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Good weasel hunting

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1. Introduction

The indispensability argument sets the agenda for much contemporary philosophy of mathematics. There are several ways of formulating the argument (see Colyvan 2001: 11–13 and Resnik 1997: 44–5 for the classical Quinean formulation). The skeleton of the argument can be expressed as follows:

(1) Mathematics is indispensable to science: that is, our best scientific explanations imply the existence of mathematical objects, such as numbers.

(2) If mathematics is indispensable to science, then there are mathematical objects.

Therefore: there are mathematical objects.

Let us call the claim that there are mathematical objects *platonism* and its negation *nominalism*. For much of the 1980s and 90s, discussion of the indispensability argument was dominated by debates over one particular nominalist response, that of Hartry Field (1980, 1989). According to Field's response, our best scientific theories can be re-written so as to avoid implying that there are mathematical objects, and so (1) is false. Field embarks on a formidable technical programme to support this claim. The consensus is that Field's programme, although illuminating, is unsuccessful (see MacBride 1999 for an excellent survey).

Another response to the indispensability argument is to deny (2). This has recently been explored by several philosophers (see e.g. Balaguer 1998, Chs 5 and 7; Yablo 2000, 2001, 2002; Azzouni 2004, Leng 2010). According to this response, mathematics may well be indispensable to science, but this does not imply that there are mathematical objects. Mark Colyvan (2010) has named this the *easy road* strategy, in contrast to the *hard road* which is to deny (1).

One prominent version of the easy road strategy is that of Joseph Melia. In a series of publications (1995, 1998, 2000, 2002, 2003, 2008, 2011), Melia has offered a distinctive version of the easy road strategy. The purpose of this paper is to clarify Melia's response to the indispensability argument and to advise Melia and his critics on how best to carry forward the debate. We will begin by presenting Melia's response and diagnosing some recent misunderstandings of it (section 2). Then we will discuss four avenues for replying to Melia. We will argue that the three replies pursued in the literature so far are unpromising (section 3). We will then propose one new reply that is much more powerful, and – in the light of this – advise participants in the debate where to focus their energies (section 4).

2. What is weaseling?

We often say things such as:

Everyone who came to the seminar had a handout. Except the person who came in late.

Melia (2000: 467) points out that there are different ways to interpret such a claim. We could take it to be self-contradictory: 'Everyone who came to the seminar had a handout and the person who came in late did not have a handout'. Or we could interpret it as consistent: 'Except for the person who came in late, everyone who came to the seminar had a handout'. Consistent interpretations are more charitable. By making these utterances, one expresses one's view by taking back something implied by the sentence one has uttered. Melia calls this practice *weaseling*.

When scientists articulate their theories, they often utter sentences which imply the existence of numbers, functions, and other mathematical objects. But according to Melia (2000: 469), 'almost all scientists … deny that there are such things as mathematical objects'. Melia says that rather than regarding these scientists as inconsistent, it is more charitable to interpret them as indulging in weaseling: they convey their views by taking back something implied by the sentence they have uttered.

Why would scientists weasel rather than stating their position more straightforwardly? According to Melia, natural languages lack the resources to state some scientific conclusions in any other way. For instance, if our language had a predicate for each distance relation, it would have infinitely many primitive dyadic predicates (‘ … is one metre from …’, ‘… is two metres from …’, etc.). But if we are willing to talk about mathematical objects, we can use just a single triadic predicate, ‘…is … metres from …’ (see Melia 1995: 227–8, 2000: 474–4). In Melia’s view, the best way for scientists to articulate their distance claims is to utter a sentence that implies the existence of mathematical objects and then cancel the implication. In other words, he holds that the best way is to use weaseling. ‘[M]athematics is used [in science] simply in order to make more things sayable about concrete objects’ (Melia 1998: 70–1).

If this is the role of mathematics in science, then the fact that our best scientific theories imply the existence of mathematical objects gives us no reason to believe in such things. Platonism is not an implication of any of our best scientific theories, on Melia’s view; rather, scientists speak as if there were mathematical objects in order to make assertions which have nothing to do with mathematical objects.

Episodes of weaseling can sound inconsistent. When someone uses an assertoric tone of voice to first utter a sentence that implies the existence of mathematical objects and then utter another sentence which says there are no such objects, charity is required to interpret the speaker as not asserting jointly inconsistent things. The ‘handout’ example, which is very close to one of Melia’s (2000: 467–8), is helpful here. In this case, it is clear that we should interpret the speaker’s first sentence in an unusual way: although they say ‘Everyone who came to the seminar had a handout’, we should not interpret them as asserting that everyone who came to the seminar had a handout. We propose that weaseling should be understood similarly: when a scientist articulating their theory utters a sentence which implies the existence of mathematical objects, then denies that there are mathematical objects, they should not be interpreted as asserting something which implies the existence of mathematical objects.

We should acknowledge that this reading of Melia is hard to reconcile with his claim that ‘Sometimes it is legitimate to assert a collection of sentences whilst denying some of the logical consequences of this collection!’ (2000: 456). This claim is independently puzzling: Melia denies (2000: 469) that he advocates that we should have inconsistent beliefs, so we would expect him not to tolerate inconsistent assertions.

Some conceptual hygiene suggests a charitable explanation of this puzzling claim. The objects of assertion are propositions. Sentences can be uttered to make assertions, but the proposition asserted need not be the proposition expressed by the sentence uttered. For instance, 'There are four eggs' can be used to assert that there are *exactly* four eggs. (See Stalnaker 1978 for a popular and plausible account of assertion in this vein.) In light of this, we interpret Melia's quoted claim as follows: 'sometimes it is legitimate to *use a collection of sentences to assert something* whilst denying some of the logical consequences of this collection'. This is a very sensible claim. Moreover, our interpretation suggests that, in the quotation, Melia is just being careless with his terminology rather than ascribing inconsistent behaviour to speakers. The interpretation we offer here makes the best sense of all that Melia says about weaseling.

Yvonne Raley (2012) argues that Melia's position is incoherent. We will now set out her argument and explain where it goes wrong.

Suppose a scientist articulating their theory utters a sentence which implies the existence of mathematical objects, and then denies that there are such things as mathematical objects. On Melia's view, what is asserted does not imply the existence of mathematical objects. Raley poses a dilemma. Can Melia tell us exactly what is asserted? If he can, then his view is just an example of the familiar paraphrase strategy for replying to the indispensability argument. On the other hand, if Melia cannot tell us exactly what is asserted, then 'why shouldn't we just conclude that the speaker is contradicting himself?' (Raley 2012: 342).

As Raley acknowledges, her dilemma echoes earlier arguments of Colyvan and Azzouni. But Raley takes the discussion further by trying to block some possible escape routes Melia might attempt to take. One of these is to interpret the speaker as consistent by not reading them as asserting the proposition expressed by the original sentence (just as in the example above we do not usually interpret the speaker as asserting that everyone had a handout). Raley contends that this leads nowhere:

> That's not an option because paraphrases of the statements that are to be taken back are not always going to be available. And if they aren't, then how can we rewrite our Best Theory so that we can see which parts of it are true and which aren't? In the absence of paraphrase, in other words, we have no way of separating the true from the false statements of the theory. (Raley 2012: 343, footnote omitted)

We have two comments to make on Raley's argument. First of all, it seems to have little prospect of establishing the stated conclusion, that Melia's position is incoherent. On the first horn of the dilemma, Melia's response to the indispensability argument turns out to be a paraphrase strategy in disguise – but it is hard to see how establishing that would show Melia's view to be incoherent. On the second horn, Melia is forced to attribute contradictory assertions to scientists, and so will probably have to interpret them as holding inconsistent beliefs. If so, then Melia would interpret scientists as, in a sense, incoherent. But it is hard to see how that would show *Melia*'s view to be incoherent.

More importantly, Raley's criticism of Melia seems to betray a misunderstanding of his view. In particular, Raley seems to have overlooked Melia's emphasis on the notion of expressibility. His view is that natural languages lack the resources to state some scientific conclusions except via weaseling. We will now argue that this blunts the second horn of Raley's dilemma.

Exactly what is asserted when a scientist articulating a theory utters a sentence which implies the existence of mathematical objects and then denies that there are any such things? Raley argues that if Melia cannot tell us exactly what is asserted, then we should take the speaker to be making two jointly inconsistent assertions. Raley presupposes that all cases of weaseling are like the 'handout' example above in that it is possible to state in other terms exactly what is asserted. But it is part of Melia's theory that some cases of weaseling lack this feature: some conclusions can be stated using weaseling but not otherwise. Thus it is question-begging for Raley to demand that Melia state these conclusions without using weaseling. Of course, if Raley had established that every conclusion that can be articulated through weaseling can also be articulated without, that would refute Melia's position. But Raley makes no attempt to argue for this.

Michael Scott and Philip Brown argue that Melia's view is committed to an implausible account of speaker meaning (2012: 353–4; 357–8). They go on to sketch their own account of scientific discourse that allegedly avoids this difficulty (2012: 358–9). We will now show that Scott and Brown's interpretation of Melia is wrong, and that their account of scientific discourse in fact suggests one possible elaboration of the correct interpretation.

Scott and Brown label Melia's position a form of *figuralist fictionalism*, which they characterise as follows:

> Utterances of indicative [sentences of discourse D] are truth-apt but quasi-assertoric. A quasi-assertion is a speech act with the outward appearance of an assertion where the speaker does not endorse the uttered sentence but presents it as adhering to some norm other than truth. (2012: 352)

This passage presupposes a notion of assertion different to the one we endorsed above. Scott and Brown assume that the objects of assertion are sentences, where asserting a sentence involves endorsing that sentence as true. This rules out the possibility of using a sentence S to assert a proposition that is not the proposition expressed by S, so forces a reading of Melia according to which scientists do not assert anything, but instead perform some other linguistic act.

On this interpretation of Melia, the speech acts scientists engage in when articulating their theories are similar to the act of telling a fictional story. Sir Arthur Conan Doyle used indicative sentences about Sherlock Holmes in a way that looked like assertion, but did not endorse those sentences as true. Scott and Brown argue that this view involves an implausible account of speaker meaning because it implies that speakers are unaware of the speech acts they are performing (2012: 353–4). They cite Stanley's (2001: 46–7) point that we should expect competent speakers to deny that they are speaking non-literally when using mathematical language. The problem, they say, 'is not that the sentences… have truth-conditions of which speakers are unaware… but that *utterances* should have or lack content in [a] way that is opaque to speakers' (2012: 354).

Scott and Brown offer their own account of scientific discourse to avoid this difficulty (2012: 358-359). They claim that scientists' assertions do not communicate the proposition assigned to the sentences they utter by semantic theory, but instead communicate pragmatically determined content that does not imply the existence of mathematical objects. Scott and Brown provide a suggestion of the mechanism responsible for this. We rarely utter 'The fridge is empty' to communicate the proposition that there is a complete absence of matter inside the fridge; rather, we use it to communicate that there is no *food* in the fridge. However, we should expect competent speakers to deny that they are speaking non-literally when they use this sentence in this way. Pragmatic theory explains this by positing a process called 'loosening', whereby the meaning of the word 'empty' is contextually changed so that the term is applicable to things just with no food in them. Scott and Brown suggest that some analogous pragmatic mechanism is at work in scientific discourse: scientists assert the sentences of their theories which pragmatically express content concerning only the physical world. This does not attribute ignorance to scientists about the speech acts they are performing.

There are four points we wish to make here. First, the evidence Scott and Brown cite in favour of their interpretation of Melia is weak. Second, their interpretation contradicts what Melia says about weaseling. Third, once weaseling is properly understood, Scott and Brown's objection misses its target. Fourth, the pragmatic apparatus appealed to in Scott and Brown's own account of scientific discourse is available to Melia as one possible elaboration of the view he intended. Let us take these in turn.

Scott and Brown cite two pieces of evidence in favour of their figuralist fictionalist interpretation. They first point to Melia's claim that a scientist who utters sentences which imply the existence of mathematical objects, but who then goes on to deny the existence of such things, should not be understood as contradicting herself (2012: 358). This claim is central to Melia's view, and does not tell in favour of a figuralist fictionalist interpretation of it. It is perfectly compatible with our own interpretation of Melia.

The second piece of evidence is that Melia apparently compares weaseling to story-telling (Scott and Brown 2012: 358). We presume the passage Scott and Brown have in mind is the following:

> My thesis is that, just 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: 470)

However, it is not clear that by 'telling a story about the world' Melia means telling a *fictional* story about the world. This locution is often used to mean 'present a theory of', 'present an explanation of', or even just 'say something about'. There is no evidence that Melia intended to establish a similarity between the kinds of speech acts scientists perform and the kinds of speech acts performed by novelists. Rather, the point of this passage appears to be that, for the purpose of conveying information, denying unwanted entailments is just as legitimate as adding previously omitted details.

The notion of assertion presupposed by Scott and Brown's interpretation rules out the possibility of using a sentence S to assert a proposition that is not the proposition expressed by S, so they are forced to interpret Melia as claiming that scientists do not really assert anything when they articulate their theories. This contradicts the claim made in the troubling quotation from Melia discussed above: 'sometimes it is legitimate to assert a collection of sentences whilst denying some of the logical consequences of this collection!' On our own interpretation, Melia is guilty of being sloppy with his terminology; according to Scott and Brown, Melia is guilty of self-contradiction. We maintain that our interpretation makes the best sense of everything Melia says about weaseling.

On our interpretation, when scientists weasel, they do not assert anything that implies the existence of mathematical objects. They use sentences which imply some mathematical content to assert propositions that concern only the physical world. As to how this communicative feat works, Melia is silent, so one can legitimately call Melia's response to the indispensability argument incomplete. However, one cannot claim that it implies an implausible account of speaker meaning. Likening scientific discourse to fictional story-telling is not an attractive route for completing Melia's account. Plausibly, for a speaker to tell a fictional story she must at least be aware that she is doing so, and we should expect many scientists to deny that they systematically engage in fictional story-telling. Melia is right to avoid this route.

A more promising route is to draw on pre-existing pragmatic theory to show that, in applied contexts, utterances of mathematical sentences communicate content concerning only the physical world. There is no reason why Melia could not appeal to the same apparatus endorsed by Scott and Brown to achieve this.

3. Anti-weaseling manoeuvres (i)

Having clarified Melia's response to the indispensability argument, we now turn to consider four ways for platonists to respond to it. In this section, we will examine three responses which have already appeared in the literature, and argue that all three of them are unpromising. In the next section, we will offer a new response that is much more powerful, and advise the participants of the debate on how to proceed in light of it.

The first strategy we consider is the most prominent in the literature. It is the response of providing examples of scientific explanations in which mathematics plays not merely an expressive role but a genuinely explanatory one. Many examples have been offered (e.g. Colyvan 2001: 81–6, Lyon and Colyvan 2008, Lyon 2012); the most discussed has been Alan Baker's (2005) example of the periodical cicadas. Baker draws attention to the fact that three species of North American cicada have life-cycles of either thirteen or seventeen years. Why are their life-cycles a prime number of years long? Baker argues that part of the best explanation is the fact that life-cycles whose length in years are prime serve to minimize intersection with other periods, a fact which crucially involves the properties of prime numbers. When combined with the biological fact that minimizing such intersection brings evolutionary advantages, this explains why the cycles have prime lengths.

In response to Baker, several authors have argued that the explanation is one in which mathematics plays an expressive rather than an explanatory role: it serves to represent periods of time and the relations between them (Daly and Langford 2009: 651–658, Leng 2010: 24–9, Rizza 2011, Saatsi 2011). None of the other examples has so far persuaded the critics.

We think that platonists would be ill-advised to reply to Melia by using this strategy. First of all, it is simply unclear what it means for mathematical entities to play a 'genuinely explanatory role'. All parties to the debate agree that planets and electrons play this role in physical theory, so if mathematical entities are to play it too, their contribution must be similar to the contribution made by planets and electrons (strictly speaking, the contribution made by mentioning mathematical entities must be similar to the contribution made by mentioning planets and electrons). But similar in what respect? We have never been told. The unclarity of 'genuine explanatory role' leads to a corresponding unclarity in how to tell when something plays this role. We need a criterion of explanatoriness to help us to classify the difficult cases, such as the cases which Colyvan, Lyon, and Baker highlight – but we do not have such a criterion. No wonder, then, that the debate has been inconclusive.

Baker concedes that since we do not have any way to decide whether the mathematics used in science plays the right sort of explanatory role to vindicate platonism, 'we seem to have reached an impasse' (2009: 624). He goes on to write:

> I do not know how to *demonstrate* that the mathematical component is explanatory. On the other hand, I think it is reasonable to place the burden of proof here on the nominalist. The way biologists talk and write about the cicada case suggests that they do take the mathematics to be explanatory, and this provides good grounds, at least *prima facie*, for adopting this same point of view. (Baker 2009: 625)

Lyon (2012: 572) is 'inclined to agree'. We disagree. It is clear that scientists are happy to regard many mathematical explanations of physical phenomena as good explanations. But it is very unclear that they agree that the mathematics in these explanations is playing a 'genuine explanatory role'. There is no evidence that scientists have any grasp of this rather unclear philosophical concept, and so there is no evidence that they are deploying it in their writings about the cicada case. It is telling that Baker does not cite any appeal to this notion by a biologist. Recognizing good biological explanations is one thing, identifying whether the role mathematics plays in those explanations counts as a 'genuine explanatory role' is quite another. To put the point another way, Baker faces a dilemma. Baker's opponents agree that scientists often regard mathematical explanations as good explanations; they are asking Baker for reasons to think that, according to scientists, the mathematics within these explanations plays a 'genuine explanatory role'. What is the nature of Baker's evidence that scientists think mathematics plays that role? If Baker claims it is implicit in what scientists say, that will not advance the debate, because that is precisely what his opponents are questioning. On the other hand, if Baker claims that there is explicit evidence that scientists believe the mathematics in scientific explanations to play a 'genuine explanatory role', then he should produce this evidence – but he has not done so.

We therefore regard Baker's attempt to saddle his opponents with the burden of proof as unsuccessful. In fact, since Baker is responding to Melia, it is arguable that the burden of proof rests with Baker. Even if it is simply unclear where the burden of proof lies, that would be enough to suggest that this way of responding to Melia does not promise to be very fruitful. Baker's talk of an 'impasse' is all too apt.

Lyon (2012) attempts to explain one way in which mathematics could play a genuinely explanatory role in science. He appeals to the notion of *program explanation* made famous by Frank Jackson and Philip Pettit (1990). A program explanation is one which appeals to a property that ensures the existence of a cause of the explanandum – just as a computer program ensures that the computer will have certain lower-level features (electrical features, for instance). Jackson and Pettit (1990: 110) give the example of a closed glass vessel which cracks when the water inside it reaches boiling point. The crack is caused, and explained, by the momenta of certain particular water molecules, but we can also explain the crack by pointing to the temperature of the water, which ensures that the water molecules have the appropriate momenta. This latter explanation is a program explanation. Lyon provides six examples of mathematical explanations of physical phenomena and argues that these are program explanations. In this way, he offers a sufficient condition for mathematical entities to play a ‘genuinely explanatory role’: it is for them to play a programming role.

Lyon’s strategy runs into a serious problem, pointed out by Saatsi (2012: 581). Even if mathematical explanations in science are program explanations, it does not follow that the mathematical entities are playing an explanatory role which supports platonism. Perhaps the mathematics used in program explanations functions as an expressive device, enabling us to pick out a non-mathematical higher-level property which ensures the existence of a cause of the phenomenon in question.

As Saatsi (2012: 582) anticipates, Lyon might respond by offering a metaphysical account of the program explanations in question, one which explains how mathematical objects ensure the instantiation of causally efficacious properties. (Saatsi actually speaks of ‘mathematical properties’ rather than ‘mathematical objects’, but the platonism/nominalism debate concerns the existence of the latter, not the former.) Since no such account has yet been provided, Lyon’s first task is to develop one. His second task is then to argue that the account applies to the explanations in question. This strikes us an uphill task. We are inclined to concede to Lyon that the six cases he cites are examples of program explanation. It does indeed seem that in each case there is a higher-level property which programs for the existence of a cause of the phenomenon. But there is no evidence to suggest that the higher-level property is *mathematical* in nature. To assume it is mathematical in nature because it is picked out using mathematical language would beg the question in the present dialectical context. For instance, consider again the cicadas case. Although Lyon is not entirely explicit here, it seems that he regards the programming property as being the property of having a length which minimizes intersection with other periods (see Lyon 2012: 567– 8). Whether this property involves numbers is a moot point. We therefore regard Lyon’s strategy as only partially successful. The appeal to program explanation helps clarify ‘genuine explanatory role’, but it does nothing to establish that the role is played by mathematical objects. (Saatsi himself offers some other proposals for what ‘genuine explanatory role’ might be, drawing on literature from the philosophy of science, but his discussion offers little comfort to platonists (Saatsi forthcoming). He argues powerfully that we have no reason to think that mathematics plays any of the explanatory roles he picks out. Platonists who are trying to defend the indispensability argument from Melia's attack would be ill-advised to appeal to Saatsi's work.)

How are platonists to respond to Melia, then, if not by producing scientific explanations in which mathematics plays a 'genuinely explanatory' role? The arguments discussed in section 2 above suggest two challenges to Melia which his critics might choose to press. The *content challenge* is to explain what the mathematical claims which figure in scientific theories serve to convey. Melia tells us that what is conveyed does not imply the existence of mathematical objects – but precisely what is it that is conveyed? (This is suggested by Raley's demand for paraphrase: for other versions of the content challenge, see, Pincock 2007: 265–273, Azzouni 2009: 157–9, Colyvan 2010: 295–6, Turner 2011, and Pincock 2012: 252–6.) The *communication challenge* is to explain *how* these contents are conveyed. When scientists utter sentences that imply the existence of mathematical objects, how do they manage to convey something which lacks that implication? (See Liggins 2014.) We will now discuss to what extent Melia's critics should rely on these challenges.

They might argue that Melia has not met either challenge. As we have already seen, that would be right. But its significance would be very limited. Much more significant – indeed, potentially fatal to Melia's response to the indispensability argument – would be to argue that one or both of the challenges *cannot* be met. But we think it will be very hard to establish that conclusion. Such an argument would rule out any answers to these challenges Melia or his supporters might provide, no matter how ingenious. Whilst we cannot eliminate that possibility, we regard it as very ambitious.

Moreover, the content challenge is illegitimate. As we saw in section 2, Melia's view is that some contents can be stated only by using weaseling – not otherwise. So it is question-begging to require Melia to state these contents without using weaseling: that is something he thinks cannot be done. As Liggins (2012: 999) puts it, to press the content challenge is 'rather like responding to the claim that some gases are invisible by demanding to see them all'.

According to Colyvan, if Melia fails to tell us anything more about these contents other than that they are consistent with nominalism, he runs the risk of 'render[ing] much of science incomprehensible' (Colyvan 2012: 1039). However, Melia does tell us more: he tells us that these contents concern physical quantities, such as distance and mass (see Melia 1995: 228, 2003: 58). These are physical properties and relations, entering into causal laws, and measured by familiar devices such as rulers and balances. Talk of incomprehensibility is therefore misplaced. That said, there is still a good deal of work for Melia to do, because it is not clear that every area of science serves to convey contents concerning physical quantities. For instance, it is controversial whether that quantum mechanics ought to be understood in this way: Margaret Morrison (2007: 552) concludes that the property of spin is 'perhaps best viewed as a curious hybrid of the mathematical and the physical'. Melia's critics might argue that Melia renders this area of science 'incomprehensible', and thereby renders his own position unacceptable (cf. Colyvan 2012: 1039–40).

To mount this case against Melia in detail requires one to enter difficult and controversial areas, such as the interpretation of quantum mechanics. The risks of getting bogged down in this treacherous terrain are great, so the chances of reaching a decisive defence of the indispensability argument by this route are slim. Melia's critics might therefore prefer to press the communication challenge. But we will now argue that this path is no more promising.

What could Melia say to explain how the contents are communicated? One option would be to follow Stephen Yablo in appealing to Kendall Walton's notion of 'prop-oriented make-believe'. Walton (1993) points out that utterances made within games of make-believe can sometimes be used to make claims about the real world. If we make-believe that tree stumps are bears, with bigger tree stumps counting as bigger bears, then the claim 'There's a big bear in the top field' can convey the claim that there's a big tree stump in the top field (see Walton 1993: 53). In this way, a sentence which implies the existence of bears can be used to convey something which lacks that implication. There is not space to set out Walton's explanation of this phenomenon in terms of the rules governing the game; suffice it to say that this is a theory to which Melia might appeal in response to the communication challenge. Yablo's appeal to prop-oriented make-believe leads him into controversy. For instance, Stanley (2001: 46–7) offers a battery of criticisms. One is that Yablo's theory is implausible because it entails that we often engage in make-believe without being aware that we are doing so. Yablo 2001 responds to these criticisms, and that is only the beginning of a group of complex, protracted debates. It seems likely that if Melia were to appeal to prop-oriented make-believe, that would lead him into similar controversies. And this is just to mention one option for Melia. Another possibility is to appeal to recent advances in pragmatics, as we mentioned in section 2 when we discussed Scott and Brown's work. Since there are a wide variety of approaches available, and each of these could be applied in many different ways, a complex, protracted debate threatens once again. So if the platonist tries to defend the indispensability argument against Melia by pressing the communication challenge,then (once again) the chances of a decisive defence are slim.

To summarize: friends of the indispensability argument are likely to find neither the content nor the communication challenge very helpful.

We will conclude this section by discussing Russell Marcus's comments on how to respond to Melia (Marcus 2014, section 8). They centre on *confirmational holism*: the thesis that scientific testing confirms entire scientific theories, not just particular parts of them. Many versions of the indispensability argument have confirmational holism as a premiss. To set the stage for his comments on Melia, Marcus quotes from Liggins (2008: 125) two arguments for platonism which make no mention of confirmational holism:

(1a) We should believe the measurement claims made by well confirmed scientific theories for instance, astronomy's claim: 'Saturn has surface area 1.08×10^{12} km^2'.
(2a) If these measurement claims are true, then there are abstract mathematical entities.
(3a) So we should believe that there are abstract mathematical entities.

(1b) We should believe the law-statements that figure in well confirmed scientific theories.
(2b) If these law-statements are true, then there are abstract mathematical entities.
(3b) So we should believe that there are abstract mathematical entities.

Marcus argues that the defence of these arguments must rely on confirmational holism (or some principle similar to confirmational holism – see below). He points out that Melia might reply to them with the claim that when astronomers seem to assert that Saturn has surface area 1.08×10^{12} km^2, they are weaseling – and, more generally, that the contents of scientific theories do not genuinely imply the existence of numbers. According to Marcus, the only way to respond to Melia's attack is to invoke confirmational holism (and, presumably, defend the doctrine from its critics). 'Without the implicit holistic premise, the proponent of [the quoted arguments] has no good response to the weasel' (Marcus 2014: 3588).

In our view, the strategy Marcus offers is unpromising. Some of the most important responses to the indispensability argument, such as Elliott Sober's and Penelope Maddy's, have consisted of attacks on confirmational holism (see Sober 1993, Maddy 2005; see also Glymour 1980 for an attack on confirmational holism from within the philosophy of science). As Joe Morrison has argued, whilst some other forms of confirmational holism are plausible, the form required to power the indispensability argument is not plausible and has never been established by argument (Morrison 2012). Those who choose to respond to Melia by establishing confirmational holism commit themselves to a substantial project in the philosophy of science. It is natural to wonder whether there is a quicker route. We will provide two such routes in the next section of the paper. This will show that Marcus's emphasis on confirmational holism is misplaced. (Marcus mentions the possibility of using a weaker premiss in place of confirmational holism to defend the indispensability argument, but he neither states such a premiss nor indicates how to argue for it.)

Why does Marcus think that the only way to respond to the weasel is by appeal to something like confirmational holism? Earlier on in the paper, Marcus presents a puzzle for proponents of the indispensability argument. Our evidence for scientific theories comes from observation of physical objects. But since mathematical objects, if they exist, are causally inert, they have no effects on physical objects. So how could there be scientific evidence for mathematical conclusions? In the face of this puzzle, Marcus argues that the indispensability argument requires confirmational holism, or some other principle which, as he puts it, 'facilitates the transfer of evidence from science to mathematics' (2014: 3579). According to Marcus, confirmational holism solves the puzzle: for if holism is true, then the empirical evidence for scientific theories is evidence for every part of them, including the parts that make claims about mathematical objects.

The puzzle Marcus raises is a good one. But confirmational holism does not solve it. The challenge is to explain *how* there can be empirical evidence for mathematical conclusions, given that mathematical objects are causally inert. Confirmational holism entails that there is such evidence, but it does not explain how there could be. If there is a puzzle about how there could there be scientific evidence for mathematical conclusions, then there is equally a puzzle about how confirmational holism could be true.

4. Anti-weaseling manoeuvres (ii)

We will now offer a way of replying to Melia which we take to be much more promising. It is to take issue with the bold sociological claims Melia makes about scientists, and his appeal to charity.

According to Melia, the vast majority of scientists are nominalists, so when they make assertions by using sentences that entail the existence of mathematical objects, they appear inconsistent. Melia claims that charity is required to interpret them as consistent after all. We should interpret them as weaseling rather than expressing inconsistent beliefs. It is certainly more charitable to interpret scientists as consistent, but whether scientists appear inconsistent in the first place can be disputed. How does Melia know that most scientists are nominalists? The claim that they are is a substantive sociological one, yet Melia provides no serious evidence for it. He does provide an amusing anecdote:

> In a set-theory class, the lecturer told me that I shouldn't go as far as to *believe* anything that he said, as I would end up like Gödel… (2008: 104)

> … On further questioning, after the class, I made sure that the teacher meant 'mad' rather than 'brilliant'. (2008: 104, fn. 3)

But anecdotal evidence does not go far in supporting generalisations about the scientific community. At best, Melia singles out one lecturer who is not keen on platonism (and it is notable that this a mathematician rather than a working scientist). We can single out a scientist who explicitly endorses platonism: Roger Penrose (1990: 123–8). Neither example warrants any conclusions about the scientific community as a whole. Melia's claim lacks justification, so there is no reason to look for a non-standard interpretation of what scientists do when they articulate their theories. One could push the point further: there is no evidence that scientists are mostly nominalists, so it is more sensible to take the assertions that scientists make at face value. The default interpretation should be that they intend to communicate the propositions expressed by the sentences they use.

Melia might respond by pointing to evidence that suggests an implicit commitment to nominalism in the scientific community. For example, it is puzzling that scientists are so willing to make statements which appear to imply the existence of mathematical objects even though the existence of such entities has not been established by typical scientific means. Melia could offer the thesis that scientists are nominalists as the best explanation of this. However, there are other explanations that seem to us at least as good: perhaps scientists are ignorant of the ontological implications of their theories with respect to mathematical objects; perhaps scientists consider the existence of certain mathematical objects to be obvious; or perhaps different scientists have different views on the matter. This move does not provide the justification Melia needs.

The burden of proof here lies squarely with Melia. To motivate his interpretation of scientists' assertions, he must provide evidence that the majority of the scientific community are nominalists. Because no evidence is currently available, there is no reason to think that scientists engage in weaseling.

We think this is a powerful objection to Melia's argument. Our view is that so long as his argument rests on such unfounded sociological claims, it fails. But this is not the end of the matter. In fact, there are promising avenues for further research on both sides of the debate. We will now outline how Melia might alter his position to avoid the above objection, and suggest how platonists should proceed in responding to this altered position.

Sorin Bangu argues that a more sensible means of deciding ontological questions is to focus on the features of *scientific practice* and *scientific theories*, rather than the beliefs and assertions of *scientists* (2012: 21). Melia's mistake was to rest his case on claims about what scientists happen to believe and say. Whether he is right or wrong about this, the question still remains as to what their beliefs *should be*, given their scientific practice and the theories that they endorse. Recall that Melia argues that the role of mathematical language in science is only that of making more things about the physical world expressible. Arguably, mathematical language does not have to be true to play this expressive role. In light of this, Melia might alter his response to the indispensability argument as follows. It is not irrational to be nominalist while simultaneously engaging in scientific practice and endorsing our best scientific theories. That is, whether or not they do, scientists *can* weasel without compromising their rationality. So the indispensability of mathematics to science does not require us to believe in the existence of mathematical objects. (Our discussion of weaseling in this section parallels previous discussion of van Fraassen's constructive empiricism. In particular, Rosen 1994 poses the question of whether constructive empiricism should be seen as a sociological claim about the beliefs of scientists, or as an epistemic claim about what it is rationally permissible to believe.)

This altered version of Melia's response avoids the above objection, and we think developing it should be a priority for critics of the indispensability argument. However, its success rests on Melia's claim that mathematics plays a purely representational or expressive role in science. Call this the *representational thesis*. According to the thesis, physical magnitude ascriptions mention numbers, but only to better represent purely physical properties of objects. The problem is that Melia offers no justification for this view, save the repeated assertion that the alternative view is implausible (see for example 1995: 229; 2000: 473; 2000: 474). According to Melia, the alternative view is that physical magnitudes, such as mass, length, and temperature, '*are* really fundamental relations holding between concrete objects and abstract numbers' (1995: 228-9). Call this view *heavy duty platonism* (HDP). Melia writes:

> I (like Field) find the Heavy Duty Platonism countenanced here implausible. But if you could convince me of it, I would retract my view… However, it is precisely [this] kind of metaphysical debate which philosophers *should* be focusing upon when trying to discover what there is. (1995: 229)

Melia clearly thinks that the representational thesis is more plausible than HDP. This explains why he takes the burden to be on the platonist to convince him of the truth of HDP. However, he does not provide any arguments for why HDP is so comparatively implausible.

Though he doesn't present any argument at this point, it is charitable to assume that Melia has in mind some objections that have previously been levelled at HDP. One such objection takes the form of a *reductio ad absurdum*. It starts with the assumption that HDP is true, and the premise that physical magnitudes are causally efficacious. The absurd conclusion is supposed to be that physical objects have the causal powers they do by being related to causally inert numbers (see Crane 1990: 225-6). Another objection highlights the fact that it is an arbitrary matter which number a given physical magnitude is measured with. It seems implausible to think that a certain magnitude is had by an object by its being related to all the numbers it is measurable by, and it seems equally implausible to think that one of these relations is in some way metaphysically privileged. From this we are supposed to conclude that HDP is implausible (see Crane 1990: 227 and Daly and Langford 2009: 643 for variations on this objection). Melia is certainly familiar with these objections, since both were presented in Crane 1990, a paper to which Melia responded (Melia 1992; Melia did not challenge the objections in his response).

Should we be persuaded by these long-standing objections to HDP? In Knowles (forthcoming), one of us argues that these objections to HDP fail – and that so do all the other objections to HDP to be found in the literature. If that is right, then Melia's assumption that HDP is less plausible than the representational thesis is too hasty. That reveals a promising means by which the platonist can defend the indispensability argument against even the altered version of Melia's response to the indispensability argument: argue that HDP in fact provides the superior account of the role of mathematics in science. Another option for the platonist is to attack the representational thesis directly. For their part, the nominalist should seek to defend the representational thesis and attack HDP. Our conclusion, then, is that discussion of weaseling will be advanced by debating the merits and demerits of HDP and the representational thesis.

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