THE BULLETIN OF SYMBOLIC LOGIC
Volume 14, Number 3, Sept. 2008

REVIEWS

The Association for Symbolic Logic publishes analytical reviews of selected books and articles in the field of symbolic logic. The reviews were published in ***The Journal of Symbolic Logic*** from the founding of the JOURNAL in 1936 until the end of 1999. The Association moved the reviews to this BULLETIN, beginning in 2000.

 Authors and publishers are requested to send, for review, copies of books to *ASL, Box 742, Vassar College, 124 Raymond Avenue, Poughkeepsie, NY 12604, USA.*

In a review, a reference "JSL XLIII 148," for example, refers either to the publication reviewed on page 148 of volume 43 of the JOURNAL, or to the review itself (which contains full bibliographical information for the reviewed publication). Analogously, a reference "BSL VII 376" refers to the review beginning on page 376 in volume 7 of this BULLETIN, or to the publication there reviewed. "JSL LV 347" refers to one of the reviews or one of the publications reviewed or listed on page 347 of volume 55 of the JOURNAL, with reliance on the context to show which one is meant. The reference "JSL LIII 318(3)" is to the third item on page 318 of volume 53 of the JOURNAL, that is, to van Heijenoort's *Frege and vagueness*, and "JSL LX 684(8)" refers to the eighth item on page 684 of volume 60 of the JOURNAL, that is, to Tarski's *Truth and proof*.

References such as 49***5*** or 280***1*** are to entries so numbered in *A bibliography of symbolic logic* (the JOURNAL, vol. 1, pp. 121–218).

Absolute generality, edited by Agustín Rayo and Gabriel Uzquiano, Clarendon Press, Oxford, 2006, ix + 396 pp.—therein:

AGUSTÍN RAYO AND GABRIEL UZQUIANO. *Introduction*. Pp. 1–19.

KIT FINE. *Relatively unrestricted quantification*. Pp. 20–44.

MICHAEL GLANZBERG. *Context and unrestricted quantification*. Pp. 45–74.

GEOFFREY HELLMAN. *Against 'absolutely everything'*. Pp. 75–97.

SHAUGHAN LAVINE. *Something about everything: Universal quantification in the universal sense of universal quantification*. Pp. 98–148.

ØYSTEIN LINNEBO. *Sets, properties, and unrestricted quantification*. Pp. 149–178.

VANN MCGEE. *There's a rule for everything*. Pp. 179–202.

CHARLES PARSONS. *The problem of absolute generality*. Pp. 203–219.

AGUSTÍN RAYO. *Beyond plurals*. Pp. 220–254.

STEWART SHAPIRO AND CRISPIN WRIGHT. *All things indefinitely extensible*. Pp. 255–304.

GABRIEL UZQUIANO. *Unrestricted unrestricted quantification: The cardinal problem of absolute generality*. Pp. 305–332.

ALAN WEIR. *Is it too much to ask, to ask for everything?* Pp. 333–368.

TIMOTHY WILLIAMSON. *Absolute identity and absolute generality*. Pp. 369–389.

This is a collection of new papers on a currently hot topic by an impressive array of authors. Enthusiasts will already have devoured the book. But what does it offer the rest of us?

1079-8986/08/1403-0005/$1.40

Well, why is there an issue about the very possibility of quantification over absolutely everything? The serious and distinctive problems arguably arise from the phenomenon of indefinitely extensibility first remarked on by Russell. However, there are other supposed worries which exercise some of the contributors more. Let's pause over three.

Geoffrey Hellman gives an argument against absolutely general quantification which rests on the supposed multiplicity of 'factually equivalent ontologies' (Shaughan Lavine and Charles Parsons consider similar arguments). 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 ... 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 defender of absolutely general quantification 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'." Really? 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 may commit myself inter alia to agreeing that points, whatever they ultimately are, are self-identical and lines, whatever *they* are, are self-identical. But why is it 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 gives some other examples, but similar points apply. So, in short, I can't see that Hellman's arguments in §4 of his paper have any force. 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 does exist, basic or otherwise.

Vann McGee offers a different line of argument. "The bothersome worry 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 my theory. So what can make it the case that I'm not talking about that submodel instead? How can I ensure that I *do* quantify over everything?

It is a good question how we should best respond to the Skolemite argument in general, and McGee offers two lines of attack (neither of which strikes me as successful). But be that as it may. Issues about the Skolemite argument are surely 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' (taking it that 'real number' doesn't indefinitely extend): this still leaves the familiar indefinite extensibility worries about 'all sets', 'all ordinals' and the like in place just as they were. Suppose on the other hand we continue to struggle to find any really happy response to the persistent 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. So the best policy is surely to separate out the distinctive issues about absolute generality and focus on those, and treat quite separately the entirely general Skolemite arguments which apply to (some) restricted and unrestricted quantifications alike.

To introduce a third line of argument against absolutely general quantification, consider the following: 'Quantifiers are to be interpreted as ranging over some domain. A domain is understood to be a set. No set contains absolutely everything, including all sets, on pain of Russell's paradox. So quantifications aren't ever over absolutely everything.' To which we can either counter with deviant set theories, or with Richard Cartwright's familiar animadversions against what he calls the All-in-One principle, i.e., against the idea that a domain itself has to be a set.

However, even if that naive version fails, we can essay a variant argument, but still starting from considerations about interpretation, due to Timothy Williamson and discussed here by e.g., Michael Glanzberg, Øystein Linnebo and Charles Parsons. Here's a version.

On an interpretative truth-theory for a language L, we'll have a clause for a monadic L-predicate P along the lines of 'for all o, P is true-of o iff Fo' (where 'all' certainly looks initially like an unrestricted quantification over absolutely everything). But if we can reflect about the business of interpretation at all, we can surely imagine running through various other possible interpretations for P, which will result in clauses in definitions of different two-place 'true-of' predicates, relativized to different interpretations.

Now, it might well seem that we needn't follow Williamson and others in thinking of the different interpretations that could be in play here as 'objects' themselves. On the other hand, we might reasonably suppose that the different relational truth-predicates could at least be *indexed* by some suitably big collection of kosher objects (some sets, for example). So the clause in a definition for an indexed relation 'true-of$_\alpha$' will be given in the form 'for all o, P is true-of$_\alpha$ o iff Fo' (still with absolutely general quantification). But now, since the indexing objects are by hypothesis kosher objects we can define a 'Russellian' property R which is had by an object o just in case not-(P is true-of$_o$ o). Since that seems to be unproblematically a property, it's available to be used as the interpretation of a monadic predicate P, so there ought to be at least one index κ such that for all o, P is true-of$_\kappa$ o iff Ro. But it is immediate that there can be no such index κ—assuming, that is, that κ falls into the range of the universal quantifier 'for all o'.

What follows? We could conclude that the universal quantifier 'for all o' just *can't* after all include the object κ in its range: so here's a case of trouble for the idea of absolutely general quantification. But *that* is hardly the only, or the most obvious, possible lesson to draw! It might equally well be said that the argument shows instead that we shouldn't treat an interpretative 'true-of$_\kappa$' relation defined in terms of R—where that property is defined by quantifications over (indices for) such true-of relations—as on a par with the true-of relations we started off with. The moral could be that we have to ramify the truth-predicates, and recognize that given some such predicates, we can always 'diagonalize out' and define another new predicate which is not co-extensive with any of them.

Parsons almost makes the point. He initially says that (in the case of unrelativized truth-theories) 'true of' had better not be in the language being interpreted on pain of paradox. Now we are generalizing and talking about different definitions of 'true of' on a range of possible interpretations. And again on pain of paradox there will be a 'true of' that isn't already among those different definitions. But why can't we say again, in Parsons's words, "this new interpretation does require 'ideology' not already present, but it does not require an expansion of ontology"? The objects we are quantifying over—absolutely everything—stay fixed, and the idea of an absolutely general quantification over them remains in good order; we just have to recognize new 'true of' relations.

It is far from clear, then, that the defender of absolute quantification is in trouble just from the Williamsonian style of argument about interpretations. (A quick aside: Linnebo too suggests that the strongest response to the Williamson style of argument is a hierarchical one—but Linnebo goes on to deploy a simple theory of types, and we've just seen that what we seem to need is rather a ramification of first-order 'true-of' relations into levels.)

So let's turn at last to the familiar Dummettian development of Russellian thoughts about indefinite extensibility: for here we *do* have an argument against absolute quantification that is less easily turned. What exactly is the argument? Again, a number of contributors touch on this, though some are surprisingly dismissive. But Dummettian concerns are explored in the stand-out paper of the collection, 'All things indefinitely extensible' by Stewart Shapiro and Crispin Wright.

Taking up a hint in Russell, Shapiro and Wright offer a neat argument for the following—at least as a first characterization of the scope of the indefinitely extensible: a concept P is indefinitely extensible (in the fullest sense) if and only if there is a one-to-one function from all the ordinals into the Ps.

But as they note, this makes genuinely indefinitely extensible totalities very *big*, while some Dummettian examples of indefinitely extensible totalities are *small*. Take for example Dummett's discussion in his paper on the philosophical significance of Gödel's theorem. He says the theorem shows that the concept of an arithmetical truth (of first-order arithmetic) is indefinitely extensible. Well, true, if we start with some nice recursively enumerable set of arithmetical truths, we can keep on extending the set by Gödelizing out. Set aside the question of why we should suppose the arithmetic truths 'ought' to be r.e. in the first place: the point we want for now is that there is evidently a limit on how far along the ordinals we can continue the process of Gödelizing out—for there are only countably many r.e. sets available to be Gödelized while there are uncountably many ordinals (even uncountably many countable ordinals). The moral seems to be this. There is a difference between saying that the concept P is such that, given any appropriately 'definite' totality of Ps, we can always find a P that isn't in that totality, and saying that the totality is (so to speak) *indefinitely* indefinitely extensible.

This observation leads into Shapiro and Wright's main discussion. They give an insightful and persuasive account of what makes for extensibility in a weak general sense (covering 'small' cases, thereby illuminating Dummett's discussions, and showing why the phenomenon in general is not paradox-generating). They then discuss the problem cases of *set*, *cardinal* and *ordinal*, which are indefinitely indefinitely extensible along the ordinals, and they show how and why these cases do indeed lead to deep problems for the thought that we can quantify over absolutely all such things (at least when combined with other seemingly compelling thoughts). In the end, they identify five possible combinations of views—none of them attractive. "It seems that every one of the available theoretical options has difficulties which would be justly be treated as decisive against it, were it not that the others fare no better."

Shapiro and Wright's discussion is far too rich to try to summarize further here: *read it* (even if you read nothing else from the collection), though you can perhaps skim their excursus into matters internal to the neo-logicist programme. For this paper must henceforth be the starting point for all further discussions of Dummettian worries about indefinite extensibility and absolute generality.

There is much else in the collection I haven't been able even to touch on here. I should add, though, that the volume is under-edited. A number of the papers would surely not have made it into the journals as they stand, and are quite unnecessarily hard to construe. Referees reading anonymized submissions and not overawed by reputations would have repeatedly pressed hard for greater clarity and concision. There really isn't any excuse for writing this kind of philosophy with less than the absolute clarity and plain-speaking directness which the editors should have insisted on.

For more details of the collection, the editors' introduction with paper summaries is available online (`http://web.mit.edu/arayo/www/Introduction.pdf`). And I have also discussed some of the papers at greater length online (at `http://logicmatters.blogspot.com`).

PETER SMITH

Faculty of Philosophy, University of Cambridge, UK. ps218@cam.ac.uk.