Mathematical pluralism

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Abstract

There is a plurality of mathematical investigations. These cannot all be reduced to proofs within the framework of Zermelo Fraenkel set theory, if only because some of them use non-classical logic (such as the various branches of intuitionist mathematics). How is one to understand this situation? In this article, I suggest that one should see this plurality as analogous to the plurality of games, any of which may be played. Various objections are considered and rejected, including the charge that the picture engenders a pernicious relativism.

Keywords: Pluralism, reductionism, games, noneism, non-classical mathematics, applied mathematics.

1 The variety of mathematics

It is clear from a very cursory review of mathematical practice, past and present, that mathematicians concern themselves with a wide variety of investigations. They investigate the structure of groups, of random variables, of the complex plane, the natural numbers, infinite cardinals and so on. There is, then, a pluralism of mathematical practices.

A natural thought—I presume a currently orthodox one—is that the pluralism is, in a certain sense, a superficial one. There is a single over-arching mathematical theory, say Zermelo Fraenkel set theory, maybe with the Axiom of Choice. The various structures that are investigated are defined within this. The different practices are therefore all investigations of the structure of the set-theoretic universe delivered by ZF(C), or of various parts thereof.

The thought, though, does not survive long. First, mathematicians do not consider just what can be done within ZF(C). They consider extensions of ZF(C)—e.g., with various large cardinal axioms. Next, it is not the case that all of standard mathematics fits into ZF(C) anyway. The programme of reducing mathematics circa 1900 to ZF(C) was spectacularly successful, but there are problems with later parts of mathematics. Category theory is an obvious example. Category theorists investigate the category of all sets, and even the category of all categories. For well known reasons, these do not live in the set-theoretic universe [14, ch. 2]. Of course, mathematicians realize this, and have suggested ways of accommodating these 'overlarge' categories: we introduce proper classes (and proper classes of proper classes, etc.); or we invoke some large cardinal axiom, and then talk about the category of sets of some bounded rank. In the end, though, these devices just defer the problem; we have simply changed the subject. We still cannot apply category theory to *all* collections (by whatever name we choose to call them).

2 Non-classical mathematics

The failure of mathematics to fit into ZF(C) holds also for reasons much more radical. Let us call the sorts of mathematical investigations we have talked about so far, *classical mathematics*. How to characterize these is somewhat moot, but let us just take it that they may be pursued using classical

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logic.[1] There are also non-classical mathematics, where the underlying logics of the investigations are non-classical.[2]

Several of these have appeared since they hey-day of mathematical reductionism. The most obvious example is intuitionist mathematics and its various branches: the theory of species, the intuitionist reals, the theory of the creative subject [8]. Clearly, there is no hope of reducing such investigations to a theory based on classical logic by giving explicit definitions of the intuitionist notions in classical terms. One might hope to be able to *interpret* all these theories by finding classical models. Thus, e.g., one can interpret intuitionist logic in classical logic plus an *S4* modal operator [16, 6.10 (11) and 20.13 (11)]. But it appears to be impossible to interpret many intuitionistic theories in such a way. Thus, some intuitionist theories of the reals contain both Brouwer's Continuity Theorem (every real-valued function defined over the closed unit interval [0,1] of a real variable is uniformly continuous on that interval) and Intuitionistic Church's Thesis (every total function from the natural numbers into the natural numbers is Turing computable). By suitable interpretations, one can understand each of these in classical terms; but not both together, by a construction of Specker.[3] Moreover, even if it were possible to interpret all intuitionist mathematics classically, this is *not* how intuitionistic mathematics is done. To view it in this way is therefore a *falsification* of the practice. It is as if one should claim that speakers of English are really speaking Latin, because everything they say can be translated into Latin.[4]

Matters are similar with respect to a more recent variety of non-classical mathematics: paraconsistent mathematics. There are now investigations of various inconsistent mathematical theories based on some paraconsistent logic or other: set theory based on naive comprehension, inconsistent arithmetics, inconsistent geometries (of impossible pictures), to name but a few [11, 12, 14 (chs. 17, 18), 19]. Again, there are various reductionist strategies that might sometimes come to mind. Thus, the inconsistent arithmetics are often defined by their (paraconsistent) models. So one can think of the investigation as one within standard model-theory. But this strategy does not apply to the development of inconsistent set theory, or of inconsistent geometries.

I interpolate that one does not have to be an intuitionist or a dialetheist to take intuitionist or paraconsistent mathematics to be legitimate. It suffices that these are interesting *mathematical* enterprises.

Non-classical mathematics are not only to be found in the developments of 20th century mathematics; they are also to be found in the history of mathematics before the 19th century 'drive for rigour'. An obvious example of this is the infinitesimal calculus in the 17th and 18th centuries. Quite self-consciously, mathematicians treated infinitesimals as non-zero at one stage of their proofs, and zero at another. Clearly, some paraconsistent reasoning strategy was being employed.[5]

[1] Whether the underlying logic *must* be seen as classical is a different, and more contentious, matter.

[2] A referee objected that using the term 'logic' here is out of place, since logic has to do with truth-preservation, and these 'logics' cannot all encode truth-preservation. In fact, I think that they do preserve truth in those worlds which realize the characterization of the objects of the investigation. More of this later. However, if one does not want to call this logic, I do not really mind. The question is only whether these are systematic investigations of something mathematically interesting using some notion of deduction.

[3] See [2]. Many thanks to David McCarty for helpful discussions on these matters.

[4] The plurality of mathematics is defended on the ground of constructive mathematics in [7]; it is defended on the grounds of both constructive mathematics and category theory in [9]. A variety of mathematics, including classical and various constructive mathematics, is defended in [16]. Sambin does this, obtaining these mathematics by varying parameters within a 'minimalist foundation' of mathematics (distinct from set theory). I do not see why, however, the variety of interesting mathematics must be constrained by *any* procrustian framework.

[5] The inconsistency of the early calculus has been contested by Vickers [17]. His ground is essentially that there were few working in the area at the time who endorsed the existence of objects (infinitesimals, fluxations) with contradictory properties. Instead, there was disagreement, confusion and uncertainty about the rationale for the procedures employed. This is hardly surprising, since the method employed *did* seem to depend on an inconsistent procedure [3, 23].

Of course, the reasoning strategy was changed later with the invention of epsilon/delta methods by Cauchy and Weierstrass. Maybe this was a better way of doing things; maybe this is what earlier mathematicians were all really *trying* to do (though I doubt it). But the point remains: the proof-procedures were *changed*; mathematical practice was *revised*. The original practice was still, therefore, an example of a mathematical practice which was highly non-classical.

3 Mathematical practices and games

It would appear, then, there is an undeniable *pluralism* in mathematics. The obvious question is how to make sense of this. In what follows, I will suggest a way.

Let us start by considering games. Pluralism is obviously true of games. Let us consider some salient facts:

1. There are many games; and any individual can play lots of different games. Thus, chess and draughts (checkers) are such games.
2. One of these might be more interesting than the other, more aesthetically pleasing, have a richer structure than the other. But, *qua* game, both are equally legitimate.
3. Games have rules. The rules may have been made explicit, as in the case of chess and checkers; or they may only be implicit in a practice, as are many children's games (or as are the grammatical rules of a language).
4. The rules may be learned explicitly—as, normally, one learns chess; or they may simply be picked up by entering into the game and having one's actions corrected until, in the end, one just 'has a feel' for what to do.
5. Whichever of these is the case, playing the game is just following the rules.
6. Typically, there is a point to following the rules: winning—which is not to say that a person must have the personal aim of winning to play the game; just that it is the institutional point.[6]

I want to suggest that mathematical pluralism is similar.[7] There is a plurality of mathematical practices: category theory, intuitionist analysis and inconsistent calculus. Each of these is governed by a set of rules—including inference rules—and engaging in the practice means following the rules. The (institutional) point of following the rules is establishing (proving) certain—hopefully interesting—things within the rules of the practice.[8] The rules may be explicit, as they typically are in contemporary mathematics; or implicit, as they were with number theory until the late 19th century. One may absorb the rules simply by being trained to follow them, as one learns a first language; arithmetic is usually learned in this way. Or one may learn the rules more reflectively, as one learns a second language; the way that a classically trained mathematician has to struggle with intuitionistic proof when they first meet intuitionist mathematics is like this. Just as with games, some practices may be more interesting, fruitful or whatever; but all practices, *qua* practices, are equally legitimate.

One may balk at this point.[9] Not all practices are equally legitimate. In particular, some of the practices (the legitimate ones) serve to establish truths; the others do not. This raises the question of

[6]Of course, in some things we are inclined to call games there are no winners or losers. These, presumably, have other points.

[7]I stress that this is an *analogy*; I am not suggesting that to do mathematics is to play a game. There are obvious dissimilarities too.

[8]If one takes the things proved to be true of some domain of entities (as I will suggest below), one might think of the aim of the practice, alternatively, as establishing (interesting) truths about the objects in that domain.

[9]Indeed, one referee of a draft of this article did.

truth, to which I will turn in due course. For the present, I note only that, as far as pure mathematics goes, mathematicians appear to be less interested in truth that truth-in-a-given-structure (or family of structures). Some structures are, of course, mathematically more interesting than others, have more natural applications, or wot not. But that is a different matter.

Something that speaks very strongly in favour of this view is the fact that it makes excellent sense of the phenomenology of mathematics. When one learns a game, such as chess, one is initially very conscious of the rules. ('This is a knight; now, how do they move?') Once one internalizes the rules, one no longer thinks of them, however. They create a phenomenologically objective space, within which one just moves around. Similarly, when one learns a new mathematical practice, one has to concentrate very hard on the rules. ('This is a group; now, what properties does the group operator have?', 'We are doing intuitionistic mathematics; now, is this a legitimate inferential move?') But once the rules are internalized, the phenomenology changes, and we again find ourselves within an objective terrain within which we move around.

This seems an appropriate place to say something about formalism as a foundationalist view of mathematics. Perhaps the most plausible version of formalism is to the effect that mathematics is simply the development of formal systems; that is, mathematics is nothing more than symbol-manipulation in each such system.[10] This view is sometimes described by saying that mathematics is a game with symbols [10, 2.3.]; and so it might be thought that I am advocating a variety of formalism. Now, while there are certainly some similarities between this version of formalism and the view that I am suggesting here, there are crucial differences. For a start, there is no suggestion that an arbitrary formal system is a mathematical one: the system must have mathematical content. Nor is there a suggestion that every mathematical investigation is a formal system. People were 'playing the game' of arithmetic for milenia before it was formalized. Perhaps it cannot even be formalized. (If it is essentially a second-order theory, it cannot.) Next, the view I am suggesting is quite compatible with the view that mathematical terms refer to objects of various kinds. (More of this in due course.) Finally, and to return matters phenomenological, the phenomenologies of doing mathematics and of manipulating symbols are quite different, as I have just stressed. When one learns a branch of mathematics initially, one may be doing little more that operating on symbols according to rules; but the phenomenology of a fully fledged mathematical practice is exactly one of acquaintance with the objects that the symbols (noun phrases) refer to.[11]

4 Mathematical interactions

One might wonder what makes all these rule-governed activities *mathematics* once we have given up the hegemony of ZF(C), classical logic, etc. The obvious answer is that provided by Wittgenstein's *Investigations* for games themselves.[12] The plurality of mathematics are bound together by a family resemblance—and one, it might be added, whose bounds we are ever enriching and stretching.

There are tighter connections between the practices than mere family resemblance, however—at least sometimes. Albeit the case that there are different mathematical practices, some bits of mathematics 'hang together'. For example, we use arithmetic to count all kinds of mathematical

[10] Something like this view is to be found in [4,6]. For a discussion of the various forms of formalism and their problems, see [20].

[11] Hence, according to the view which I am putting forward, mathematics has a content. That formalism cannot account for the content of mathematics was the most substantial of Frege's arguments (in *Grundgesetze*) against the formalists of his day.

[12] See, e.g., §§ 65, 67.

objects, and we apply group theory in arithmetic and geometry. One might try to turn this observation into an objection against the view suggested here. Mathematical practices are essentially different from games because they can hang together in a way that games do not.

Of course, one could take the bits that hang together to be parts of one over-arching game (of ultimate truth), such as ZF(C), as, perhaps, some who work in the foundations of mathematics do. That most practicing mathematicians do so, I find hard to believe. (I suspect that most have no interest in the foundations of mathematics whatsoever, and would be hard pressed even to state the axioms of ZF(C).) In any case, the reduction of them to ZF(C) appears to be simply a *post facto* reconstruction of parts of mathematics. Actual history is, in fact, much more interesting; and the objection is simply oblivious to the many inter-relationships between games that there may be.

For a start, games can overlap. Thus, the rules of rugby union and rugby league have an overlap. The rules of one game can even subsume those of another. Thus, consider the game chess$^-$, which is the same as chess, except that there is no castling. The rules of chess subsume those of chess$^-$. So it can be with mathematical practices. Thus, as observed, in many parts of mathematics, objects are counted. For example, we may count the number of groups of a certain kind. This means that, in such practices, number theory, or at least an important part of it, is subsumed.

Practices can also evolve from others practices. (We will note that the same is true of games in a moment.) Take group theory. In the 19th century, mathematicians such as Galois abstracted from the structures of numbers, geometries and other things, to formulate the notion of a group. In writing down the axioms of group theory, and investigating their consequences, they initiated the practice of group theory. But this allows us to apply a theorem of group theory to any one of the areas from which it was abstracted. For the fact that certain moves can be made, generally, in the group-theoretic game means that they can be made, specifically, in the game for, say, number theory, since they are equally moves there. Group theory provides, as it were move-schemata.

Another example: one game can be absorbed into a larger game (which does not mean that the original game cannot be played in its own right). Thus, the usual 2D chess can be extended to a game of 3D chess—in such a way that the restriction of the 3D game any 2D plane would coincide with the 2D game. In a similar way, one can see elementary number theory as being incorporated in analytic number theory.[13]

The chess example shows, by the way, that there may be natural and not so natural ways of extending a game. Thus the natural extension of ordinary chess into the third dimension still has, e.g., bishops moving along diagonals, rather than some other trajectory. Mathematics may be the same. Thus, assume (mootly) that to do arithmetic is to follow the rules of first-order Peano Arithmetic. The extension of this to second-order *PA* (or some axiomatizable fragment thereof), where axiom schemata are turned into quantified sentences, is clearly natural. Such an extension is much more natural than simply adding ¬Con(PA) to PA.[14]

The history of mathematics provides a rich ground for those who would investigate the interaction between mathematical practices. But enough of this for now.

[13] This raises an interesting question about the identity of objects across practices. Suppose that we prove something about a number, *n*, using analytic number theory, which cannot be proved using elementary number theory. Are they the same *n*? I will turn to matters ontological at the end of the article, but the answer is essentially yes. There is no reason why different practices must be about different objects. In the same way, different stories can be about the same object, e.g., Napoleon or Sherlock Holmes. In other words, the identity of an object need not be practice-bound.

[14] This should assuage the concerns voiced by P. Koellner [22] concerning certain kinds of pluralism.

5 Further objections

Let us move on to some other objections. According to this view, each of the multiplicity of mathematics is defined by a set of rules. One might worry that mathematics is not a rule-bound activity. Of course, in one sense it clearly is: proving is a tightly constrained activity. But, one might think, mathematicians break the rules sometimes. They may invoke new axioms or new methods of proof, not legitimized by the game being played. This, however, is easily accounted for. About 200 years ago, according to the standard story, during a game of soccer at Rugby School, someone picked up the ball and started to run with it. They broke the rules; but what they did was instigate a new game, rugby. In the same way, when someone breaks the rules of a mathematical game, and they are not simply making a mistake, they are, *ipso facto*, no longer playing that game. The new set of rules constitutes a new game.[15]

Another concern one might have is that it may be impossible to explicitly formulate the rules. Thus, Brouwer thought that the rules of intuitionist reasoning could not be formally circumscribed. Or one might hold that Gödel's first incompleteness theorem shows that the rules for arithmetic cannot be explicitly formulated, since the theory is not axiomatic. The particular examples raise many interesting issues. Maybe Brouwer was shown to be wrong when Heyting axiomatized intuitionist logic. Maybe the axiomatizability of arithmetic is to be accommodated by the possibility that arithmetic is inconsistent (and complete). But whatever the case with the particular examples, there is no real worry here. There is no reason why, in general, the rules of some practice (a game, a branch of mathematics) *must* be explicitly formulable. The question is only: can we follow them? If there are cases where the rules cannot be explicitly formulated (perhaps because they use second-order logic), this means, presumably, that the rules cannot be taught by giving them to another person explicitly. But maybe in such cases, the rules are, in some sense, hard-wired within us—in the way in which, according to Chomsky, a universal grammar is hard-wired in us—and in virtue of which, after sufficient prompting, we just 'catch on'.

A more worrying objection is posed by the spectre of relativism. What has happened to truth on this picture? The answer is as follows. Distinguish between pure and applied mathematics. What we have been talking about so far is pure mathematics: proving theorems according to some set of rules. Here there is a relativity of truth. What is acceptable or unacceptable is defined by the rules of the practice itself. The criteria of truth are *internal* to the practice. This is relativism of sorts, but not, as far as I can see, a worrying one. In particular, it is no threat to objectivity. From within a practice, results will be objectively right or wrong, just as, within a game, a move is objectively legitimate or not. You just have to be clear what the practice in question is.

But in that case, what are we to make of people who argue that some bits of mathematics are just plain wrong? For example, Brouwer and similar intuitionists famously held that classical mathematicians had got it wrong; and paraconsistent set-theorists have argued that ZF(C) gets our set theory wrong. Since classical mathematics, intuitionist mathematics, paraconsistent set theory and ZF(C) are all equally legitimate games, are such disputes simply mistaken?

No. What is at issue here is this. In each case there is a received practice, number theoretic reasoning, set theoretic reasoning or whatever. But there can be legitimate disputes about what, exactly, the correct norms of that practice are. We formulate different sets of rules, trying to capture these. There can be a fact of the matter about who, if anyone, gets it right. In the same way, linguists

[15] Lurking in the background here is the thorny question of how to individuate practices. What are the synchronic and diachronic identity conditions for these? Arguably, a minor modification of the rules of test cricket leaves it as cricket, but one-day cricket and test cricket are not the same game, even though the one evolved from the other. Fortunately, we do not need to try to resolve these matters here.

can take a spoken language and try to formulate a set of rules which capture its grammar. Some grammars can just be wrong. Of course, once a set of formal rules is set up, they do characterize some language or other; and even if it is not the one targeted, it can still be spoken. Similarly, once rules for a mathematical practice are explicitly formulated, it can be followed. Thus, an advocate of paraconsistent set theory with unlimited comprehension does not have to claim that ZF(C) is wrong. ZF(C) is just as good a practice (*qua* practice) as paraconsistent set theory. It is just that those who adhere to it are wrong if they claim that it correctly characterizes our naive practice about sets.

6 Applied mathematics

Let us turn from pure mathematics to applied mathematics. The matter of relativism is quite different there. We apply mathematics for many purposes: to simplify electronic circuits, to compute the orbits of satellites, to test biological hypotheses statistically.[16] It is always an important question as to which mathematical theory is to be used in each application. Sometimes, a branch of pure mathematics may arise out of an application: arithmetic was presumably like this. Sometimes, a branch of mathematics may be developed with an eye on an application: the infinitesimal calculus was like this. Sometimes, when we want to apply mathematics, we can take a pre-existing system (which may, before that, have had no application) off the shelf: group theory was like that for modern physics. But always there is an important conceptual distinction between the mathematical theory itself and its application to this, that or the other.

Once we apply a mathematical theory, there are criteria of correctness external to the pure practice. We need to get the *right* mathematical theory for the application in question. What makes it right? Here the story depends on one's philosophy of science. If one is an instrumentalist, one cares for nothing save the directly testable consequences. The mathematical theory is right if, when one applies it, the predictions check out. That is all there is to the matter. If one is a realist, success of this kind will be a (fallible) mark of something deeper. The mathematics must describe what is really there. What this means is that, under the correlation deployed in the application (for example, between points in Euclidean space and space-time points, or between functions in a complex Hilbert space and quantum states *in re*), the mathematical structure and the physical structure are isomorphic. That is why facts about the domain of application can be read off from facts about the mathematical structure [3, 7.8].

Speaking of applications, one should note what seems to be a significant disanalogy between pure mathematics and games: there is, as far as I can see, no phenomenon of the application of games similar to the application of mathematics.[17] A natural question is: why? Why can mathematics be applied in a way that games cannot? Part of the answer is that mathematical practice deals with things that are propositional (and so truth-apt—or better, truth-in-a-structure-apt); games (generally) do not. That we are dealing with propositional objects allows a certain kind of application. This cannot be the whole answer, however. Writing fiction is also dealing with propositional objects. Yet this has no application similar to mathematics either. Why is this? Presumably, a large part of the answer is that the origin of much pure mathematics (arithmetic, geometry and calculus) was in some application or other, as I have already noted. Unsurprisingly, then, mathematics has applications: it was designed to do so. But what of those parts of mathematics which were not designed with

[16]One can think of metamathematics as an applied mathematics too. Formulas are just finite strings of symbols, '$\wedge$', '$\exists$', etc.; and axiom systems are collections of these, structured in a certain way. A metatheory establishes certain results about these.

[17]Games can be applied, of course, to develop fitness, problem-solving ability, team spirit or wot not. But this would seem to be quite a different matter.

an eye to application? There would seem to be no *a priori* reason why such parts of mathematics sometimes find application. Perhaps we just have to accept this as a contingent feature of the world in which we live [21]. Perhaps, if we develop enough pure mathematical systems, some of them are bound to find application sooner or later.

7 Matters ontological

Let me end by commenting on the ontological story about mathematics that goes with the picture I have painted. In fact, many ontological stories fit happily with it. For example, one might be a plenitudinous, or 'really full-blooded' platonist.[18] Every theory characterizes a domain of existent objects—not just the one and only one on which mathematical platonists are usually fixated. Alternatively, and just as plausibly, one might endorse a conventionalism, of the kind advocated by Carnap in 'Empiricism, Semantics, and Ontology' [5]. According to this view, questions of existence have meaning within, and only within, a linguistic framework (practice), and are settled by the rules thereof. Outwith such a context, questions of existence are meaningless.

Another possibility which goes very well with the view, and the one which I, in fact, prefer, is the noneist position sketched in *Towards Non-Being* [13, ch.7]. A mathematics practice can be taken to characterize an object, or collection of objects. This characterization is guaranteed to be true, not necessarily at the actual world, but at some world or other. The world may be impossible, however, in the sense that its logic may be different from that of the actual (and other possible) worlds. Thus, the pursuance of the practice can be seen as the exploration of the structure of some non-existent objects—or, at least, objects that do not exist at this world. All games are, then, equally legitimate, in the sense that they all capture the way things are at some world (or worlds).

According to this view, mathematics and fiction are very similar activities. Mathematical theories (practices) and stories are free creations of the human spirit, and we can invent whatever we like. Having done so, we may then follow the inferential rules in play, to discover more about the mathematical or fictional situation characterized [13, 7.7]. One can, if one likes, think of mathematical assertions or fictional assertions as coming prefixed with a tacit 'In the practice/story, it is the case that ...', just as we can think of legal assertions as prefixed by 'In such and such jurisdiction ...'. But of course, we are so used to operating certain practices, or of operating within a certain jurisdiction, that the prefix may become invisible to us.

This ontological position will answer another question that is likely to come up in connection with the story I have told. The view is mathematical pluralism. Does it entail a logical pluralism? Yes and no. Given the perspective of *Towards Non-Being*, worlds are many; and logic may differ from world to world. In that sense, yes. But there is only one actual world, only one actual truth, and so only one true logic. In that sense, no.

8 Conclusion

Like it or not, mathematical pluralism seems to be a fact of mathematical life—both diachronic and synchronic. Ours it is to make sense of this fact. One can, if one wishes, declare that there is one true mathematics (ZF(C)?), and that the rest is all mistaken. Such would seem to be a procrustean position of desperate proportion—and one, moreover, with a good deal of hubris. The position I have sketched is, I hope, much more plausible than this. There is a genuine plurality of mathematical

[18] As mooted in [1].

practices—a motley, as Wittgenstein puts it[19]—each with its own set of rules. We are free to pursue any of them. All practices are equal. Though, of course, in terms of intrinsic interest, richness, beauty, application, etc., there will be significant differences. Some animals will always be more equal than others.[20]

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[19] 'I should like to say: mathematics is a MOTLEY of techniques of proof.—And upon this is based its manifold applicability and its importance.' [22], III, 46.

[20] Versions of this article have been given at various gatherings: the conference *This Conference Has No Name*, held at the Graduate Center, City University of NY, December 2009; the Melbourne Logic Group, March, 2010; an Arché FLC Seminar, University of St Andrews, May 2010; the University of Auckland, July 2010; and McGill University, October, 2010. I am grateful to the members of those audiences for their thoughtful comments, which improved the article greatly. I am similarly grateful to a number of anonymous referees for this publication. The final draft of this article was produced in the light of these, at a time when I had a heavy load of other commitments. I have no doubt that more time to reflect would have produced a smoother and polished paper.

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