On *Absolute Generality*

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On my blog (logicmatters.blogspot.com), I've very rather intermittently commenting on the papers in *Absolute Generality*, edited by Augustín Rayo and Gabriel Uzquiano (OUP, 2006), preparatory to writing a review of the book. This document brings together the comments so far, pretty much unedited.

The editors take the easy line of printing the papers in alphabetical order by the authors' names, and they don't offer any suggestions as to what might make a sensible reading order. So we'll just have to dive in, taking the papers in more or less their given order, and see what emerges.

1 Kit Fine, 'Relatively Restricted Quantification'

First up is a piece by Kit Fine. And it has to be said straight away that this is, presentationally, pretty awful.[1] This isn't just me being captious: sitting down with three very bright and knowledgeable graduate students and a recent PhD, we all struggled to make sense of it. There really isn't any excuse for writing this kind of philosophy with less than absolute clarity and plain-speaking directness. It could well be, then, that my comments on this paper – such as they are – are based on misunderstandings. But if so, I'm sure this is not entirely my fault!

1.1 The All in One principle

Fine holds that if there is a good case to be made against absolutely unrestricted quantification, then it will be based on what he calls "the classic argument from indefinite extendibility". So the paper kicks off by presenting a version of the argument.

Suppose the "universalist" purports to use a (first-order) quantifier $\forall$ that ranges over *everything*. Then, the argument goes, "we can come to another understanding of the quantifier according to which there is an object ... of which every object, in his [original] sense of the quantifier, is a member". Then, by separation, we can define another object R whose members are all and only the members of *that* object (the universalist's original domain) which are not members of themselves – and on pain of the Russell paradox, this object cannot be in the original domain. So we can introduce a quantifier $\forall^+$ that runs over this too, and hence the universalist's quantifier wasn't absolute general.

Well, *this* general line of argument is of course very familiar (even if Fine's version isn't the all-time clearest). What I initially found a bit baffling is Fine's claim that it *doesn't* involve any appeal to what Cartwright famously calls the All in One principle. Here's a statement of the principle at the end of Cartwright's well-known paper:

[1] Length issues aside, no way would something written like this have got into *Analysis* when I was editing it.

> Any objects that can be taken to be the values of the variables of a first-order language constitute a domain, where a domain is something set-like.

Which looks to be exactly the principle appealed to in the first step of Fine's argument. *So why does Fine say otherwise?*

Well, Fine picks up on Cartwright's initial statement of the principle:

> to quantify over certain objects is to presuppose that those objects constitute a 'collection' or a 'completed collection' – some one thing of which those objects are members.

And then Fine leans heavily on the word 'presuppose', saying that his extendibility argument isn't claiming that an understanding of the universalist's $\forall$ already presupposes a conception of the domain-as-object and hence an understanding of $\forall^+$; it's the other way around – an understanding of $\forall^+$ presupposes an understanding of $\forall$. Well, indeed. But I'm sure that Cartwright was *not* saying otherwise, but at worst slightly mis-spoke in presenting the argument that he is discussing. His idea, as the rest of his paper surely makes clear, is that the extendibility argument relies on the thought that where there is quantification over certain objects then we must be able to *go on* to take those objects as a completed collection – but I don't think that Cartwright is committed to any claim that the argument supposes that the *very business of understanding quantification* requires thinking of the objects quantified over as constituting another object.

Anyone persuaded by the criticisms of extendability arguments in Cartwright's paper, then, won't find Fine's version of the extendibility argument any more convincing than usual.

1.2 Showing and saying

Suppose I *do*, however, think that there is something problematic about absolutely general quantification. So I try to say 'You can't quantify over absolutely everything'. But either that 'everything' is absolutely general, and I've illustrated how you can quantify over absolutely everything after all. Or else my 'everything' is restricted, and I fail to say what I meant to say. Either way, my attempted saying misfires.

So that easy argument disposes of the anti-absolutist? Well, hardly! I just need to be a bit more dialectically supple: to be an anti-absolutist *I shouldn't assert a position myself, but rather stand ready to reveal the tempting confusion that the absolutist has fallen into.* Faced with a philosopher who stakes out an absolutist position, the enlightened opponent hits him with an extensibility argument ('Ah, take those things you are quantifying over all together as one big domain; now consider the bit of the domain which contains all the non-self-membered things you were quantifying over; then that isn't one of the things you were quantifying over, on pain of Russell's paradox'). Then – assuming of course the cogency of such extensibility arguments – the absolutist is in trouble. Which is something the enlightened philosopher, to coin a phrase, *shows* rather than *says*.

Now, Fine, at the end of §2 of his paper, floats the possibility of taking this rather Wittgensteinian line. And it would have been good to explore it more. But in fact, Fine himself doesn't endorse this line. Rather there are another fifteen pages in which he tries to find the words in which one might, after all, cogently state an anti-absolutist position. The idea is to go modal, and talk in particular about "postulational modalities". But this, however, all gets deeply obscure.

1.3 Postulational modality

And indeed, I think I'm going to have to admit defeat. Even after re-readings, I'm still stumped by Fine's positive claims about 'postulational modality'.

The defender of indefinite extensibility thinks that "whatever interpretation [of the supposedly absolutely general quantify] our opponent might come up with, it will be possible to come up with an interpretation that extends it". And supposedly the second modality here, at any rate, is "postulational". Whatever exactly that means.

Presumably the thought is something along the lines that whatever objects you are quantifying over, I can *postulate* another one – the set-like collection of all the sets you are quantifying over which aren't members of themselves – which can't already be in your domain of quantification, on pain of paradox. But how precisely does this differ from there *being* such a set-like collection? On the one hand, if Fine is to be making a new move here, there better be a difference; on the other hand, it is difficult to understand what the move is without a clear account of the difference – i.e. a treatment of the metaphysics of (some) mathematical entities as postulated entities, which Fine doesn't give us here.

But set aside those worries. Let's suppose that we agree that, while there isn't any sense in which you can just postulate new donkeys into existence (so 'there are no talking donkeys' isn't, so to speak, vulnerable to a legitimate postulated extension of the domain of quantification), you *can* 'just' postulate new sets (or set-like collections). Well, *so what?* Why can't the defender of absolute quantification just aver that when he says, e.g. 'Everything is self-identical' or 'Nothing is a talking donkey' he already means to cover whatever your postulational ingenuity might come up with – and then dig his heels in when you insist that you can still find another entity which might comprise all those things at once (so he is vulnerable to the extensibility argument). Rather he takes the argument of Russell's paradox as showing us that there is no such single entity available to be postulated.

Which is a now familiar dialectic of course. So what I'm missing is how talk of 'postulational modality' is really supposed to move things forward. As I say, I'm stumped.

2 Michael Glanzberg, 'Context and Unrestricted Quantification'

2.1 Contextualism

According to Michael Glanzberg, quantifiers always have to be understood as ranging over some contextually given domain; and paradoxes like Russell's show that, "for any given context, there is a distinct context which provides a wider domain of quantification". So he is defending "a contextualist version of the view that there is no absolutely unrestricted quantification".

The aim of *this* paper of his, however, isn't to directly defend the contextualist thesis as the best response to the paradoxes (Glanzberg has argued the case elsewhere), so much as to explore more closely how best to articulate the thesis – and in particular to explore how the idea that quantifiers always have to understood in terms of a background domain which is set contextually relates to more common-or-garden cases of quantifier domain restriction.

Consider, for example,

1. Every graduate student turned up to the party, and some undergraduates did too.

2. Everyone left before midnight.

In the first case there are already *explicitly restricted* quantifiers. But we of course don't mean every graduate student in the world turned up: there is also a *contextually* supplied restriction to e.g. students in the Cambridge philosophy department. In the second case, context does all the restricting – e.g. to the people at the party.

So far, so familiar. But what about

3. Absolutely everything that exists, without exception, is self-identical?

Here there is no explicit restriction to a subclass of what exists; nor need there be any common-or-garden-contextual restriction of the ordinary kind. Still, Glanzberg wants to say, in *any* given context – including here – there is a *background domain* ("the widest domain of quantification available" in that context). This is the domain over which quantifications as in an occurrence of (3) range, when there is no explicit restriction and no common-or-garden-contextual restriction. And, his argument goes, *there is still a kind of contextual relativity in fixing this background domain* (so, in a sense, the likes of (3) involve contextually relative although unrestricted quantifiers):

Whereas the absolutist holds there is one fixed background domain, which is simply 'absolutely everything', Glanzberg's contextualist holds that different contexts can have different background domains. But of course the contextualist needs to say more than that: it isn't just that different contexts might give different extensions to 'absolutely everything', it is also the case that there is no way of setting up a 'maximal' context in which our quantifiers do succeed in being maximally, not-further-expandably, all-embracing. For example, the contextualist must say that, even given a context of sophisticated philosophical reflection, when – in fall awareness of the issues – we essay a claim like

4. Absolutely everything that might fall under our quantifiers in any context whatsoever is self-identical,

we *still* somehow *must* fall short of our aim, because the context can be changed in a way that will expand what counts as everything. But just how plausible is this? Well, we'll have to see how the explanations develop over the rest of the paper.

2.2 Restricting quantifiers in ordinary ways

§3 of Glanzberg's paper gives an overview of the ways in explicit and common-or-garden-contextual restrictions on quantifiers work (advertised as background to a discussion in later sections about how his so-called "background domains" are fixed). But this isn't intended to be more than just a quick set of reminders about familiar ideas, so we can be equally speedy here.

Going along with Glanzberg for the moment, suppose the putative "background domain" in a context is M (the absolutist says that we can locate one fixed ultimate background domain containing *everything*: the contextualist says that M can vary from context to context). More or less explicit restrictions on quantifiers plus common-or-garden-contextual restrictions carve out from this background a subdomain D (so that *Every A is B* is interpreted as true just when the As in D are B, and so on). But carve out how?

Explicit restrictions are relatively unproblematic. But how is the contextual carving done? There are cases and cases. For example, there is carving by "anaphora on predicates from the context", as in

1. Susan found most books which Bill needs, but few were important,

where ‘few’ is naturally heard as restricted to the books that Susan found and Bill needs.

Then second, there is “accommodation”, where we rely perhaps on some Gricean mechanisms to read quantifiers so that claims made are sensible contributions to the conversational context. For example, as we are about to leave for the airport, I reassuringly say that, yes, I’m sure,

2. Everything is packed

when maybe some salient things (the passports, say) are in plain view in my hand and my keys are jingling in my pocket. Here, my claim is heard, and is intended to be heard, as generalizing over those things that it was appropriate to pack, or some such.

There’s a third, rather different way, in which context can constrain domain selection, that isn’t a matter of domain restriction but rather a matter of how, when an object which is already featuring prominently enough as a focal topic of discourse at a particular point, “we will expect contextually set quantifier domains to include it”. (Though I guess that this point has to be handled with a bit of care. The taxi for the airport arrives very early: we comment on it. ‘But,’ I say, ‘we might as well leave in the taxi now. Everything is packed.’ The quantifier of course doesn’t now include the taxi, even though it is the current topic of discourse! So the idea has to be that contextually set quantifier domains will include suitably appropriate objects that are brought into focus – whatever exactly ‘appropriate’ comes to here.)

Well, so far, let’s suppose, so good. *But how are these reminders about common-or-garden contextual settings of domains going to help us with understanding what is going on in fixing “background” domains?* The story continues ...

2.3 Expanding background domains

What I was expecting, after the first three sections, was a story about how “background domains” do get contextually set, a story somehow drawing on the thoughts about what goes on in more common-or-garden contextual settings of restricted domains. Though I confess I was sceptical about how this could be pulled off – given that the business of restricting quantifications to some subdomain of everything available to be quantified over in a context and the business of (so to speak) reaching out to everything would seem to be intuitively importantly different. But in fact, what Glanzberg next gives us is *not* the whole story about background-domain-fixing (“very little has been said here about how an initial background domain is set” p. 71), but rather a story about how we might go about domain-expansion when we are (supposedly) brought to acknowledge new objects like the Russell class which cannot (on pain of paradox) already be in the domain of objects that we are currently countenancing as all that there are.

Now, Glanzberg says (p. 62) that although domain-expansion isn’t a case of setting a restricted domain, “it is still the setting of a domain of quantification ... [so] it should be governed by the principles” discussed in §3 of the paper that we’ve just been talking about. But in fact, as we’ll see, at most one of the principles to do with common-or-garden domain fixing arguably features in the discussion here about domain-expansion (and I’d say not even that one).

What Glanzberg does discuss is the following line of thought (I’ll use a more familiar example, though he prefers to work with Williamson’s variant Russell paradox about interpretations). Suppose I’m cheerfully quantifying over everything, including sets. You then – how? invoking an All in One principle? – get me to countenance that domain as itself a something, an object, and then show me that it is one which on pain of paradox can't be in the domain I started off with. Ok, so now this new object is a 'topic of discourse', and what is now covered by '(absolutely) everything' should now include that new object – and I suppose we could see *this* as an application of the same principle about domains including current 'focal topics' which we mentioned as governing ordinary domain setting. (But equally, as I said before, it isn't entirely clear how we should handle that supposed general principle in the case of ordinary domains. And in fact the thought in the present case just needn't invoke any wobbly notion of contextually set 'topic' but comes down to the following more basic point: if we are brought *explicitly* to acknowledge the existence of an object outside the previous domain of what we counted as '(absolutely) everything', then that forces an expansion of what we must now – in our new situation – include in '(absolutely) everything'.)

So, to repeat, suppose you bring me to acknowledge e.g. a set-like object beyond those currently covered by 'all sets'. I expand my domain of quantification to contain that too. But not *just* that too. I'll also need to add . . . well, what? At least, presumably, all the other objects that I can define in terms of it, using notions that I already have. And so then what? Thinking of all these objects together with the old ones, I can – by the same move as before – take all those together as a domain, and now we have a new object again. And off we go, iterating the procedure. It is, of course, not for nothing that Dummett called this sort of expansion *indefinitely* extensible!

So we are now in very familiar territory, but territory seemingly unconnected with the early sections of Glanzberg's paper. We can now ask: just how far along the ordinals should we iterate? Maybe – Glanzberg seems to be saying – it isn't a matter of indefinite extensibility, but there is a natural limit. But I found the discussion here to be not very clear.

Where does all this leave us then? As I say, *the early sections about common-or-garden domain setting in fact drop out as pretty irrelevant.* If the paper has *does* have anything interesting to say, it is in the later sections, particularly in Sec. 6.2 about – so to speak – how indefinitely extensible 'indefinitely extensible' concepts are. So, OK, let's have another bash at that. (Though I will grumpily add that I think the editors could have taken a firmer line in getting Glanzberg to make his arguments more accessible.)

2.4 Indefinitely expanding?

I mentioned that Glanzberg's paper focuses on Williamson's version of Russell's paradox for interpretations. I can't say that I find that version very illuminating, but there it is. But it does shape Glanzberg's discussion, and he tells the story about background domain expansion in terms of someone's reflecting directly about the interpretation of their own language. But actually, I don't think that this is of the essence, nor the clearest way to present a discussion about what Dummett would call indefinite extensibility.

For what *is* central to the discussion here is – as I just indicated – Glanzberg's discussion on *how far* we should iterate the expansion of our domain of 'absolutely everything', once we grasp the (supposed) Dummettian imperative to start on the process. Dummett's talk about indefinite extensibility suggests that *he* thinks that there is no determinate limit point (which, I take it, isn't to say that the expansion definitely goes on for ever, but that there is no point where we have a clear reason to stop). Glanzberg, by contrast, thinks there *might* be reason just to embrace iteration just up to the first non-recursive ordinal (or just up to the first $\alpha + 1$-stable ordinal, or up to the first

$\alpha+$-stable ordinal). But he doesn't actually develop any considerations that would settle this. Indeed he says

> In considering multiple options, I do not want to suggest that there is nothing to distinguish among them ... Rather, I think the moral to be drawn is that we do not yet know enough to be certain just how far iteration really does go.

Which is a rather disappointing upshot! We seem to have learnt fairly little from Glanzberg's journey.

3 Geoffrey Hellman, 'Against "Absolutely everything"'

There are four main sections in Hellman's paper – an attempt to state a version of anti-absolutist skepticism, an argument for anti-absolutism based on indefinite extensibility, an argument based on the possibility of 'factually equivalent' ontologies, and then a section explaining e.g. how the anti-absolutist makes sense of apparently absolutely general quantifications as in 'there are no talking donkeys'. I'll take these sections in turn.

3.1 Hellman on the problem

Hellman's attempt to state a version of anti-absolutist skepticism is actually a bit of a fumble. He starts off by saying that the skeptic (if that's the right word) can state a position 'without self-destruction' by mentioning the purported quantifier 'absolutely everything' and saying, negatively, that in the end he can't give a stable coherent content to it. So far so good.

However, Hellman then asks whether there is a defensible *positive* thesis that the skeptic can articulate. He starts talking about 'the intensional aspects of ontological commitments' in a way which I found a bit baffling (it's hardly a Quinean notion of ontological commitment that's in play). But then in the end, Hellman says that 'essentially the same idea' can be given presented in the negative mode, with the skeptic standing ready to offer e.g. an indefinite extensibility argument whenever the absolutist attempts to use a supposedly absolutely general quantifier, thereby backing up his (the skeptic's) claim not to be able to give coherent content to it. The excursus looking for a 'positive' thesis seems to achieve nothing. So let's pass on.

3.2 On extensibility

In the next section of his paper, Hellman expounds a version of the Dummett indefinite extensibility argument. We already know the sort of thing: 'Take some ordinals; then, whatever we start with, there's an operation which gives us a new ordinal (take the successor of the greatest, or if there is no greatest take the limit ordinal) ... Hence there can be no determinate domain containing, once and for all, all ordinals'.

What Dummett actually says (in a passage Hellman quotes) is "Given any precise specification of a totality of ordinal numbers, we can always form a conception of an ordinal number which is the upper bound of that totality, and hence of a more extensive totality." But, as the version above shows, it seems we don't need to lean heavily on the notion of a 'totality' to get the argument going – a point that Hellman also makes. Oddly, however, Hellman seems to think that the argument presupposes a plenitudinous platonism that involves the thought that "the very possibility of mathematical objects suffices for their actuality". But that doesn't seem right. You could surely be a selective platonist (a sort of Quinean platonist?), who thought that there were kosher mathematical entities that really exist and which are to be contrasted with the mere fictions of mathematical game-playing, and who thought that – among the kosher entities – are ordinals, indispensable for theorizing about the order structures we find in the world. But you could still be struck by the thought that, once we *do* countenance ordinals and the standard ways of getting from old ordinals to new ordinals, then there is no non-arbitrary way of calling a halt which is true to the very concept of an ordinal. And being struck by that thought just doesn't require being in thrall to a plenitudinous platonism.

Could it be, though, that we might consistently accept the argument that there is no determinate domain of ordinals (for example) but still countenance quantification over absolutely everything? The thought would be that we can talk determinately about everything that there is, even if we cannot determinately corral off just that portion of what there is that ought to count as an ordinal (any attempt will leave outside it entities with just as much right to be called ordinals). Hellman thinks that this is unpromising for the reason that the "mathematical operations appealed to in connection with pure mathematicalia [as in forming new ordinals] can also be applied to mathematicalia-cum-non-mathematicalia". But this goes too fast. Suppose we have some ordinals plus some other things (making up everything there is!); then we can apply the familiar operation to the given ordinals to give us another thing. But this operation need not extend the tally of everything there is: it could just be that one of those "other things" that made up everything there is has turned out to have as much right to be deemed an ordinal as the ordinals we started with.

3.3 On ontologies

Hellman's second line of argument against absolutely general quantification rests – according to the title of Section 4 of his paper – on the multiplicity of 'factually equivalent ontologies'.

The claim is that 'The same underlying factual situation [can be] described accurately and adequately in ontologically diverse ways. It would be arbitrary and unwarranted to say that just one is "really correct".' What sorts of case does Hellman have in mind? 'Familiar examples cited long ago by Goodman ... and others come from geometry (pure or applied), e.g. a framework with points and lines (say, in the two-dimensional case) vs. a framework with just lines, points being definable as (suitably selected) pairs of intersecting lines.' But hold on, both frameworks agree that there are points and lines. Either way, then, in promiscuously quantifying over absolutely everything, I'll be quantifying over both points and lines. So what's the problem?

Ah, says Hellman, the absolutist must claim that there is one correct answer to the question 'Are there sui generis points, i.e. points which are distinct from pairs of lines or nested volumes, etc., not constructed out of anything else? ... We may not ever know [the answer], but it shows up one way or another in the range of "absolutely everything".' Eh? Why is someone who claims that we can sensibly quantify over everything committed to the quite different claim that issues about what is ontological basic or sui generis have determinate answers? When I say that everything is self-identical (for example), I commit myself inter alia to agreeing that points, whatever they ultimately are, are self-identical and lines, whatever *they* are, are self-identical. But I just can't see why it is supposed to follow that I'm thereby committing myself to supposing that the ontology game (in the form of raising the question of what's really, really fundamental?) is even a game with determinate rules let alone delivers determinate answers.

Hellman offers variants on the points/lines case. He asks us to consider an ontology of space-time regions that doesn't recognize the existence of ordinary objects like books; strictly, instead of saying that there are books over there, we should say that a certain region is "booked", etc. And Hellman's thought is that in this sort of case, 'no entity recognized in the ontology of this theory is literally a book' (contrast the points/lines case, where there are entities available to be identified as points in either ontology). But so what? What has this to do with the question of the possibility of absolutely general quantification over everything?

It has turned out that there are no gods (which was a bit of a surprise in some quarters); so those who thought – in quantifying over everything that there is – that they were including gods along with the books and the points and lines would have been just flatly mistaken. But the possibility of being mistaken about what exists doesn't in itself undermine the possibility of quantifying over everything that *does* exist! Similarly, suppose it turns out that there are wonderfully conclusive arguments for an ontology of space-time regions that makes it false that there are books. That would be more than a bit of surprise! But, playing along with that fantasy, it would then have just turned out that we are mistaken in thinking – when we quantify over everything – that books are included, just as our ancestors were in thinking that gods were included. That still doesn't undermine the possibility of quantifying over everything.

So, in short, I can't see that Hellman's arguments in §4 of his paper have any force at all as they stand. He just seems to be running together issues about 'what exists absolutely, "in Reality"' (his phrase, indicating some tally of the *basic* constituents of the world) with the question about whether we can quantify over absolutely everything that exists.

3.4 On talking donkeys

The final section of Hellman's paper is called 'Making do with "less"', and concerns strategies that the sceptic about the coherence of absolutely general quantification can use to makes sense of (true!) assertions like 'there are no talking donkeys' or 'there are no gods' which seem on the face of it to make absolutely general claims.

I found his discussion murky.[2] But the main point Hellman makes, if I'm understanding him aright, is what strikes me as the obviously right point. 'There are no talking donkeys' stands or falls with 'No animal is a talking donkey', and there are no problems about the common-or-garden restricted quantification involved there. Issues about indefinite extensibility are beside the point because 'animal' (unlike 'ordinal' or 'set') is *not* indefinitely extensible; and issues about relativity to alternative conceptual schemes are beside the point too, since (in talking about donkeys at all) we are already talking within a certain scheme that recognizes animals.

'There are no gods' can't be handled quite so straightforwardly (pace Hellman): but not because of issues about absolute generality so much as because of issues about the lack of clear content of 'gods'. (Who knows what a boojum is? Especially if boojums are described as having all sorts of daft and seemingly incompatible properties. If I then impatiently say there are no boojums, I'm not making a bold speculation about the contents of the universe but rather rejecting – though perhaps not in the most

[2]At least as far as philosophy is concerned, I'm with Isabella Dale in *The Small House at Allington* [ch. xliv]: 'I hate books I can't understand,' said Bell, 'I like a book to be as clear as running water, so that the whole meaning may be seen at once.'

transparent way – the presupposition that there is any clear content to claim that there are boojums. It is pretty similar with gods.)

4 Vann McGee, 'There's a rule for everything'

I was intending to discuss the papers in *Absolute Generality* in the order in which they are printed. But Glanzberg's piece is followed by a long one by Shaughan Lavine which is in significant part a discussion of Vann McGee's views, including those expressed in the latter's contribution to this book. So it seems sensible to discuss McGee's paper first.

4.1 General semantic skepticism

The first section of this paper addresses "semantic skepticism in general". McGee writes

> The prevalent skeptical view, which is sometimes called deflationism or minimalism, allows that a speaker can say things that are true, but denies that her ability to do so depends on the linguistic practices of herself and her community. . . . [D]isquotationalism doesn't connect truth-conditions with patterns of usage. . . . The (T)-sentences for own language are, for the deflationist, an inexplicable brute fact.

And there is more in the same vein. Well, I thought *I* was a kind of deflationist, but certainly I don't take myself to be wedded to the idea that truth-conditions aren't connected to patterns of usage. Au contraire. I'd say that it is precisely because of facts about the way I use 'snow is white' that I am interpretable as using it to say that snow is white. And because 'snow is white' is used by me to say that snow is white, then indeed 'snow is white' on my lips is true just in case snow is white. So, I'd certainly say that the truth of such a (T)-sentence *isn't* inexplicable, it *isn't* an ungrounded brute fact. But the core deflationist thought – that there is in the end, bells and whistles apart, no more to the content of the truth predicate than is given in such (T)-sentences (the notion of truth, so to speak, lacks metaphysical weight) – is surely quite consistent with *that*.

Still, let's not fuss about who gets to choose which position counts as properly 'deflationist'. My point is merely that the sort of extreme position which McGee seems to talking about (though he is far from ideally clear) is remote from plausible versions of deflationism, is therefore to my mind not especially interesting, and in any case – the key point here – hasn't anything particularly to do with issues about absolute generality. *So exactly why does he think it is going to be illuminating to come at the topic this way?* I'm completely stumped. So I have to pass over this first section with a rather puzzled shrug.

4.2 Skepticism about the quantifiers in particular

In §2 of his paper, McGee reviews a number of grounds that might be offered for skepticism about absolutely unrestricted quantification. But he doesn't take the classic indefinite extensibility argument very seriously – indeed he doesn't even mention Dummett, but rather offers a paragraph commenting on the disparity between what Russell and Whitehead say they are doing in *Principia* to avoid vicious circles, and what they end up doing with the Axiom of Reducibility. Given the actual state of play in the debates, just ignoring the Dummettian version of the argument seems pretty odd.

But be that as it may, "the bothersome worry," according to McGee, "is not that our domain of quantification is always assuredly restricted [because of indefinite extensibility] but that the domain is never assuredly unrestricted [because of Skolemite arguments]". Here I am, let's suppose, trying to quantify all-inclusively in some canonical first-order formulation of my story of the world, and by the Löwenheim-Skolem theorem there is a countable elementary submodel of story. *So what can make it the case that I'm not talking about that instead?*

OK, it is a good question how we should best respond to the Skolemite argument in general, and McGee offers some thoughts. He suggests two main responses. The first appeals very briefly to considerations about learnability. I just don't follow the argument (but I note that Lavine is going to discuss it, so let's hang fire on this argument for the moment). The second is that "[t]he recognition that the rules of logical inference need to be open-ended . . . frustrates Skolemite skepticism." Why so? The argument goes like this.

> The Löwenheim-Skolem construction requires that every individual that is named in the language be an element of the countable subdomain S. If the individual constant c named something outside the domain S, then if '$(\forall x)$' is taken to mean 'for every member x of S', the principle of universal instantiation [when c is added to the language] would not be truth-preserving. . . . Following Skolem's recipe gets us a countable set S with the property that interpreting the quantifiers as ranging over S makes the classical modes of inference truth-preserving, but when we expand the language by adding new constants, truth preservation is not maintained. The hypothesis that the quantified variables range over S cannot explain the inferential practices of people whose acceptance of universal instantiation is open-ended.

But this line of argument by itself needn't faze the more subtle Skolemite. After all, she could reasonably insist, there seems to be a limit to the number of constants that a finite being like me can add to his language while (even with the help of context) successfully picking out different things. They are no more, she might insist, than countably infinite. So start with my actual language L. Construct the ideal language L^+ by expanding L with all those constants I could add (and add to my theory of the world such sentences involving the new constants that I would then accept). Now Skolemize on *that*, and we are back with trouble that McGee's response, by construction, doesn't touch.

Be that as it may, however, it seems to me that issues about the Skolemite argument are orthogonal to the distinctive issues, the special problems, about absolute generality. For suppose we *do* have a satisfactory response to Skolemite worries when applied e.g. to talk about 'all real numbers' (supposing here that 'real number' doesn't indefinitely extend): that still leaves the Dummettian worries about 'all sets', 'all ordinals' and the like in place just as they were. Suppose on the other hand we struggle to find any response to the Skolemite skeptic. Then it isn't just quantifications that aim to be absolutely general that are in trouble, but even some seemingly tame highly restricted ones, like generalizations about all the reals. Given this, I'm all for trying to separate out the distinctive issues about absolute generality and focussing on those, and then treating quite separately the entirely general Skolemite arguments which apply to (some) restricted and unrestricted quantifications alike.

4.3 A rule for 'everything'

In the final section of McGee's paper he argues that "the semantic values of the quantifiers are fixed by the rules of inference". The claim rests on noting that (i) two universal quantifiers governed by e.g. the same UE and UI rules will be interderivable (McGee credits "a remarkable theorem of J.H. Harris", but it is an easy result, which is surely a familiar observation in the Gentzen/Prawitz/Dummett tradition). McGee then claims that, assuming the quantifier rules don't misfire completely [like the tonk rules?], this implies that (ii) they determine a uniquely optimal candidate for their semantic value. And further, (iii) "the Harris theorem ... gives us reason to anticipate that, when we develop a semantic theory, it will favor unambiguously unrestricted quantification."

The step from (i) to (ii) needs some heavy-duty assumptions – after all, the intuitionist, for example, doesn't differ from the classical logician about the correct *quantifier* rules, but does have very different things to say about semantic values. But McGee seems just to be assuming outright a two-valued classical background; so let that pass.

More seriously in the present context, the step on to (iii) is just question-begging, if it is supposed to be a defence of an absolutist reading of unrestricted quantification. Consider a non-absolutist like Glanzberg. He could cheerfully accept that the rules governing the use of unrestricted universal quantifiers fix that in any context they run over the whole "background domain", whatever that is (and that a pair of quantifiers governed by the same rules would both run over that same domain): *but that leaves it entirely open whether the background domain available to us at any point is itself contextually variable and can be subject to indefinite expansion.*

Of course, says the anti-absolutist, there is no God's eye viewpoint from which we can squint sideways at our current practice and comment that right now '(absolutely) everything' on our lips doesn't really run over all the exists. 'Everything' always means everything. What else? *But that isn't what the anti-absolutist denies*, and so it seems that McGee fails in the end to really engage with the position.

5 Shaughan Lavine, 'Something about everything'

Shaughan Lavine's is one of two fifty-page papers in *Absolute Generality* (I'm not sure that the editors' relaxed attitude to overlong papers does either the authors or the readers a great service, but there it is). In fact, the paper divides into two parts. The first six sections review four anti-absolutist arguments, and criticize McGee's response on behalf of the absolutist to (in particular) the Skolemite argument. The last five sections are much more interesting, arguing that we can in fact do without absolute unrestricted generality – rather, "full schematic generality will suffice", where Lavine is going to explain at some length what such schematic generality comes to.

5.1 Lavine on the problems

But first things first. What are the four anti-absolutist arguments that Lavine considers? It's worth quickly reviewing his list.

(1) First, there's the familiar argument from the paradoxes that suggests that certain concepts (set, ordinal) are indefinitely extensible and that it is not possible for a quantifier to have all sets or all ordinals in its domain. Now, unlike McGee, Lavine recognizes that that the "objection from paradox" (as he calls it) raises serious issues. However, he evidently thinks that any direct engagement with the objection just leads to a stand-off

between the absolutist and anti-absolutist sides, "each finds the other paradoxical", so he initially sets the argument aside.

(2) The second argument is the "framework objection" that we saw Hellman also discusses in his contribution.

> Different metaphysical frameworks differ on what there is . . . . If the answers to [questions like are there any mathematical entities? is space composed of points?] are not matters of facts, but of choice of framework, . . . [then there is only] quantification over everything there is according to the framework [and not] absolutely unrestricted quantification.

Well, as I noted before, if two frameworks differ just in what they take to be ontologically *basic* and what they take to be (in some broad sense) constructions out of the basics, then that is beside the present point. We can still quantify over all the same things in the different frameworks – for quantifying over everything isn't to be thought of as restricted quantification over whatever is putatively basic. So to make trouble here, the idea would have to be that there can be equally good rival frameworks, with only a conventional choice to be made between them, where Xs exist according to one framework, and cannot even be constructed according to the other. If there are such cases, then there may be an argument to be had: but that is a pretty big 'if', and Lavine doesn't give us any reason to suppose that the condition can be made good, so let's pass on.

(3) "The third objection to everything is technical and a bit difficult to state, and in addition it is relatively easily countered," so Lavine is brief. I will be too. Start with the thought that there can be subject areas in which for every true $(\exists x)Fx$ – with the quantifier taken as restricted to such an area – there is a name c such that Fc. There is then an issue whether to treat those restricted quantifiers referentially or substitutionally, yet supposedly no fact of the matter can decide the issue. So then it is indeterminate whether to treat c as having a denotation which needs to be in the domain of an unrestricted 'everything'. And so 'everything' is indeterminate.

Lavine himself comments, "the argument . . . works only if the only data that can be used to distinguish substitutional from referential quantification are the truth values of sentences about the subject matter at issue". And there is no conclusive reason to accept that Quinean doctrine. Relatedly: the argument only works if we can have no prior reason to suppose that c is operating as a name with a referent in Fc (prior to issues about quantifications involving F). And there is no good reason to accept that either – read Evans on *The Varieties of Reference*. So argument (3) looks a non-starter.

(4) Which takes us to the fourth "objection to everything" that Lavine considers, which is the Skolemite argument again. Or to use his label, the Hollywood objection. Why that label?

> Hollywood routinely produces the appearance of large cities, huge crowds, entire alien worlds, and so forth, in movies . . . the trick is only to produce those portions of the cities, crowds, and worlds at which the camera points, and even to produce only those parts the camera can see – not barns, but barn faades. One can produce appearances indistinguishable from those of cities, crowds, and worlds using only a miniscule part of those cities, crowds, and worlds. Skolem, using pretty much the Hollywood technique, showed that . . . for every interpreted language with an infinite domain there is a small (countable) infinite substructure in which exactly the same sentences are true. Here, instead of just producing what the camera sees, one just keeps

> what the language 'sees' or asserts to exist, one just takes out the original structure one witness to every true existential sentence, etc.

That's really a rather nice, memorable, analogy. And the headline news is that Lavine aims to rebut the objections offered by McGee to the Skolemite argument against the determinacy of supposedly absolutely unrestricted quantification.

One of McGee's arguments, as we noted, appeals to considerations about learnability. I didn't follow the argument and it turns out that Lavine too is unsure what is supposed to be going on. He offers an interpretation and readily shows that on *that* interpretation McGee's argument cuts little ice. I can't do better on McGee's behalf (not that I feel much inclined to try).

McGee's other main argument, we noted, is that "[t]he recognition that the rules of logical inference need to be open-ended ... frustrates Skolemite skepticism." Lavine's riposte is long and actually its thrust isn't that easy to follow. But he seems, inter alia, to make two points that I made in my comments on McGee. First, talking about possible extensions of languages won't help since we can Skolemize on languages that are already expanded to contain terms "for any object for which a term can be added, in any suitable modal sense of 'can'" (though perhaps neither Lavine nor I am quite clear enough about those suitable modal senses: there is probably more work to be done there). And second, Lavine agrees with McGee that the rules of inference for the quantifiers fix (given an appropriate background semantic framework) the semantic values of the quantifiers. But while fixing semantic values – fixing the function that maps the semantic values of quantified predicates to truth-values – tells us how domains feature in fixing the truth-values of quantified sentences, that just doesn't tell us what the domain is. And Skolemite considerations aside, it doesn't tell us whether or not the widest domain available in a given context (what then counts as 'absolutely everything') can vary with context as the anti-absolutist view would have it.

So where does all this leave us, already some twenty pages into Lavine's long paper? Pretty much just where we were. Considerations of indefinite extensibility have been shelved for later treatment. And, for Lavine, the Skolemite argument is still in play (since McGee's responses, at any rate, haven't convinced) – though nothing has yet been said that really shakes me out of the view that, as I said before, issues about the Skolemite argument are in fact orthogonal to the interestingly distinctive issues about absolute generality. However, there is a lot more to come, ...

5.2 Schematic generality

In §7 of his paper, Lavine argues that there is a distinct way of expressing generality, using "schemes" to declare that "any instance [has a certain property], where 'any' is to be sharply distinguished from 'every'" (compare Russell's 1908 view). In fact, Lavine goes further, talking about the kind of generality involved here as "more primitive than quantificational generality".

We are supposed to be softened up for this idea by the thought that in fact distinctively schematic generalization is actually very familiar to us in the most elementary of contexts:

> When, early on in an introductory logic course, before a formal language has been introduced, one says that NOT(P AND NOT-P) is valid, and gives natural language examples, the letter 'P' is being used as a full schematic letter. The students are not supposed to take it as having any particular domain – there

> has as yet been no discussion of what the appropriate domain might be – and it is, in the setting described, usually the case that it is not 'NOT(P AND NOT-P)' that is being described as valid, but the natural-language examples that are instances of it.[3]

But Lavine's initial motivating example doesn't really impress. To be sure, in an early lecture, I may say that any proposition of the form NOT(P AND NOT-P) is logically true in virtue of the meanings of 'NOT' and 'AND'. But to get anywhere with this idea, I of course have to gloss this quite a bit. For a start, some effort has to go into explaining what counts as an instance of this schema, given that just plugging in a declarative English sentences won't even yield a well-formed sentence. And indeed, in glossing such principles like non-contradiction and excluded middle, I for one certainly remark e.g. that we are setting aside issues about vagueness ('it is kinda raining and not raining, you know'), and issues about weird cases (liar sentences), and issues about sentences with empty names, and so on.

But true enough, I – no doubt like Lavine – will still leave things not entirely pinned down. If you like, things are left 'open-ended' at this stage. Does that mean, however, that I'm engaged in something *other* than 'quantificational generality'? I *thought* I was saying something along the lines of 'every statement of such-and-such a type is logically true', leaving it a bit vague what counted as being of the relevant type. Our everyday quantifications are often cheerfully a bit rough and ready, with the domain only rather roughly indicated: why isn't this is a case in point?

5.3 More about the distinction?

'Ah,' it will be said, 'you are forgetting the Lavine's explanation that talk about a *full* schematic variable in the case of NOT(P AND NOT-P) indicates that "what counts as an acceptable substitution instance ... expands as the language in use expands".' But again, more needs to be said about the significance of *this* before we get a difference between schematic and ordinary quantificational generalizations. If I say 'every statement of such-and-such a type is logically true', intending to cover not just statements that we can make now, but statements to be expressed in versions of English yet to come, why is that so very different from saying, e.g., "all cats have whiskers", intending to cover not just cats that we can encounter now, but cats that could come to exist as the population changes. In both cases, we are giving a modal twist to the generalization, treating it as applying to more than the here-and-now. But it needs a lot more argument to show that what we are modalizing in each case isn't a common-or-garden quantification is going on (and anyway, it would be very odd indeed to suppose that the modalized form of claim here is more primitive that quantificational generality).

In a subsection entitled 'Schemes are not reducible to quantification', Lavine goes on to write

> Schematic letters and quantifiable variables have different inferential roles. If n is a schematic letter then one can infer $S0 \neq 0$ from $Sn \neq 0$, but that is not so if n is a quantifiable variable – in that case the inference is valid only if n did not occur free in any of the premisses of the argument.

[3] Utterly beside the present point, of course, but surely it isn't a great idea – when you've been trying to drill into students the idea that truth is the dimension of assessment for propositions and validity is the dimension of assessment for inferences – to turn round and mess up a clean distinction by calling logically necessary propositions 'valid'. I know quite a few logic books do this, but why follow them in this bad practice?

But, in so far as that is true, how does this establish the claim that generalization via schemes is something different from quantification generality?

Of course, one familiar way of using schemes is e.g. as in Sec. 8.1 of my Gödel book where I am describing a quantifier-free arithmetic I call Baby Arithmetic, and say 'any sentence that you get from the scheme $S\zeta \neq 0$ by substituting a standard numeral for the place-holder "ζ" is an axiom'. And to be sure, the role of the metalinguistic scheme $S\zeta \neq 0$ *is* different from that of the object language $Sx \neq 0$. Still, it would be pretty misleading to talk of *inferring* an instance like $S0 \neq 0$ from the metalinguistic schema. And the generality in the rule for *using* the schema, signalled by 'any', can – at least pending further, independent, argument – be thought of as unproblematically quantificational (though not quantifying over *numbers* of course). So this sort of apparently anodyne use of numerical schemes can't establish Lavine's claim, unless he can offer some additional considerations. So again we are forced to ask: what *does* he have in mind?

Perhaps an important thought comes out here:

> One who doubts that the natural numbers form an actually infinite class will not take the scheme $\varphi(n) \rightarrow \varphi(Sn)$ to have a well-circumscribed class of instances and hence will not be willing to infer $\varphi(x) \rightarrow \varphi(Sx)$ from it; for the latter formula involves a quantifiable variable with the actually infinite class of all numbers as its domain or the actually infinite class of all numerals included in its substitution class.

We seemingly get a related thought e.g. in Dummett's paper 'What is mathematics about?', where he argues that understanding quantification over some class of abstract objects requires that we should 'grasp' the domain, that is, the totality of objects of that class – which seems to imply that if there is no totality to be grasped, then here there can be no universal quantification properly understood.

But do note two initial things about this. First, a generalization's failing to have a well-circumscribed class of instances because (i) we are talking in a rough and ready way and haven't bothered to be precise because we don't need to be, and its failing because (ii) we *can't* circumscribe the class because there is no relevant completeable infinity (e.g. because of considerations about indefinite extensibility), are surely entirely different cases. Hence Lavine's moving from an initial example of the first kind when he talked about arm-waving generalizations we make in introductory logic lectures to his later consideration of cases of the second kind is a significant slide. Second, I can still see no reason at all to suppose that such self-consciously schematic talk adopted in order to avoid being committed to certain actual infinities (e.g. in the light of awareness of issues about indefinite extensibility) is "more primitive" than quantificational generality.

5.4 Pressing on

There are *still* over twenty pages of Lavine's paper remaining. But now I am at a loss to give a precise interpretation of his claims about schematic generality. And even on a re-reading, I've not got any really clear understanding of how these claims are supposed to help us with puzzles about absolute generality.

Should I struggle on? Well, Lavine doesn't bother to write with a light touch or transparent clarity – making these pages quite unnecessarily hard going. So, to be frank, he exhausted my patience, and so (at least for the moment) I'm not going to bother to say more about this ill-written paper here.

So we'll press on ...