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Immodest and Proud

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Abstract In his 'Ambitious, Yet Modest, Metaphysics', Hofweber (Metametaphysics, Oxford University Press, Oxford, pp 260–289, 2009a) puts forward arguments against positions in metaphysics that he describes as 'immodest'; a position he identifies as defended by Jonathan Lowe. In this paper I reply to Hofweber's arguments, offering a defence of immodest metaphysics of the type practiced by Lowe (The possibility of metaphysics, Oxford University Press, Oxford, 1998) inter alia.

1 Introduction

In his 'Ambitious, Yet Modest, Metaphysics', Hofweber (2009a) puts forward arguments against positions in metaphysics that he describes as 'immodest' a position he identifies as defended by Jonathan Lowe. In this paper I reply to Hofweber's arguments, offering a defence of immodest metaphysics of the type practiced by Lowe (1998) *inter alia*.

2 Science and Metaphysics: Introducing Immodesty

There is an important project in (meta)metaphysics; we must determine whether or not there are questions to which metaphysics can give answers. Specifically, we must delimit the domain of metaphysical enquiry in such a way as to ensure that there are questions of metaphysics to which the natural sciences do not already deliver answers. This might appear challenging. Indeed, according to Hofweber,

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things look grim for traditional metaphysics, for a number of traditional metaphysical questions appear to be answered very straightforwardly by the natural sciences.

For instance, metaphysics has traditionally sought to answer the questions of whether and how objects may change their properties over time. There is a sense in which this question is fully answered *without* recourse to metaphysics.

> Consider a candle that is bent after being left by the window during a sunny day. How was this possible, how could it have happened? The answer to this, empirical, problem is complicated, but known. It comes mostly from materials science and physics, and includes stories of the effects of sunlight on solid matter, the particular features of wax, and their dependency on temperature, and so on and so forth. The sciences have answered the question how this candle changed in this particular way, how it was possible, even though no one touched the candle. But once we know how a particular change was possible, don't we then know that change is possible, and how? What is left for metaphysics to do? (Hofweber 2009a: 261)

Similarly, consider the question of whether or not there are numbers. This question is traditionally conceived of as a question for metaphysics. But since mathematicians have established that there are infinitely many prime numbers, so mathematicians must be taken to have shown that there *are* numbers (cf. Hofweber, 2009a: 260–1). Once again, it is unclear as to what might be left for metaphysics to do.

There is an option available: an immodest option.[1] What might be left for metaphysics to do is to play a role that is *prior* to that played by physics and the other natural sciences. This, according to Hofweber, is for metaphysics to be both immodest and radical. Hofweber identifies Lowe (1998) as a defender of immodest metaphysics and describes Lowe's position as follows:

> The sciences by themselves do not answer the question how the candle changed its shape, and mathematics by itself does not answer the question whether or not there are infinitely many prime numbers. Rather they assume or presuppose that change is possible at all/that numbers exist at all. And only under these assumptions do they then establish that there are prime numbers/how the candle changed. (2009a: 262)

The assumptions in question are then to be discharged by metaphysics. Thus, *if* metaphysicians can show that there are numbers, *then* the mathematicians may be in a position show that there are infinitely many prime numbers; *if* metaphysicians can

[1] In fact, there are (at least) two options available. According to Hofweber, there is scope for us to carry out modest metaphysics. To do this, Hofweber claims, we must deploy a distinction between internal and external quantifiers. As he (2009a: 288) notes: 'Metaphysics will be alright, but it will be different than how most metaphysicians think of it.' What I'm interested in, here, is whether or not we can continue to think of metaphysics 'as most metaphysicians think of it'. I don't engage, then, with Hofweber's positive re-construction of the project of metaphysics. Rather, I'm interested (in this paper) in defending the traditional approach from his attack. For more on Hofweber's distinction between internal and external quantifiers, see Sect. 4.

show that change is possible, *then* the natural sciences may be in a position to show how this occurred. This leaves a clear role for metaphysics.

But, according to Hofweber, it is not a role that metaphysics can play. Indeed, Hofweber offers two arguments against immodest metaphysics being practiced in the way described. The first focuses on the way in which science is carried out. The second levels a charge of regress against the immodest metaphysician. In Sect. 2 I will reject the first charge; in Sect. 3 the 2nd. In Sect. 4 I will argue that I am not attacking a straw-man. In the final section of the paper, I offer an argument intended to show that, regardless of the traditional focus in metaphysics, we have some reason to think that certain distinctively metaphysical questions will forever remain beyond the reach of the natural sciences.

3 Immodesty and Scientific Practice: The Scientific Track-Record

Here is Hofweber (2009a: 263—my italics).

> The mistake on the immodest philosopher's side is to think that scientific theorizing works this way: it first makes certain general assumptions (that there is a material world, that it contains objects, that they change, that there are numbers, etc.) and then given these assumptions science tries to find out some more of the details. *To the contrary, the sciences establish their results without needing any further vindication from philosophy. That there are numbers and that change is possible is implied by the relevant theories, not assumed or presupposed.*

Crudely, then, we seem to be faced with two views. On the one hand, it seems that Lowe thinks that positions in metaphysics are prior to positions in physics. We are entreated to settle the question of *whether* there are xs before we can engage in scientific enquiry, where that scientific enquiry seems to be about the xs. On the other hand, Hofweber thinks that positions in physics are prior to positions in metaphysics. We are entreated to consider what is entailed by the physics and treat the results as our ontological commitments. But then there is no role for metaphysics.[2]

3.1 Who to Believe?

We face a choice: who to believe? Lowe or Hofweber? Elsewhere, Hofweber makes his case:

> The success of the sciences is so impressive that it would be anything but excessive immodesty on the part of philosophy to think that anything it can do could turn this success into failure. When science and philosophy clash, it seems wise to put ones chips on the sciences. (2009b: 288)

[2] Or, at least, that whatever role there is for metaphysics it is not the same as that described by Lowe.

On the face of it, this is what we might call a 'track record argument'. It proceeds from the assumption that science has been hugely successful, adds in the assumption that metaphysics has been less successful, and concludes that we should side with science. I do not think that this is at all persuasive.

As is made clear elsewhere in the literature, track record arguments *generally* tend not to be successful. Typically, these arguments are discussed in the philosophy of mathematics and follow this quotation from Lewis:

> Mathematics is an established, going concern. Philosophy is as shaky as can be. To reject mathematics for philosophical reasons would be absurd … Even if we reject mathematics gently – explaining how it can be a most useful fiction, 'good without being true'– we still reject it, and that's still absurd. …
>
> That's not an argument, I know. Rather, I'm moved to laughter at the thought of how presumptuous it would be to reject mathematics for philosophical reasons. How would you like the job of telling the mathematicians that they must change their ways, and abjure countless errors, now that philosophy has discovered that there are no classes? (Lewis 1991, pp. 58–59)

Daly and Liggins (2011) note three objections to track-record arguments in the context of the philosophy of mathematics. Daly and Liggins (2011: 326) offer one argument against the *general* use of track-record arguments against philosophical positions; I shan't repeat that, here, though I do think it correct. My concern is more specific. I want to allow that *in general* perhaps it's right to think that track record arguments can do some work. What I want to show is that in this particular case, a track record argument simply will not do the job required.

Daly and Liggins go on to offer an argument against the use of track record arguments against fictionalism in the philosophy of mathematics—where they treat fictionalism as the view that mathematical claims are false but useful. I want to briefly show how both arguments carry across to the debate surrounding persistence. By doing so I show that, regardless of the *general* success of track-record arguments, Hofweber's use of the general argument form fails.

3.1.1 Unpacking the Case in Fictionalism; Moving to Persistence

To begin, Daly and Liggins (2011: 324) 'describe deference to mathematics', which they take to be expressed by the quotation from Lewis:

> (Deference to Mathematics) If a philosophical theory is inconsistent with mathematics, then we should reject the philosophical theory for that reason.

Here, I'll follow Daly and Liggins as treating mathematical fictionalism is the view that all mathematical claims are false.

Given that view of mathematics, it would therefore be question-begging in the extreme to adopt a track-record argument in favour of the view that mathematics has a better track record than philosophy. After all, what is at issue *is*, at least in part, the track-record of mathematics: if mathematical fictionalism is true, then all historical mathematical putative truths were false. But if historical mathematical putative

truths were false, then we cannot appeal to their truth in order to establish that the discipline of mathematics has generated many mathematical truths. When the track-record of mathematics is in question, we cannot appeal to the track-record of mathematics.

I will now demonstrate that the same point can be made in discussion of persistence. To the best of my knowledge, this claim is novel; I am aware of no-one demonstrating that, in debates about persistence, scientific-track-record arguments fail to establish that persistence occurs.

Recall the quotation from Hofweber: 'The mistake on the immodest philosopher's side is to think that scientific theorizing works this way: it first makes certain general assumptions (that there is a material world, that it contains objects, that they change, that there are numbers, etc.) and then given these assumptions science tries to find out some more of the details.'

To give us some traction, here, let us consider a view according to which there is no persistence. Thus, all of the putative theories of persistence fail to apply to the actual world. Whilst there is a moment-by-moment change in what exists, there are no objects undergoing any change. Just as the mereological nihilist (who denies that there are composite objects) claims that our talk about composite objects is useful but false by recourse to paraphrase, so too will our change-denier—who I'll call a delta-nihilist.

The mereological nihilist denies that there are composite objects, but agrees that there are collections of mereological simples (objects without parts) that arrange themselves in particular ways. Thus, although there are no molecules, there are mereological simples arranged molecule-wise. In the same way, then, the delta-nihilist denies that there are persisting objects, but agrees that there are instantaneously existing objects, such that they can be said to be arranged persisting-object-wise.

To illustrate delta-nihilism further, start off with the presumption that perdurance is the correct view of persistence, according to which an entity can be said to persist through time by virtue of having different parts at different times. From this starting position, then deny that there is such a relation as (temporal) parthood. From this, delta-nihilism follows. Or, to go about matters another way, suppose that the world is as the exdurantist has it, and objects can be said to persist by virtue of standing in counterpart-theoretic relations to objects in the future. From this picture, simply excise counterpart relations (more perspicuously, perhaps: deny that the counterpart relation is sufficiently strong a connection to make it true that an object persists).[3] Roughly, then, we should say that there are un-persisting objects arranged persisting-object-wise.

According to Hofweber's reasoning, we should take the view delta nihilism is false, as the theories of science clearly commit us to the existence of persisting entities. To illustrate that they really do show this, Newton's first law provides a nice illustration:

[3] In effect this is to say that exdurance is right about how world is (entities are counterpart related to one another) but that the endurance theorist or perdurantist is right about what is required for persistence.

> When viewed in an inertial reference frame, an object either is at rest or moves at a constant velocity, unless acted upon by an external force.

An object cannot be said to *move* a constant velocity (which is to say, *change* its location) unless it persists. Thus, Newtonian physics entails that objects change.

For a more up-to-date case, consider the following (chosen entirely at random) from Physical Review Letters:

> The real question is the decay rate of a supposed tetraquark meson. If the width of the tetraquark grows as some power of N, while its mass is independent of N, then for very large N it may not be observable as a distinct particle. Although Coleman did not address this issue, his discussion may suggest that the rate for a tetraquark meson to decay into two ordinary mesons must grow with N. As we will now see, this is not correct. (Weinberg 2013, 261601-1)

If a tetraquark meson can be said to *decay* into two ordinary mesons, and two ordinary mesons *grow*, then the tetraquark meson must be able to persist—otherwise the language simply makes no sense. Thus, given the track-record of physics and the fact that it commits us to the existence of persisting objects, delta-nihilism is false.

But, once again, this is too quick. The delta-nihilist will reply: the majority of physics is not true; it is merely useful. What is true is that there are non-persisting entities arranged persisting-object-wise. And whilst the truth of claims about non-persisting entities arranged persisting-object-wise is sufficient to explain why we speak of there being objects that persist (in the same way that there being mereological simples arranged composite-object-wise suffices to explain why we frequently assert that there are composite objects), it is nonetheless false to speak of there having been persisting entities.

The salience of this point should be clear. All (or nearly all, I'll return to this in a moment) science is predicated on there existing persisting entities. If the track-record of mathematics cannot be appealed to when the historical truth of mathematics is in question, then the track-record of physics cannot be appealed to when the historic truth of physics is in question. And since so much of our best physics appeals to claims about persisting objects, that pretty much writes off any appeal to the historic track record of physics.

Of course, it's easy enough to imagine a reply. That is, although it's true that we can't straightforwardly appeal to the track record of physics, that's ok. We don't need to. The point that Hofweber is looking to make is not a track-record argument. Rather, it's a familiar argument from the realist camp. We must explain the success of science somehow. There's one very obvious explanation; the reason that science is so very successful is that it's tracking the truth. And, even if some of the more technical aspects of our best science aren't currently true, they're pretty close, and getting closer all of the time. All of our best physical theories entail that there are persisting objects. The only way to explain the success of these theories is that they *are* tracking the truth. And the only way that they can be doing that is if there *are* persisting objects. Thus, we have a perfectly good argument against delta-nihilsm.

This reply faces an obvious problem. Call the view that objects do persist, 'persistence-realism'. The delta-nihilist claims that persistence-realism is observationally equivalent to persistence anti-realism (the view that objects do not persist); there is no observation that would tell either way. There are no observable differences between delta-nihilism and persistence realism.

And, recall, the delta-nihilist is offering us a paraphrase of all seemingly true talk about persistence; although there are no persisting objects, there are instantaneously existing objects arranged persisting-object-wise. So, not only can the delta-nihilist point-out that no observation could ever tell between the two positions, they can also offer a perfectly coherent explanation of the success of our best, false, science. These scientific theories are false, but there is a systematic connection between every false claim that they make about persisting objects and a nearby true claim of the form, 'there is a collection of instantaneously existing objects arranged persisting objects-wise'. Because the connection between the false science and true paraphrase is systematic (it holds right the way across our language and cases in the world), and because there is no observable difference between the commitments of the two views, we have our explanation: the success of science is explained by it systematically tracking the truth, whilst narrowly missing due to an observationally undetectable error; since the error does not make any difference to what is (or could be) observed, the falsity of the science is no obvious threat to its success.

As it will prove useful in what follows, let me summarise. The immodest metaphysician (e.g. Lowe) claims that we need to settle what's possible in order to settle what's actual. As a consequence, we must engage with metaphysics before we can hope to determine the truth of physical theories. The modest metaphysician (e.g. Hofweber) claims that settling what's actual *does* settle (at least some of) what's possible (as well as what's actual), and so there's no need to engage in metaphysics before we try to carry out our scientific enquiry.

The reason that I urge that we come down on the side of the immodest metaphysician, here, is that, as we have seen, science is blind to various claims in metaphysics: if there are no observable differences between two metaphysical hypotheses, and we have a perfectly good explanation of the success of a putative scientific theory, then even if our metaphysics claims that our physics is false, that is no reason to reject the metaphysical hypothesis. There are many metaphysical possibilities—possibilities that seem *reasonably* plausible—that physics simply cannot help us choose between, and where not all of which are compatible with the truth of our current physical theories. Thus, it would seem, if we are to determine the *truth* (as opposed to mere usefulness) of the claims of the natural sciences, then we would be well advised to engage in metaphysics before we engage in physics.

4 Immodest and Regress

Let me flag, explicitly, that what immediately follows is a presentation of Hofweber's argument—albeit one that is unpacked a little. I don't endorse it. Nonetheless, here it is.

> Immodest metaphysicians claim that metaphysics is prior to physics. They claim that metaphysics must settle what's possible, before physics can come along and settle what's actual. But, just as the modest metaphysician makes the assumption that physics *is* possible and ignores the alleged priority of metaphysics, so the immodest metaphysician must assume that *metaphysical enquiry* is possible, prior to engaging in their studies. And if the physicist should defer to the metaphysician, then the metaphysician had better defer to whoever it is that determines whether metaphysical enquiry is possible—the meta-metaphysician. Now the regress looms: if the metaphysician should defer to the meta-metaphysician, then the meta-metaphysician had better defer to the meta-meta-metaphysician, to adjudicate upon the question of whether or not *their* enquiry is possible. There seems to be no end in sight for this regress. (Good news for the growth of sub-disciplines in philosophy; bad news for the immodest metaphysician.) If physics requires metaphysics to get at the truth, then we can never get at the truth, as metaphysics requires meta-metaphysics, which requires meta–meta-metaphysics, which requires….etc.

I'm a little more optimistic than Hofweber (I suspect that it goes hand-in-hand with the immodesty). I think that there is a striking disanalogy between the cases.

According to the immodest metaphysician, the task for metaphysics is to determine what's possible and what's necessary; the task for physics is to determine what's actual. But where Hofweber thinks that we can then ask about some other task that must be completed before we engage in inquiry, I do not. Let me start with a question: what task is supposed to be left for the meta-metaphysician? We've settled what's necessary. We've settled what's possible. We've settled what's actual. Everything not on one of our three lists is impossible. There's nothing left to ask about; there is no task to be completed. What question, then, is the meta-metaphysician supposed to be asking? I find it hard to tell. And, of course, if there are no questions left to be asked, then it's hard to see how the regress gets going.

The problem in engaging in this task is that Hofweber himself says so little about how exactly the regress is supposed to get going. I suggest two candidate subjects: truth and knowledge. I will argue that there is no obvious reason to think that either generates a regress. Thus, there is no obvious reason for the immodest metaphysician to worry.

4.1 Truth and a Regress

One thing that we might say is that in order for physical theories to be true, corresponding metaphysical theories must be true. That is, in order for various claims about what's actual to be true, various claims about what's possible and necessary must be true. And in order for various claims about what's possible and necessary to be true, various *other* claims about what's possible and necessary must be true.

In a bit more detail: in order for physical theories to be true, various metaphysical hypothesis must be true. For example, in order for extant theories about tetraquark mesons to be true, at least *a* theory of persistence must be true. But in order for a theory of persistence to be true, it must be the case that certain *other* metaphysical theories

are true (for instance, a particular theory about the nature of time must be true, for an object cannot persist through time, unless there is time). In order for that metaphysical theory to be true, other metaphysical theories must be true—and so on and so forth.

Now, while this all strikes me as correct, it's hard to see it as a threat to the immodest metaphysician. Everything that has been said so far is right. But what of it? There is no *regress*, here. All that has been stated is a commitment to some kind of holism, where the truth of claims made in one sphere depend upon the truths made in another. The truths of the various spheres are connected. That's well and good. But, to repeat, it's hard to see this as a problem for the immodest metaphysician. There is certainly no evidence of a regress. Perhaps there is something lurking here. But then we should be given its details. At the moment, there simply are none.

4.2 Knowledge and a Regress

Perhaps we should treat *knowledge* as the basis for the proposed regress argument. *Perhaps* what Hofweber *wants* to do is to make this about *knowledge*, rather than truth. Thus, the desired argument would be of the form: in order to *know* that our scientific theories are true, we first have to come to know that the corresponding metaphysical claims are true. Once we endorse that step, then it's easy to get the regress going. In order to *know* that metaphysical claims are true, we have to first *know* that some other claims are true. And in order to *know* that the meta-metaphysical claims are true, we have to *know* that the meta–meta-metaphysical claims are true. And so on and so forth.

Suppose that's right; suppose Hofweber's concern is about knowledge. If it is, then I don't see that the argument has much force. The immodest metaphysician (e.g. Lowe) claims that there is genuine metaphysical work to be done, if we're to *know* that our best physics is true. Why? Well, because physics is blind to what's possible: physics doesn't give us the tools to decide between competing metaphysical hypotheses. But note: these really are *competing* hypotheses that the metaphysician thinks that we must choose between.

The immodest metaphysician is, thus, not simply engaged in the (tiring) game of the knowledge-sceptic, repeatedly asking 'but how do you know *that*?' of each proposition asserted as true by their interlocutor. If the immodest metaphysician were behaving in such a fashion, then, yes; then we would face a regress. For at every level we would be prone to the 'but how do you know that?' question.

But if we're to set-off on a regress when taking the approach adopted by the immodest metaphysician, then it would have to be the case that, at every level of the regress, we had genuinely competing theories that nothing at that level could tell-between, and so would require a move to a higher level. That, after all, is what is now supposed to be driving the move to think that there is something (metaphysics) that is prior to physics. That our best physical theory couldn't tell between two competing (and, we're supposing) plausible metaphysical theories is precisely what required the metaphysical questions to be settled prior to the physical questions.

Now, perhaps that's right. But if it is right, two thoughts come to mind: first, it would need to be proven. That is, Hofweber would sure have to *show* that these

competing meta-metaphysical theories exist and that we have nothing to choose between them given our best metaphysics. Then we would have a proper analogy with the physical case to drive us to settle meta-metaphysical questions prior to settling the metaphysical questions.

Second, and perhaps more worryingly, it seems highly unlikely that this could be done. For, quite simply, we don't have that many theories! Remember, what Hofweber is after isn't *merely* a move to meta-metaphysics; it's a regress. What we're supposed to face is an infinite regress that troubles the very possibility of metaphysis and physics. But in reality, beyond some meta-metaphysical theories, matters go dark: to the best of my knowledge, at any rate, there *are* no meta–meta-metaphysical theories, competing for primacy. There certainly don't seem to be any meta–meta-meta-metaphysical theories, each of which equally plausible, such that meta–meta-metaphysical theories cannot settle which is right, and which therefore force us to choosing between meta–meta-meta-metaphysical theories *before* we engage in meta–meta-metaphysics. And we certainly cannot get all the way to an infinite regress. Perhaps Hofweber has something else in mind with the charge of regress; but in that case, he should be forthcoming with it.

5 Questions of Interpretation

There is an aspect of Hofweber's (2009a) that I've not yet mentioned, but which is a central focus of that paper. This additional element of Hofweber's position is that it is a crucial question whether we take the quantifiers of mathematical and scientific statements to be 'internal' or 'external'; further that this question is to be answered by considerations in the philosophy of language. These two readings of the quantifiers are important and (or so we might think) bear directly on the current project.

Before we get to the relevance to the current project, let's review the claim about the quantifiers.

> On one of them they make a claim about the domain of objects that they range over, a claim about what the world contains. This reading is the active one in a common utterance of:
>
> (9) Someone kicked me.
>
> Call this the *domain conditions reading*, or *external reading*. (Hofweber, 2009a: 277)

In contrast, now consider the internal reading.

> In addition, they have a reading tied to an inferential role, a certain way in which quantified statements inferentially relate to quantifier free ones. An example to illustrate this use of quantifiers is a common utterance of:
>
> (10) There is someone we both admire.
>
> when I have forgotten who it is. All I want to say is that:

(11) You admire X and I admire X.

It is supposed to be the very same X, although I can't remember who X is. To get that across I need a quantifier, but not one that ranges over what the world contains. (Hofweber, 2009a: 277)

Hofweber claims that the kind of number-talk that 'the folk' and scientists deploy, involves merely using internal quantification. That is, there is no ontological commitment loaded into this use of number-talk. There is only an inferential role played by the quantifier phrases in question. If we wanted to use these considerations to generate ontological commitment, we would have to demonstrate that the quantification involved was external. And it is not.

We can now turn our attention to the relevance to the current project. It would seem to follow from this that all that the successes of the sciences justify is the fairly trivial conclusion that the internalist readings of quantified mathematical and scientific statements are true. This concession does not imply that, say, numbers really exist or belong in one's ontology; again, there is no ontological commitment attached to the internal reading. Call this Hofweber's 'internality argument'.

What this shows, importantly, is that Hofweber doesn't mean to treat either the track record argument or the regress argument as persuasive on their own terms. Rather, what Hofweber intends is that the 'internality argument' shows that immodest metaphysics does not succeed.

There is an interesting question about whether or not the internality argument is intended to be regarded as independent from the track-record and regress arguments. I claim that it is—but I also claim that it doesn't matter too much whether I'm right about that (though, again, I think that I am).

5.1 The Right Interpretation

When Hofweber (2009a) presents the track record argument, briefly, the presentation does not presuppose any of the results later generated by the internality argument. Indeed, the dialectical role of the track-record argument—at least as it is presented—is to militate against Lowe-style positions. Hofweber then presents his own view, based on the two readings of the quantifiers, as a way of steering a course between the immodest position (that has already been rejected) and the view that there is nothing for metaphysics to do.

As Hofweber himself puts matters (2009a: 263–4)

Besides the immodest attitude there is another extreme, which we'll call the unambitious attitude. A philosopher who has this attitude will look at the closest science to see what it implies for a certain question which is traditionally thought of as a metaphysical one. Is everything water? No, various sciences found other stuff. Is time travel possible? Let's look at physics and see what it says. And so on. The unambitious attitude works out the consequences that other parts of inquiry have for questions that are traditionally considered philosophical. It is like popular science journalism,

> getting clear on the consequences of the sciences without contributing to them, for a general audience. If metaphysics is a legitimate project it has to find a place in between these two extremes.

Thus, we see, the immodest approach has already been ruled out. With it ruled out, Hofweber (2009a: 264) thinks, we must now move on to see whether an appropriately modest project can succeed.

I think, therefore, that it's very tempting to read Hofweber as treating the track record argument as a stand-alone argument against immodest metaphysics that is intended to apply quite independently of whether or not we should ultimately conclude that typical number-talk involves internal or external quantification. *That* immodest metaphysics cannot succeed is presented to us as an argument that moves in the direction of seeking modest metaphysics.[4]

Further evidence for this interpretation of Hofweber is generated by the fact that in his (2009b) presentation of the track-record argument against immodest metaphysics, he makes no appeal to, and engages in no discussion of, the distinction between internal and external quantification. The arguments presented in that (2009b) paper certainly read as if they are intended to be stand-alone arguments designed to show that the immodest metaphysician should give up their ways—and they should give up their ways quite independently of whether or not what Hofweber has to say about internal and external quantification is correct. As a consequence, the responses that I offer seem to be to be legitimate responses to the view that Hofweber defends and not to target a mere straw-man.

(It's also worth noting that, if Hofweber is right about the internality argument, then both the track record and regress arguments are superfluous. After all, the internality argument *itself* is supposed by Hofweber to show that questions about whether (for instance) numbers exist are properly settled by engaging in the philosophy of language and not metaphysics. If such questions are not settled by metaphysics, then there is no need to argue explicitly against immodest positions, such as that taken by Lowe. After all, that the immodest metaphysician is in the wrong is guaranteed by the internality argument. I do not think it likely that Hofweber would (twice—2009a, b) offer superfluous arguments.)

5.2 How Much Does it Matter?

So, I think that the right interpretation of Hofweber is that both the regress and track record arguments are taken to be pressing against the immodest metaphysician[5] and that they are intended to be doing genuine, independent dialectical work within the papers where they're offered.

Nonetheless, one might think that this doesn't matter too much. For instance, one might simply take the arguments in Sects. 2 and 3 to show, not that immodest metaphysics is acceptable, but that we should simply reinterpret Hofweber's project as one in which the internality argument *is* to be viewed as essential to the success

[4] Similar remarks apply to the regress argument.

[5] Or, at the very least, that this is the interpretation suggested by the texts.

of both the track-record and regress arguments. Thus, although track record arguments against immodest metaphysics aren't persuasive, a track record argument in conjunction with the internality argument *is* persuasive; although the regress arguments against immodest metaphysics aren't persuasive, a regress argument in conjunction with the internality argument *is* persuasive.

Perhaps this isn't the most natural interpretation of the texts—indeed, I have argued that it is not. But to reflect this, we can easily just describe this package of arguments as Hofweber's* argument. Given the obvious availability of Hofweber* arguments, why bother with the arguments I've presented here?

There are still, I think, two reasons for caring about the track record and regress arguments. First, it is an independently interesting question as to whether or not the track record and regress arguments are enough for us to reject immodest metaphysics. It is certainly common enough to find philosophers who (to put it crudely) think that science trumps metaphysics (the track record argument—see, *inter alia*, Ladyman and Ross (2007: e.g. pp. 6–7), and there are also those who would question how metaphysics can be viewed as prior to science.[6] Thus, in presenting such arguments as these, Hofweber is hardly alone. It is therefore of independent interest as to whether the arguments Hofweber presents can be run independently of the internality argument

Second, from the current state of the literature, it is not at all clear that Hofweber's internality argument succeeds. As the reader will recall from Sect. 4, Hofweber claims that there are two readings of the quantifier, and that in the in the mathematical case and the case involving change, the kind of that 'the folk' and scientists deploy involves merely using internal quantification. That is, there is no ontological commitment 'loaded' into their use of number-talk. To explain and illustrate why we don't also get metaphysical commitment loaded in, Hofweber (2009a: 278) reasons as follows:

> the loaded counterparts are, in the relevant uses, focus constructions. They present the same information with a different emphasis.

Again, in his own words (2009a: 278–9—with Hofweber's numbering):

> The basic idea is this: sentences like
>
> (21) I had two bagels.
>
> and
>
> (22) The number of bagels I had is two.
>
> have a quite different role in actual communication, despite the fact that they are, apparently, truth conditionally equivalent.

[6] See, *inter alia*, Hawley (1999: 479). Of course, it's not at all clear that Hawley's very brief remarks on this score entail a regress argument. However, her concerns about how knowledge of metaphysical possibilities can be generated, indicate that a regress argument is one natural way to unpack Hawley's brief remarks (cf. 3.2 and the 'knowledge regress'). See, also, Ladyman and Ross (2007: 6–7).

As Hofweber points out, sentences like (21) do not seem to commit us to the existence of numbers; sentences like (22), do.

So, Hofweber presents arguments intended to support the claim that what is going on *in the mathematical* case and *the case involving change* are instances merely of differing force constructions. If he is right, this leaves us with *seemingly* metaphysically loaded statements like (22) not in fact being metaphysically loaded. But the arguments in support of this claim are ones that he provides elsewhere.[7] The point salient to us, in any case, is that there are a range of responses to Hofweber's claims concerning force construction, as they apply to these cases. In particular, Balcerak-Jackson (2013, 2014) presents what I take to be compelling arguments *against* Hofweber's account of what occurs in the mathematical case. I don't repeat these here, though I do recommend them to the reader.

My point, in any case, is not that Hofweber's claims about the readings of the quantifiers are wrong. My only intention here is to point out that whether or not the *seemingly* metaphysically loaded paraphrases of sentences like his (21) mean that sentences like 'I had two bagels' commit us to metaphysically loaded counterparts, remains an open question. It certainly still seems possible that they do so commit us and that the mathematical case and the case involving change involve instances of external—metaphysically loaded—quantification.

That possibility then leaves it independently interesting as to whether or not Hofweber's arguments (denuded of the internality argument) would still suffice for us to reject immodest metaphysics. My suggestion, as should be clear, is that they do not, and that immodest metaphysics survives the track record and regress arguments.

6 A Final Thought

For the bulk of the paper I've focused on rebutting various remarks from Hofweber, that purport to show that we should not treat metaphysics as prior to science. In this final section, I want to suggest (though I concede somewhat speculatively) that there is an important reason to think that there are at least some metaphysical questions that science can never hope to answer, and that the ultimate success of science depends upon the possibility of metaphysics.

In particular, I want to focus our attention on the following sentence,

(S) Science is complete

I think that it's reasonable to suppose that (S) entails various consequences. In particular, (S) entails that:

(S)* There are no (observable?) phenomena that are unexplained by our scientific theories

Certainly, I fail to see how science could be viewed as complete if (S)* is false. Moreover, I take it that for science to be said to be an *unqualified* success, it ought to be complete. For whilst I'm happy to restrict S* to including only cases involving concrete

[7] He references Hofweber (2000, 2005).

objects, it would seem odd to say that there are phenomena involving concrete objects that science cannot fully explain, and yet also that science is complete.

So, let us assume, then, that (S) entails (S)*. (S)* is a negative existential. It is a true claim about what does not exist: it says that what 'does not exist' is a phenomena that is unexplained by our scientific theories. There is then an issue. Nothing in our best science would appear to explain how negative existential truths get to be true. I cannot see—even in principle—how such an answer would go (though I concede I would be interested to see such an answer).

This question is distinctively metaphysical. It has received extensive treatment in the philosophical literature: perhaps such truths require truthmakers (e.g. Cameron (2008)); perhaps they do not (e.g. Tallant (2010)). So whilst I cannot rule it out, I see no prospect for our best science delivering an account of how (S)* gets to be true. Thus, the very possibility of it being true that science is complete *establishes* that there is work for the metaphysician to do: working out a convincing theory about how truths are connected to the world and of determining how negative existential propositions get to be true.

Of course, one might ultimately find that locating truthmakers for sentences like (S)* is a project for philosophy of science. Or, one might think of this as a project for meta-science. And, I concede, we might end up in such a situation 1 day.

But that day is not today. For the time-being, at least, sentences like (S)* require consideration from metaphysics. Only within metaphysics do we find suitable discussion of how truth might come to be grounded in the world, such that sentences like (S)* turn out to be true. Things may change, of course. But, for the time being at any rate, I cannot see how such a change would be affected. And, in that case, it would seem that the very possibility of a complete science rests upon an answer to a distinctively metaphysical question—namely, how the truth of (S*) gets to be grounded by reality. Of course, since the possibility of a complete science rests upon there being a satisfactory answer to this question, so the possibility of a complete science rests upon metaphysical considerations.

7 Concluding in Immodesty

It seems, then, that immodest metaphysicians may continue a little while longer in their efforts. The traditional questions that have vexed them (whether there is change, etc.) are sensible questions. They are not questions to which science provides an answer. Rather, the questions of metaphysics are questions to which answers must be given, *before* answers can be given to scientific questions. This is immodest. But for everything that's been said, it still seems to be true.[8]

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[8] I'm very grateful to three referees for this journal for their comments on an earlier version of the paper.

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