Road Work Ahead: Heavy Machinery on the Easy Road

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Abstract: In this paper I reply to Jody Azzouni, Otávio Bueno, Mary Leng, David Liggins, and Stephen Yablo, who offer defences of so-called "easy road" nominalist strategies in the philosophy of mathematics.

1. Introduction

In "There is No Easy Road to Nominalism" (Colyvan 2010), I argued against three prominent and initially-promising ways of shirking ontological commitments to mathematical entities—so-called "easy roads" to nominaism. I then overstated my case in the title, suggesting that there was no such route to nominalism. The overstatement was not simply a result of a faulty induction based on three cases. Rather, the overstatement was due to the fact that the three attempts at easy roads I considered—those engineered by Jody Azzouni, Joseph Melia, and Stephen Yablo—were among the most well developed and plausible accounts on offer. Moreover, I argued that all three suffered from similar problems. My conjecture was that any such attempt at an easy road to nominalsim would fall foul of these same problems.

The very interesting and thoughtful papers in this volume from Jody Azzouni (2012), Otávio Bueno (2012), Mary Leng, (2012), David Liggins (2012) and Stephen Yablo (2012) all indicate various ways in which some of the problems I raised might be addressed. These five papers thus constitute several attempts at completing particular easy-road strategies. I am not convinced that these attempts are successful but they are pushing in fruitful directions. In any case, these five papers really do help to focus the key issues.

There are many good points made in the papers in question. I cannot do justice to all that has been raised in them so here I will be content to highlight what I take to be some of the most interesting issues and note some of the points of residual disagreement. I do this in the same spirit as each of the five discussants: with a genuine interest in advancing the debates in question. I will address each paper in turn even though, as we shall see, there are some common themes in the five papers.

2. Azzouni

Jody Azzouni clarifies his version of nominalism and, in particular, clears up some misconceptions (at least I had) about the excuse clause in his account. Recall that Azzouni allows ontological commitment to: (i) entities to which we have thick epistemic access and (ii) to entities we have only thin access, but for which there is a suitable excuse clause explaining why such entities are not accessed thickly. The excuse clause does a lot of work and yet I was having trouble getting a grip on what counted as a legitimate excuse. Azzouni's answer (2004, 2012) is that the acceptability or otherwise of the excuse in question must come from within scientific practice. This is surely right. So we have scientific theory positing theoretical entities—the thin posits—then those thin posits lucky enough to find themselves with an excuse clause are then promoted to the status real (or worthy of ontological commitment, if you prefer). For a thoroughgoing naturalist, this two-stage process also needs to find support from scientific practice. Azzouni takes it that there is such support. He suggests that scientists are not satisfied with mere theoretical posits of our best scientific theories (the thin posits). Rather, they want further evidence and if they find obstacles to gathering such evidence, they report the details of the obstacles in question. But it is far from clear that the excuse clause plays such a crucial role in promoting mere posits to fully-fledged ontological commitment.

Consider a case where we are ignorant of why we do not have better access to a thin posit. Gravitational waves may be such a case. We know that gravitational waves are hard to (directly) detect, but I take it that we are not in a position to say why we are having quite as much trouble as we are in detecting them. Azzouni will resist ontological committed to gravitational waves because, despite being theoretical predictions of our best theory of space-time, they do not have an excuse clause (or so we are supposing for the purpose of the argument, at least). But if an excuse arises from within science, Azzouni *would* then allow ontological commitment to gravitational waves. But this is odd. We still do not have anything like direct detection; we are in exactly the same position as before, except that after the excuse is provided, we know why we cannot detect them.

If one is driven by epistemology, as Azzouni clearly is—and I take this to be a strength of his position—it is hard to see what it is about the excuse clause that warrants the promotion of entities from mere posits to real. If thick epistemic access is the gold standard (as it is for Azzouni), it would seem more natural to say that we should resist ontological commitment until we have thick access, full stop. The excuse just tells us why we fall short of the gold standard: why we are stuck with thin access. Alternatively, we might allow ontological commitment to thin posits without the excuse clause. The point is that the excuse clause—even after Azzouni's helpful clarification—does not have the epistemic motivation that the rest of his account enjoys. I see no conflict with scientific practice in the following suggestion: we have good reason to believe in gravitational waves but, all other things being equal, we would like more, perhaps direct, evidence. Were we to discover that such direct evidence is impossible, we would then give up the search. But I do not see why the presence or absence of an excuse should change our ontological attitude to gravitational waves.

A final point about the role of mathematics in science. I claimed that "the causal idleness of mathematical entities does not entail that they play no real role in scientific theorizing" (Colyvan 2010, p. 293). I then went on to suggest that mathematics can play non-causal explanatory roles in science and that this is a real role. Azzouni responds (emphasis in the original):

> It's not (at least not if Colyvan wants to avoid begging the question against those who reject Quine's ontic thesis). Mathematical *terminology* has a real role to play in explanation: the indispensability of mathematical theories to scientific theories guarantees that quantification over mathematical entities will appear in scientific explanations despite the nonexistence of mathematical objects. But the indispensability of mathematical theories to our scientific explanations does *not* automatically correspond to a role for mathematical objects themselves. For there are no such objects, even if quantification over them is indispensable: this is the position in logical space that rejecting Quine's ontic thesis allows. (Azzouni 2012, p. ??)

Perhaps we reach a stand off here. Azzouni takes it that it is possible to get all the benefits of the mathematical explanations I offer from the notation alone. I am inclined to take the scientific explanations in question literally and see them as cases of mathematics (not just mathematical notation) doing the heavy lifting in the cases in question. I will not say any more about this here, because the issue arises again in some of the other papers. I agree with Azzouni, though, that there is a position in logical space that his version of nominalism aims to colonise; I disagree about the tenability of this position, especially when it comes to accommodating mathematical explanations.

3. Bueno

Otávio Bueno offers a number of suggestions for completing the sought-after easy road. Let me start with a quibble about how much of the explanatory load mathematics must carry before we can take the explanation of some physical phenomenon to be mathematical. I argued that in a number of cases mathematics is carrying a significant portion of the explanatory burden (Colyvan 2010, p. 302) and that this is enough to make the explanations in question mathematical. To be sure, there is some unclarity about what "carrying a significant portion of the explanatory burden" amounts to, but this is not what worries Bueno. Bueno suggests that pretty-much any reading of this is too weak and that for my purposes I need to establish that mathematics is the sole reason for the phenomenon in question. But this strikes me as far too demanding. After all, we do not require anything like this in more mundane cases of explanation. Take a simple, familiar case of a rock breaking a window. The rock is not the *sole* reason for the window breaking. There is the fragility of glass, the velocity of the rock and so forth. Still, it is true that the rock hitting the glass carries a significant portion of the explanatory load here. The explanation here would crumble without the rock. It is in this sense, in the cases in question, I meant that mathematics carries a significant portion of the explanatory burden: there would be no explanation without the relevant mathematics. We do not require that a biological explanation appeals to only biology or that a chemical explanation appeals to only chemistry, so why insist on this for cases of mathematical explanation? We need to be careful not to set the bar higher for mathematical explanations than we do for other explanations in science.

It is interesting to note that in all the cases of mathematical explanation on the table, the mathematical component complements a rather useless causal explanation. The

Kirkwood gaps in the asteroid belts have a causal explanation in terms of the causal histories of all the particles in the universe and why they are not in one of the Kirkwood gaps. This story, were it possible to articulate, would undoubtedly be true but it is the mathematical explanation in terms of the eigenanalysis that is enlightening here. Moreover, the causal story makes it sound as though it is an accident that the Kirkwood gaps are where they are. The mathematical explanation makes it clear that it is no accident—the location and width of the gaps can be accounted for by the eigenanalysis.

It is also worth noting that in some of the cases of mathematical explanation, there is a modal component to the explanandum. We are not merely interested in why there are no asteroids in the Kirkwood gaps, rather, we are interested in why, in some sense, there could not have been asteroids there. Mathematics is very well suited to supplying such modal explanations. Such modal information is typically missing from any accompanying causal explanation.

Bueno argues that what really does the work in the Kirkwood gaps is the gravitational fields of the Sun and Jupiter. While it is true that this is what supplies the mechanism (if indeed, general relativity is a "mechanistic" theory), I deny that this is the full explanation. (And note that the authors quoted by Bueno, Murray and Dermott, talk in terms of mechanism and this is *not* the same thing as an explanation.) The explanation for why a square peg of side length l will not go in a round hole of diameter l is (arguably) geometric but the mechanism for any specific attempt is the repulsive forces of the electrons in the relevant parts of the peg and material surrounding the hole. The mechanism is one thing but the full explanation (arguably) involves more than just the mechanical details.

I agree with Bueno that the mathematics does not explain on its own; the mathematics needs an interpretation. For example, Bueno rightly suggests that a given differential equation has multiple interpretations. But far from this being an obstacle to mathematics explaining, this is exactly why mathematics is able to deliver the very general explanations I have in mind. Consider the logistic equation:

$$dP/dt = rP(1-P/K).$$

One interpretation of the terms in this equation are as follows: P is the abundance of some population of organisms at a given time t, K is the carrying capacity of the environment (the maximum population the environment can sustain), and r is the growth rate of the population in question. Under this interpretation, this equation represents, for example, the growth of a rabbit population towards their maximum abundance in some region. But the same equation, under a different interpretation, represents the course of autocatalytic reactions in chemistry.[1] But herein lies the power of mathematics for the kind of high-level explanations I am interested in. The mathematics does not care about causal details; it does not matter whether it is populations of rabbits approaching carrying capacity or the disintegration of the tin buttons on Napoleon's army's uniforms

[1] These are reactions where the catalyst is a product of the reaction. Examples include Tin Pest, which is the disintegration of the metal tin into the powder grey tin.

(LeCouteur 2003). In so far as the equation faithfully represents salient features of the systems in question, at least some of the features of these systems (such as the behaviour of the population as it approaches carrying capacity or the progress of the chemical reaction as the reactants are nearly exhausted) will be best explained at the mathematical level and by ignoring the causal details. This is not to deny the importance of the causal details. It is just that such details do not help with the explanation and can even get in the way. While I admit that such a view is controversial it does enjoy some support (and, to be fair, opposition as well) in scientific practice.[2]

Finally, there are intra-mathematical explanations: mathematical explanations of mathematical facts (Baker and Colyvan 2011; Colyvan 2012). Take some mathematical theorem with an explanatory proof. When we find a physical system that the theorem (under a suitable interpretation) is applicable to, we have the potential for a mathematical explanation. In such cases, there will be causal explanations in the vicinity, but the mathematical explanation holds for any system with the relevant structure. The mathematics seems to be telling us that some causal mechanism or other is needed but that the details are beside the point; the explanatory action is in the mathematical formalism itself. Such cases lead us to ask after a philosophical account of intra-mathematical explanation and their relation to other forms of scientific explanation. This is an area in need of much further work, irrespective of debates about realism and anti-realism in the philosophy of mathematics.

There is much more to be said about whether mathematics can play a genuinely explanatory role in scientific theories or whether mathematics always plays a merely representational role.[3] Here I merely want to say why I am inclined to stand fast with my claim that the mathematics does play an explanatory role. But Bueno is quite right to push this point; there is more work to be done here.

4. Leng

Mary Leng also appreciates the importance of mathematical explanation to this debate. She agrees that mathematics does more than play a merely representational role in science. She thinks that sometimes mathematics plays an indispensable role in scientific explanation—in what she calls *structural explanations*. In such explanations, she thinks, the mathematics does explanatory work over and above the work done by the nominalistic content of the explanans; the mathematics explains the explanandum by showing it to be a consequence of structural features of the physical system. She is right to point out that there are a two options left open to the nominalist here: "either show that such explanatory uses of mathematics can be dispensed with in favour of equally good nonmathematical explanations (a route that takes them perilously close to the hard road they wished to avoid), or show that such uses of mathematics are coherent even from a nominalistic perspective." (Leng 2012, p. ??). Once put this way, it is not surprising that she plumps for the second option. This is an interesting suggestion and I admit that I did not do justice to it in my earlier paper.

[2] See Colyvan and Ginzburg 2010 for further discussion on this point.
[3] Bueno and I find some common ground on this issue in Bueno and Colyvan (2011).

Leng's basic idea here is to treat mathematics as a kind of if-then theory. The mathematics, according to Leng, just tells us what must be true of any system that has the relevant mathematical structure but there is no reason to take the explanations in question to involve mathematical objects. So "we are committed only to the physical instantiation of a mathematical structure, not to a system of abstract mathematical objects over and above that physical instantiation." (Leng 2012 p. ??)

One concern with this proposal is that Leng seems to have mathematical explanations singled out for special treatment. When an explanation invokes mathematical objects, Leng advises us not take the ontological commitments of that claim seriously, just read it as a claim about the structural properties of the physical system we are interested in. But why stop there? Why not follow van Fraassen (1980) in reassessing apparent explanations involving unobservables? This is not an argument against Leng; it is just a word of warning about a nearby slippery slope. If the aim is to advance a nominalist account, while remaining a scientific realist, more needs to be said about why mathematics is singled out for such special treatment and why the special treatment stops there.

A more substantial issue with Leng's account is that she is assuming that straightforward cases are as follows: there is a physical structure (which the nominalist can be realist about) that has approximately the same structure as some mathematical structure. Invoking the intermediate-value theorem to explain why you need to cross the equator in order to travel from Sydney to London has this kind of pattern. She goes on to discuss more complicated cases where the mathematics–physical connection has the intermediary of an idealized model. Here she suggests that such cases can be dealt with, as she has elsewhere (Leng 2010), by the kind of make-believe advanced by Kendall Walton (1993). The difficulty, as I see it, is that there are cases that do not fit either of these patterns. For instance, some of the explanations in question involve a mathematical structure representing non-actual physical states in order to explain something about actual physical states.[4] The explanation of galactic stability discussed by Aidan Lyon and myself (Lyon and Colyvan 2008) has this pattern. Perhaps Leng can use a Walton-style pretence theory to deal with these cases as well, but it is important to note that these cases are very different from the idealised intermediary models that Leng discusses.

5. Liggins

David Liggins (2012) defends Joseph Melia's (2000; 2002) approach to the easy road. He takes issue with my claim that weaseling—taking back the implications of what one has just said—only has plausibility in cases where it is clear what is being taken back and what is left after the weaseling. I suggested that the only plausible way to clarify the content after weaseling is to provide a translation, and this leads back to the hard road to

[4] This is a point that David Malament (1982) raised in response to Field: when the underlying space is not something a nominalist can be realist about, the Field approach does not look promising. Examples include infinite-dimensional phase spaces of quantum mechanics, but also more mundane ones such as Hamiltonian formulations of classical physical theories.

nominalism. More specifically Liggins argues that (i) my demand for a nominalistic restatement of all our best scientific theories is unreasonably strong; (ii) Melia has already met a more reasonable demand; (iii) as a consequence Melia's view does not render the contents of our best scientific theories obscure; and (iv) the view does not give rise to technical difficulties such as those facing Field's (1980) nominalisation program.

Liggins rightly notes that it is part of Melia's view that not all the content of our best scientific theories can be expressed in nominalistic language. He goes on to suggest that this means that my request for a translation of sentences such as:

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

is unreasonable. According to Liggins, it is like insisting on being shown some invisible gases. But the analogy with invisible gases is not appropriate. The sentence (1), I claim, is simply not intelligible. Adding "and I can not make it any clearer" does not get Melia and Liggins off the hook here. I am not asking to see invisible gases; I just want to know what core sentences of weasel science mean. My original point was that the weaseling view, as it stands (with or without the "we can not do any better clause"), renders much of science incomprehensible. This is not an outcome anyone should be satisfied with. My claim that translations of sentences such as (1) are required was an attempt, on Melia's behalf, to avoid this unpalatable conclusion.

While Liggins takes my request for a translation of all the sentences in question to be unreasonable, he does concede that I am entitled to ask after the contents of the nominalistic theory. This more modest request, though, he seems to think is met by providing translations of some of the less troublesome sentences. This is odd. I do not understand (1), so I am offered a translation of some basic measurement statements concerning distance and mass. It is instructive to see the difference between what Liggins offers at this point and Hartry Field's (1980) strategy.

Field too failed to offer translations of all of science. Instead, he offered a demonstration of how the differential fragment of classical gravitational theory would look. The hope here was that once this was done, it would be clear how, in principle at least, the rest would follow by much the same method. Malament (1982) and others (Lyon and Colyvan 2008) have challenged whether the strategy offered by Field generalizes to non-space time theories or even to Hamiltonian formulations of classical space-time theories. These are genuine problems but at no stage did Field suggest that by doing a small part of the job, the job was done. But Liggins seems to suggest that we will be in better shape to understand (1) after seeing a bit of nominalistic measurement theory.[5]

[5] Liggins suggests that since Melia's goal is different from Field's, Melia's project does not face the same well-known technical difficulties facing Field's. But my point is that in order to understand what the weasel is saying (e.g. in sentences such as (1)), we need something like the completion of Field's nominalisation project. I agree that Melia would like to avoid facing such technical difficulties, but simply having a different goal is not sufficient to ensure this.

Liggins is right that "it is in Melia's interests to nominalize some parts of our best scientific theories" (Liggins 2012 p. ??). The problem with Liggins's suggestion is that he focuses on trivial scientific claims such as "the chair is 8 kg". If that were all there were to science—stating the mass of chairs and the distances between them—I agree that the weasel would be in good shape. But the range of roles mathematics plays in science is much broader. Part of the purpose of my earlier paper (Colyvan 2010) was to stress some of these roles. In particular, I was keen to emphasise the ways in which mathematics contributes to scientific explanations.[6] I know the weasel has to start somewhere, and it is tempting to start with easy cases, but it is the more complicated cases where the action is. I, for one, would be genuinely interested in Liggins account of how the weasel can deal with more substantial applications of mathematics in science.[7]

6. Yablo

Stephen Yablo (2012) does two things: he gives us a very nice account of logical subtraction—how we can retract ontological commitments—and he distinguishes three grades of explanatory involvement, suggesting that with these distinctions in mind, it is not clear that mathematical explanations are of the right kind to warrant ontological commitment to mathematical objects. Both these contributions are genuine advances on his earlier treatments of this topic (e.g. in Yablo (2005)) and are interesting in their own right. Here I just express a couple of doubts I have about Yablo's account of the mathematical explanations in question.

Yablo argues that there are three grades of mathematical involvement in physical explanations. The first is *descriptive*, where mathematics helps in describing the physical circumstances, the outcomes, or the generalization that links the outcomes with the circumstances. The second is *structural*, where mathematics is needed in order to present the relevant explanation at the right level of generalization. The third grade of mathematical involvement is *substantive*, where mathematics is actually providing the explanatory generalizations. Yablo goes on to argue that it is only this third grade of mathematical involvement that carries with it ontological commitment. The examples of mathematical explanation in the literature have not been established to be of the crucial third type, or so claims Yablo. In a constructive spirit, he offers a number of questions, each of which requires an appropriate answer before we can establish that we have mathematical explanations of the third kind.[8]

Adequately addressing each of Yablo's questions deserves a more extended treatment than I can offer here. Instead, let me say a few words about why I think that mathematics

[6] Joseph Melia appreciates the significance of such cases, as seen in an earlier exchange (Colyvan 2002; Melia 2002). This is not to say that Melia agrees with me on the prospects of the weaseling strategy, but he agrees that the weasel needs to say something about cases where I claim mathematics is explaining. See also Baker and Colyvan (2011) for more on weaselling and mathematical explanation.
[7] As a helpful suggestion here, I think Yablo's (2012) account of logical subtraction is just the kind of account Melia and Liggins might find useful.
[8] Mary Leng is thinking along similar lines with her resistance to mathematics playing anything more than a structural role in the explanations in question.

playing any substantial role in scientific explanation (either grade 2 or grade 3) is a problem for the nominalist. Yablo says in relation to my claim that the mathematical explanation of the Kirkwood gaps is problematic for the nominalist:

> Colyvan suggests it is a problem for nominalism if what the orbits have in common, by virtue of which they're unstable, is mathematical rather than physical. He doesn't say why it's a problem, however. The nominalist rejects mathematical ontology, not mathematical typology. Why should she not agree that math enables the scientist to carve physical phenomena at the explanatory joints? (Yablo 2012, p. ??)

I take it that what Yablo has in mind is that in the Kirkwood gaps case, there are, as I suggest, many different causal stories but that the mathematics unifies these in the right sort of way. Mathematics unifies these causal stories so that the explanatory joints are revealed.[9] But mathematics is not doing anything more; the real explanatory work is being done by nominalistically kosher entities. In the case under consideration, the nominalist explanation involves all the possible causal histories of the different particles in the vicinity. But what of cases where the mathematics is suggesting explanatory joints that run across different subject matters? In such cases it is hard to see how there is anything other than a mathematical explanation that unifies the different kinds of phenomena.

What I have in mind here are cases such as the way the intermediate-value theorem tells us why you need to cross the equator when travelling from Sydney to London, and how it also tells us that if the temperature is 10 degrees Celsius in the morning and 20 degrees Celsius early in the afternoon, there must be a time between when it is 15 degrees Celsius.[10] Think for a moment what the nominalist's explanations would look like. The former will be about the motion of bodies in space and the latter will be about changes in the mean-kinetic energy of collections of gas molecules around a thermometer. The point is that it would be very odd for a nominalist to admit any such cross-subject-matter mathematical explanations as revealing any explanatory joints at all. All the causal details are different, not just in mere detail (as in the different causal stories coinciding with the Kirkwood gaps), but different in subject matter as well.

Let me also repeat the warning about slippery slopes I gave earlier in relation to Leng's proposal. If Yablo's account of grades of explanation is adopted and we need to prove that the relevant mathematical theory featuring in an explanation has the third grade of involvement, we need to make sure that we are not guilty of adopting double standards. If mathematical explanations have to live up to the lofty standards set by Yablo, we need to rethink the way inference to the best explanation is used by scientific realists. After all, Yablo's proposal amounts to a rejection of inference to the best explanation in its naïve

[9] It is also not clear how the nominalist explanation in terms of instability is supposed to work. 'Instability' here is a metaphor; it is a metaphor that can be cashed out in the relevant mathematics but I cannot see how an appeal to instability is supposed to do explanatory work otherwise.

[10] Another example I used earlier about autocatalytic reactions and population growth also illustrates this cross-subject-matter style of explanation.

form, at least. In its place he offers us something like "inference to the best third-grade explanation". This may well be an advance and help in overcoming some of the problems and unclarity surrounding the naïve use of inference to the best explanation. But this new form of inference to the best explanation opens the door for a kind of constructive-empiricist challenge: electrons only make the second grade and fail to feature in any third-grade explanations. Whenever electrons *appear* to feature in third-grade explanations, it cannot be shown that the explaining is being done by the electrons and not by some electron-free constructive empiricist "as if " theory. Again, this is not an argument against Yablo; I am just offering a word of caution against double standards and nearby slippery slopes.[11]

I agree with Yablo that more needs to be said about what it is for mathematics to be playing the lead role in scientific explanation. I was happy to rely on an intuitive conception of what this amounted to. Yablo's contribution in this regard is a welcome addition. I am not sure I agree with the details but it serves as a useful starting point for discussions of mathematical explanations in science. In tandem with this project, we need to pay attention to intra-mathematical explanations—mathematical explanations of mathematical facts—for as I have suggested elsewhere (Lyon and Colyvan 2008; Baker and Colyvan 2011) at least sometimes these are the ultimate explanations of the physical facts. It is not clear how such intra-mathematical explanations fit in Yablo's three-tiered classification of explanations. To be fair, Yablo's classification was not intended to deal with such cases. But we cannot ignore intra-mathematical explanation in this discussion. Ultimately we want an account of mathematical explanation for both mathematical and scientific applications. Yablo's account is a good first step, but it cannot be the full story.

7. Conclusion

As I suggested in my earlier paper, a great deal hangs on the role of mathematics in scientific explanations. If mathematical entities do not play the right sort of role in scientific explanations, then this needs to be spelled out in a way that distinguishes mathematical entities from other entities quantified over in our best scientific theories. Or perhaps the alleged mathematical explanations Alan Baker (2005, 2009, forthcoming) and I have presented are better understood as garden-variety scientific explanations and there is nothing especially mathematical about them. In any case, as Stephen Yablo suggests, we need to get clearer about what it would be for mathematics to carry the bulk of the explanatory load in the kind of explanations in question. All these are issues that deserve further work. I would add to this list: gaining a better understanding of intra-mathematical explanations.[12] As I have already mentioned, mathematical explanations

[11] And recall that in the current context, giving up on scientific realism is not on the table. Of course rejecting scientific realism is a live option in a different context, but here the easy road strategies under consideration are put forward as ways of being both a scientific realist and a mathematical nominalist. Descending to a more global instrumentalism is simply not where the easy road is supposed to go.

[12] Mathematical explanation has come to prominence in the context of the indispensability argument (e.g. Colyvan 2001; 2002 and Baker 2005; 2009), but the central issues arising from both intra-mathematical explanation and extra-mathematical explanation were aired much earlier by Mark Steiner (1978a; 1978b) and J.J.C. Smart (1990).

can spill over into empirical contexts.

If there is a point of agreement in this debate, it is that we could do with a better understanding of mathematical explanation. This, of course, would help in assessing the viability of easy-road nominalist proposals. But the topic of mathematical explanation is also interesting in its own right. A better understanding of mathematical explanation is relevant for understanding aspects of mathematical practice (e.g. explanatory versus non-explanatory proofs) as well as for developing general philosophical theories of explanation elsewhere in science. With or without the mathematical realism debate, we need a better understanding of mathematical explanation.[13]

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[13] First, let me express my sincere appreciation to Jody Azzouni, Otávio Bueno, Mary Leng, David Liggins, and Steve Yablo for their insightful discussions of my earlier paper. They have all helped to push the debate forward. I am also indebted to Mary Leng, and Steve Yablo for reading and commenting on drafts of the sections devoted to their respective views. The research for this paper was funded by an Australian Research Council Future Fellowship Grant (grant number FT110100909) and was completed while I was a visiting fellow at the Stellenbosch Institute for Advanced Studies (STIAS) in Stellenbosch, South Africa.

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