all the entities referred to in fluid mechanics, Euclidean geometry, set theory, and number theory.[16]

At this point it should come to no surprise, then, that scientific practice does not, perhaps cannot, play the dominant role in determining which scientific theories philosophers take to be true. What physicists and mathematicians say about which theories are true is, in fact, often overridden on other methodological grounds. Scientific theory then has to be reinterpreted accordingly, that is, certain parts of scientific theory that scientists treat as true now have to be labeled false. And in order to make such labeling compatible with the actual practice of science, one might then be forced to say (along with Field) that certain parts of science, or mathematics, are only true within the story of science, or the story of mathematics. But while the reasons for thus rewriting scientific doctrine may be serious enough to give us pause, they are not compatible with ontological naturalism.

III. Ontological Commitment

Not too long ago, reading ontology out of science was thought to be simple at least in one respect: once it had been determined which parts of scientific discourse were to be taken as true, all one had to do is regiment that discourse and then see which items in that discourse were quantified over. These items were then taken to be the only ones that discourse is committed to. (Or: if we follow the path laid out by Field, once it has been decided which entities we should not take to exist, all the theories that quantify over those entities are treated as false.)[17]

Recently, however, this straightforward approach to ontological commitment has taken some hits. For instance, Yablo (2000) argues that sometimes, the quantifiers are used metaphorically, not literally; and worse, that these metaphors are sometimes essential, that is, it isn't possible to rewrite discourse in such a way that this metaphorical use of the quantifiers disappears.[18] And "not even Quine thinks that figurative quantification is ontologically committal".[19]

A discussion of the idea that the quantifiers do not have to have ontological force can also be found in Azzouni's 1997, where he suggests that Tarski's formalism could be adopted "*without* the standard ontological interpretation that usually accompanies it".[20] In his 2004, Azzouni amplifies his argument that, once scientific discourse is regimented, the resulting objectual quantifiers need not be seen as ontologically committing.[21]

But perhaps all this recent fuss over the correctness of Quine's criterion can be circumvented if we just resort to the ordinary English language. We might say that ordinary English "there is . . ." statements simply assert the existence of the objects the phrase "there is . . ." refers to because, after all, that is what "there is . . ." means. It is therefore *unnecessary* to apply a criterion for ontological commitment such as Quine's to a discourse. Ordinary English "there is . . ." statements *already commit us to the existence of the objects these statements are about*. On this view, regimentation of these statements is entirely unnecessary, at least for the purposes of recognizing ontological commitment.

In point of fact, however, philosophers cannot simply resort to ordinary English "there is . . ." statements to figure out where our ontological commitments lie. In his 2002, Varzi provides a number of examples (some familiar, some new) that show that ordinary English is full of "ontological traps".[22] Here's a sampling:

(1) There is a difference in age between John and Tom.[23]
(2) There is a strong chance that Professor Moriarty will come.[24]
(3) There is a hole in this piece of cheese.[25]
(4) There are many virtues which Tom lacks.[26]

Taken at face value, these would seem to commit us to age differences, chances, holes and virtues. But many philosophers, the list of which surely includes Cartwright, Ellis and Field, would *not* want to be committed to such entities. While they would want *some* "there is . . ." statements to be taken as ontologically committing (such as, perhaps, "there is a cow blocking the road"), they wouldn't feel this way about statements talking of entities of the sort provided in the examples above.

In light of this, perhaps we could sort ordinary English "there is . . ." statements into two categories. Category A would be the set of statements that are to be taken as ontologically committing, and category B the set of statements that aren't.[27] The statements in category A (i.e. statements (1)–(4) above) would then have to be suitably paraphrased. According to Varzi, (1), if paraphrased, might become:

It is not the case that John is as old as Tom.[28];

(2) could be turned into:

It is very likely that Professor Moriarty will come.[29];

(3) might turn into:

This piece of cheese is perforated.[30];

and (4) could become:

Tom might conceivably be much more virtuous than he is.[31]

As Varzi argues, however, the tools we have available for the kind of linguistic analysis sketched out here "are pretty useless unless we *already* have a pretty good idea of what sort of [ontological] inventory we want to draw up, and of how we are going to count our items".[32] But to presuppose an already established ontology, of course, is verboten to the ontological naturalist unless it pretty clearly arises from ordinary language. Therefore, resorting to ordinary English is not a move that is available to him.

It seems, then, that we are thus required to return to Quine's criterion and face the challenges leveled against it. To make matters worse, in addition to the criticisms that are raised to the criterion directly, it appears that Quine's criterion has alternatives. In his 1951, Quine himself explores, but then rejects, one other alternative to his: reading the ontological commitments off the nouns in one's theory.[33] Of greater interest to us, however, is the idea that one could coin a predicate, say 'is causally efficacious', 'is observable', 'is mind-independent' or 'is concrete' and

take a theory to be committed to all and only those entities that fall under this predicate.

The most recent version of this suggestion, and one that explicitly targets Quine's criterion, can be found in Azzouni's 1998.[34] His thought is that scientific discourse that is *not* regimented "*already has* one or more phrases which are taken to carry the burden of ontological commitment".[35] The predicate 'is causally efficacious' (or some other such predicate) would be an example. When we then go on to regiment that discourse, we would develop a predicate that would play the role the predicate 'is causally efficacious' plays in non-regimented discourse.[36] This does not mean that we could no longer use the quantifiers. It *would* mean that they no longer have an ontological role (recall the earlier observation that Azzouni does not think objectual quantifiers need to be ontologically committing).

But why should this 'predicate approach' to ontological commitment concern us? Moreover, why should anyone favor this approach over Quine's criterion? Well, assume, for example, that we took it that only entities with causal powers exist. This might be appealing for a number of reasons, as we've seen in the discussion of Cartwright, Ellis and Field in section II. If one only took entities with causal powers to be real, then focusing on the predicate 'is causally efficacious' would be a better way of reading ontological commitments out of science than using Quine's criterion, for it offers a way out of the possible conundrum of having to label as false all the sentences of scientific theory that involve quantification over objects without causal powers (such as numbers or sets). Discourse about causally idle entities would no longer have to be ostracized from scientific theory. Rather, we could now leave it up to scientists to decide which theories (or which parts of theories) are true. Quantification over acausal entities would be fine, so long as we did not interpret the quantifiers as having ontological force.

I think that the idea of a theory being committed to whatever falls under the predicate 'is causally efficacious', rather than whatever falls under the quantifier looks very promising indeed. But it also illustrates something that is, at least for our purposes, more important: taking one or other criterion for ontological commitment as the correct one implies that we "already have a pretty good idea" about what sorts of entities we think exist, or don't exist.[37] In fact, it's those metaphysical views that we would employ in justifying the correctness of one or other such criterion. And that, again, is bad news for ontological naturalism because *it* is the view that matters are just the other way around.

What illustrates this moral *graphically* is that examples of the predicate approach to ontological commitment can easily be multiplied. If one believed that all and only concrete objects existed, for instance (that, incidentally, may well come to the same as the view that only objects with

causal powers exist, but I will treat it as a separate option for illustrative purposes), then one might prefer to use the predicate 'is concrete' to carry ontological commitment. And so on, and so on. In fact, a predicate (or a set of predicates) of this sort could be coined to fit any view about ontology whatever.[38] This would leave us with rather a plethora of criteria for ontological commitment to adjudicate between; and the only decisive reasons for or against these, so far as I can see, would come from the arena of metaphysics, not science. That is, it is the *ontology* we take to be the correct one that tells us which of these *criteria* for ontological commitment is the right one. The reason metaphysics suddenly takes precedence over science is that we cannot read an ontology out of science until we have such a criterion.

Let's take it into evidence, then, that not only are there alternatives to Quine's criterion for ontological commitment, but that there are also some reasons for taking them seriously. This creates an interesting dilemma for the ontological naturalist, for he is now required to adjudicate between them, but the only resources available for doing so stem from the metaphysical arena: in order to know which criterion for ontological commitment is correct, he needs to have decided which entities he wants to take as real. Alternatively put, for him to be able to read the ontological commitments off of science, he needs a criterion for ontological commitment, but before he can know *which* criterion for ontological commitment is the right one to use, he needs to have an ontology *already in place.*

IV. Conclusion

At first glance, ontological naturalism really did seem rather appealing. This is mainly because it promised an escape from the metaphysical complications that can so easily arise from trying to decide what there is. Recall that according to ontological naturalism, the scientist is to determine ontology for us, and we philosophers are simply going to heed his commands, and not the reverse.

Just this kind of sentiment is expressed by Maddy (1997) when she claims that "if our philosophical account of mathematics comes into conflict with successful mathematical practice, it is the philosophy that must give".[39] But I think the observations in this paper show that this dictum cannot be observed. Not only is it not true that when scientific practice and philosophy conflict, philosophy must take the back seat, it doesn't even seem possible.

Ontological naturalism, as I've here defined it, is not feasible. While its simple and straightforward appeal to scientific practice is very attractive, one cannot simply read off ontology from science. Rather, matters are the

other way around: we first need to settle some undeniably philosophical questions. We must decide which parts of scientific discourse are true, and (or) which criterion for ontological commitment is correct. Such philosophical maneuvering undercuts the ontological naturalist's official strategy. Philosophy may well be continuous with science, as Quine claimed, but this can't mean that philosophers can retire from doing metaphysics.

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NOTES

[*] I would like to thank Jody Azzouni and Rohit Parikh for helpful suggestions.

[1] I here take the view that, since science includes a great deal of mathematical doctrine, that doctrine, at least, should be considered part of science. Whether that means that pure mathematics, too, should be included, is a difficult issue. Maddy (1997, p. 193), for instance, believes that "there is no principled distinction to draw between mathematics . . . and science". But the arguments in this paper do not turn on how this issue is resolved.

[2] Ellis, 1990, p. 19.

[3] Ibid.

[4] Ibid.

[5] This step and the previous one belong together, but I am separating them because I subsequently focus only on (1). Regimenting a discourse has its difficulties, as the literature in that area can attest to, but those difficulties are not the subject of this paper.

[6] See her 1983, pp. 1–3, for this distinction.

[7] Cartwright, 1983, p. 87.

[8] Feynman, 1963, Part 1, p. 1.

[9] Feynman, 1963, p. 1–2.

[10] Quine, I think, would not have been happy with this development.

[11] Field, 1989, p. 2.

[12] Field, 1989, p. 6. See also pp. 8, 14, 25 and 26.

[13] Ellis, 1990, p. 21.

[14] Ellis, 1990, pp. 59 and 21/22.

[15] Ellis, 1990, pp. 21–27.

[16] According to the philosophers discussed here, there is an explanation as to why scientists so happily engage in the exploration and development of theories that aren't to be taken as true. First of all, they only engage in the exploration and development of *some* such theories, of course. The laws of mechanics, for instance, are considered worthwhile, whereas the theory about phlogiston isn't. The distinction between the two is this: some theories are useful even though they aren't to be taken as true. For Cartwright, fundamental laws still have explanatory power, and Ellis says the same of model theoretic laws. Field holds that mathematics, while false, is conservative (that is "consistent with every internally consistent theory about the physical world", but not necessarily true – 1989, p. 58), as well as being interesting, elegant, and having "applications outside mathematics" (1989, p. 4).

[17] Actually, this is an interesting point. Which direction *does* the argument take? For an ontological naturalist, of course, the direction must be the first. Scientists tell us what's true, and the quantifiers then tell us what exists. But as the previous section indicates, steps (1) and (3) are not so easily separated from one another.

[18] Yablo, 2000, p. 304.

[19] Ibid.

[20] Azzouni, 1997, p. 206, footnote 19 (emphasis added). He takes this up again briefly in his 1998, p. 3.

[21] Azzouni, 2004, p. 52 ff.

[22] Varzi, 2002, p. 3.

[23] Varzi, 2002, p. 4.

[24] Varzi, 2002, p. 5.

[25] Varzi, 2002, p. 5.

[26] Varzi, 2002, p. 6. For other examples, see Hale, 1987, p. 22.

[27] There are objections to this strategy. See, for instance, Azzouni, 2004 (Part I. Section 3). We will pursue it nevertheless.

[28] Varzi, 2002, p. 5.

[29] Ibid.

[30] Ibid.

[31] Varzi, 2002, p. 6.

[32] Varzi, 2002, p. 3. Furthermore, Azzouni (2004) argues that even if an ontological inventory can be taken as common ground, this strategy of paraphrasing still won't work for all cases.

[33] Quine, 1951, pp. 1–12.

[34] And, more recently, in his 2004, Part 1, section 3.

[35] Azzouni, 1998, p. 3.

[36] Ibid.

[37] The echo of Varzi's words here is deliberate.

[38] Azzouni (1998) discusses a number of these variants of the predicate approach.

[39] Maddy, 1997, p. 161.

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