

# There is No Easy Road to Nominalism

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Hartry Field has shown us a way to be nominalists: we must purge our scientific theories of quantification over abstracta and we must prove the appropriate conservativeness results. This is not a path for the faint hearted. Indeed, the substantial technical difficulties facing Field's project have led some to explore other, easier options. Recently, Jody Azzouni, Joseph Melia, and Stephen Yablo have argued (in different ways) that it is a mistake to read our ontological commitments simply from what the quantifiers of our best scientific theories range over. In this paper, I argue that all three arguments fail and they fail for much the same reason; would-be nominalists are thus left facing Field's hard road.

## 1. The lure of the easy road

Nominalist strategies in the philosophy of mathematics are popular options. They are less ontologically extravagant than their platonist rivals and they do not face the same epistemological problems. They are not problem free though. The most notable difficulty with which they must contend is the Quine-Putnam indispensability argument. Central to this argument is the Quinean ontic thesis that *we are committed to the existence of all the entities we (indispensably) quantify over in our best scientific theories.*<sup>1</sup> There are, of course, many nominalist responses to this argument—many of these involve trying to reinterpret mathematical discourse, or attempting to show that mathematics is not in fact indispensable to our best scientific theories.<sup>2</sup> Perhaps the most significant of the latter approaches is Hartry

<sup>1</sup> This is an argument for platonism based on the indispensable role quantification over mathematical objects plays in our best physical theories. From a broadly naturalistic and (epistemological) holistic standpoint, mathematical objects are on a par with other theoretical posits of our best scientific theories. This argument thus presents serious problems for any scientific realist who is inclined towards nominalism. See Quine 1981a, Putnam 1971, and Colyvan 2001 for further details.

<sup>2</sup> See Burgess and Rosen 1997 for a very good critical survey of such approaches.

Field's (1980) fictionalism. Field denies that mathematics is indispensable to science and sets about showing how to eliminate quantification over abstracta from our scientific theories. He also demonstrates the conservativeness of platonistic theories over their nominalist counterparts, thus justifying the everyday use of platonist methods in science.<sup>3</sup>

Field's road is not for the faint hearted, though. There are substantial technical obstacles facing Field's nominalisation project (Burgess and Rosen 1997) and these obstacles have prompted some to explore other, easier options. In recent years, a number of quite different nominalist strategies have emerged. The strategies I have in mind attempt to sidestep the Quine-Putnam indispensability argument by admitting the indispensability of mathematics to our best scientific theories, but denying that this gives us any reason to believe in the existence of mathematical entities. Proposals along these lines have been put forward by Jody Azzouni (1997a, 1997b, 2004), Mark Balaguer (1996, 1998), Mary Leng (2010), Penelope Maddy (1995, 1997), Joseph Melia (2000, 2002), Chris Mortensen (1998), and Stephen Yablo (1998, 2002, 2005, 2009).<sup>4</sup> What all these approaches have in common is that they attempt to provide an easier route to nominalism than the hard road mapped out by Field.

In this paper, I will focus on the proposals of Azzouni, Melia, and Yablo.<sup>5</sup> I choose these three not out of any capriciousness, but because I think that they are representative of the recent flurry of activity centred around finding an easy road to nominalism. I also think that these three are strong and insightful proposals and their failures are thus significant. That they should all fail for much the same reason is also significant. As we shall see, all three have been critical of the Quinean ontic thesis. They have suggested (in different ways) that it is a mistake to read our ontological commitments simply from what the quantifiers of our best scientific theories range over. I will argue that

<sup>3</sup> According to Field, mathematical sentences such as 'there exists an even prime number' are false because there are no prime numbers. He thus takes it to be an error to consider such sentences to be true, even though they are true in the (fictional) story of mathematics.

<sup>4</sup> Neither Maddy nor Yablo claim to be advancing a nominalist position, but I include them here because their arguments can be (and have been) put to this purpose, irrespective of their own stances on the nominalism-platonism debate. And Balaguer does not, in the end, endorse the nominalist position he argues for in Balaguer 1996. Still, the nominalist arguments in here belong in this company.

<sup>5</sup> I have discussed Balaguer (Colyvan 2001 and Colyvan and Zalta 1999) and Maddy (Colyvan 1998a and 2001) elsewhere. I have also discussed Azzouni and Melia elsewhere (on the former, see Colyvan 2001 and 2005; on the latter, Colyvan 2002) but it will be instructive to revisit these proposals here.

each of these three proposals cannot succeed without presupposing the success of Field's nominalisation program — or something like it. So in the end, these are not easy roads at all; they are merely interesting detours which ultimately lead back to the hard road. And, as we shall see, they rejoin the hard road at the hardest part. Ultimately it is the hard road where the action is, and it is here nominalists should focus their energies.

Although Azzouni's, Melia's, and Yablo's aims are the same — to find an easy road to nominalism — their arguments are different enough to warrant separate consideration. I begin with Azzouni.

## 2. The epistemic route

Jody Azzouni has been developing a very interesting nominalist strategy for the philosophy of mathematics. Although, in the past, he presented the strategy in rather tentative terms and without explicit endorsement, it is clear that his proposal, if successful, amounts to an easy-road strategy deserving serious attention. In his book-length treatment of the topic, Azzouni (2004) explicitly endorses the nominalist strategy in question. Moreover, in this work, he presents a more detailed and more sophisticated version of the position. It is this presentation I will largely focus on here.

Azzouni's central idea is to distinguish between those scientific posits we ought to take to be real and those to be treated instrumentally. It is important to note that Azzouni is a realist about unobservable entities so he does not take the observable–unobservable distinction to mark the relevant cleavage here. He does admit, however, that there is something special about direct observation. With direct observation as the example of epistemic access, par excellence, Azzouni then considers the important features of this kind of access.

He isolates four conditions direct observation satisfies (Azzouni 2004, pp. 129–36): *robustness*, *monitoring*, *refinement*, and *grounding*. Epistemic access satisfies *robustness* when the access does not depend on the expectations of the epistemic agent; for example, our theory about genetics might prove to be incorrect or might otherwise surprise us by outstripping our expectations. Epistemic access satisfies *refinement* when there are ways of adjusting and refining the epistemic access we have to the posit in question; for example, we can use more powerful microscopes to get a better look at micro-organisms. Epistemic access satisfies *monitoring* when we can track the posits in

question by either detecting their behaviour through time or by exploring different aspects of the posits in question; for example, we can follow a particle via its track in a cloud chamber or we can walk around a mountain to view it from different aspects. Epistemic access satisfies *grounding* when particular properties of the entity in question can be invoked in order to explain how the epistemic access we have enables the discovery of those and other properties of the object; for example, we can identify the heart in a chest x-ray because its relative density means that it appears as a region of greater x-ray absorption and this, in turn, enables us to determine other properties of the heart, such as its size. As should be clear from some of these examples, direct observation is not the only kind of epistemic access to satisfy these conditions. These four conditions can be thought of as generalizations of features of typical direct observation. When we have access to unobservable particles such as alpha particles via a cloud chamber, we find that such access also satisfies these four conditions. Azzouni calls such access *thick epistemic access*. And as a generalisation of direct access, it enjoys the privileged epistemic status of the latter.

Now contrast thick epistemic access with the kind of access we have via the role an entity plays in a scientific theory enjoying the usual aesthetic virtues of simplicity, familiarity, and so on. With Azzouni, let us call such theoretically-motivated access *thin epistemic access*, and contributing to the theoretical virtues of the theory in question ‘paying its Quinean rent’. Entities accessed thinly may play indispensable roles in our best scientific theories, but intuitively they do not have the same kind of privileged status as entities accessed thickly. In addition to paying their Quinean rent, entities accessed thinly must also have a story in place explaining why they are not accessed thickly. For example, we might not be able to have thick access to the black hole at the centre of the Milky Way, but our very understanding of what a black hole is delivers a story of why we fail to have thick access (because black holes do not reflect or emit light). This ‘excuse clause’ turns out to do a lot of work for Azzouni and I will have more to say about it in what follows. Indeed, the excuse clause lies at the heart of Azzouni’s proposal, yet it remains unclear what counts as a legitimate excuse.

The third kind of access Azzouni considers is *ultra-thin access*.<sup>6</sup> Entities so accessed we can think of as *mere posits*; they can be posited

<sup>6</sup> Strictly speaking, it is the access to posits that is thick, thin, or ultra thin, although often it is convenient to speak of the posits themselves as thick, thin, or ultra thin.

by anyone at anytime without regard for reality. The posits of fiction are paradigmatic examples here. They need not play indispensable roles in our best scientific theories and they do not have excuse clauses for why they are not accessed thickly. Now we are in a position to draw the line between what is real and what is not. According to Azzouni, the thin–ultra-thin distinction is the crucial one: posits accessed either thickly or thinly are to be thought of as real.<sup>7</sup> The ultra-thin, unsurprisingly, are not taken to be real, since they do not earn their keep. Azzouni explains how a thin posit can be demoted to ultra-thin, and the difference in attitude towards the two.

Should [a thin posit] fail to pay its Quinean rent when due, should an alternative theory with different posits do better at simplicity, familiarity, fecundity, and success under testing, then we have a reason to deny that the thin posits, which are wedded to the earlier theory, exist—thus, the eviction of centaurs, caloric fluid, ether, and their ilk from the universe. (Azzouni 2004, p. 129)

It is also important to note that if a thin posit fails to deliver its excuse for why it is not thick—even if its Quinean rent is paid—it will also find itself classified as ultra-thin and thus evicted from Azzouni’s ontology.

What we end up with is a way of distinguishing those portions of our scientific theories that are taken to be real, from those that are to be treated instrumentally. Indeed, the cleavage produced is very similar to the causal–acausal distinction. The thick posits are typically entities with which we have causal contact, the thin are typically causal entities required by our best scientific theories but with an excuse as to why we fail to have thick (causal) access to them, and the ultra-thin are typically acausal entities. This rough aligning of the causal–acausal cleavage and the real–instrumental cleavage, presumably, is no accident. Earlier Azzouni (1997a) toyed with the idea of using the former as the means of distinguishing the real from the instrumental. The problems associated with using a causal criterion however, are serious. Indeed, without independent motivation, such an approach is simply question begging (Colyvan 1998b). The beauty of Azzouni’s thick and thin epistemic access approach is that it does not seem to beg the question against platonism and yet, according to Azzouni, it does rule against ontological commitment to abstract entities such as numbers. If all this were to work, we would have a plausible easy road to nominalism.

<sup>7</sup> Recall, that the thin posits require a story about why they are not accessed thickly.

On closer examination, however, there are problems with this approach. First, note that the thick, thin, and ultra-thin distinction is not sharp and yet it needs to be in order to do the work required of it. The point is that epistemic access can have the four crucial features—robustness, refinement, monitoring, and grounding—in degrees. Take refinement, for example. Using a more powerful optical telescope makes a big difference when looking at Saturn, it makes less difference when looking at Alpha Centauri, and even the most powerful optical telescopes make no difference at all when looking at a very distant star whose presence is theoretically established (because, say, it is having an influence on the motion of an observable binary partner). So do we say that epistemic access to Saturn satisfies the refinement condition, epistemic access to Alpha Centauri *partially* satisfies it, and epistemic access to the distant star does not satisfy refinement at all? That seems reasonable enough. But now notice that although Azzouni will presumably accept all three posits as real, he will do so for three quite different reasons. He will accept Saturn as real because it is a thick posit. He will accept the star in the distant galaxy as real because it is thin—the defeasibility condition kicks in to explain why the access is not thick: the star in question is too far away. But what of Alpha Centauri? According to Azzouni, it will be a thick posit. But it will not be as thick as Saturn (or so we are supposing for the purpose of the example). Being less thick, it might plausibly require a partial excuse for not being as thick as we would like. The excuse, of course, is that the star in question is a fair way away (but not too far away for refinement to be impossible). But now this raises a serious question about the strength and nature of the defeasibility condition. It seems that some posits can be borderline thin? And given the above suggestion that the excuse clauses might come in varying strengths—some excuses are better than others—it may well be that the crucial thin–ultra-thin border is not sharp either.<sup>8</sup>

But things get worse for Azzouni. There would seem to be clear cases of entities which do not fall into Azzouni's tripartite classification. Those I have in mind enjoy the Quinean virtues but do not come equipped with an excuse for their lack of thick epistemic access. Let us call the access to such entities *very-thin*. It is worth drawing attention to the importance of Azzouni's excuse clause concerning the lack of

<sup>8</sup> Azzouni might invoke kinds of entities here and suggest that astronomical bodies such as stars and planets are accessed thinly because at least some of them are accessed thickly. Such a move, however, would ride roughshod over the original epistemic motivation for his account. See Colyvan 2001 (pp. 42–5) for a discussion of such a proposal and why it is not promising.

thick access. Recall, that an entity is thin if we do not have thick access to it, but the entity in question pays its Quinean rent and has an excuse for its failure to support thick access. This excuse clause is important in order to avoid obvious counterexamples such as stars and planets outside our light cone. The latter are uncontroversially real but we cannot have thick access to them. They pay their Quinean rent and there is also a well-accepted story as to why we do not have thick access to such stars and planets: they are too far away. But what if the excuse were not forthcoming? What should we say about very-thin posits? Azzouni does not think that there are any, so does not tell us whether to count such posits as real or not. Such posits are thus confined to a kind of ontological purgatory: neither real nor unreal.

Some examples of such posits may help. Consider a 'gap' in the fossil record. This is a creature posited in order to make sense of the standard evolutionary story but with which we have no contact, let alone thick epistemic access. The crucial question is whether there is a story in place as to why we do not have thick access with such creatures. This is crucial because *with* a story, Azzouni is able to deliver the intuitively-correct result that we are justified in taking such creatures to be real; *without* a story, such creatures turn out to be very-thin and thereby sentenced to ontological purgatory. One excuse might simply be that such creatures are now extinct and so cannot be tracked. The fact that they were extant in the past but are now extinct might be all that is required. But this seems too cheap. Surely we want a more substantial story about why such creatures never fell into tar pits or the like. But I take it (or at least we can suppose for the purpose of this example) that we do not have such a story. We are thus faced with two possibilities: (i) either these gaps in the fossil record are accessed very thinly and Azzouni gives us no advice about their ontological status, or (ii) they are accessed thinly because they come equipped with a fairly trivial and obvious story about why they are not accessed thickly.

Now to return to the case of interest: mathematical entities. These are not accessed thickly, on that (almost) everyone agrees. The question is whether they are accessed thinly, very-thinly or ultra-thinly. Mathematical objects (at least *prima facie*) enjoy the Quinean virtues, so they are (at least *prima facie*) not ultra-thin. Whether they are thin or very-thin depends on what can count as an excuse for not being accessed thickly. I have suggested elsewhere (Colyvan 2005) that mathematical objects, being acausal, have such an excuse. But is this excuse acceptable? Unfortunately Azzouni does not give us any guidance; he offers no systematic story about acceptable excuse clauses. Moreover,

the excuse clauses play a central role in Azzouni's account, so independently of concerns about mathematical entities, a well-motivated and detailed account of what passes for an excuse is required.

It is an interesting feature of Azzouni's account that many of our scientific (and historical) posits do not enjoy thick epistemic access: dinosaurs, Gondwanaland, the inflationary phases of the big bang, and Plato, to name a few. (Posits from past times are not able to be tracked and so cannot be accessed thickly.) It is, thus, clear that the issue of what counts as a permissible excuse for lack of thick access is crucial. If Azzouni is fairly liberal about such stories, then the excuse that mathematical entities are abstract may be acceptable. If he is too restrictive, Azzouni risks sliding into some form of scientific antirealism — a kind of presentism, where only present objects can be thought to be real. In any case, whether the account of permissible excuses is liberal or restrictive, it needs to be independently motivated. After all, the ontological status of a large number of our theoretical posits will depend on the excuse clause. And without an independently-motivated account of what excuses are admissible, we have no reason to take mathematical entities as unreal.

So we see that Azzouni's more sophisticated version of his nominalism is at best incomplete and is unable to deliver the promised easy road to nominalism. A little more work on this road is required before it leads anywhere, let alone to nominalism. But it is worth reflecting on Azzouni's motivation and on the less sophisticated approaches of his earlier papers on the topic (1997a and 1997b). Azzouni's main motivation in his earlier articles was the idea that mathematical entities are causally idle and therefore idle *simpliciter*. As I have already mentioned, I think that this idea is still prominent in his current thinking. We have causal stories (albeit complicated and perhaps incomplete ones) about why we do not see fossil records of missing links in evolutionary chains, and for why we cannot track Gondwanaland, but appeal to the lack of causal powers of mathematical entities is not admissible (or so I am suggesting is the natural response for Azzouni at this point). Why should Azzouni avoid appealing to lack of causal power? Well, it would be like explaining the lack of thick access to a fictional character by offering the excuse that the character in question does not exist. The latter would not be a legitimate excuse because the excuse itself gives the game away — it admits that the entity in question should not be taken as real. I suspect that this is why Azzouni will, indeed, resist allowing the lack of causal power to stand as a legitimate excuse. Admitting lack of causal power, according to Azzouni, is to



admit that the entity in question has no ontologically-committing role to play in any theory in which it appears. But such a response is little more than a hunch that total theory will tell us that mathematical entities are not required. Nominalist intuitions and sympathies are not enough here; we require something more. The obvious way to discharge the burden of proof here would be to demonstrate the dispensability of mathematics. The latter requires the success of Field's project (or something like it).

To be clear, Azzouni denies that mathematics is dispensable to our best science. Indeed, that is part of what is distinctive about his and the other brands of nominalism discussed in this paper. Azzouni takes quantification over mathematical entities to be indispensable to science, but that such indispensability gives us no reason to take mathematical entities to be real. My case against Azzouni here has two parts. First, I argued that without a more-detailed account of the excuse clauses, Azzouni is not able to sustain the distinction between thin and very-thin posits that he requires. Without this distinction, his proposal simply does not deliver nominalism. After all, mathematical entities might be thought to come equipped with a ready-made excuse that appeals to one of their important properties: their causal idleness. The second part of my argument was to offer a limitation on the kind of excuse clauses allowable, one that ruled out appeal to causal idleness as a reason for failure to gain thick access. According to this line of thought, mathematical entities are causally inert and therefore the entities themselves do not play any real role in scientific theorising (even though quantification over them may be indispensable). Moreover, this way of fleshing out the permissible excuse clauses is faithful to Azzouni's earlier presentations (1997a, 1997b) of his view, where he explicitly appealed to the causal idleness of mathematical entities as a motivation for nominalism. But the problem with this suggestion is that the causal idleness of mathematical entities does not entail that they play no real role in scientific theorising — at least not without further argument.<sup>9</sup> So we see that, at best, Azzouni's 'easy road' is an interesting, scenic detour that leads back to the hard road at its steepest and rockiest point — the point where the dispensability of mathematics must be demonstrated. At worst, it is a

<sup>9</sup> And as we shall see in the next two sections, mathematics may play an explanatory role in scientific theories, even though mathematical entities are causally idle. This would suggest that mathematical entities are playing a very similar role in our theories to the thin posits. This in turn casts considerable doubt on the prospect of making a principled distinction between mathematical entities and Azzouni's thin posits.

frustrating, incomplete path that dead-ends somewhere short of nominalism.

### 3. The way of the weasel

Joseph Melia (2000) argues, contra Putnam 1971, that it is not inconsistent or intellectually dishonest to quantify over mathematical objects, and yet deny the existence of such objects.<sup>10</sup> Melia points out that often we say things such as:

- (1) All Fs are Gs, except *b*

Melia quite rightly suggests that the way to understand such claims is not as the contradictory claim

- (2) All Fs are Gs but *b* is an F that is not a G

but, rather, as the claim

- (3) All Fs, except *b*, are Gs

Melia calls this strategy of retracting what you have previously said: *weaseling*. In a similar vein, Melia suggests that when a nominalist says something like

- (4) There exists a differentiable function that maps from the space-time manifold to the real numbers, but there are no mathematical objects

she need not be understood as being committed to the obvious contradiction. In short, Melia suggests that we ought to grant some charity in our interpretation of claims like (4).

[J]ust as in telling a story about the world, we are allowed to add details that we omitted earlier in our narrative, so we should also be allowed to go on to take back details that we included earlier in our narrative. (Melia 2000, p. 470)

Why should we wish to take back bits of our scientific story of the world in this way? Surely it is just sloppiness to assert something we do not take to be true, only to retract it later. Whenever we say things like (1) we really ought to take more care and say (3), if that is what we mean. Melia (2000, pp. 468–9) suggests, however, that this is not

<sup>10</sup> Putnam suggests that '[i]t is like trying to maintain that God does not exist and angels do not exist while maintaining at the very same time that it is an objective fact that God has put an angel in charge of each star and the angels in charge of each of a pair of binary stars were always created at the same time!' (Putnam 1979a, p. 74).

always possible; sometimes the only way we can say what we want is by weaseling. He goes on to suggest that this should not overly concern us because we can legitimately weasel away our ontological commitments to abstracta as in (4).

There are problems with Melia's argument here. Perhaps the most serious of which is that if we cannot say what we want any other way except by weaseling, it is just not clear what we are saying. I agree with Melia that (1) is understandable and can be read consistently as (3), but this is, in part, *because* of the availability of (3). Indeed, this is true of any simple retraction, such as those Melia uses to illustrate the strategy in question. We can change the story we are narrating by adding or subtracting minor details, but we can hardly be thought to be telling a consistent story (or in some cases, any story at all) if we take back too much.<sup>11</sup> In short, there are limits to how much weaseling can be tolerated. J. R. R. Tolkien could not, for example, late in the *Lord of the Rings* trilogy, take back all mention of hobbits; they are just too central to the story. If Tolkien did retract all mention of hobbits, we would be right to be puzzled about how much of the story prior to the retraction remains, and we would also be right to demand an abridged story—a paraphrase of the hobbitless story thus far.

So too for weaseling wherever it arises—at least whenever the weaseling in question is radical enough. I simply do not know what to make of sentences such as (4) where no obvious paraphrase presents itself. Moreover, Melia is committed to the existence of such sentences—if not (4), there are other such sentences (Melia 2000, p. 469). The problem we are confronting is that when the weasel oversteps the mark and tries to take back too much (as would be required to purge *The Lord of the Rings* of hobbits, or science of mathematical entities), we no longer have a grip on what is being said.

How does the weasel respond to someone who simply does not understand sentences like (4), except as the obvious contradiction? One way of clarifying things would be to provide the appropriate translation. Indeed, this is the only way of replying that is acceptable. When the weaseling is minor, the translation is fairly trivial so need not be provided. But when we are talking about weaseling away all mathematics in science, the weasel is committed to providing translations of all of current science—not as weasel sentences but in sentences where there is no commitment to mathematical objects.

<sup>11</sup> Or if we add too much, for that matter—when adding we can only fill in the gaps, not add inconsistent detail.

So we see that this easy-road strategy is dependent on the success of a hard-road strategy.<sup>12</sup>

Another reply that the weasel might make to someone who fails to understand the content of sentences such as (4) is to suggest that the weasel sentences are to be understood in terms of nearby possible worlds. So, for instance, consider a weasel sentence without obvious paraphrase:

- (5) The homunculus is making the person cheer at the football game, but there are no homunculi

The first conjunct of (5) has us consider a world,  $w_1$ , where homunculi control people, and it is the will of the homunculi that is responsible for, and explains, the person cheering. In  $w_1$  the homunculi is the reason for the person cheering. The weasel sentence—(5) in its entirety—then invites us to consider the nearest world to  $w_1$ ,  $w_2$ , where there are no homunculi, but the counterpart of the person cheers regardless.

There are a couple of problems with this understanding of (5). First, there is the non-uniqueness of the closest homunculi-free worlds to  $w_1$ . Sentences such as (5), drastically under-describe the situation. There are many homunculi-free worlds and it is not clear which one  $w_2$  is supposed to be. The second problem is that it is hard to understand the purpose of uttering sentences such as (5). After all, any explanation afforded by the homunculi in  $w_1$  is not available in  $w_2$ . It seems that weasel sentences like (5) can neither be descriptive (because of non-uniqueness) nor explanatory (because of the way the explanation is tied to the non-existent homunculi). There simply seems to be no useful role for such sentences in our theories of the world.

But perhaps this understanding of weaseling fares better with sentences such as (4). For a start, it might be thought that the mathematics in question is not contributing to explanation; such sentences are only in the business of describing and here the picture is clear: the physical world is arranged in the way it needs to be for the sentence to

<sup>12</sup> There is a further issue: even if we grant that the nominalist is a weasel rather than an inconsistent hypocrite (Melia's (2000, p. 469) words), this should not be confused with an *argument* for nominalism. At best, it is a way of disarming the Putnam charge of intellectual dishonesty. Better a weasel than a hypocrite, says Melia; I say, better an honest platonist than either. The point is that Melia needs to convince us not only of the *coherence* of the nominalist's position, but he must also convince us that it is *preferable* to the platonist's position. He takes up this issue in the latter part of Melia 2000 and in Melia 2002. I raise an objection to his account in Colyvan 2002. I resist the temptation to revisit this debate here.

come out true. As Mark Balaguer (1998, p. 134) puts it ‘the physical world holds up its end of the “empirical-science bargain”’. The idea is that if mathematized theory is only describing the behaviour of the non-mathematical realm and it gets this largely right, then weaseling away commitment to mathematical entities might not be thought to be problematic. Mathematical entities are, after all, on this account, not doing any essential work. For purposes of merely representing, we might represent the eight planets and the binary dwarf planet Pluto with existing entities (like nine marbles) or with non-existing entities (like the Greek muses). Putting aside the problems I raised earlier—non-uniqueness issues and the descriptive adequacy of the weasel sentences—there is a serious issue about the role mathematics plays in science. The response under consideration depends on mathematics playing no explanatory role in science, for it is hard to see how non-existent entities can legitimately enter into explanations. But this purely descriptive role for mathematics in science cannot be simply stipulated, it needs to be demonstrated (and Melia acknowledges this challenge in Melia 2002). What would it take to demonstrate that mathematics plays no explanatory role in science? The only way I can see to do this is to show that any explanation offered by the mathematised theory is available in a non-mathematised counterpart theory.<sup>13</sup> In order to do this, one is required to provide a mathematics-free version of our current best scientific theories and demonstrate that this theory has the same explanatory power as its mathematized counterpart. But this is the hard road. To be sure, there are some substantial issues here and I will have more to say about them in the next section. If I am right about mathematics contributing to the explanatory power of our best scientific theories, whichever way Melia jumps on justifying his weaseling strategy, his proposed easy road merges with the hard road.

#### **4. The hermeneutic trail**

In a number of fascinating papers, Stephen Yablo (1998, 2002, 2005, 2009) has been developing an anti-realist approach to mathematics. Yablo is more inclined to see his position as an anti-metaphysical position, according to which there is no fact of the matter about whether there are mathematical objects. He is thus neither a platonist

<sup>13</sup> Think of how it was demonstrated that the luminiferous ether was not doing any explanatory work or that phlogiston was not doing any explanatory work—ether-free and phlogiston-free theories with the same or better explanatory resources were tabled.

nor a nominalist. Nevertheless, his approach does open up a path that might be used (and indeed, *has* been used, for example, by Leng 2010) by nominalists in search of the elusive easy road. Yablo's approach draws on the work of Walton (1993) and harks back to a more Carnapian (Carnap 1956a) style of metaphysics. I cannot do justice to all the details of this very rich account here. Instead, I will give a brief sketch of the view, focusing on one feature that is crucial for our purposes. I will then raise a problem for Yablo-style nominalism.

Yablo begins by noting that it would be a mistake to take metaphorical statements (and figurative language, generally) to commit us to the objects apparently quantified over in such language.<sup>14</sup> Take, for example, Bob Dylan's famous line from the song, 'Visions of Johanna', on the 1966 album *Blonde on Blonde*:

The ghost of 'lectricity howls in the bones of her face

Clearly we should not take the statement's existential claims — either explicit or implicit — seriously; there is no reason to entertain the existence of ghosts of electricity, or anything howling in the bones of anyone's face, even if we take the sentence to truly describe the woman in question. The metaphors here are supposed to conjure up an image (and, arguably, refer to the 'spark' of creativity and perhaps to Dylan's own transition to electric music a year earlier).<sup>15</sup>

Next, Yablo notes, with Walton (1993), that we can invoke metaphors (and other forms of non-literal language) to truly describe actual situations. To use one of Walton's examples, we can describe the Italian town of Crotona as being located on the arch of the Italian boot. Here the metaphor draws our attention to the similarity between the shape of Italy and a boot. We then engage in the pretence that Italy is a boot and this pretence allows us to give (more or less) accurate information about the location of Crotona. Moreover, such non-literal language is present in our scientific discourse — average stars, and so on — and such uses are arguably ineliminable.<sup>16</sup> Yablo then argues that there is no clear boundary between the portions of scientific discourse

<sup>14</sup> Unless indicated otherwise, I will mostly be referring to Yablo's influential paper Yablo 1998.

<sup>15</sup> And like all interesting metaphors, the possible interpretations are never exhausted, and require some interpretive work on the part of the reader. As Davidson (1978, p. 29) once suggested '[m]etaphor is the dreamwork of language, and like all dreamwork, its interpretation reflects as much on the interpreter as on the originator'.

<sup>16</sup> At least, it will be very difficult to convey what is intended by claims about the mass of an average star, say, in literal language.

intended literally and those that are merely metaphorical. This leads to a serious problem for the Quinean. Clearly we should only read off our ontological commitments from literal parts of our scientific theories, but if these theories are shot through with figurative language, we need to be able to separate the literal from the figurative, before we can begin ontology. But here is the kicker: according to Yablo, there is no way of separating the literal from the figurative.

To determine our commitments, we need to be able to ferret out all traces of non-literality in our assertions. If there is no feasible project of doing that, then there is no feasible project of Quinean ontology. (Yablo 1998, p. 233)

Yablo only considers descriptive uses of language in science — language intended to describe the state of some system. He does not consider uses of scientific language intended to explain why some system is in a particular state.<sup>17</sup> Does this make a difference? I will argue that it does. It may well be right that metaphorical language intended only to describe, need not carry ontological commitment (or at least, it need not carry the obvious, literal ontological commitments — ‘ghosts of electricity’ and the like), but it is not clear that language intended to deliver explanations can be thought to be free of such commitments.

So let us grant that metaphorical language (and figurative language generally) can be used for purposes of true description, as Walton and Yablo argue. The important question for our purposes is whether figurative language can be explanatory. Take the sentence ‘The coach is unhinged’. This sentence invokes a metaphor to describe certain psychological features of the coach, but it might also be thought to explain why many feel that a change of coach is needed. And, as we have already seen, there is no need to take the ontological commitments of the metaphorical language seriously — no need to expect the coach to consist, in part, of broken hinges or to make noises like a broken shutter. But how can a metaphor, invoking non-existent entities, explain? Answer: The explanation of the metaphor stands proxy for some further real explanation. The real explanation being that a new coach is required because the team and supporters fear that the present coach cannot be relied upon to make sensible and rational decisions (where the latter, in turn, stands proxy for a more complicated story about the coach’s cognitive

<sup>17</sup> In particular, in Yablo 2009, when discussing mathematical entities, Yablo does not consider the possibility that mathematical entities may feature in scientific explanation.

states and capacities). The important point to note here is that, to the extent that the metaphor is explanatory, any explanation delivered by the metaphor is really just standing proxy for another more complicated explanation. In any case, the ontological baggage of the metaphorical explanation, the hinges, for example, *do not play any essential role in the explanation*.

This raises the question of whether there are cases where fictional entities, invoked by a metaphor, carry some of the explanatory load. Yablo argues for a number of different ways in which metaphors are essential, but one way he does not consider is: *metaphors essential for explanation*. Suppose you want to explain why someone has the particular facial expression she does, and you answer that it is because the ghost of electricity howls in the bones of her face. Clearly this will not do. For the metaphor to function as an explanation, either there must literally be a ghost of electricity howling in the bones of her face, in which case it is not a metaphor at all (and it is presumably straight-forwardly false), or it is not an explanation because it relies on non-existent entities. It seems that metaphors can carry explanations only when the metaphor in question stands proxy for some non-metaphorical explanation. It is hard to see how there could be metaphors essential to explanation. At least, as things stand, there is no reason to believe that there are any such cases.

My suggestion is that when some piece of language is delivering an explanation, either that piece of language must be interpreted literally or the non-literal reading of the language in question stands proxy for the real explanation. Moreover, in the latter case, the metaphor in question must clearly deliver and identify the real explanation. It is important to note that I am not denying that explanations invoking metaphors abound. What is at issue is whether there can be genuine explanations essentially invoking metaphors—that is, where the metaphor is not standing proxy for the real explanation.

Take, for example, an explanation for why someone changed his or her career: the stock market crashed.<sup>18</sup> The crash of the stock market is, indeed, a perfectly respectable explanation and, moreover, it is an explanation that invokes metaphorical language. What is not clear, however, is whether the metaphor is standing proxy for some more literal description of the events in question. The difficulty is that cashing out the metaphor in literal language is not so easy here. After all, a stock market crash is a very complex series of events and

<sup>18</sup> Thanks to Jody Azzouni for suggesting this example and for pushing me on the issue.



is arguably much more than merely the loss of value on a large proportion of stocks. Be that as it may, there must be at least a partial translation of the metaphor in question that engenders the explanation. This is not an argument, I know, but I just cannot see how — on any account of explanation — metaphors can explain without at least some understanding of the literal meaning of the metaphor. I am not suggesting that metaphors can be completely cashed out in non-metaphorical language; I take it that accepted wisdom on this issue is that they cannot, and I am inclined to go along with this accepted wisdom. All that I am claiming is that some partial, literal translation of the metaphor is carrying the explanatory load.

To see how this works, let us return to the example of the stock-market crash. It might be that the person in question changed their career because the particular industry they worked in found itself in financial difficulties. As a result, most companies in the sector were unlikely to be hiring or offering career advancement opportunities in the near future. And all this was a result of the stock market crash. At no stage did we require a full, literal translation of the metaphorical expression ‘stock market crash’, but we did require the partial translation that involved many industry sectors being placed under financial stress and that this was the motivation for the change of career of the person in question.<sup>19</sup> Without any, even partial, translation, it is hard to see how the metaphor can be explanatory. Indeed, it is crucial to the explanation here in terms of the stock market crash that we have some idea of what a stock market crash involves, even though none of us has a full (literal) understanding of stock market crashes in their full detail.<sup>20</sup>

If all this is right, we have the makings of an at least partial response to Yablo’s challenge to mark the boundary between the literally true

<sup>19</sup> The translation here is not literal — ‘financial stress’ is still metaphorical — but this need not concern us. All we require is a partial translation of these terms as well, so that in the end we arrive at a partial, literal translation of the original metaphor that is capable of carrying the explanatory load.

<sup>20</sup> Perhaps similar concerns can be raised about metaphors in descriptive roles. At least, you might want to know why I am focusing on explanation and prepared to concede so much to Yablo on the descriptive role of metaphors. Apart from anything else, many believe that metaphors without literal translation *can* carry descriptive content. For the purposes of the present debate, I will grant this and instead press the point in terms of explanation. Indeed, I think a better case can be made in terms of explanation since it is very common among scientific realists, at least, to take explanations to be ontologically committing. The case for descriptions being ontologically committing is in some ways more controversial, in part for the reasons that Yablo gives, and in part because of concerns about reading too much into representations. See, for example, Russell (1923, p. 62) on what he calls ‘the fallacy of verbalism’ in relation to ontological vagueness.

parts of our theory and the figurative: whenever we have an explanation we ought to treat the language in question as literal and thus as being ontologically committing, unless the explanation invokes a metaphor which is standing proxy for some other real explanation. It remains to show that there are cases in scientific discourse where mathematics features in explanations. If I can show this, then a nominalist tempted by Yablo's hermeneutic path will have one of two options: (i) provide suitable and well-understood translations of the mathematical explanations I offer, or (ii) show why the alleged explanations in question are not really explanations at all. If neither of these is possible, we have good reason to accept the explanations at face value and to take their ontological commitments seriously.

Elsewhere, I have given a number of examples where mathematics seems to carry a significant portion of the explanatory burden (Colyvan 1998b, 2001, 2007, Lyon and Colyvan 2008).<sup>21</sup> Here I will be content to sketch one example that illustrates the central role mathematics can play in scientific explanation. The Kirkwood gaps are localized regions in the main asteroid belt between Mars and Jupiter where there are relatively few asteroids. The explanation for the existence and location of these gaps is mathematical and involves the eigenvalues of the local region of the solar system (including Jupiter).<sup>22</sup> The basic idea is that the system has certain resonances and as a consequence some orbits are unstable. Any object initially heading into such an orbit, as a result of regular close encounters with other bodies (most notably Jupiter), will be dragged off to an orbit on either side of its initial orbit. An eigenanalysis delivers a mathematical explanation of both the existence and location of these unstable orbits (Murray and Dermott 2000). It is interesting to note that we can seek out a non-mathematical, causal explanation for why each particular asteroid fails to occupy one of the Kirkwood gaps. Each asteroid, however, will have its own complicated, contingent story about the gravitational forces and collisions that that particular asteroid in question has experienced. Such causal explanations are thus piecemeal and do not tell the whole story. Such explanations do not explain why *no* asteroid can maintain a stable orbit in the Kirkwood gaps. The explanation of this important astronomical fact is provided by the mathematics of eigenvalues (that is, basic functional analysis).

<sup>21</sup> See also Alan Baker's (2005) discussion of this for a very nice biological example.

<sup>22</sup> More carefully, the explanation involves the eigenvalues of the relevant operator associated with the system in question (under a suitable mathematical description).

We thus have scientific statements involving mathematical entities (the eigenvalues of the system) explaining physical phenomena (the relative absence of asteroids in the Kirkwood gaps).<sup>23</sup>

In effect, I have argued that even though the literal–figurative distinction might not be sharp, that does not mean that there is no distinction to be had. When we are dealing with explanations, presumably, we are dealing with literal scientific language, and here we find reference to mathematical entities. That is enough for present purposes.<sup>24</sup> So a nominalist motivated by Yablo’s discussion of metaphor has the two options outlined two paragraphs back. The first option (providing a nominalistically-acceptable translation of any language that features mathematics in explanatory contexts) clearly involves the success of a Field-style nominalization program. But so too does the second option. After all, if one were to disallow explanations such as the one I offered above, one would be obliged to provide a nominalistically-acceptable alternative explanation. For if one’s theory of the world denied many of the usual scientific explanations without offering acceptable alternatives, mystery would be increased. Surely the nominalist is not willing to buy ontological parsimony at the price of increased mystery. So whichever option the Yablo-style nominalist chooses, we are led back, yet again, to Field’s hard road.

## 5. The end of the road

The prospect of an easy road to nominalism is certainly seductive, and there has been no shortage of attempts to find such a road. In this paper, I have considered three such attempts and I have argued that each fails. Moreover, I have argued that each fails for much the same reason: they all require the success of a hard-road strategy such as Hartry Field’s nominalization program. Of course, strictly speaking,

<sup>23</sup> If you are unconvinced by this example, perhaps because you think that the absence of asteroids does not count as a physical event, consider the case of the collapse of the Tacoma Narrows Bridge in Washington in 1940. This wind-induced collapse is generally thought to be explained by the eigenvalues of the operator associated with the physical system in question. Again this is a mathematical explanation. As a result of this bridge collapse, eigenanalyses now feature prominently in modern engineering—especially the engineering of suspension bridge construction.

<sup>24</sup> Although there is still the issue of what to say about entities posited in borderline literal language—perhaps centres of mass and average stars. For all I have said here, Yablo might well be right that there is no way to *fully determine* our ontological commitments.

the failure of the three candidates I have considered in this paper does not show that there is no easy road. But as I have already suggested, these are three of the best shots at discovering such a route to nominalism, so the prospects do not look good. I thus feel entitled to overstate my case just a little and, for the time being at least, proclaim that there is no easy road to nominalism. Would-be nominalists are thus left facing the hard road mapped out by Field.

Another interesting issue to emerge from this discussion is the important role that mathematical explanation plays in the debate. The existence of mathematical explanations of empirical facts raises problems for both Melia's and Yablo's proposals. I also argued that Azzouni needs to do more to demonstrate that science can survive without countenancing the existence of mathematical entities, and this task is all the more difficult if mathematics is playing a central role in scientific explanation. The debate over platonism and nominalism would be genuinely advanced by a better understanding of explanation—especially those explanations that have mathematics playing the leading role.<sup>25</sup>

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