Possible Thesis Topics (as annotated by the diabolical Dr Holmes)

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Now that it is finally official that NF is consistent, there is a huge range of questions about it that have all suddenly become live. As well as questions arising from the challenge of understanding Holmes' proof, there are questions of long-standing which suddenly acquire *point*. And it is these latter questions that i want to start with.

Clarify the status of TTT_n for all sufficiently small n. Randall says that for all n is TTT_n equivalent to NF. I don't understand this.

What is the status of the Prime Ideal theorem? How many ultrafilters on V might there be?

Study the GFP for $x \mapsto$ set of transitive (proper) subsets of x

How strong is NF^{pf}, the parameter-free fragment of NF? Kaye did some work on NF^{pf} in the 1980s which i can dig up. Can Specker's proof of $\neg$AC be carried out in it? There is a URL devoted to a formalisation of Specker's proof on `https://us.metamath.org/nfeuni/nchoice.html`

Holmes: Surely very weak. I do not know how to address this.

Is NF_3 finitely axiomatisable?

Holmes: Surely it is not. But I don't have a handle on techniques to derive this kind of result.

Good notion of restricted quantifier in theories with $V \in V$ presumably only for those theories that are synonymous with theories of wellfounded sets.

Holmes: Perhaps quantifiers restricted to s.c. sets have interesting properties, since types of variables restricted to such sets can be freely raised and lowered. I have actual work in this in theorem prover settings. I mean here, quantifiers restricted to a specific s.c. set.

The fact that NF is consistent doesn't mean that it is the "correct" stratified theory of sets in the way that ZF is the "correct" theory of wellfounded sets. Zermelo is a consistent theory of wellfounded sets but it has identifiable flaws ... it is only a *first attempt*. Perhaps NF, too, is only a first attempt, and there is a stronger "correct" stratified theory of sets . . . obtainable perhaps by adding axioms already known in the NF literature. Obvious candidates are the axiom of counting, Henson's axiom of Cantorian Sets, or Holmes' axiom of small sets. If we had a clearer picture of what models of NF looked like we would know which of these axioms to invite to the party, but at the moment the only models we know are those obtained by Professor Randall ("Faustus") Holmes at the cost of selling his soul to the Devil and those models are – well – diabolical. And they are diabolical because the Fraenkel-Mostowski method used to obtain is a method that throws up models satisfying symmetry conditions that will violate choice. In consequence Holmes' models violate choice much more gravely than the axioms of NF seem to require.

Holmes: the extensions of NF with unstratified axioms already known which I have studied as extensions of NFU presumably work.

It would be nice if there were a method of obtaining new models of NF from old that output models that satisfy more choice than the input models.

If there are natural "correct" axioms to add to NF where might we find them? There is a theorem of Pétry-Henson-Forster in the style of 1950s model theory that characterises stratified expressions as those preserved by the Rieger-Bernays construction. There is a closely related but distinct characterisation of stratified expressions as those preserved by certain group actions. Both these are standard in the NF literature (see [], [], [] . . .). The symmetry conditions have given rise to a theory discovered by Holmes []. The same ideas of symmetry gave rise to some bizarre models of stratified fragments of ZF with stratified replacement and dramatic failures of choice [**?**].

Holmes: The system of my second symmetry paper is philosophically very charming but some disconcerting things happen.

Do we expect the natural axioms we recruit for addition to NF to tell us anything about the wellfounded part of a model of NF? One can express this in a way that makes more sense to ZF-istes by asking if there are assertions about the big sets that have consequences for wellfounded sets and can give us a steer on the continuum hypothesis?

Holmes: I express my continuing conviction that it is simply not natural or appropriate to talk about the well-founded model of a model of NF(U). It can be done, but it is entirely alien to the nature of the theory.

This has got garbled: what did he mean?

Holmes: Something you can take away from my model construction is that NF has nothing to tell us about the continuum hypothesis. Nor do I think there is any way that properties of big sets would influence CH. What properties of big sets might do is give enlightening formulations of large cardinal axioms. CH is a weak, stratified assertion.

1 Synonymy

Two theories are synonymous iff they "have the same models". *e.g.* Theory of partial order and theory of strict partial order, or boolean algebras and boolean rings. Any model of one can be turned into a model of the other in a definable invertible way. equivalently: there are interpretations of the two theories into each other whose compositions are the identity up to logical equivalence. A theory is *tight* if any two synonymous extensions of it are identical. ZF is tight, PA is tight, Zermelo + "every set belongs to a level" is tight. Tightness seems to be something to do with second-order categoricity.

Recent work (Button, and Forster-and-Holmes, [3], [14]) has shown that NF is not synonymous with any set theory with foundation, but that ZF however is synonymous with Church's universal set theory CUS. NF is not tight but it is 'stratified-tight'.

There are several remaining questions in this area, and they now matter. Now that we know that NF is consistent, and that it is not just syntactic sugar for ZF, we conclude that it is reporting on parts of Mathematics that ZF cannot reach.

Holmes: I don't really believe in this program. NFU is equally not syonymous with any theory of well-founded sets in the strict sense (basically because it adds "nonstandard" objects to the universe) but in fact its mathematical world is demonstrably the same as that of ZFC. Nonstandard analysis is not syonymous with the theory of the reals, but it actually is essentially the same theory, and this is what is going on with NFU and Zermelo style set theory; this analogy is actually fairly exact. Nonstandard analysis has very interesting technical advantages over the usual formulation of the reals, but it will not do anything fundamentally new; the same is probably true of NFU, and of NF.

There is a lovely result of Kaye-Wong [20] that says that PA is synonymous with KWo, the theory ZF $\setminus$ infinity + $\neg$infinity transitive containment. We need to think about what the Ackermann bijection does to weak fragments of arithmetic and stratified fragments of KWo. There's the obvious tho'rt that IO is something to do the exponential function $n \mapsto 2^n$ being total. IO is not a theorem of KF. (This follows from the consistency of NF).

Another thing one could usefully do in this connection is go over [20] reworking everything using the Oswald bijection instead of the Ackermann bijection.

2 Category Theory in/of NF

SCU is the principle that a strongly cantorian family of strongly cantorian sets has a strongly cantorian sumset. Consistent with symmetric comprehension? **Holmes:** I do not know.

Holmes: My own evil project: find a model of NFU in which SCU is false. It looks quite tricky to do so.

Is the category of sets of str(ZF) cartesian closed? Does it become cartesian closed if you add IO? **Holmes:** I would think no for the first, maybe for the second. Scratches hives.

Category of situated sets?

2.1 Concretisable Categories

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.

Holmes: These are interesting questions but I am not at all certain that NF(U) will provide much assistance. I am quite certain that use of NF per se has no particular advantages of use of NFU.

3 Permutation Models

These are permutation models in the sense of Rieger-Bernays, not Fränkel-Mostowski. It is the techniques used to demonstrate the independence of the axiom of regularity from ZF \ regularity. Since the construction preserves stratified expressions it is a powerful technique for demonstrating the independence of unstratified expressions from NF, first introduced in NF studies by Dana Scott. It takes up a whole chapter in both editions of [**?**], and there are worse places to start reading.

There is a nice theorem about Rieger-Bernays permutation models, which I made a small contribution to proving: a formula is equivalent to a stratifiable 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 additionally considering judiciously chosen special proper subsets of the available permutations one gets more interesting modal logics. It is already clear that if you restrict attention to *definable* permutations (permutations fixed by all automorphisms) then you get a different logic. 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 something cute to be said about why this might be so. I mean, *clearly* we were doing something wrong, but it would be nice to know what! 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 the 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.

Holmes: I'm willing to believe that this might be a useful area of investigation. I'm interested in working on how to systematically find permutations that cause propositions of interest to have desired truth values, possibly developing a toolkit for this purpose.

Nathan Bowler has recently had some helpful things to say to me about what he calls the "return" permutation in Rieger-Bernays models. Does V^σ contain a permutation that will take us back to V? Yes, always, but it might not be definable even if σ is. If both σ and the return permutation are definable then $\mathrm{Th}(V)$ and $\mathrm{Th}(V^\sigma)$ are synonymous. But it is known that stratified formulæ are precisely those that are invariant under RB model constructions. So this looks as if its trying to tell us that NF is synonymous with any unstratified extension of itself. This isn't true of course, because of the axiom of counting. but something with that flavour should be. Perhaps there is a theorem along the lines that if ϕ is unstratified then either NF $+\phi \vdash$ Con(NF) or NF and NF$+\phi$ are synonymous. In this connection i have been able to show that NF + no-Quine-atoms is synonymous with NF + there is a unique Quine atom and it belongs to every set that has no $\in$-minimal element. That's quite nice (and it enables us to show that NF is not tight) but i'm sure it's just the tip of the iceberg.

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.

Holmes: This goes along with the thought that one should develop a toolkit for proving results about RB models.

4 The $\forall^*\exists^*$ Problem

The Universal-Existential problem is still open. There is a family of conjectures about universal-existential sentences (no restricted quantifiers allowed) in NF and T$\mathbb{Z}$T. One conjecture is that T$\mathbb{Z}$T decides every universal-existential sentence. A lot of progress has been made with special cases (and some is still being made: Diamant Pireva did an M.Phil. thesis on it in 2023) but it's still open.

Holmes: I have never understood where this question came from or why it might be true.

5 NF, Proof Theory and Constructive Logic

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, [5] and [6]. 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, beco's we *NFistes* are surely missing something.

The late Daniel Dzierzgowski did some important work on constructive TST in the 1980s but nobody has properly taken up the baton. And the situation with constructive NF (*i*NF to its friends) is annoyingly unclear. The obvious strategy of extending the negative interpretation to NF doesn't seem to work, or rather nobody has been able to make it work. So is constructive NF weak? It's strong enuff to prove that the universe is not Kuratowski-finite.... But is that enough to interpret Heyting arithmetic? I've tried, but so far without success. I have a long-standing conjecture that the obvious constructive version of NF (weaken the logic but keep the same axioms) is consistent and weak. It is only fair to say that Holmes doesn't believe that constructive NF is any weaker than NF. Michael Beeson spent a lot of time and effort on this question and produced some `lean` code that is helpful, but he didn't solve either question. He ended up describing *i*NF as a "tar-baby" ... an American expression i had to look up. `https://en.wikipedia.org/wiki/Tar-Baby`

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

Holmes: I agree that this is an interesting topic.

6 T$\mathbb{Z}$T

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 for a long time we did not know whether or not it has any ω-standard models. (I think Randall Holmes has recently shown that it has).

Holmes: Indeed it appears to be settled though there is something routine to check: I need to verify that Jensen's argument for ω-models of NFU carries over to NF without difficulty.

There is (are?) a wealth of open questions about it, more than enough to keep a Ph.D. student occupied. My thoughts on this are publicly available in the borderline-publishable (but not so far published) `www.dpmms.cam.ac.uk/~tf/TZTstuff.pdf`.

7 Acyclicity

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 the whole of 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, and one would expect the acyclic fragment to be even better behaved..

Holmes: It seems to me that acyclicity might be interesting in proof theory.

Extending Calliope's work to acyclic formulæ.

Stratified comprehension in $L_{\omega_1,\omega}$ is consistent as long as only finitely many levels are used. What happens if you restrict further to acyclic formulæ. Does Bowler's proof work in the infinitary case?

8 KF

The theory KF of my joint paper [13] 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' [21] 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. There is one particular case of this general question that piques my interest: 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?

Holmes: well, of course, because NF is consistent... One might want to phrase the question in a way which rules out NF.

9 Logial Duality

It is a curious fact about NF that if one replaces '$\in$' by '$\not\in$' throughout in its axioms one obtains another axiomatisation—at least if one's logic is classical!

Let $\phi^{\circlearrowleft}$ be the result of doing this to a set theoretic formula ϕ. Obviously $\phi^{\circlearrowleft}$ is a theorem of NF iff ϕ is. Is $\phi \longleftrightarrow \phi^{\circlearrowleft}$ 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.

With the halp of Nathan Bowler i have been able to show the consistency wrt NF of the subscheme $\phi \longleftrightarrow \phi^{\circlearrowleft}$ where ϕ is "stratifiable-mod-2". This emerged as part of a project to examine formulæ that are "stratifiable-mod-n". Have a look at `www.dpmms.cam.ac.uk/~tf/stratificationmodn.pdf`. It's interesting stuff, but perhaps not of sufficient moment to justify a Ph.D.

Holmes: Thinks about the desired RB toolkit.

10 Stratification

There is an M.Phil. thesis (or Part III essay) to be written on stratification. A kind of survey article—one that could avoid set theory altogether actually. There is a result of Calliope Ryan-Smith to the effect that the language of stratifiable formulæ of the language of set theory is not context-free, but that the language of strati**fied** formulæ *is* context-free. There is also a result of Jamie Gabbay's to the effect that the system of rewrite rules for stratifiable set comprehension is confluent and terminating. Such a document could include a discussion of acyclicity and stratification-mon-n.

Could be something to be said about stratified unification.

11 The theory of wellfounded sets in NF

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 on 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 must surely be lots of theorems with this flavour and it would be a useful exercise to ascertain the correct formulation and perhaps prove an omnibus result.

It might be an idea (in this connection) to think about NZF (= NF $\cap$ ZF). It's recursively axiomatisable but not finitely axiomatisable.

Holmes: I reiterate my view that there is essentially no reason to talk about pure sets or the well-founded part of the universe in a theory with stratified comprehension. It is mildly interesting that one can sort of talk about this, but it isn't a natural thing to talk about. It is more to the point to talk about the isomorphism classes of well founded extensional relations with top under isomorphism (which one can turn into well founded sets with an RB permutation, though one should check that this works as expected).

12 Implementing λ-calculus in NF

No-one has ever sat down and written this out.

Holmes: I have. It is even in print.

13 Cardinal Arithmetics in NF

Specker pointed out years ago that it's not clear whether or nor NF proves the existence of infinitely many infinite cardinals. This question is still open! Holmes' models of NF all have lots of inifinite cardinals, because AC fails in them so badly.

A model of NF that has only (internally) finitely many infinite cardinals satisfies some nontrivial forms of choice: $n = 2n$ for every infinite cardinal n for example.

Holmes: My conjecture would be that this question will be answered positively, but I certainly don't know how to do it.

14 The Baltimore Model

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. And—if i remember correctly—Vu Dang showed that if one starts with a model of ZF, takes the inner model of hereditarily symmetric sets and then considers the family of isomorphism classes of [wellfounded] set pictures in this inner model, it turns out to be isomorphic to the model one started with. I find this a very striking result: it *looks as though* the move to the inner model of hereditarily symmetric sets destroys unstratified information, but it turns out that all that information has been safely squirrelled away in the set pictures.

Holmes: These things are interesting, but they end up surprisingly ZF-like as I recall.

15 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.

Holmes: Countable choice, DC, the axiom of determinacy, all have settled status (things which speak about limited cardinalities).

Linear order on the universe and the prime ideal theorem remain inaccessible to us with my methods. My methods do show that it is possible for the power sets of all well ordered sets to be well ordered and indeed all sets supporting well founded extensional relations to be well orderable.

16 Stratified Model Theory

This last topic is a favourite of André Pétry's—or was. Develop model theory for stratifiable formulæ. There is a completeness theorem for stratifiable formulæ that he and I put the finishing touches to: a formula is equivalent to a stratifiable 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.

Holmes: This is good stuff.

17 NFU

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 in any case). 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. citation needed

Holmes: The question is answered, since the set of urelements can be empty. The question of whether indiscernibility of urelements gives a theory with the strength of NF is interesting.

18 Stratification

The project is to show that all of mathematics is stratified.

Colin McLarty showed that all the Grothendieck stuff can be done in Mac Lane. So presumably it can be done in KF + IO

Holmes: Can all of mathematics be done in a stratified style? Should this be encouraged? That is how I would put it.

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Adam Epstein is asking me how the isomorphism between a vector space and its double dual plays out in NF

Colin McLarty showed that all the Grothendieck stuff can be done in Mac Lane. So presumably it can be done in KF + IO Is the category of sets of str(ZF) cartesian closed? Does it become cartesian closed if you add IO?