Possible Thesis Topics

Thomas Forster

September 1, 2016

- There is a standard trick of Ackermann's that makes $\mathbb{N}$ look like V_ω: ordain that $n\,E\,m$ iff the nth bit of (binary) m is 1. There is a suggestion in [4] that there is an analogue for Church's set theory CUS, that in some sense it contains no mathematics not already in ZF—that the existence of a universal set is just a *trompe l'oeil* trick. There is a mathematical notion of *synonymy* which is in play here. (The theories of Boolean rings and of Boolean algebras are synonymous, for example. See work of Visser and Friedmann.) The question is: "is CUS synonymous with ZF?" My guess is that the answer is a fairly straightforward 'yes'—possibly with a bit of tweaking. (Mind you, i know of no proof; i did try to get David Matthai to sort this out but i was unable to twist his arm hard enough). So far so good. One can of course ask the same question about NF: "Is NF synonymous with some theory of wellfounded sets?" and *that* question is going to be a lot harder. It's an interesting question beco's there is the thought that perhaps the mathematical world painted by NF is so alien that NF cannot be synonymous with *any* ZF-like theory. I'm not sure what a proof of such an assertion would look like! Any result like that would provide a powerful argument for studying large sets in NF, beco's it would mean that the large sets are not just wellfounded sets-in-drag (as they are in the Church-Oswald models) but are genuinely alien things—and what NF is telling us about them cannot be faulted on the grounds of mere inconsistency. (NF might still prove false theorems of course, but at this stage we have no means of detecting them. And the mere fact that it contradicts ZF doesn't give any trouble because it seems that NF does not contradict what ZF has to say about *wellfounded* sets.)

- Now that NF is known to be consistent we have to review all the results that say that certain mathematical concepts are not concretisable as sets: polymorphism, homotopy . . . . There are ideas in geometry that are popularly supposed to need some set theory beyond ZFC—Grothendieck universes—but i think that has all been sorted out by Colin McLarty. There is a literature on certain categories not being concretisable, and i free-associate to *Isbell's criterion*. These are things I want to think about, but i'm going to have to learn some more topology and geometry to get to the bottom of them. If you want to think about this stuff i will be happy to join you.

- There is a nice theorem about Rieger-Bernays permutation models, which I made a small contribution to proving: a formula is equivalent to a stratified formula iff the class of its models is closed under the Rieger-Bernays construction. That's a nice fact. A slightly less striking fact is that in theories where one can quantify over the permutations that give rise to the models one has a natural interpretation of modal syntax: $\Box p$ will mean that p holds in all permutation models. In NF this gives rise to a particularly degenerate modal logic: S5 + Barcan + Converse Barcan + Fine's principle H. Not very interesting. It may be that by addionally considering judiciously chosen special proper subsets of the available permutations one gets more interesting modal logics. Another thing that needs to be looked at is whether the topologies on the symmetric group on the carrier set have anything to tell us about the family of permutation models. Is the set of models containing Quine atoms dense, for example? Does that topology interact in any useful way with the usual (Stone) topology on the space of permutation models? Olivier Esser and i looked at this a decade ago and got precisely nowhere; there may be someting cute to be said about why this might be so. One circumstance that I am sure is significant but whose significance is obscure to me is the fact that although composition of permutations has some meaning in terms of the model theory and modal logic, inversion seems to have no meaning at all. The fact that the family of permutations is a group seems to do nothing over and above it being a semigroup with a unit. Is this anything to do with the fact that we should really be dealing with *setlike* permutations rather than permutations that are sets, and the inverse of a setlike permutation might not be a set? The matter cries out for investigation.

- There is a deep connection between permutation models and unstratified assertions about virtual entities (such as cardinals) which arise from congruence relations on sets. Cardinal arithmetic is that part of set theory for which equipollence is a congruence relation. All assertions—even unstratified assertions—of cardinal arithmetic are invariant. There seems to be a tendency for the unstratified assertions to be equivalent to assertions of the form $\Box$ or $\Diamond$ prefixed to a purely combinatorial assertion about sets. ($\Box$ and $\Diamond$ are as in the modal logic of Rieger-Bernays permutation models, as above.) For example the axiom of counting is equivalent to the assertion "$\Diamond$(the von Neumann ω is a set)". It would be nice to know whether this happens generally and if so why.

- NF and proof theory. We all know that we haven't really got the right concept of proof-as-mathematical-object—yet! Theoretical computer scientists are at work on it even as I write. Proof theory of set theory is a problem because the axiom of extensionality is a proof-theoretic nightmare. If we drop it from NF the resulting theory has a sequent presentation for which one can prove cut-elimination. This important result is due to Crabbé. [2] and [3]. Anyway, it would be good if some member of the tribe of theoretical computer scientists who work on proof theory were to have a look at the possible ramifications of their work for NF studies. Constructive theories generally have nicer proof theory than their classical counterparts, and in the particular case of NF there are extra reasons (namely the breakdown of the double-negation interpretation) to support my long-standing conjecture that the obvious constructive version of NF (weaken the logic but keep the same axioms) is consistent and weak. There are good reasons to expect this, and I suspect the proof will be quite easy. It has been very elusive so far. It is only fair to say that Holmes doesn't believe that constructive NF (INF to its friends) is any weaker than NF.

Might there be a clever way of coding constructive NF inside the theory of recursive functions ... ?

- The theory T$\mathbb{Z}$T, of simple typed set theory with levels indexed by $\mathbb{Z}$ rather than by $\mathbb{N}$, is a strange and interesting theory. It is consistent by compactness, but we do not know if it has any ω-standard models. There is (are?) a wealth of open questions about it, more than enough to keep a Ph.D. student occupied.

- In [1] Bowler, Al-Johar and Holmes prove that if you add to extensionality a principle of acyclic comprehension (You can probably guess what that means: it's like stratified comprehension only stronger) you don't get a system weaker than NF, you actually get NF. Remarkably, Nathan Bowler has recently shown that—modulo a very weak system of set theory—every stratifiable formula in the language of set theory is logically equivalent to an acyclic formula. This is a very striking discovery that needs to be followed up. Unfortunately Bowler has not published this fact anywhere. There may be implications for the proof theory of NF—after all Crabbé was able to show cut-elimination for the stratified fragment of the comprehension scheme.

- The theory KF of my joint paper [5] with Richard Kaye in the JSL 1990 is quite interesting. There is a vast sequel to that paper which has never been turned into anything publishable. Some of the material is alluded to and explained in Mathias' [7] survey article on Weak Set theories: Annals of Pure and Applied Logic, **110** (2001) 107–234. One very interesting question about KF is whether or not it is consistent with the assertion that there is a set that contains wellorderings of all lengths. (Think of this assertion as "The ordinals are a set") This question is interesting because it is related to the question of how far it is possible to separate the paradoxes. The paradoxes can all be seen off in one of two ways: either (i) the problematic collection turns out not to be a a set, or (ii) it remains a set, but one can't manipulate it as freely as one would wish. It is natural to wonder to what extent decisions one takes about how to knock one of the paradoxes on the head affects decisions about how to knock the others on the head. If the collection of all ordinals is a set must the universe be a set too? Or at least, does the sethood of the collection of all ordinals smoothly give rise to a model of a set theory with a universal set?

- It is a curious fact about NF that if one replaces '$\in$' by '$\notin$' throughout in its axioms one obtains another axiomatisation—at least if one's logic is classical!

  Let $\hat{\phi}$ be the result of doing this to a set theoretic formula ϕ. Obviously $\hat{\phi}$ is a theorem of NF iff ϕ is. Is $\phi \longleftrightarrow \hat{\phi}$ always consistent with NF? The obvious weapon to use is Ehrenfeucht games, but I have not been able to make any significant progress using them.

- What does NF prove about wellfounded sets? Is the theory of wellfounded sets of NF invariant under Rieger-Bernays permutations? Probably not. Is the stratified part of it invariant? Perhaps more natural questions concern the content of that theory rather than the metatheorems once can prove about it. All we know at present is that it contains the theory KF alluded to above. (Not obvious that it satisfies either infinity or transitive containment, for example). If that is the best one can do, then every wellfounded model of KF is the wellfounded part of a model of NF. There are probably quite a number of theorems like that that one can prove, and a fairly straightforward example (every wellfounded model of ZF is the wellfounded part of a model of NFO) is one that can be found in my Church *festschrift* paper. It would be nice to have converses: "KF is the theory of wellfounded sets in NF" would be nice, and now that we have Holmes' consistency proof for NF this problem is in principle tractable. My guess is that every wellfounded model of KF is the wellfounded part of a model of NF. I also suspect it's true (and easy to prove) that every model of KF has an end-extension that is a model of NFU.

- There is a version of Gödel's L constructed by stratified rudimentary functions; AC fails in this model. This is an interesting structure—or family of structures—about which very little is known. Look at my BEST paper: `www.dpmms.cam.ac.uk/~tf/strZF.ps`

- A rather more interesting topic, which is the chief topic of that paper, is the model of hereditarily symmetric sets, which models a stratified fragment of ZF and refutes choice. This structure was investigated by my students at Cambridge but there is plenty of work still to be done. There is a significant body of unpublished work which could be made available to anyone who wants to start work on it. For example, Nathan Bowler has shown that that model obeys IO, the principle that says that every set is the same size as a set of singletons . . . which is what one seems to have to add to the stratified fragment of ZF to obtain a theory that interprets ZF.

- Weak choice principles in NF. We need DC to do forcing, for example. Also no version of choice talking only about small sets has been refuted. As far as we know the continuum can be wellordered. Indeed, Holmes has shown that if DC and the axiom of counting hold, then there is a forcing model in which the continuum can be wellordered.

- This last topic is a favourite of André Pétry's—or was. Develop model theory for Stratified formulæ. There is a completeness theorem for stratified formulæ that he and I put the finishing touches to: a formula is equivalent to a stratified formula iff the class of its models is closed under the Rieger-Bernays permutation construction. Theorems about cut-elimination and stratification have been proved by Marcel Crabbé. It does seem that it should be easier to prove cut-elimination for stratified formulæ but the situation is clearly complex: every provable stratified formula has a cut free proof, and will also have a stratified proof—but there is no guarantee of a proof that is both. The issue is subtle. It seems that the assertion $(\forall x \in y)(\exists z)(z \notin x) \to (\exists w)(w \notin y)$ has a stratified proof, and a cut-free proof, but if you eliminate the cuts from the stratified proof, the result is not stratified. This proposition comes in distinct classical and constructive versions. The situation cries out for the attentions of a Ph.D. student. This kind of syntactic monkeying around with model theory is very much in the spirit of Finite model theory: sexy stuff these days: definitely worth a look.

- I don't promote questions about NFU here: if you want to study NFU you should go to Boise and work under Randall Holmes. In fact if you come to work on NF with me you will be sent off to Boise to study with Holmes at some point or other). I do have one question about NFU tho': can there be a model of NFU in which the set of atoms forms a set of indiscernibles? Holmes thought for a long time that they are always indiscernible, but recently has shown that in the usual ZFJ models the atoms are all discernible. I think that it's a strong assumption possibly equivalent to the consistency of NF.

References

[1] Zuhair Al-Johar, M. Randall Holmes and Nathan Bowler "The Axiom Scheme of Acyclic Comprehension". Notre Dame Journal of Formal Logic **55** (1):11-24 (2014)

[2] Crabbé, Marcel "Stratification and Cut-elimination". Journal of Symbolic Logic **56**, 1991. pp. 213-226

[3] Crabbé, Marcel "The Hauptsatz for Stratified Comprehension: a semantic Proof". Mathematical Logic Quarterly **40**, 1994. pp. 481-489.

[4] Forster, T.E. "The Iterative Conception of Set" *Review of Symbolic Logic*. **1** (2008) pp 97−110.

[5] Thomas Forster and Richard Kaye "End-extensions preserving Power Set" Journal of Symbolic Logic, **56** pp 323-328).

[6] Richard Kaye and Tin Lok Wong. "On interpretations of arithmetic and set theory". Notre Dame Journal of Formal Logic Volume 48, Number 4 (2007), 497–510.

[7] A.R.D. Mathias "On MacLane Set Theory" Annals of Pure and Applied Logic, **110** (2001) 107–234.