Scrapbook on Set Theory with a Universal Set

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Contents

Chapter 1

Stuff to fit in somewhere

It's just struck me: $|V|$ is an annihilator for cardinal addition and cardinal multiplication!

1.1 NF$_3$

I have been thinking in a small way about permutation models for NF$_3$. The standard result is that if $\mathfrak{M}$ is a model of NF and σ a setlike permutation, then $\mathfrak{M}^\sigma$ is also a model of NF and satisfies the same stratified expressions. Naturally there are specialisations of this for k-setlike permutations and k-stratifiable formulæ. That is to say, we have ... If σ is k-setlike then $\mathfrak{M}$ and $\mathfrak{M}^\sigma$ agree on $k+2$-stratifiable sentences. Now NF = NF$_4$, so in particular we get: if $\mathfrak{M}$ is a model of NF and σ a 2-setlike permutation, then $\mathfrak{M}^\sigma$ is also a model of NF and satisfies the same 4-stratified expressions. And of course we also get: if $\mathfrak{M}$ is a model of NF$_3$ and σ a 1-setlike permutation, then $\mathfrak{M}^\sigma$ is also a model of NF$_3$ and satisfies the same 3-stratified expressions. Also: if σ is 1-setlike then $\mathfrak{M}^\sigma$ satisfies the same 3-stratified sentences as $\mathfrak{M}$: $(x \in y \in z)^\sigma$ is $(x \in \sigma(y) \in \sigma``(\sigma(z)$ which is OK as long as σ is 1-setlike. Similarly 2-setlike permutations preserve 4-stratifiable sentences and therefore preserve NF. Presumably there are 5-stratifiable sentences that are not 4-stratifiable.

Might be an idea to write out a proof that if π is $2-$setlike and $\mathfrak{M}$ is a model of NF then $\mathfrak{M}^\pi$ is also a model of NF.

Now we have some examples of permutations that are 1-setlike but not 2-setlike. We can invoke these in models of NF and thereby get funny models of NF$_3$. Look at remark 21 on p. 134

1.1.1 Is NF$_3$ finitely axiomatisable?

There is this question about whether or not NF$_3$ is finitely axiomatisable. I have stumbled across a reflexion that *just might* have some bearing on it. There seems to be a problem with axiomatising NF$_3$. Bear with me.

[snip]

But that's not what i am thinking about now. NF_3 proves the existence of plenty of closed set abstracts that use more than three levels. Take a banal example:. NF_3 proves the existence of $\{\{\{\{x\}\}\}\}$ for all x. However it does it in two stages, rather the way in which NF proves the existence of all $x \cup \{x\}$. It does rather seem to me that $\mathrm{NF}_3 \vdash (\forall x)(\{\{\{\{\emptyset\}\}\}\}$ exists) but that the obvious (indeed the sole apparent) proof has a maximal formula. It's not entirely clear, beco's the obvious proof contains complex terms such as '$\{\{x\}\}$ which strictly aren't part of the language.

Something to think about there.

NF_3 has no bound on the number of quantifiers allowed in the eigenformula of its comprehension axioms, so we need something that does projections inside three levels. Here's a start

> For all x^1 and y^2 there is a set z^1 of all those w^0 s.t. $\exists$ pair^1 $p = \{w, u\} \in y$ with $u \in x$.

or

> For all x^2 there is a set^2 of all those pairs^1 p s.t there is a triple^1 $t \in x^2$ with $p \subseteq t$.

and there are other things can we cay inside three types along these line. The definiability of the successor functions fites in here somehow.

Rosser points out somewhere that constants like '$\mathbb{N}$' can be given any type in a stratification. Definable things always can. But *local* version of things like $\mathbb{N}$ cannot, because they have an embedded variable. 28/iii/2025 Need to think – in this connection – about parametrised NFU, as Albert was saying

1.2 A factoid to be fitted in

Write $D(x)$ for x `XOR` $B(x)$. I think i can show that D has no finite cycles. That is to say, we can prove by meta-induction on n that

REMARK 1 $(\forall x)(D^n(x) \neq x)$.

Proof:

Start with $n = 1$. If $x = D(x)$ then $x = x$ `XOR` $B(x)$, which is clearly impossible since $B(x)$ is never empty.

What about $n = 2$? $D^2(x)$ is $(x$ `XOR` $B(x))$ `XOR` $B(x$ `XOR` $B(x))$.

If this is equal to x we would need $B(x)$ `XOR` $B(x$ `XOR` $B(x))$ to be empty. And $B(u)$ `XOR` $B(v)$ is never empty.

The same is going to work with larger n: $D^n(x)$ is x `XOR` (a complex boolean combination of Bs) and no such things are empty.

■

"$(\forall x)(D(x)$ exists)" is an unstratified $\forall_3$ sentence which, together with extensionality, has no finite models. It's a theorem of NF0, for what that's worth.

Does it give us an unstratified implementation of arithmetic into NF0? Well, yes! Take $0 = V$ and successor of x is $D(x)$. But of course it doesn't support induction.

We can't show that D is injective, sadly. After all, if $x = B^2(x)$ we have $D(x) = D(B(x))$, but – for what it's worth – the assertion that D is injective is universal-existential:

$$x \texttt{ XOR } B(x) = y \texttt{ XOR } B(y) \text{ iff } x \texttt{ XOR } y \texttt{ XOR } B(x) \texttt{ XOR } y = \emptyset$$

Is it consistent ?

Of course it's highly unstratified. But it is stratifiable-mod-2! Is that any use? No, beco's even in $\mathrm{TC_2T}$ we don't have comprehension for formulæ that are stratifiable-mod-2.

I can't now remember why i was interested in this operation.

1.3 Does every ordinal contain a wellordered partition?

Consider a wellordering $\langle X, <_X \rangle$. Consider what i used to call the *minimal extensional generator* – the **MEG** – of X: the set of atoms of the atomic boolean algebra generated by X. (I had a sister called Meg). You can also think of that algebra as the intersection of all power sets that $\supseteq X$. Call this set of atoms At. It's clearly a partition of $\bigcup X$. Is it wellordered? It might not be, for consider: Take a set Y, and look at $\mathcal{P}^2(Y)$, and the subset $B``Y$ of it. The set of atoms of $\mathcal{P}^2(Y)$ is of size $2^{|Y|}$, so merely being able to worder $Y$ and therefore $B``Y$ isn't enuff to wellorder that set of atoms.

But this has made me think about something that i should've sorted out years ago. Suppose i define a lexicographic order on the **MEG** by saying $a < b$ iff the $<_X$-minimal $x \in X$ that is a superset of one but not the other is a superset of a but not b. This is the lexicographic order of course. But how does one prove it is transitive? I confess i have never worked through a proof. But here is one.

Consider alements of *At three at a time*. a, b and c. Consider x the $<_X$-minimal element of X that contains some but not all. There are two cases to consider: (i) it contains one but not the other two; (ii) it contains two but not the third.

Case (i) it contains one but not the other two.

Without loss of generality it contains a not b or c. Then we have $a < b$ and $a < c$. Further analysis will tell us that $c < b$ or $b < c$ but we don't care which. Either way we do not get any counterexample to transitivity

Case (ii) it contains two but not the third.

Without loss of generality it contains a and b but not c. Then we have $a < c$ and $b < c$. Further analysis will tell us that $a < b$ or $b < a$ but we don't care which. Either way we do not get any counterexample to transitivity.

So we have established that, for all triples a, b and c, precisely one of the six possibilities $a < b \wedge b < c \wedge a < c \ldots$ holds. So – for example – if we have $a < b$ and $b < c$ then the only possibility of the six that accommodates this is $a < b \wedge b < c \wedge a < c$ so we infer $a < c$, as required by transitivity.

1.4 A Question from Zuhair

He asks: can there be a wellfounded set the same size as its power set?

(I take 'wellfounded' to mean 'belongs to every set that $\supseteq$ its own power set.)

I am looking for the answer to be 'no', so suppose X is such a set, and π a map between X and $\mathcal{P}(X)$. π can be extended to a permutation of the universe – and this might be a fruitful move – but its restriction to X is a wellfounded set (of ordered pairs) and is code-able (somehow) in $\mathcal{P}(X)$.

Clearly we are going to get a model $\mathfrak{M}$ of NF with carrier set X and membership relation $\in \cdot \pi$. The fact that π is a wellfounded set of pairs means that it appears in $\mathfrak{M}$ somehow. The strategy from this point is to show that the membership relation $\in$ of the original model (or rather its restriction to the transitive closure of X – or something!!) is encoded by a definable set of $\mathfrak{M}$. This relies on $\mathfrak{M}$ being able to see the whole of the transitive closure of X, and i'm not seeing this clearly enuff at the moment. Then we invoke Bowler-Forster JSL 2025, where it is shown that there is no definable wellfounded extensional relation on V. The impossibility doesn't flow from the relation being a set, but from it being definable. The point is that any wellfounded extensional relation is rigid, so one can prove by induction on it that there are no nontrivial $\in$-automorphisms. But (Bowler-Forster) every model of NF has a permutation model which has a nontrivial $\in$-automorphism of order 2 that is a set of the model.

1.5 Strongly Cantorian

Remark 2 *If all ordinals below α are cantorian then α is strongly cantorian.*

Proof:

If $\beta = T\beta$ for all $\beta < \alpha$ then the set of ordinals below α is a strongly cantorian set and the order type of its wellordering-by-magnitude is strongly cantorian. By Cantor's counting principle the ordinals below α are a wellordering of otype $T^2\alpha$. So $T^2\alpha$ is strongly cantorian, so α is strongly cantorian.

■

Notice that this argument needs *all* ordinals below α to be cantorian; α merely being a sup of cantorian ordinals is not enough ... tho' it does give us *something*.

Remark 3 *A sup of cantorian ordinals is cantorian.*

Proof:

Suppose X is a set of cantorian ordinals – in fact suppose merely that X is closed under both T and T^{-1}. Then the set of upper bounds to X is closed under both T and T^{-1}, so its least member must be fixed by T – *i.e.,* be cantorian.

■

REMARK 4 *A sup of strongly cantorian ordinals is strongly cantorian.*

Proof:

Reflect that every subset of a strongly cantorian set is strongly cantorian, so any ordinal below a strongly cantorian ordinal is strongly cantorian. Let's prove it. Suppose $\langle A, <_A \rangle$ is a wellordering, and $\iota \restriction A$ exists, so that the order type α of $\langle A, <_A \rangle$ is strongly cantorian. Let β be an ordinal $< \alpha$. Then there is $B \subseteq A$ with $\langle B, <_A \restriction B \rangle$ of order type β. This, too, is a wellordering, and $\iota \restriction B$ exists by separation, so β is strongly cantorian as desired.

So if X is a set of strongly cantorian ordinals then the set $\{\beta : (\exists \alpha \in X)(\beta \leq \alpha)\}$ is an initial segment of NO consisting entirely of strongly cantorian ordinals so its sup must be strongly cantorian too, by remark 2.

■

So a wellordering all of whose proper initial segments are strongly cantorian is itself strongly cantorian.

1.5.1 SCU and variants thereof

SCU says that

A union of a strongly cantorian family of strongly cantorian sets is strongly cantorian.

How about

S1 "A nested union of stcan sets is stcan"

S2 "A directed union of stcan sets is stcan"

How strong are they? One has to assume that the indexing family is at least a set o/w one could take the directed family of strongly cantorian sets and get V, so this is all going on inside NF not ML.

If one is to avoid contradiction we also have to require the family to be strongly cantorian, at least if we have counting.

AxCount refutes S2! Consider the set of finite sets. AxCount implies that it's a directed family of strongly cantorian sets, but its sumset is V which is not strongly cantorian.

So we have to repurpose the name 'S2' for

S2′ 'The sumset of a stcan directed family of stcan sets is stcan'.

On the face of it S2′is weaker than SCU.

Let $\mathcal{F}$ be a strongly cantorian family of strongly cantorian sets. A finite union of strongly cantorian sets of strongly cantorian sets is strongly cantorian. (This needs AxCount). So close $\mathcal{F}$ under finite unions. The result is a strongly cantorian directed family of strongly antorian sets. So by S2′ its sumset is strongly cantorian.

So S2′ plus AxCount implies SCU.

This doesn't seem to mean that AxCount refutes S1!!

Then AxCount + S2′ implies SCU.

Here we reach for the fact that the sup of a family of stcan ordinals is stcan. Suppose i have a chain of stcan sets under $\subseteq$. Is there a wellordered cofinal subchain? This presumably needs choice. If there is i can argue that its union is stcan. So it seems that we can prove that the union of a (wellorderable) chain of stcan sets under $\subseteq$ is stcan.

So we really need to weaken S1 and S2′ by requiring that the family be strongly cantorian. Then the new versions are obvious consequences of SCU and we can sensibly ask if either of them implies SCU.

What about a nested union of strongly cantorian sets? One thing one can say is that every proper initial segment of the index set I is strongly cantorian. For $i \in I$ consider $\{j : j < i\}$. The map $j \mapsto X_j$ injects $\{j : j < i\} \hookrightarrow \mathcal{P}(X_i)$, and $\mathcal{P}(X_i)$ is strongly cantorian beco's X_i is – whence $\{j : j < i\}$, too, is strongly cantorian. So the index set is a total ordering every proper initial segment of which is strongly cantorian. Doesn't seem to be quite enough to show that it is strongly cantorian itself.

Observe that not every set can be the union of a nested family of strongly antorian sets. Consider a wellordered noncantorian set. If it is a union of a chain of sets then every element of the chain is wellordered. But then the cardinality of the union is the sup of a set of cantorian alephs and so must be cantorian.

1.6 Some Arithmetic

Randall and i are corresponding about the arithmetic of strongly cantorian naturals in NF. It's better than ordinary arithmetic beco's it's easy to show that there is no last strongly cantorian natural.

What about the arithmetic of natural numbers that are cardinal numbers of hereditarily finite sets? Suppose there is a last such natural. It's the cardinal of a hereditarily finite set x. Now we must have $x \in x$ lest $x \cup \{x\}$ be a bigger hereditarily finite set. But then $x = \mathcal{P}(x)$ giving a model of NF + ¬Infty which of course is impossible. So there is no largest such natural.

Are these two classes of naturals the same?

Well, it's easy to cook up a permutation model in which every stcan natural contains a von Neumann ordinal, and vN ordinals are hereditarily finite, so that's a start. That raises the question of whether the arithmetic of stcan naturals is the same as the arithmetic of vN naturals. Presumably it is. But the class of natural numbers of hereditarily finite sets might be bigger.

So there are (so far) four implementations of arithemtic (with no last element)
strongly cantorian naturals
von neumann naturals
cardinals of hereditarily finite sets
numbers arising from hereditarily finite sets *via* Ackermann

Zach asks: is every stratifiable theorem of ZF(C) provable from stratified collection + IO?

I now think i can prove, using stratified collection and IO, that ω_α exists for every α, by which i mean that for every wellordering there is another wellordering isomorphic to it which is a set of wellorderings wellordered by cardinality. So it's not the counterexample we would have liked...

You prove it by wellfounded induction on the class of wellorderings ordered by end-extension. At limit stages, you have a wellordering W, and you form the set of all its initial segments, and each of those has isomorphic copies as above. You use collection to obtain a family of such copies. The point then is that (since given any two worders one is isomorphic to a unique initial segment of the other) there is a canonical way of turning that family of wellorderings into a directed system, and you can then form the direct limit, using IO.

Randall sez that the principle that every set that injects into $\iota^n``V$ for all n is cantorian is strong. He sez: consider the BFEXTs and the copy of L therein, and the model of NFU+AC that it gives you. That model of NFU believes the principle, and it also believes choice, which means - apparantly - that it believes Henson's axiom CS and that - apparently - implies the existence of n-Mahlo cardinalss for every concrete n. So the principle has at least that much consistency strength.

This principle is a kind of dichotomy. It says that every set is either v well behaved (cantorian) or very badly behaved (big enuff to be $\not\leq |\iota^n``V|$ for some n). Dichotomy principles are consistent with weak theories like NF_2. They say that there are no intermediate sets. And let us not forget that - as Church says - the paradoxical sets are all intermediate. Church-Oswald constructions give us models of dichotomy principles, and they also give us synonymy. The NF dichotomy principles are *Beschränkheitsaxiome*

I used to say that one important difference between Quine atoms and empty atoms is that distinct Quine atoms have distinct complements. Randall sez that that isn't the point, the point is rather than every set of Quine atoms is strongly cantorian. It now seems to me that one can put it like this: if your point of departure is NFU, so you are doing NF with atoms ("atoms NOS[1]" as it were) and you then require that your atoms be Quine atoms then this has considerable consistency strength, since Quine atoms are extensional so you get the whole of NF! So if you want there to be a difference between NF and NFU you'd better go for empty atoms.

I now think i have some sort of understanding of this situation. Suppose you

[1] `https://en.wikipedia.org/wiki/Not_otherwise_specified`

are worling in a set theory whose name has a suffix 'U' - ZFU etc. You want to turn a model of this theory into a model with no *urelemente* so as to restore extensionality. First thought: turn the (empty) *urelemente* into Quine atoms. How do you do that? Well, you add ordered pairs to the membership relation of the model so that each *urelement* becomes its own singleton. Sadly this doesn't work beco's the resulting model is not extensional: if u was a *urelement* then in the new model u and $\{u\}$ have the same extension but are distinct. However what you can do is a kind of Rieger-Bernays construction: let i be the function that sends every *urelement* u to $\{u\}$ and sends $\{u\}$ to $\{\{u\}\}$ and so on, and fixes everything else. Then the new membership relation ϵ is given by $x \epsilon y \longleftrightarrow x \in i(y)$. The new model is extensional, he says defiantly. (Should supply details). If we do this in NFU we experience obstacles. The collection of *urelemente* is a set (its membership condition is stratified) However when we turn the set of *urelemente* into a set of Quine atoms we find the the set is strongly cantorian in the new model, which means it must have been strongly cantorian in the original model – beco's R-B constructions preserve strong cantorianness. (This isn't exactly an R-B construction but we hope that won't matter).

x is finite if every y s.t. $x \in y$ and y closed under subcision contains $\emptyset$. So "Every set is finite" is "Every set closed under subcision contains $\emptyset$".

$$(\forall x)((\forall y \in x)(\forall z)(y \setminus \{z\} \in x) \to \emptyset \in x)$$

How many quantifiers?

$y \setminus \{z\} \in x$ is both $(\forall w)((\forall u)(u \in w \longleftrightarrow u \in y \wedge u \neq z) \to w \in x)$
which is $\forall\exists$, and
$(\exists w)((\forall u)(u \in w \longleftrightarrow u \in y \wedge u \neq z) \wedge w \in x)$
which is $\exists\forall$, and the first formulation is more useful to us, co's it makes the conditional $\exists^3\forall$ making the whole thing $\forall\exists^3\forall$.

Are there formulations of Infty or $\neg$Infty with different quantifier prefixes?

Try this: there is a set x with $\emptyset \notin x$ but whenever $z \in y \in x$ there is $w \in y$ with $y \setminus \{w\} \in x$

this is
$(\exists X)((\forall v \in x)(\exists w)(w \in v) \wedge \forall w \in y \in X)(\exists w \in y)(y \setminus \{w\} \in X)$
which has even more quantifiers.

The fact that acyclic comprehension turns out to be the same as stratified comprehension underlines how natural a notion stratification is. You tweak the definition slightly and you get the same thing. Rather like the fact that general recursive functions, λ-representable functions and functions computable by register machines all turn out to be the same is telling us that we've got the concept right ... tho' those aren't minor tweaks so much as different approaches.

Can we show that the transitive closure of a wellfounded set is wellfounded if it exists? It should be possible to do that

Would i be correct in thinking that Pabion's result can be expressed by saying that every countable model of TST_3 can be expanded to a model of TTT with three levels? Or is it the assertion that every saturated model of TST_2 (every infinite element can be split into two infinite elements) can be expanded to a model of TST_3?

Really should get this straight!

And what about TST_4? Presumably any countable model of TST_4 (which has four levels, $0, 1, 2, 3$) can be expanded by adding a membership relation between levels 0 and 3? (It costs nothing). However adding a membership relation between levels 0 and 2, or between levels 1 and 3 (let alone both!) presumably needs strong assumptions. Worth getting straight.

How many levels do we have to postulate for a model of TTT for the postulate to be as strong as the consistency of NF..?

Randall sez: "Infinitely many. TTT proves the consistency of TTT_n for each n.

It isn't analogous to the situation with ambiguity."

Randall sez that all his models of TTT are β models. This is possible only beco's the natural ordering on the levels is a wellordering; any model of TTT that had a reduct of a substructure that is a model of T$\mathbb{Z}$T cannot be a β-model beco's of the Ω at each level.

A conversation with Peter Lumsdaine.in january 2025

Something Peter sez prompts the question whether or not Nathan's proof that jc is universal works also for any $\sigma \in C(J_1)$. (I've now checked it, and the answer is that it doesn't. However it has made me think about what use one makes of the injectivity of an embedding-of-involution. . .)

Peter sez that you can erect TTT on any relation whatever, even a multigraph – with multiple edges! That way one can encompass TCT! TTT on a Quine atom is NF!

A message from Jonathan Kirby 24/ii/25

Dear Dan, Thomas,

Unfortunately the timing for tomorrow's talk doesn't work for me so I won't be there even virtually. I have read the notes and have one question which maybe you, Thomas, could answer:

Dear Peter and Jonathan,

This message is prompted by an interesting suggestion you have both made to me in recent weeks.

This is what Jonathan wrote:

"Is it known whether ZF and NF are bi-interpretable, or mutually interpretable? Or indeed some variation? I ask because another reasonable foundational theory of sets is Lawvere's ETCS, which is equiconsistent with ZFC-Replacement but not bi-interpretable. They are mutually interpretable and if you start with a model of ZFC $\setminus$ Replacement, interpret the model of ETCS (the elementary theory of the category of sets) and then in that interpret a model of

ZFC-R (a set is a subset of a transitive set considered as a special sort of tree) then what you get is isomorphic to what you start with. But if you start from a model of ETCS and interpret twice, what you end up with is equivalent to the category you started with, but not necessarily isomorphic. And this distinction indicates that ETCS is strictly more expressive than ZFC $\setminus$ Replacement, and indeed in what way.

> (The lack of replacement is not important. One can add replacement to both theories, or any other axioms, and the interpretations work the same way.)

So, although I don't really have any feel for NF, it seems a reasonable question to ask about mutual interpretability and bi-interpretability, since they are somewhat stronger results than equiconsistency."

A first – quibbling – response will be that NF is presumably not bi-interpretable with ZF(C) beco's ZF(C) is much stronger than NF (as far as we know). So you want to substitute 'T' for 'ZF(C)' in the above, where T is some fragment of ZF(C) equiconsistent with NF. The question thus becomes:

"Is there a natural fragment of ZF(C) that is bi-interpretable with NF?"

which i am taking to be your question. It's a fairly pointed question because if we take T to have foundation then we know that it can't be synonymous with ZF(C).

In my notes of a conversation with Peter Lumsdaine i find:

"Two theories S and T are *bi-interpretable* iff every model of S houses a model of T and every model of T houses a model of S – probably in lots of ways – but certainly so that we have freedom to choose these models so that every model $\mathfrak{M}_1$ of S houses a model $\mathfrak{M}_2$ of T and M_2 (being a model of T) houses a model of S which is isomorphic to M_1 – seen from outside. And presumably it's the same – *mutatis mutandis* – starting from a model of T instead of a model of S. I don't think there is any suggestion that $\mathfrak{M}_1$ and $\mathfrak{M}_2$ have to be sets of the models they inhabit, and the claimed isomorphism is not required to be visible except in the metatheory."

Peter's example of a pair of bi-interpretable theories is PA and the theory of hereditarily finite sets. For one direction you use the Ackermann bijection and for the other you use von Newmann Naturals (or Zermelo naturals . . . the point is you don't use Ackermann)

Look at "when bi-interpretability implies synonymy" by Albertus Magnus available at: `https://dspace.library.uu.nl/handle/1874/308486`

Peter's challenge is . . . Can we exploit the argument using automorphisms to show that NF is not even *bi-interpretable* with any ZF-like theory?

[What does Peter mean by "the theory of hereditarily finite sets"? Isn't this just the theory i have been calling KWo, namely ZF $\setminus$ infty + $\neg\infty$? – which is the theory that Kaye-Wong prove is actually *synonymous* with PA (beco's of the Ackermann bijection)? Or is his point that the two interpretations he mentions are two interpretations that make the two theories bi-interpretable? (They are synonymous, as it happens, but not in virtue of those two interepretations) Is that what you mean, Peter . . . ?]

Jonathan says:

"Here is a paper by my former student which explains carefully what bi-interpretation means, albeit in the slightly more general context of coherent theories.

`https://link.springer.com/article/10.1007/s00153-022-00825-7`

Your gloss has the right gist, but is not precisely correct. What is wrong is 1) you don't define housing, and 2) you assert that some things exist, but there have to be some uniformities in their definition. Essentially things have to be functorial and you need naturality of the isomorphisms in an appropriate category (of theories and interpretations). There is a big gap between mutual interpretation and bi-interpretation which your definition is not precise enough to see.

A reasonable place to start would be to take a model of NF and construct its category of sets and functions. Suppose you start with two models of NF and get isomorphic categories. Does that mean your original models were isomorphic? And indeed if there are automorphisms of the model of NF, are they visible in the category?

Best, Jonathan

Suppose I split V into pairs, so $\mathbb{P}$ is a partition of V into pairs. How many sets are there that are unions of subsets of $\mathbb{P}$? There ought to be $|V|$ of them.

Check the finite axiomatisability of *i*NF.

Changes to SEP
APG's NDJFL 2012
"Again you say... refer to NFU book

For a detailed pædogogical treatment of stratification
There now follows a discussion of the stratification of
'$(j\tau)^{Tn}(x) = j(\tau^n)(x)$'

> (i) '$x = \tau(y)$' is stratified with 'x' and 'y' having the same type, which is some fixed number (can be taken to be 1 but we have to stick to whatever we decide) greater than the type of 'x' and 'y'. The two variables 'x' and 'y' have to have the same type for reasons which we explain elsewhere in this document. Let κ be the type-difference `type-of`('τ') `-` `type-of`('x'). The precise value of κ depends on our implementation of ordered pair and is at least 1. It can be taken to be 1 but it's important that nothing depends on this fact, so we won't assume it.

(ii) '$\sigma = \tau^n$' is stratified with 'σ' and 'τ' having the same type. The type of 'n' is ...some fixed number greater than the type of 'σ' and 'τ'. Let's call this type difference ν; ν can probably be taken to be 1 but – again – it's important that nothing depends on this fact, so we won't assume it, and we have to stick to whatever we decide.

(iii) '$\sigma = j\tau$' is stratified with 'σ' having a type one higher than τ'.

By the above

$w = (j\tau)^{Tn}(x)$ is stratified with 'w' and 'x' having the same type – call it 0 to keep things simple. '$(j\tau)^{Tn}$ now has type κ. By (i) we can infer that '$j(\tau)$', too, has type κ, and – by (ii) – that 'Tn' has type $\kappa + \nu$.

Now we consider '$w = j(\tau^n)(x)$'. By (ii) 'x' must have the same type as 'w', namely 0. Then, by (i), '$j(\tau^n)$' must have type κ. So – by (iii) – τ^n must have type $\kappa - 1$. So (by (ii)) 'n' must have type $\kappa + \nu - 1$, and 'Tn' will have type $\kappa + \nu$.

Thus '$(\forall n \in \mathbb{N})((j\tau)^{Tn}(x) = j(\tau^n)(x))$' is stratified, with:
`type-of`('n') $= \kappa + \nu - 1$;
`type-of`('x') $= 0$;
`type-of`('τ') $= \kappa - 1$

Easy to show that $\langle X, R\rangle$ is a wombat (as it might be, a wellordering) iff $\langle \iota``X, \texttt{RUSC}(R)\rangle$ is a wombat. This doesn't mean they are *isomorphic* wombats!

We know there are wellorderings of length $T^{-1}\Omega$, whose carrier sets are too big to be the size of any set of singletons. Can any of them be definable by a set abstract? If so, then there are stratifiable formulæ $\phi(-)$ and $\psi(-,-)$ such that $\{x : \phi(x)\}$ is of size $\aleph(|V|)$, $\psi(-,-)$ is homogeneous (so that the graph $\{\langle x, y\rangle : \psi(x, y)\}$ of ψ is a set) and wellorders $\{x : \phi(x)\}$ to otype $T^{-1}\Omega$. Now ψ is m-stratifiable for some m so its graph is fixed by the action of j^m"Symm(V) aka J_m. J_m acts as a group of automorphisms on the graph $\{\langle x, y\rangle : \psi(x, y)\}$ of ψ. Now the graph $\{\langle x, y\rangle : \psi(x, y)\}$ of ψ is a wellordering and is therfore rigid, so every permutation in J_m fixes every member of the graph $\{\langle x, y\rangle : \psi(x, y)\}$ of ψ. But this is simply to say that everything in the domain of ψ (aka $\{x : \phi(x)\}$) is m-symmetric. This is a Key Fact!

This gives us a taste of the kind of situation that has to happen but (we hope) can't. Can we really find as many as $\aleph(|\iota``V|)$ m symmetric sets?

It would be nice to be able to argue that $\{x : \phi(x)\}$ is a family of pairwise disjoint sets, but that doesn't follow from them all being m-symmetric. We would need them to be m-equivalence classes, which they aren't – just unions of such.

$\{x : \phi(x)\}$ generates a complete boolean algebra, and the atoms of this CBA are pairwise disjoint and each atom is a union of m-equivalence classes. There can't be too many of these equivalence classes so the partition can't be too large. Call this set of atoms P. We have $2^{|P|}$ exists but is $\not\leq T|V|$.

The partition admits a definable total order arising from its nature as morally the power set of the wellordered set $\{x : \phi(x)\}$. Now think about how J_m acts on this total order of P. Every permutation in J_m induces an automorphism of the total order, so there is a homomorphism from J_m to the group of automorphisms of the total order. The automorphism group for a total order is torsion-free. So what can this homomorphism possibly be? It must kill every torsion element, but – by Bowler-Forster – every permutation is a product of involutions so that means it must be the zero map. So it must fix every member of P. So every member of P is m-symmetric...

But we probably knew that anyway, since every member of P is an intersection of m-symmetric sets. So this argument about automorphism of total orders doesn't seem to give us anything new. Perhaps i'm just not deploying it properly.

Well, one thing the argument about automorphism of total orders does show is that if $\langle X, \leq_X\rangle$ is a m-symmetric total order then every member of x is m-symmetric. But all that's going to do is show that there is no symmetric total order on V – which we knew anyway.

So we are looking for theorems that say that a set of k-symmetric sets cannot be too large. As far as we know a collection of arbitrary symmetric sets can be as big as you please – there doesn't seem to be anything to say that V cannot be a set of (all) symmetric sets – but the hope is that if you bound the degree of symmetry then you get something.

We are interested in formulæ of $\mathcal{L}(TTT)$ which are expressible in extracted models of TSTU. Information couched in a form that requires us to use both the predicates $\in_{i,j}$ and $\in_{i,k}$ with $i \neq k$ gets lost in the passage to an extracted model of TSTU. We will need a special adjective for expressions Φ that respect the constraint that ...

If '$\in_{i,j}$' appears in Φ then, for $i \neq i'$ and $j \neq j'$ neither '$\in_{i',j}$' nor '$\in_{i,j'}$' appear in Φ.

Altering our choice of f for the extractions will not affect the truth value of sentences that bear this adjective.

I think the formulae that are well-behaved in this sense are simply those that are stratified if you do it Holmes' way and have only one membership relation for the whole of TTT

Theorem 1

$$(\forall xy)(|x| = |y| \longleftrightarrow (\exists x_1, x_2, y_1, y_2, z_1, z_2)\bigwedge\begin{pmatrix} x = x_1 \sqcup x_2 \\ y = y_1 \sqcup y_2 \\ x_1 \approx z_1 \approx y_1 \\ x_2 \approx z_2 \approx y_2 \end{pmatrix}$$

Proof: .

The right-hand side is simply the assertion that x and y are J_0-equidecomposable with two pieces, with '$x_i \sim_1 y_i$' replaced by their equivalents using the preceding remarks. ■

That is to say '$|x| = |y|$' is equivalent (assuming GC) to a 3-stratified 2-formula. We have to be cautious in drawing conclusions about the existence of (Frege/Russell−Whitehead) cardinals in $NF_3 + GC$ since some of the proofs above may not be reproducible in $NF_3 + GC$. Although the (Frege/Russell−Whitehead) 0, 1, 2, . . . are all sets in NF_3, in general cardinal numbers do not seem to be provably sets in NF_3. It suggests, curiously, that the addition of a small amount of choice (GC) to NF_3 may make it much easier to conduct cardinal arithmetic.

Not sure whence came *that!*

One has to emphasise that induction over a wellfounded relation works in NF for stratified formulæ only. However one does not need *the wellfounded relation over which we are performing the induction* to be (captured by a) stratified (expression). Its graph doesn't have to be a set; all that is needed for it to support induction is for it to be wellfounded. Mathematical induction and transfinite induction over th ordinals are inductions over homogeneous relation, but we can (and do) perform induction on stratified inhomogeneous relations, like $\in$ and $<^T$.

1.7 SCUM

Isn't `SCUM` exactly what we need to prove that the strongly cantorian sets form a category? No! It's provable outright that a composition of stcan maps is stcan. Let f and g be strongly cantorian maps: they are sets of ordered pairs, and their domains are strongly cantorian. Any surjective image of a stcan sets is a stcan set, so the two graphs of f and g are stcan. The (graph of) $f \cdot g$ is a surjective image of (the set, the cartesian product) $f \times g$,. This last is a cartesian product of two stcan sets and is theefore stcan, as are all its quotients.

Thinking about `SCUM` (a stcan union of stcan sets is stcan) while lying awake in the middle of the night. It seems to be exactly what one needs to prove that a sup of a stcan family of stcan ordinals is stcan; but it then occurred to me that that last assertion is provable anyway.

So: what do we know about can and stcan ordinals?

Every ordinal below a stcan ordinal is stcan
If the ordinals below α are closed under both T and T^{-1} then $\alpha = T\alpha$.
If every $\beta < \alpha$ is stcan then so is α.
The sup of an increasing family of can ordinals is cantorian
BAD JOIN

Is the sup of a set of stcan ordinals stcan? Let X' be a set of stcan ordinals. Close downwards so that we now have an initial segment X of `NO` consisting entirely of stcan ordinals. This X cannot be `NO` so there is a least ordinal not in it. Call this ordinal $T^2\alpha$ (This T^2 is OK beco's we know there are suff many ordinals above our heads). α is $\mathtt{otype}(\langle X, <_{\mathtt{NO}}\rangle)$. But now – or so it seems to me – X is strongly cantorian, as follows. If $\beta \in X$ then $T\beta = \beta$ so we can get the function $\beta \mapsto \{T^{-1}\beta\} \mapsto \{\beta\}$. (Notice that for our purposes it is sufficient that $\beta = T\beta$, we don't need β to be stcan!)

Have i made a mistake here? Or have i misremembered your result..?

Any two levels of a model of T$\mathbb{Z}$T have the same arithmetic. But different copies of Z might give you different arithmetic. So there isn't a good notion of "The arithmetic of $\mathfrak{M}$" where $\mathfrak{M}$ is a model of T$\mathbb{Z}$T co's it could be a disjoint union of such models!! I s'pose it cld be the intersection of all the arithmetics of each $\mathbb{Z}$...

Might there be a simpler proof of AxInf in NF? It would be an odd thing, beco's it would have to exploit extensionality. Two useful facts ...

If every finite set is the same size as a set of naturals then every finite set is the same size as a set of singletons

If every finite set is the same size as a set of singletons then V is infinite.

So let's try proving by induction that every finite set is the same size as a set of naturals. We want to prove that if n contains a set of naturals then so does $n + 1$. If n contains a set of naturals then it comtains a set of singletons. So $T^{-1}n$ exists. That sounds promising but it probably doesn't do anything.

Naturals are equipollence classes of finite sets. They are pairwise disjoint. Every finite set that is the same size as a set of naturals is the same size as a set of singletons. (That's AC for finite sets) V is not the same size as any set of singletons. (That's Cantor's theorem, NF-style). So V is not the same size as any finite set of natural numbers. If V is the same size as a subset of $\mathbb{N}$ then by Cantor-Bernstein it is the same size as $\mathbb{N}$. So can V be the same size as $\mathbb{N}$? If it is the same size as a proper subset of itself then AxInf follows as desired. So we consider the other horn: $|V| > |\mathbb{N}|$. We want it to imply the axiom of infinity. Presumably not, beco's all this can be done in NFU.

Ivan Vorobiev `<ivanexplay2000@gmail.com>` writes on 5/vii/24 ...

There is some result that made me think about TC$_2$T again, namely the paper "Periodicity in the cumulative hierarchy" by Gabriel Goldberg and Farmer Schlutzenberg: `https://arxiv.org/abs/2006.01103`. In this paper they demonstrate the 2-periodicity of the high floors of the cumulative hierarchy in the absence of the axiom of choice in the presence of a Reinhardt cardinal in ZF. It became interesting, if we take $\mathcal{P}^n(V_\alpha)$ as a model for TST, where α is the Reinhardt cardinal and choose only 2-stratified formulas about it, what would be the result? As before, the question is motivated by the fact that one wants to study "small" and "large" sets in the same way. And as seen in "Categories with New Foundations" NF handles this not badly, but it seems that [reinforcements of] TC$_2$T can give a more comprehensive picture.

Zachiri sez that Π_n collection plus Π_{n+1} foundation implies Σ_{n+1} separation.

I have been thinking about a construction of Crabbé's that Randall showed me. Crabbé shows how to interpret NFU in SF. This is like the CUS situation in the following sense:

> We have a theory T_1 and a construction $\mathcal{C}$ which, whenever it is applied to a model of T_1, gives a model of T_2. We seek *Beschränkheitsaxiome* $\mathcal{B}$ to add to T_2 so that T_1 and $T_2 + \mathcal{B}$ are synonymous.

Can this always be done? Tim has managed to do it where T_1 is ZF (or anything like it) and T_2 is CUS (or something like it). My guess would be that it requires special conditions. Can we say anything about what those special conditions are? In particular what happens if T_1 is SF and $\mathcal{C}$ is Crabbé's construction?

Randall sez: Crabbé's construction probably doesn't witness synonymy because it destroys information. Objects which are coextensional in SF are simply identified in NFU, and the information about the extensions of objects in SF whose extensions are not unions of equivalence classes under coextensionality is lost.

Zuhair has this idea for a different stratification algorithm. The idea, as usual, is to decorate every variable with a natural number. You give all occurrences of any one variable to same natural number of course. And if you find the subformula $x = y$ you decorate 'x' and 'y' with the same natural number. The difference comes with $x \in y$. If you have decorated 'x' but not 'y' you give 'y' a decoration that is bigger than the decoration given to 'x' ... and analogously if you have decorated 'y' but not yet 'x'.

He posts this in `https://mathoverflow.net/questions/437961/is-stratified-sorted-rendering-of-naive-set-theory-equivalent-to-tangled-type-th` Zuhair thinks it is something to do with Tangled type theory but it's nothing of the sort; it clearly identifies as stratifiable exactly the same sentences as were identified as stratifiable under the old regime.

1.8 A question for Randall

Consider the following restriction/fragment of TTT. Suppose (to keep things simple) your levels are indexed by Z, the integers. You have a relation $\in_{i,j}$ whenever $j = i + 1$ or $i + 2$, but no others. (Plus the usual conditions that the extracted models are moels of TZT etc etc.

One can also consider the weakening which has $\in_{i,j}$ where j is $i + 1$. $i + 2$ or $i + 3$. And so on.

Randall sez that skipping one type is as good as skipping lots.

One wonders what happens constructively.

A conversation with Randall 27/xii/23

Randall, too, has noticed that the indices that decorate variables in TZT aren't numbers. cf stratificationmodn. We need to find the correct things to say about this.

Randall sez: take the one-sorted version of set theory. Add a predicate that says "the equivalence class of y is higher than the equivalence class of x" and axioms to say it is a strict order. That way you can prove that it has no loops. This makes the theory finax!

Think about how to expunge unstratified separation in the one-sorted version of type theory.

Somewhere in these notes i point out that there is a choice function on the set of cofinite sets. But there can't be a choice sunction on $|V|$.

$\{y \in x : y \notin y\} \notin x$. Now substitute $(V \setminus x)/x]$
$\{y \in (V \setminus x) : y \notin y\} \notin (V \setminus x)$. In other words:
$\{y \notin x : y \notin y\} \in x$. Or
$\{y : y \notin (x \cup y)\} \in x$.
But this is a global choice function!
It might help to see a proof in na¨ive set theory.
Can we use this to get a global choice function on V_ω?
$\{y \in x : y \notin y\} \notin x$. Now substitute $(V_\omega \setminus x)/x]$
$\{y \in (V_\omega \setminus x) : y \notin y\} \notin (V_\omega \setminus x)$.
$\{y \in (V_\omega \setminus x) : y \notin y\} \notin (V_\omega \setminus x)$.
All this shows is that $\{y \in (V_\omega \setminus x) : y \notin y\} \notin V_\omega$.

Randall points out that in NFU you can't identify atoms with Quine atoms beco's of Cantor's theorem. There are fewer Quine atoms than sets beco's of Cantor's theorem

Which subsystems of NF give easy relative consistency of duality?

Prove NF_3 is not finitely axiomatisable.

1.9 An article on matters arising from the third edition?

pseudo.tex; pairsntuff.tex; SCU; axiom of infinity and Amb(arith)

small-scale open problems. Does parameter-free NF prove infinity?

Notice that the set NO does not have an easy inductive definition. It's a quotient of an inductively defined set but isn't easily inductively defined itself.

1.10 Fagin and Ambiguity, a false Start

Does Fagin's 0-1 theorem, in connection with my result that every countable model of TST is a direct limit of finitely generated models of TST, have anything to tell us about ambiguity? Possibly with the cofinite quantifier..?

Fix $k \in \mathbb{N}$, and consider the countable family of finite structures that are models of TST_k where the cardinality of the bottom level is a beth number. Apply Fagin to this.

Fagin allows us to choose between ϕ and $\neg\phi$. This appears to give us a complete extension of T$\mathbb{Z}$T+ Ambiguity AC – and that cannot be right.

The notion of synonymy that i'm happy with is one where theories T_1 and T_2 are synonymous if any model of one can be turned into a model of the other by defining new predicates, and in such a way that the composition of the two interpretations is the identity up to logical equivalence. (e.g., the two theories of partial order and strict partial order, or boolean algebras and boolean rings). The theorem i'm looking for will state that: if $\mathcal{K}$ is a CO-like construction then ZF is synonymous with the theory of models obtained from models of ZF by whacking them with $\mathcal{K}$.

Consider the basic CO construction over a model of ZF, that just gives every set a complement. The challenge is to show how to recover a model of ZF from a suitable model of NF_2. If we can do that then we get a synonymy result. (It's worth pointing out at this stage that on the face of it there is no reason to suppose that, for a given CO-like construction, the set of models obtained from models of ZF by means of it is an axiomatisable class. There may be some such general argument – and my conjecture that CO theories in general are synonymous with ZF relies on there being one – but that sounds like a tricky thing to prove that requires much thought.)

Suppose $\mathfrak{M}$ is a model that arises from a model of ZF by means of the basic CO construction. Every set is low or co-low. We will assume that there is a bijection $k : V \longleftrightarrow \mathtt{low} \times \{0, 1\}$. (we will worry *later* about what 0 and 1 are, and what ordered pairs are; at the moment we have quite enuff on our plate as it is).

I think that to define the $\in_1$ relation that turns the carrier set of $\mathfrak{M}$ into a model of ZF we next say something like: $x \in_1 y$ iff either

- y is low and $x \in \mathtt{fst}(k(y))$ or
- y is co-low and $x \notin \mathtt{fst}(k(y))$.

I think that works; *something* like that should work, anyway[2]. Let's suppose it does, and think about what the fallout is. We needed the special assumption "everything is low or co-low". This assumption is true in every model of NF_2 obtained by the basic CO construction so (it seems to me) we are going to have to write it in from the very start. This axiom is of course a *Beschränkheits axiom*, and the way in which it crops up here tells us that there are always going to be *Beschränkheits axiome*, simply beco's the models created by the CO-like construction (whatever it be) are going to be special in uninteresting annoying ways.

So we are stuck with *Beschränkheits axiome*. Can we always express them in the language $\mathcal{L}(\in, =)$ of Set Theory? (Or at worst in that language expanded

[2]writing out a proper proof could be quite laborious but someone should do it

by adding pairing-and-unpairing ...) Or are we going to have to expand the language? In the simple case we are considering it's pretty clear that we can remain within $\mathcal{L}(\in, =)$ (with added pairing/unpairing). A set x is low iff $\iota``x$ exists. (If $x$ is low so is $\iota``x$; if x co-low then $\iota``x$ is intermediate – neither low nor co-low and is not a set.)

The reason i am slightly uncomfortable about this is that altho' – in this case – we have a cute formulation of the *Beschränkheits axiom*, it does seem rather *ad hoc* and doesn't seem to promise that any sensible CO-like construction will bring with it an obvious *Beschränkheits axiom*, and therefore it's not obvious that the theory of the models constructed by that sensible CO-like construction will be axiomatisable. And if it isn't axiomatisable it clearly can't be synonymous with ZF.

This – apparently banal – observation that a theory that is not axiomatisable cannot be synonymous with a theory that is axiomatisable doesn't seem to prevent nonaxiomatisable theories from being synonymous with other nonaxiomatisable theories. Is the theory of finite boolean algebras synonymous with the theory of finite boolean rings? Surely the answer is 'yes'? Suppose $\mathfrak{M}$ is a model for the theory of finite boolean algebras. We can turn it into a boolean ring. Is this boolean ring a model of the theory of finite boolean rings?

I think so. Any formula ϕ true in $\mathfrak{M}$ is true in any finite b.a. $\mathcal{B}$. So then the boolean ring $\mathcal{B}'$ corresponding to $\mathcal{B}$ believes the ring formula ϕ' corresponding to ϕ. But every finite boolean ring is obtained from a finite b.a. in this way. So every finite boolean ring believes ϕ'. Done

Does NFP refute choice? No. It's consistent with everything being countable (Can't prove any version of Cantor's theorem)

NFP. It proves infinity. Does it refute choice? Presumably not.

Notice that altho' NFP doesn't believe that every set has a sumset it *does* believe that every singleton has a sumset – simply beco's of transitivity of the model. Finite sets then have sumsets by induction.

Notice that the set of Kfinite sets exists by the axioms of NFP. Also the collection of sets that have sumsets is a set by the axioms of NFP. So we can prove in NFP that every finite set has a sumset.

If NF believes $\{x : \phi\}$ exists, then NFP believes $\{\iota^k(x) : \phi\}$ exists, for sufficiently large k. So NFP + Sumset = NF. But we don't actually need *all* sets to have sumsets, just all sets of singletons. Now what happens if, instead of adding that, you add "every set of singletons^k has a sumset^k"? As Randall says, *of course* you get NF, beco's if you want x from $\iota``x$ you apply your double sumset to $\iota^2``x$. Duh.

Question: If NF$\vdash s \in t$ for two closed terms s and t, does NFP prove $\iota^n(s) \in \iota^n``t$ for all suff large n? The worry is that this might give rise to an interpretation of NF into NFP which of course can't happen.

Let's think a bit about how one might prove this. NF might prove $s \in t \in u$ then one might want two different prefixes on t. We might want $\{s\} \in \iota``t$ and

$\iota``t \in j^2\iota(u)$. In fact what matters about the prefix is only how many types it raises by... free-associate to Generalised T-functions.

If NF $\vdash t$ exists where the parameters are k levels higher than the eigenvariable, then NFP will only prove the existence of $\iota^k``t$. But actually the prefix doesn't have to be specifically $j(\iota^k)$. The prefix can be a product of k things from $\langle j^n(\iota) : n \in \mathbb{N}\rangle$. Notice that the sequence starts with $j\iota$ not ι; this is beco's if $\{x\}$ exists so does x – by transitivity. However $\iota``x$ can exist without x existing. One thing that makes life easy is that if (according to NFP) $p(s)$ exists so does $p'(s)$ as long as p and p' are equivalent in the obvious sense. What is the obvious sense? Well $\bigcap^k p(s)$ had better be equal to $\bigcap^k p'(s)$ at the very least. (Of course neither of them might exist!)

I think we should help ourselves to the concept of a **prefix of height** k. It is a composition of k things from $\langle j^n(\iota) : n \in \mathbb{N}\rangle$ – the prefixes of height 1.

I think it's clear that NFP believes that, for all prefixes p of height k, x exists iff $p(x)$ does.

What is certainly true is that if NF thinks that $\{x : \phi\}$ exists, then NFP believes that $p(\{x : \phi\})$ exists, for any prefix p of suff great height.

So the idea is this, for any two closed terms s and t there is k such that if NF proves $s \in t$ then, for all prefixes of length k, NFP will prove $p(s) \in (jp)(t)$.

TNT is the theory of strictly negative types: levels indexed by the negative integers. Does every model of TNT have an upward extension? Obviously every model of TNTP has an upward extension. Consider the theory of all models of TNT. Is it axiomatisable? Isn't it just $\phi \to \phi^+$ for all ϕ?

1.11 TTT and *i*TTT

Tangled T$\mathbb{Z}$T has this funny substructure property. Starting with a model of Tangled T$\mathbb{Z}$T, if you discard some levels, and all the $\in$-relations associated with those levels, then you still have a model of Tangled T$\mathbb{Z}$T.

That makes it sound as if Tangled T$\mathbb{Z}$T should be a Π_1 theory in some language. It might be worth thinking about what that language might be like. Randall thinks of TTT as a theory in $\mathcal{L}(\in, =)$, with only one membership relation. If $\mathfrak{M} \models$ TTT we are interested in substructures that are closed under the relation "are of the same type". so presumably we want the language to contain that predicate.

God help us there is also a tangled version of TC$_n$T.

Any model of T$\mathbb{Z}$T has a canonical expansion (well, it's not quite an expansion but never mind) to a model of Tangled T$\mathbb{Z}$TU.

There is an injection from the set of models of T$\mathbb{Z}$T to the set of models of Tangled T$\mathbb{Z}$TU. It's an injection not a surjection. The same idea (the obvious expansion) gives a map from models of T$\mathbb{Z}$TU to models of Tangled T$\mathbb{Z}$TU. This map is not injective. Put it this way: there is an obvious map (the *expansion* map) from [the set of] models of T$\mathbb{Z}$TU to [the set of] models of Tangled T$\mathbb{Z}$TU. Its restriction to models of T$\mathbb{Z}$T is injective.

What happens if you try to backfill an arbitary model $\mathfrak{M}$ of T$\mathbb{Z}$TU? If $\mathfrak{M}$ arose as a result of extracting from a model of T$\mathbb{Z}$T you can recover that model. It all depends on whether the cardinality of each level is a precise beth number of the cardinal of the level immediately below it.

But suppose we start from the other end. Start with a model $\mathfrak{M}$ of Tangled T$\mathbb{Z}$TU. Throw away the tangles to obtain a model of T$\mathbb{Z}$TU. Then canonically expand. Do you get back to where you started? I think so: this time you do.

So i think T$\mathbb{Z}$TU and Tangled T$\mathbb{Z}$TU are synonymous. However T$\mathbb{Z}$T and Tangled T$\mathbb{Z}$TU are *not* synonymous.

Consider the class of models of T$\mathbb{Z}$TU that arise from extracting every second (or nth, *mutatis mutandis*) level from a model of T$\mathbb{Z}$T. The theory of these models is axiomatisable, and is synonymous with T$\mathbb{Z}$T.

Is it always possible to expand a model of TST$_4$ to a model of TTT$_4$? Randall sez no. In contrast it is always possible to expand a (countable) model of TST$_3$ to a model of TTT$_3$ – Grischinology. (There may be a connection here with Pabion's work). There is probably a set Σ of first-order sentences in $\mathcal{L}(TST_4)$ that one can add to TST$_4$ to get a theory all of whose countable models can be expanded to a model of TTT$_4$. Randall sez that Σ isn't as strong as NF and nor is he putting any money on its refuting AC.

Dear Randall,

I now think i understand this business of expanding models of TST to models of TTTP, or at least understand an initial segment of it. I still have post-COVID brain fog, so writing this out is actually part of my convalescence. Can i get you to cast a brief eye over it and tell me that i am not crazy?

A trivial but perhaps helpful observation is that we can represent the first part of what Jensen shows as a demonstration that every model of TST (T$\mathbb{Z}$T) has an expansion to a model of TTTU. (What we think of as Jensen's clever idea of using Ramsey theory is not actually *the* clever idea of the paper, but is rather the second of *two* clever ideas in that paper.) This (first) expansion-idea suggests a project of finding analogues for other theories TTTX.

There is one of these possibilities that has been intruiging me of late: TTTP. *Can every model of TST/T$\mathbb{Z}$T be expanded to a model of TTTP?* Randall suggests doing this by recursion on levels (so that, in the first instance, we are doing this to TST not T$\mathbb{Z}$T). He also suggests considering countable models of TST with the splitting property: *every (externally) infinite set of the model can be split into two (externally) infinite sets of the model* so that we can use Grischinology. So let's see how we might carry this out. The first case where there is anything to do is with a model of TST$_3$, where we somehow cobble together a bijection between level 0 and level 2. I don't think it matters how we do it, tho' we might change our mind about this later. At later stages we perform a recursion. Suppose we have turned the first n levels into an initial segment of a model of TTTP. What about the $n+1$th level? We need to define a membership relation between (sets of) level k ($k < n$) and (sets of) level $n+1$. By induction hypothesis we already have a membership relation between (sets of) level k and (sets of) level n. So it will suffice to compose that relation with a suitable bijection between level n and level $n+1$. Thus the task of expanding a model of TST to a model of TTTP boils down to the task of finding a suitable tsau.

A tsau? At this point i find i need to explain to myself why this isn't the same as the task of finding a model of TST with an *actual* tsau. We need a family of bijections $V_n \longleftrightarrow V_{n+1}$, so why isn't this a tsau? And why doesn't it imply Con(NF)? The answer is that there is no requirement that the bijections in our conjectured construction be setlike! Setlikeness would enforce typical ambiguity, which is much more than we want. What is necessary is that the image of a level-n set $\{x : \phi(x)\}$ in the bijection should be a set as long as ϕ is predicative. That's obviously a weaker condition than being setlike so we may be in with a chance.

If we want a membership relation between (elements of) level i and (elements of) level j with $i+1 < j$ then it will suffice to cook up a bijection between level $i+1$ and level j. This suffices beco's we have a membership relation between level i and level $i+1$ and we can compose it with the bijection between levels $i+1$ and j to get a membership relation between (elements of) level i and (elements of) level j. How do we get such a bijection??

What is wrong with the following? Assume our model of TST is countable. Every level is a countable atomic b.a. Let us assume it has the splitting property. This gives us a bijection. Is that bijection sufficient?

The thing that has been bothering me – and which i now think i might be getting a handle on – is this. Suppose every countable model of TST with infinite bottom level can be expanded to a model of TTTP. Reflect on the fact that TTTP proves the axiom of infinity: there is a Dedekind-infinite set. But there is no Dedekind-infinite set in the model we start with! I think the strain is taken by the added gadgets appearing in the expansion. In the expansion there are sets believed by the expansion to be infinite, but that infinitude can be expressed only by means of the new relations. So there is no contradiction.

So the challenge is to perform the recursion outlined above. This will need a refinement of Grischinology. I suppose the first thing to do is to reprise the proof that the theory of infinite atomic boolean algebras is complete. Zachiri tells me it's proved by quantifier elimination.

Aargh! There is the following annoying elementary fact:

If σ is a type-raising permutation that respects $\in$ then it is 1-setlike.

Let y be an arbitrary element of our model; we want to show that σ“y is also an element of the model. It will in fact be $\sigma(y)$.

Let x be arbitrary. Then

$x \in \sigma$“y iff

$\sigma^{-1}(x) \in y$. This is OK beco's σ is a bijection. Next we use the fact that σ preserves $\in$:

$x \in \sigma(y)$. But all these steps are reversible. So

$(\forall x)(x \in \sigma``y \longleftrightarrow x \in \sigma(y))$. So

$\sigma``y = \sigma(y)$ by extensionality.

So $\sigma``y$ exists and is equal to $\sigma(y)$ by extensionality. So σ is 1-setlike.

However i don't see any reason why it should be 2-setlike.

Ah! The bijections we need don't have to preserve $\in \ldots$!

There is the faint possibility – which may be worth exploring – that TST and TTTP could be synonymous.

1.11.1 TTTP made easy, by Randall Holmes, 20/v/24

We define type i of our model of TTTP as $\mathbb{N} \times \{i\}$.

We will define relations $\in_{i,j}$ for $i < j$.

Suppose we have defined $\in_{i,j}$ for all i and j below a fixed k.

For each $i < k$, let Δ_i be the (countable) collection of subsets of type i which we can define using all $\in_{u,v}$'s $u < v \leq i$, allowing use of previously defined sets in Δ_i as parameters (one can manage this by constructing Δ_i in ω stages). One might want to restrict to sets definable along a single path through the types, but one does not need to.

For each $i < k$, provide a bijection $f_{k,i}$ from type k to Δ_i.

Define $x \in_{i,k} y$ as $x \in f_{k,i}(y)$.

And that is it.

A predicatively defined set $\{x_i : \phi\}^k$ is handled because the metatheoretical set $\{x : \phi\}$ was constructed during the construction of Δ_i and is the image of some element of type k under $f_{k,i}$.

There is no reason to expect the axiom of union to work, because a subcollection of type i defined by union is defined with essential reference to what happens in higher types.

One can get a model of TTTI by constructing Δ_i differently: construct a countable model of the theory of types $\leq i$ with all the $\in_{u,v}$ with $u < v \leq i$, take the full power set of type i as type $i+1$ in the obvious way, then build a countable model of the theory of these types $\leq i+1$ and let Δ_i be the type $i+1$ of the countable model. This gives you NFI because you can define sets in type $i+1$ impredicatively. But you still do not get union, because there is no reason to believe that a subset of type i defined by union, involving essential reference to a higher type, will appear in the type Δ_i thus constructed: it would appear in the full power set, but it would not appear in the countable model, because it is not defined in terms of the theory used to construct the countable model.

1.12 Sets Hereditarily the Same Size as a Set of Singletons in str(ZF)

I'm not sure which file to put this stuff in, so, for the moment, it's in a file of its own

Consider the following sequence of properties:

$$I_1(x) \longleftrightarrow (\exists y)(|x| = |\iota``y|)$$
$$I_{n+1}(x) \longleftrightarrow (\exists y)(I_n(y) \wedge |x| = |\iota``y|)$$

I think i can prove that every [wellfounded] set that is hereditarily I_1 is I_n for all n. The proof uses $\in$-induction and choice so is is bit *profligate* but may be worth noting all the same.

I'm still not 100% clear how to go about it, but here is a start.

Every set that is hereditarily I_1 is I_2.

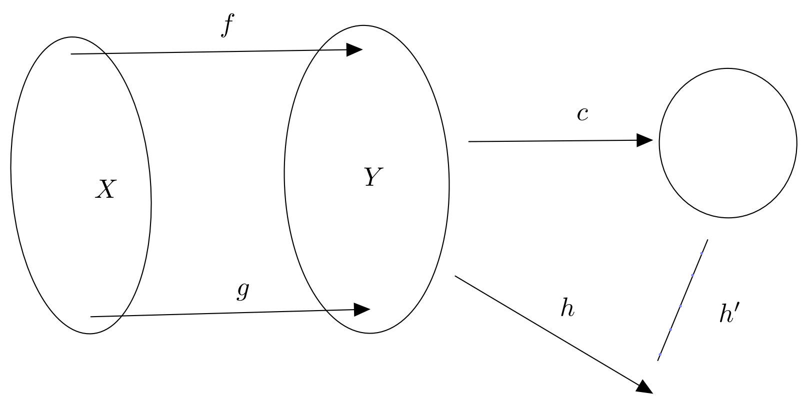
Suppose X is hereditarily I_1 and every member of x is I_2. By induction hypothesis every $y \in X$ is I_2 so for each $y \in X$ pick an I_1 set y' with a bijection $f_y : y \longleftrightarrow \iota^2``y'$.

The y' might not be distinct. However we are given that X is I_1 so there is $\iota``X'$ with a bijection $f : X \longleftrightarrow \iota``X'$.

So we send each $y \in X$ to $\{\{\}\}$

1.13 Coequalisers, and some remarks about Partitions

Functions $f : X \to Y$ and $g : X \to Y$. We want a coequaliser.



c is the coequaliser, h is any function such that $h \cdot f = h \cdot g$.

There is an equivalence relation $\sim_{f,g}$ on Y s.t $z_1 \sim z_2$ iff $(\exists x \in X)(f(x) = z_1 \wedge h(x) = z_2)$. Any $h : Y \to$ wide blue yonder gives rise to an equivalence relation on Z: the equivalence classes are the fibres of h. Now if h is any function such that $h \cdot f = h \cdot g$ then this equivalence relation given by h is at least as coarse as $\sim_{f,g}$. How about we take the intersection of all the equivalence relations that arise from these h? What do we get? Do we get exactly $\sim_{f,g}$? Adam sez it's not obvious that we do. He is concerned by the possibility that the intersection of all these equivalence relations might be strictly coarser than $\sim_{f,g}$.

But what is he worried about? Even if it isn't, and the intersection is precisely $\sim_{f,g}$, it's of no help to us in our quest for the Holy Coequaliser, because the problem all along was the inhomogeneity of the quotient map.

Actually he has a point. What happens if there *happens* to be, *somehow*, an $h : Y \to$ the wide-blue-yonder such that the equivalence relation that it gives rise to is the unique $\subseteq$-minimum equivalence relation? Then this h really is the Holy Coequaliser! *And this can happen even if this equivalence relation is not* $\sim$*!*

[*continuing to think aloud* ...] the intersection of all the equivalence relations arising from morphisms $h : Y \to$ wide blue yonder that satisfy $h \cdot f = h \cdot g$ is a perfectly well-defined object of whose existence we can be confident. It is true – as Adam says – that **if** this equivalence relation arises from some $h : Y \to$ wide blue yonder **then** that h is the Holy Coequaliser *even if the equivalence relation isn't* $\sim_{f,g}$. The trouble is that there doesn't seem to be any way of obtaining such an h.

It's probably worth spelling out what happens if X and Y are sets of singletons. We obtain $\sim$ as above; every equivalence class is a set of singletons; so we consider the result $\bigcup(Y/\sim)$ of raising[3] the type of the quotient ("rub out one layer of curly brackets"). The result genuinely is a coequaliser. But there is no reason to suppose that it is T of anything.

The category of NF sets has coequalisers iff every partition injects into $\iota``V$. My guess is that this assertion is independent of NF but is not strong.

Might there be any hope of proving it? Who knows! After all i saw no hope of proving that there are precisely as many pairs as singletons (and nor did Specker!) until Nathan showed us how to do it.

For α a reasonably small cardinal α (as-a-set) must be the same size as $\iota``V$. "Finite" is certainly sufficient. (It follows from Nathan's work that $|FIN| \leq T|V|$). So if a partition $\mathbb{P}$ has $|\mathbb{P}| \not\leq T|V|$ then it must have some infinite pieces. One might think there is some leverage in that the larger the pieces in a partition the fewer there can be of them, but it doesn't do very much for us. Just how little it does is illustrated by the following factoid: If $\mathbb{P}$ is a partition of V then $\{V \times p : p \in \mathbb{P}\}$ is a partition of V the same size as $\mathbb{P}$ all of whose pieces are of size $|V|$. So if there is a bad partition there is a bad partition every one of whose pieces is as big as can be!

Reflect that, in general, if $\mathbb{P}_1$ and $\mathbb{P}_2$ are partitions of V then $\{p_1 \times p_2 : p_1 \in \mathbb{P}_1 \wedge p_2 \in \mathbb{P}_2\}$ is also a partition of V, and there are natural embeddings ...

Let us say that an equivalence relation on V is *of small index* if the quotient injects into $\iota``V$. Then an intersection of two equivalence relations of small index is another equivalence relation of small index.

Later

Let's think about quasiorders on the set of all partitions of V. There is the partial order of refinement, which gives us a poset. There is also $\mathbb{P}_1 \leq \mathbb{P}_2$ if there is an injection $f : V \hookrightarrow V$ s.t $j(f) : \mathbb{P}_1 \to \mathbb{P}_2$. That is to say that if p is a piece of $\mathbb{P}_1$ then $f``p$ is a piece of $\mathbb{P}_2$ (not merely a subset of a piece of $\mathbb{P}_2$). This definition admits a C-B theorem and the equivalence classes are conjugacy classes.

There is also the quasiorder $\mathbb{P}_1 \leq \mathbb{P}_2$ if there is an injection $f : V \hookrightarrow V$ such that if p is a piece of $\mathbb{P}_1$ then $f``p$ is a subset of a piece of $\mathbb{P}_2$.

It is easy to see that there is no C-B theorem for this quasiorder. Set $\mathbb{P}_1$ to be a partition of V into lots ($|V|$-many) of finite pieces all of *odd* size, and set $\mathbb{P}_1$ to be a partition of V into lots ($|V|$-many) of finite pieces all of *even* size. Evidently $\mathbb{P}_1 \leq \mathbb{P}_2 \leq \mathbb{P}_1$, but they are clearly not conjugate.

Probably worth pointing out that this second quasiorder says that $\mathbb{P}_1 \leq \mathbb{P}_2$ if there is an injective homomorphism $\mathbb{P}_1 \to \mathbb{P}_2$ where we are thinking of the partitions as equivalence relations: $x \sim_1 y \to f(x) \sim_2 f(y)$.

However if we are considering these two quasiorders in a context where all the pieces in all our partitions have at most two members then counterexamples like the one we have just considered cannot arise.

[3] lowering...?

1.14 A Question of Alice Vidrine

Can there be an infinite set of pairwise disjoint sets that has, up to finite difference, precisely one transversal?

The answer is yes.

The construction of an example will be done in KF, or rather a version of KF + not-AC, in which we assume that there is an infinite set of pairwise disjoint sets, no infinite subset of which has a transversal. I do not know offhand of any construction of such a model, but i imagine that there is a standard FM construction that effects it.

Remark 5 *KF + "there is an infinite set of pairwise disjoint sets, no infinite subset of which has a transversal" proves that there is an infinite set of pairwise disjoint sets that has, up to finite difference, precisely one transversal.*

Proof: Let $\{X_i : i \in I\}$ be an infinite family of pairwise disjoint sets such that for no infinite $I' \subseteq I$ does $\{X_i : i \in I'\}$ have a transversal. Now make a copy $\{\iota``X_i : i \in I\}$ of $\{X_i : i \in I\}$, and add one element to each $\iota``X_i$ to obtain $\{\iota``X_i \cup \{X_i\} : i \in I\}$.

Observe that this new family, unlike $\{X_i : i \in I\}$, does actually have a transversal, namely $\{\{X_i\} : i \in I\}$. Observe further that this transversal is unique up to finite difference.

■

One could use $\{X_i \cup \{X_i\} : i \in I\}$ to the same effect, but i wanted something that was stratified all the way and that therefore worked in KF.

I think this is a better version of Alice's question:

"Can there be a family of pairwise disjoint sets that has precisely $\aleph_0$ transversals?"

I suspect this is equivalent to the question:

"Can there be a countably infinite profinite structure?"

to which the answer really ought to be 'no!'!

1.15 Quantifier Complexity of the Axiom of Infinity

A set is infinite iff it is both even and odd. So we can express AxInf by saying that V is an odd set. (It's obviously even). So we can say: $(\exists x)(V \setminus \{x\}$ has a partition into pairs). How many quantifiers..?

There is a set x and a set P...

$(\forall u)(\forall p, p' \in P)(u \in p \wedge u \in p' \to p = p')$

$(\forall p \in P)(\forall u, u'u'' \in p)(u = u' \vee u = u'' \vee u' = u'')$

$\exists\forall$ so far

$(\forall y \neq x)(\exists p \in P) y \in p$

$\exists\forall\exists$ so far

$(\forall p \in P)(\exists u, v \in p)(u \neq v)$

So: $\exists\forall\exists$. But some of these quantifiers are restricted, making it Σ_2 ... but then *everything* is Σ_2!

We know that AxInf cannot be $\exists^*\forall^*$. [Why!? I've forgotten!] It's not yet clear that it cannot be $\forall\exists$. The ordering principle is $\exists\forall\exists$. This means that my project of adding all consistent $\forall\exists$ sentences and then all consistent $\exists\forall\exists$ sentences and so on will not give NF...

Unless of course $\neg$AC is $\exists\forall\exists$. "There is a partition without a transversal"

There is $\mathbb{P}$

"$\mathbb{P}$ is a set of pairwise disjoint sets" is $\forall$;

"T is a transversal for $\mathbb{P}$" is

$(\forall p \in \mathbb{P})(\exists y \in T)(y \in p)$;

$(\forall t, t' \in T)(\forall p \in \mathbb{P})(t, t' \in p \to t = t')$

So "T is not a transversal for P" ie $\exists\forall$.

Now allow the partition to have an empty piece. Then being a disjoint family is $\forall_1$. Being a transversal for a disjoint family possibly containing an empty piece is

$(\forall p \in \mathbb{P})(\forall x)(x \in \mathbb{P} \to (\exists y \in T)(y \in p)$

is $\forall_2$. So "there is a disjoint family without a transversal" is

$(\exists F)(F$ is a disjoint family $\wedge(\forall T)(T$ is not a transversal for R)

So $\neg$AC seems to be $\exists_4$.

One would like to do better

How complex is "$\mathbb{P}$ is a partition of X into pairs"?

$(\forall y \in X)(\exists p \in \mathbb{P})(y \in \mathbb{P})$ and
$(\forall p1, p2y)(y \in X \wedge p_1, p_2 \in p \to \neg(y \in p_1 \wedge y \in p_2)$ and
$(\forall p \in \mathbb{P})(\forall abc \in p)(a = b \vee b = c \vee a = c)$ and $(\forall p \in \mathbb{P})(\exists a, b \in P)(a \neq b)$

which is $\forall_2$

And a partition into triples is also $\forall_2$. So "V has a partition into triples" is $\exists_3$. So the axiom of infinity is $\exists_3$

"For all x there is a partition of $V \setminus \{x\}$ into pairs" is "there is an involution with a unique fixed point". Is this easily expressible in the language of group theory?

The axiom of infinity in T$\mathbb{Z}$T.... I think it is Δ_2. This is beco's it is equivalent to both

(i) The universe has a partition into (unordered) triples;

(ii) No ordernesting of the universe is closed under both $\bigcup$ and $\bigcap$.

Ad (i) the point is that V is a boolean algebra so if it is finite its size is a power of 2 and is therefore not a multiple of 3. I'm hoping (fingers crossed) that if V is Dedekind-infinite then it can be partitioned into triples. Can't see how to do it offhand but i'm planning to brazen this one out. (i) is Σ_2.

Ad (ii) the point is that every ordernesting of a finite set is closed under $\bigcup$ and $\bigcap$ whereas all infinite total orders have subsets with either no maximal element or no minimal element. These subsets are not elements of the ordernesting.

Our linear orders will be ordernestings.

"is an ordernesting of V" and "... is a partition of V" are both Π_1.

"O is an ordernesting of X" is

$(\forall x \in X)(\exists o \in O)(x \in o)$ and $(\forall o1 o2 \in o)(o1 \subseteq o2 \vee o2 \subseteq o1)$ and $(\forall x_1 \neq x_2 \in x)(\exists o \in O)(x_1 \in o \longleftrightarrow x_2 \notin o)$

which is $\forall_2$

It's pretty obvious that (i) is Π_2; for (ii) we need one existential quantifier to assert the existence of a subset X of the ordernesting O, and we want to say that $\bigcup X \notin O$. This is $(\exists y)(y = \bigcup X \to y \notin O)$ and $y = \bigcup x$ is Δ_0. So (ii) is

$$\exists O, X, y \quad O \text{ is an ordernesting and } X \subseteq O \text{ and } y = \bigcup x \text{ and } y \notin O.$$

This shows that (ii) is Σ_2.

Upshot: AxInf is Δ_2.

Does this help ...?

No, it doesn't do anything. I'd forgotten the old result that in T$\mathbb{Z}$T *everything* is Δ_2.

We'll have to count the restricted quantifiers as well. And this calculation is wrong anyway.

OK, so get out the back of the envelope. "O is an ordernesting" is

$(\forall y \exists z)(y \in z \in O) \wedge (\forall x_1 x_2 \in O)(x_1 \subseteq x_2 \vee x_2 \subseteq x_1)$

which is $\forall^*\exists^*$.

$y = \bigcup x$ is $(\forall zw)(z \in w \in x \to z \in y) \wedge (\forall z)(\exists u)(z \in y \to z \in u \in x)$

which is $\forall^*\exists^*$.

So (ii) seems to be:

for all ordernestings O there is $X \subseteq O$ $\bigcup X \notin O \vee \bigcap X \notin O$

$\forall^*\exists^*\forall^*\exists^*$.

What about (i)?

(i) is clearly $\exists\forall\exists$.

How about "for every nested family of sets covering V there are two elements it cannot distinguish"?

$(\forall \mathcal{F})(\forall A, B \in \mathcal{F})(\forall x)((\bigcup \mathcal{F} = V \wedge (A \subseteq B \vee B \subseteq A)) \to (\exists a \neq b)(\forall u \in \mathcal{F})(a \in u \longleftrightarrow b \in u))$

$(\forall \mathcal{F})(\forall A, B \in \mathcal{F})(\forall x)((\bigcup \mathcal{F} \neq V \vee (A \not\subseteq B \wedge B \not\subseteq A)) \vee (\exists a \neq b)(\forall u \in \mathcal{F})(a \in u \longleftrightarrow b \in u))$

which looks $\forall^*\exists^*\forall^*$

This is just the negation of the ordering principle, whci we knew to be $\exists_3$ all along.

Just noticed ...

Trying to formulate a $\forall^*\exists^*$ sentence that says there is no total ordering of the universe. We think of a total ordering as an *ordernesting*, a family of sets totally ordered by $\subseteq$ with special properties. One wants to say that for any two things there is a member of x that contains one but not the other. However that requires too many quantifiers.

$(\forall X)($

if x is totally ordered by $\subseteq$

(which is to say $(\forall x_1, x_2 \in X)(x_1 \subseteq x_2 \vee x_2 \subseteq x_1)$, which is $\forall^*$ as desired

then

$(\forall a, b \in X)(a \neq b \to |a \texttt{ XOR } b| \geq 2)$

The effect of this is that if x_2 "is an immediate successor of" x_1 then there are at least *two* things in $x_2 \setminus x_1$ so X cannot distinguish them.

$(\forall a, b \in X)(a \neq b \to (\exists u \neq v)(u, v \in a \setminus b) \vee (u, v \in b \setminus a)))$

But we also have to somehow compel X to cover everything, to contain $\emptyset$ and V.

so we want to say: $\forall X$ either X is not totally ordered by $\subseteq$ or $V \notin X$ or $\emptyset \notin X$ or all symmetric differences between its members are of size at least 2

$V \notin X$ is $(\forall x \in X)(\exists y)(y \notin X)$;

$\emptyset \notin X$ is $(\forall x \in X)(\exists y)(y \in X)$

Now this is a $\forall^*\exists^*$ sentence. What does it say? It certainly implies that if there is an ordernesting of V it must satisfy the symmetric difference condition, but that implies that V is infinite, so it says that if there is a total order of V then V is infinite. But if there is no total order of V then V is infinite. So it is a $\forall\exists$ sentence that implies infinity.

Is it consistent? No! Because it implies that all total orders of V are dense. If V has any total orders at all it must have some that aren't dense.

Let's hope that there is a way of modifying it into something sensible.

1.16 definable automorphisms – ain't none.

Let's minute the fact that there is no $\in$-automorphism definable by a stratifiable expression. Suppose $\phi(x, y)$ were such an expression. Think about the level of the variable 'x' in ϕ. We certainly have $(\forall x, y)(\phi(x, y) \longleftrightarrow \phi(\{x\}, \{y\}))$. But observe that, in $\phi(\{x\}, \{y\}))$, the variable x is one level lower than it is in ϕ. So 'x' can be taken to be of level 0 – which is of course absurd.

No, come on, Forster, that it not a proof!

Is there anything at all that one can say about the group of all $\in$-automorphisms? I'm guessing not. It's a subgroup of J_∞, but that doesn't tell us much. It raises the question (which i have worried about elsewhere) about sentences preserved under directed intersections. J_∞ is a directed intersection of the J_n which are all elementarily equivalent (tho' the inclusion embeddings are not elementary) so perhaps we can say *something* about J_∞...?

It's presumably something to do with $\forall\exists!$ expressions. What has only just occurred to me is that the arguments that $\forall$ and $\exists!$ are preserved work also for the higher-order language.

One thing that should be fitted in somehow: any τ in J_∞ (in fact anything in J_3) is "almost" an automorphism, in the sense that τ and $j\tau$ are conjugate (as long as we have GC). Thus "τ is conjugate to $j\tau$" is equivalent to something stratified as long as we have GC. I *think* that (if we have GC) then "τ is conjugate to $j\tau$" is equivalent to "τ is conjugate to something in J_2". Or even "τ has the same cycle type as something in J_2".

This suggests a refinement to the thoughts about Fine's principle that i had years ago. I proved that for any set and any (satisfiable) one-place predicate ϕ, there is a permutation model in which that set has ϕ. However i now think that we should consider x to be predisposed to be ϕ if there is a permutation $\tau \in J_n$ s.t $V^\tau \models \phi(x)$ for some suitably large n. We can say

Definition 1
"x is n-predisposed to be ϕ" iff there is $\tau \in J_n$ such that $V^\tau \models \phi(x)$.

Then the theorem in my monograph says that, for all x and ϕ, x is 1-disposed to be ϕ.

"π is an $\in$-automorphism" is

$\pi = j(\pi)$ and

$V^\sigma \models \pi$ is an $\in$-automorphism" is

$\sigma_n(\pi) = j(\sigma_{n+1})(\pi)$.

So we want $\sigma(\pi)$ to be a permutation. So we want σ to be in J_n for some n depending on our choice of pairing function.

1.17 Two questions about extracted Models

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From Thomas Forster <tf@dpmms.cam.ac.uk>
To:  Randall Holmes <m.randall.holmes@gmail.com>
Date: 21 Sep 2020 04:20:56 +0100
Subject: Just given a talk..
```

in which i presented Jensen's proof. I got two quite good questions[4]:

> (1) Is every model of TℤTU extracted from a model of TℤT?
>
> (2) Why not define the membership of the extracted model by $x \in' y$ iff $y = \iota^n(z) \wedge x \in z$?

I now think i have two helpful answers.

[4]One of them (i can't remember which) was asked by Linus Richter.

Question 1

The answer to (1) is: no. One thing that is clear is that, if a model $\mathfrak{M}$ extracted from a model of T$\mathbb{Z}$T is actually a model of T$\mathbb{Z}$TU, then each each level $l+1$ of the extracted model is of size $\beth_n$ of the size of level l, for some concrete n depending on i. For each n and i this is a first-order condition expressible in $\mathcal{L}$(T$\mathbb{Z}$T).

Definition 2
• *Let $\phi_{l,m}$ be the formula of $\mathcal{L}$(T$\mathbb{Z}$T) that says that the cardinality of level $l+1$ is $\beth_n$ of the cardinality of level l.*
• *For each $l \in \mathbb{Z}$, let Σ_l be the type $\{\neg\phi_{l,m} : m \in \mathbb{N}\}$.*
• *Let us give the name 'T$\mathbb{Z}$T(Omit)' to the smallest theory that locally omits all the Σ_l.*

T$\mathbb{Z}$T(Omit) is axiomatisable but (presumably) not recursively axiomatisable. (It's obviously not vacuous!) Let us minute the following fact.

Remark 6
A model of T$\mathbb{Z}$TU is a model extracted from a model of T$\mathbb{Z}$T iff, for each $l \in \mathbb{Z}$, it omits Σ_l.

Proof:
One direction is obvious: clearly an extracted model omits all the Σ_l.
For the other direction, consider a model $\mathfrak{M} \models$ T$\mathbb{Z}$TU that omits all the Σ_i. Such a model knows (for each l) that the size of its level $l+1$ is $\beth_n$ of the size of its level l, for some concrete n (depending on l). But then it knows about sets of sizes of all these intermediate $\beth$ numbers, and can use these sets to fake up a model in which no levels between l and $l+1$ have been discarded. Evidently all models obtained from $\mathfrak{M}$ in this way are isomorphic. ■

Let us call this process **backfilling**.

Backfilling

So the extracted model knows whence it came. One might have thought that all information in the discarded levels is irretrievably lost, since the atoms cannot be distinguished, but – as we have seen – the information is retained. This may be something to do with the fact that all the information is stratified[5]. This is probably worth flagging.

Remark 7 *Any model of T$\mathbb{Z}$T can be recovered from any model extracted from it.*

[5]There are echoes here of an old question about how discernible the atoms in a model of NFU can be.

Clearly by compactness there are models of T$\mathbb{Z}$T that realize plenty of these types, so the answer to question (2) is 'no'.

Let's retrace Jensen's original proof (or – perhaps i should say – my recollection of it!). We start with a model $\mathfrak{M}$ of T$\mathbb{Z}$T, and successively extract models $\mathfrak{M}_i : i \in \mathbb{N}$ from it, with the $\mathfrak{M}_i$ satisfying ambiguity for ever more expressions as i increases. All the $\mathfrak{M}_i$ are models of T$\mathbb{Z}$T(Omit). We then take an ultraproduct, $\mathfrak{M}_\infty$, which will be ambiguous. (That was the point, indeed). However it will also be a model of T$\mathbb{Z}$T(Omit). Now, although it is a model of T$\mathbb{Z}$T(Omit) it obviously realizes all the Σ_l, and therefore cannot be an extracted model. Observe, too, that altho' Th($\mathfrak{M}_\infty$) extends T$\mathbb{Z}$T(Omit), it does not itself locally omit the Σ_l. If it did, it would have a model that omitted the Σ_l, and that would be an extracted model, and the model from which it was extracted would be an ambiguous model of T$\mathbb{Z}$T. Finite extensions of theories that locally omit a type will locally omit that type, but infinite extensions might not, and the example to hand is a useful illustration.

1.17.1 Wherein we spell out the connection between TC$_n$T and special models of NFU

Remark 8 *The following are equiconsistent, for any concrete n:*

(1) TC$_n$T;
(2) NFU + $|V| = \beth_n|\texttt{sets}|$;
(3) T$\mathbb{Z}$T+ the scheme $\{\phi_{l,n} : l \in \mathbb{Z}\}$.

Proof:
If $\mathfrak{M}^*$ is a model of T$\mathbb{Z}$TU extracted from $\mathfrak{M}$ a model of T$\mathbb{Z}$T, and $\mathfrak{M}^*$ is ambiguous then, for some n, it satisfies $\phi_{l,n}$ for every l. This means that when we backfill to recover $\mathfrak{M}$ we find that it is a model of Amb^n. And clearly if $\mathfrak{M} \models$ Amb^n then we can extract an ambiguous model of T$\mathbb{Z}$TU + the scheme $\phi_{l,n}$ for all l. ■

There is the thought that T$\mathbb{Z}$TU and Tangled T$\mathbb{Z}$TU might be synonymous.

At the very least if we start with a model of T$\mathbb{Z}$T and extract we can backfill and end up where we started. Going in the opposite sense doesn't work, beco's not every model of T$\mathbb{Z}$TU is an extracted model.

If we have a model of T$\mathbb{Z}$TU + ambiguity obtained by extracting, then, for some n, we retained every nth level. By backfilling we recover a model of T$\mathbb{Z}$T, and that model satisfies Amb^n. So you get a model of TC$_n$T[6]! See [**?**].

In fact i claim that, for each concrete n, the two theories TC$_n$T and NFU + $|V| = \beth_n|\texttt{sets}|$ are synonymous. If we backfill a model of NFU + $|V| = \beth_n|\texttt{sets}|$ we obtain a model of TC$_n$T; if we discard all but one level of a model of TC$_2$T we obtain a model of NFU + $|V| = \beth_n|\texttt{sets}|$... and the two constructions are mutually inverse.

[6] I am not sure where this fact is written up!

[Are they actually *synonymous*?]

So here is a question about models of NFU. There are these two methods of obtaining models of NFU:

(i) Start with a model of T$\mathbb{Z}$T; extract lots of models from it, getting more and more ambiguity. Get a saturated ultraproduct which will be *glissant*; take a quotient over the tsau.

Is every model of NFU elementarily equivalent to a model arising in this way?

(ii) There are the models arising from nonstandard models of KF. Are they ever models of NFU + $|V| = \beth_n|\texttt{sets}|$ for any $n \ldots$? One suspects not. But what if one starts with a nonstandard version of the Baltimore model?

One might have to allow permutations

Suppose $\mathfrak{M} = \langle M, \in^{\mathfrak{M}}\rangle$ is a model of NFU + $|V| = \beth_n|\texttt{sets}|$ for some n.

(i) We obtain a *modèle glissant* $\mathfrak{M}^*$ of T$\mathbb{Z}$TU + Amb by taking $\mathbb{Z}$-many copies of $\mathfrak{M}$. We then

(ii) "backfill" to obtain a *modèle glissant* $\mathfrak{M}^{**}$ of T$\mathbb{Z}$T+ Amb^n. That is to say, we interpolate $n-1$ new levels between any two levels of $\mathfrak{M}^*$.

(iii) Any *modèle glissant* $\mathfrak{M}^{**}$ of T$\mathbb{Z}$T+ Amb^n can quotient out to give a model of TC$_n$T.

It might be an idea to spell out the details.

(i) is old tech. $\mathfrak{M} = \langle M, \in^{\mathfrak{M}}\rangle$, so M is the carrier set of $\mathfrak{M}$. Construct a model $\mathfrak{M}^*$ of T$\mathbb{Z}$TU by declaring level l to be $M \times \{l\}$; we then define a membership relation $\in^{\mathfrak{M}^*}$ between (sets belonging to) level l and (sets belonging to) level $l+1$ by saying $\langle x, l\rangle \in^{\mathfrak{M}^*} \langle y, l+1\rangle$ iff $\mathfrak{M} \models x \in y$. The operation of incrementing the second component of the ordered pairs is of course a tsau for $\mathfrak{M}^*$, making $\mathfrak{M}^*$ a *modèle glissant* $\mathfrak{M}^*$ of T$\mathbb{Z}$TU + Amb as claimed.

Next (ii) we have to explain how to backfill $\mathfrak{M}^*$ to obtain a model of T$\mathbb{Z}$T+ Amb^n as promised.

We have to set up a bijection between the object at level l that started off in $\mathfrak{M}$ as `sets` and the (internal) power set of the whole of level $l-1$ (that started off as M). Every element of `sets` of level l corresponds to a subset of level l, which is to say a subset of M. *Via* the tsau, it now corresponds to a subset of level $l-1$. This means we can use `sets` $\times\{l\}$ as the level to be interpolated ("inserted"?) immediately above level l. What is to be the next level above that? Obviously we want $\mathcal{P}(\texttt{sets})$ – which is a perfectly respectable set of $\mathfrak{M}$. We insert $\mathcal{P}^2(\texttt{sets})$ similarly. And so on up. The newly inserted levels mean that the expanded structure is now a model of extensionality. There is still a tsau (the same tsau as before, in fact) but it shifts everything up by n levels not 1, so it is a *modèle glissant* all right but of Amb^n rather than Amb.

(iii) A *modèle glissant* of T$\mathbb{Z}$T+ Amb^n can quotient out by the tsau to reveal a model of TC$_n$T.

Actually, for any concrete n we can go directly from a model of NFU + $|V| = \beth_n|\texttt{sets}|$ to a model of TC$_n$T without the *detour* through T$\mathbb{Z}$T. Suppose, as before, that $\mathfrak{M} \models$ NFU + $|V| = \beth_n|\texttt{sets}|$. For each $1 \leq k < n$, $\mathfrak{M}$ contains sets of size $\beth_k|\mathtt{sets}|$ and we take these sets (one for each k, only finitely many choices) together with V, to be the levels of our model of $\mathrm{TC}_n\mathrm{T}$.

Somewhere i should re-use this snippet from the monograph:

THEOREM 2 *(Holmes) (NFU + AC)*
For each concrete n, $\beth_n|\mathcal{P}(V)| < |V|$

Proof:

Suppose not. Then there is a concrete n such that $\beth_n|\mathcal{P}(V)|$ does not exist. Let n be the smallest such. Observe that $\beth_{n+1}|\mathcal{P}(V)|$ does not exist, and that $\beth_n|\mathcal{P}(\iota``V)|$ does exist.

Let m be the smallest cardinal such that $\beth_i(m)$ does not exist for some i. Let $j+1$ be the smallest such i. Now look at the sequence of iterated images of Tm under exp. The $Tj+1$st element of this sequence exists and is greater than $T|V| = |\iota``V|$, so it has no more than n iterated images under exponentiation; between 1 and $n+1$ new terms are added to the sequence. Thus, the number of terms in the sequence for Tm is finite and differs from the number of terms in the sequence for m mod $n+2$ (say); recall that n is standard,so m is different from Tm. Thus $m < Tm$ (by minimality of m). But then $T^{-1}m < m$, and $T^{-1}m$ is easily seen to have between 1 and $n+1$ fewer terms in its sequence of iterated images under exponentiation than m, violating minimality of m.

■

Notice that this refutation of AC is different in nature from the refutation of AC from $\beth_n|\mathcal{P}(V)| = |V|$. That is a stronger assumption, strong enuff to power the connection to $\mathrm{TC}_n\mathrm{T}$.

1.17.2 The ambiguity scheme is not finitisable

While we are about it, let us record that the ambiguity scheme is not finitisable, in the following strong sense. (It's pretty obvious that it is not finitisable in the obvious sense; we mean something stronger and more interesting.) For each stratifiable expression ϕ of $\mathcal{L}(\in, =)$ there is a scheme of biconditionals between the results of decorating the variables in ϕ with level subscripts. Let us call that scheme $\mathrm{Amb}(\phi)$. Clearly there are infinitely many such schemes. The ambiguity scheme is the union of all of them. We will show that it is not axiomatised by any finite set of them.

Suppose *per impossibile* that ambiguity is entailed by finitely many $\mathrm{Amb}(\phi)$, arising from $\phi_1 \ldots \phi_n$. Let $\mathfrak{M}$ be an arbitrary model of $\mathrm{T\mathbb{Z}T}$. Perform the Jensen/Ramsey extraction for $\phi_1 \ldots \phi_n$, successively, thereby obtaining a model $\mathfrak{M}^*$ of $\mathrm{T\mathbb{Z}TU}$ + Amb. Each level l of $\mathfrak{M}^*$ knows how many levels have been discarded between it and the level immediately below it. This number must be finite, since $\mathfrak{M}^*$ is an extracted model, and further it must be the same at each level l, beco's of ambiguity. Call this number k. That means that when we backfill $\mathfrak{M}^*$ to obtain a model of $\mathrm{T\mathbb{Z}T}$ (without atoms) it must be a model of $\mathrm{T\mathbb{Z}T}+ \mathrm{Amb}^k$. Now the model obtained by backfilling is of course $\mathfrak{M}$. So $\mathfrak{M}$ was a model of $\mathrm{T\mathbb{Z}T}+ \mathrm{Amb}^k$. But $\mathfrak{M}$ was arbitrary.

Maybe there is a cuter proof of this using van der Waerden.

Some duplication here. . . ?

A tho'rt about extracted models and finite axiomatisability of ambiguity

Suppose the ambiguity scheme is finitely axiomatisable in the sense that there are finitely many stratifiable formulæ wiithout type indices such that the finitely many schemes $\Sigma_{i \in \mathbf{Z}} \phi_i \longleftrightarrow \phi_{i+1}$ axiomatise the whole of Amb when added to T$\mathbb{Z}$TU. That means we have an extracted model of T$\mathbb{Z}$TU plus Ambiguity. Now in this extracted model each level V_i knows how many levels have been left out between it and V_{i-1}, *and this number must be the same for all* i. This gives us a model for NFU + $|V| = \beth_k(|V|)$ for some k, and this refutes AC.

But NF is finitely axiomatisable, so does that mean that *over T$\mathbb{Z}$T rather than T$\mathbb{Z}$TU*, the ambiguity scheme is finitely axiomatisable in the sense that there are finitely many stratifiable formulæ wiithout type indices such that the finitely many schemes $\Sigma_{i \in \mathbf{Z}} \phi_i \longleftrightarrow \phi_{i+1}$ axiomatise the whole of Amb when added to T$\mathbb{Z}$T?

Question 2

The answer to (2) is that, no, it doesn't make any difference. In fact there is, up to isomorphism, only one way to discard any family of levels.

If we are to extract level X and the level $\mathcal{P}^n(X)$ above it ($n > 1$ obviously) then we discard[7] the intermediate levels. We fix an injection $i : \mathcal{P}(X) \hookrightarrow \mathcal{P}^n(X)$, and then say that:

> x (a member of X) is a "member of" Y (a member of $\mathcal{P}^n(X)$) iff $x \in i^{-1}(Y)$.

Notice that things not in the range of i are empty, just as they should be.

[Does this i have to be a boolean injection? Does it have to arise from an injection $X \hookrightarrow \mathcal{P}^{n-1}(X)$?]

A fundamental requirement is that this new membership relation should support axioms of comprehension (it clearly supports extensionality for nonempty sets) in the extracted model, and for this it is necessary that the expression "x is a member of y in the new sense" should be a formula of $\mathcal{L}$(T$\mathbb{Z}$T). We now show that any two injections which are definable in this sense give rise to the same model (up to isomorphism).

Remark 9 *The model obtained by extracting some chosen levels depends only on the levels chosen and not on the manner in which the extraction is performed.*

Proof:

Key fact: all injections satisfying the above condition are *conjugate*.

If we are to discard the levels between i and k then we need an injection from $V_i \hookrightarrow V_k$. Think about the subsets of V_k that could be the ranges of such an injection. We want them all to be the same size (which is easy) and that

[7] For obvious reasons we don't want to use the word 'omit'!

their complements are all the same size. That way for any two injections there will be a permutation of V_k that maps one range onto the other. There are two cases to consider, depending on whether or not AxInf holds. If V_k is finite then the good behaviour of subtraction makes everything OK. If we have AxInf then (since we are working in T$\mathbb{Z}$T) we can use the corollary of Bernstein's lemma that says that whenever α is the cardinality of a level, and $\alpha + \beta = 2^\alpha$, then $\beta = 2^\alpha$.

However, for the sake of completeness, let us consider TST as well. If we are working in TST then either
(i) the bottom level is inductively finite (internally) in which case everything is easy, or
(ii) the bottom level is not inductively finite, in which case – after a small finite amount of grinding of gears – everything works as in the T$\mathbb{Z}$T case. But since we are interested in infinitely many levels, a finite amount of gear grinding costs us nothing: we can always discard an initial segment of badly behaved levels.

Notice that all the reasoning in either case (TST or T$\mathbb{Z}$T) can be carried out inside the model and makes no use of AC.

■

I think this makes for a nicer way of presenting Jensen's extracted models than the usual method: we don't need to know what the injection is; all we need to know is that there are injections and that it matters not which one we use. I think one should just say: if we want to discard levels $n \cdots m-1$, then one calls to mind any internally definable injection from level n to level m, and feeds it into the above construction.

A couple of additional, minor, points...

(i) This presentation does a better job of making it clear that a composition of two extractions is an extraction. Compose the two injections with ι in the middle: $i \cdot j\iota \cdot j$.

(ii) The sets that one retains as nonempty in the original setting and the sets one retains as nonempty in the modified setting suggested by my questioner at Vic are related to each other by the involution $\prod\limits_{x \in V}(\iota^n(x), \iota^n``x)$, which is striking but is evidently a red herring.

(iii) Might TST be synonymous wih TSTU + axioms that bound the number of atoms?

1.18 Van der Waerden

Not profound, but it might be nice.

Think about models of T$\mathbb{Z}$T, and extracting levels from them to get models of T$\mathbb{Z}$TU.

Notice that, in all extracted models, each level knows how many levels have been discarded between it and the level immediately below it in the extracted model: for concrete k, the assertion “there are k levels that have been discarded immediately below me" is expressible as a first-order formula of $\mathcal{L}(\mathrm{T}\mathbb{Z}\mathrm{T})$. This means that if the levels you extract to get your extracted model are not evenly spaced, then your extracted model is *guaranteed* to violate ambiguity – for that expression at least. Of course you don't expect it to obey all of ambiguity anyway, but this is an extra thing to think about. What it does mean is when we think of the Ramsey construction of iterated extractions it might make for an extra cuteness if we do a little bit of Van der Waerden to ensure that the monochromatic set we extract is an AP. I don't know how much difference this will make, but it may be worth thinking about.

I suppose it does *something*. Suppose i start with a model of T$\mathbb{Z}$T; i want to enforce ambiguity for $\phi_1 \ldots \phi_n$. Then i 2^n-colour the k-tuples from $\mathbb{Z}$ and i get a monochromatic set containing a suff long AP. So i not only get Ambiguity for $\phi_1 \ldots \phi_n$ but this shows it's compatible with ambiguity for the formulæ that tell us about the number of levels omitted. But we know that full ambiguity is consistent anyway. So it doesn't really do anything after all.

1.19 Some stuff concerning ordinals

Ordinals are isomorphism classes of wellorderings. Is there a canonical family of representatives? Morally, yes, beco's each ordinal is the order type of the set of its predecessors in their natural order. So you implement an ordinal as the set of its predecessors, and the recursion must succeed. But that of course relies on a certain amount of comprehension (enuff to show that each initial segment is a set and then enough separation to ensure that the desired bijections are sets.) These representatives are to be had without AC and they cohere.

What is the situation in NF? Small ordinals have canonical representatives, but big ones don't. I'm pretty sure that NF doesn't prove the existence of a set of representatives for NO, and even more sure that it shows that there is no coherent set of representatives. If there were, its union would be a wellordering of maximal cardinality, and that would mean there is a last aleph. Mind you, there might be!

1.20 Must get straight this business of NF and internal automorphisms

Facts to be understood properly:

Every model of T$\mathbb{Z}$T is elementarily equivalent to a strongly extensional model. So every ambiguous model of T$\mathbb{Z}$T is elementarily equivalent to one without an (internal) automorphism. Doesn't seem to work for NF

Every model of NF has a permutation model with an (internal) automorphism.

No internal automorphism can be got rid of by a definable permutation

Is every model of NF elementarily equivalent to a strongly extensional one?

Can automorphisms be got rid of by permutations? NF $\vdash \Diamond$(There are no automorphisms)?

1.21 A message from Nathan in (Northern) Spring 2019

"I had some thoughts on the question of symmetric models. First of all, as you mention, downward extensions play an important role here, and it would be bothersome if there could be multiple possible downward extensions. It seems at first that there could be, since there can be multiple cardinals κ with $2^\kappa = |V_0|$. But we can always identify the cardinality of the previous level uniquely as the $\leq$-largest among them. More precisely, let W be $\iota''V_{-1}$. Then we have $2^{|W|} = |V_0|$, but also for any cardinal κ with $2^\kappa = |V_0|$ there must be some set X such that $\kappa = |\iota``X|$ and $|\mathcal{P}X| = |V_0|$. Since $\iota``X$ is a subset of $\iota``V_{-1}$, it follows that $\kappa \leq |W|$. Thus $|W|$ can be uniquely identified (uniqueness follows from Cantor-Bernstein), and the same goes for the cardinalities of all previous levels."

If i understand this correctly then it must mean that in NF we can show that $(\forall x)(|\mathcal{P}(x)| = T|V| \to |x| \leq T|V|)$. So, in particular, any surjective image of $\iota``V$ injects into $\iota``V$.

This "in particular" is worth spelling out. It's certainly true that if $|A| \leq^* T|V|$ **and** $|\mathcal{P}(A)| = T|V|$ then $|A| \leq T|V|$. But is it the case that for every A s.t $|A| \leq^* T|V|$ we can find a B with $|A| \leq |B|$ such that $|\mathcal{P}(B)| \leq T|V|$? I think we just take B to be $A \sqcup \iota``V$. Then $|\mathcal{P}(A \sqcup \iota``V)| = |\mathcal{P}(A)| \cdot |\mathcal{P}(\iota``V)|$ which is $\gamma \cdot T|V|$ for some $\gamma \leq T|V|$. Then $|A| \leq |B|$ and, since $|\mathcal{P}(B)| = T|V|$ which gives $B \leq T|V|$, we get $|A| \leq T|V|$.

Can we do the same for $T^2|V|$? Sse $|A| \leq^* T^2|V|$. Then $|\mathcal{P}(A)| \leq T^2|V|$. So $|A| < 2^{|A|} \leq T|V|$. So WLOG A is a set of singletons: $A = \iota``B$. Rerunning the argument we get $|\iota``B| \leq^* T^2|V|$, whence $|B| \leq^* T|V|$ and then $|B| \leq T|V|$ and then $|A| = T|B| \leq T^2|V|$. So it works for $T^2|V|$ as well.

Not sure how much use this is!

It means that every partition of V is the same size as a set of singletons...

If it is true (and Randall says it isn't) then we can show that $|V|$ is indecomposible. Sse $T|V| = \alpha \cdot \beta$, with $\alpha, \beta < T|V|$. Then, by Bernstein's lemma, $\alpha, \beta \leq^* T|V|$ whence $\alpha, \beta \leq T|V|$ after all. So $T|V|$ is indecomposible.

HOWEVER Randall is now insisting that Nathan got it wrong, and he's convinced Nathan. Let's go thru' this with a fine-toothed comb. Suppose we are in a model of TST, and we want to consider downward extensions. A downward extension by one is a set X such that $\mathcal{P}(X)$ is in 1-1 correspondence with $\iota``V_1$. (Here we are using the more inclusive definition of exponentiation, from my Ph.D. thesis and Crabbe's article 'A propos de 2^x' under which $2^{|x|}$ is $T^{-1}|\mathcal{P}(x)|$ if this second thing is defined.) Such an X will give us a new level 0. It will be an element of level 1, and so a subset of level 0. Nathan's original thought would have been that there is obviously a $\subseteq$-maximal such X, namely the whole of level 0. What's not to like?

I'm still not convinced. Randall's argument, as i understand it, runs as follows.

Suppose $T|V|$ is the maximum α s.t $2^\alpha = |V|$. (We use the Crabbe/Forster definition of exponentiation.)

Then we can show that

$T^2|V|$ is the maximum α such that $2^\alpha = T|V|$.

and indeed

$T^n|V|$ is the maximum α such that $2^\alpha = T^{n-1}|V|$.

for each concrete n.

Notice we do not have a uniform proof of this, since the exponent on the T is not a quantifiable variable.

That is why i am not happy......

1.22 A message from Alice in (northern) spring 2019

Hope you're keeping well! I'm afraid I've been neglecting pretty much all of my social obligations lately, so apologies for being even scarcer than usual.

I have a puzzle which may be relevant to doing a certain kind of realizability model over NF.

Say you have a strongly Cantorian, complete Heyting algebra, H. Define an H-set as a set X equipped with an H-valued equality relation (i.e. an $e : X \times X \to H$ with $e(x, y) \leq e(y, x)$ and $e(x, y) \wedge e(y, z) \leq e(x, z)$–no reflexivity requirement). A strict predicate on (X, e) is a $P : X \to H$ such that $P(x) \leq e(x, x)$ and $P(x) \wedge e(x, y) \leq P(y)$. Given a strict predicate on (X, e) we get an H-subset defined by $(X, \lambda xy.P(x) \wedge e(x, y))$.

The question: In NF, is there an H-set (U, e) such that every H-set arises by restriction of this H-set to a strict predicate? I'm trying to figure out if the appropriate category of H-sets actually has a universe; if it doesn't then that rules out one way of using realizability to get NF(U)-like business.

Looking forward to our Spring Break Rager!

-A

Xmas at the farm 2018. Zachiri points out that in the Baltimore model construction the original model injects into the Baltimore model. Send $\emptyset$ to V_ω. (What do you do subsequently??)

I think Holmes' clever permutation that kills off infinite transitive wellfounded sets will work for any infinite ordinal $\alpha > T\alpha$. For any ordinal $\alpha > T\alpha$ one orders the finite sets of ordinals below α in the clever Holmes fashion, biject, extend the bijection to a permutation and one obtains a model wherein are no infinite transitive wellfounded sets. As far as i can see none of the goodies that we have so far extracted from the Holmes permutation rely on α being Ω. Now what about finite $n > Tn$? By extending an arbitrary injection $\mathcal{P}(\{m : m < n\}) \hookrightarrow \{m : m < n\}$ to a permutation we get a permutation model in which every wellfounded set is finite. What extra information do we get if the injection is a clever Holmes injection?

See material on Holmes' weird order in logicrave.tex

Do we get a proof that every transitive wellfounded set is strongly cantorian? Notice that there are an awful lot of injections $\mathcal{P}(\{m : m < n\}) \hookrightarrow \{m : m < n\}$ and they might give us different stories. I think we should think about this!

1.23 NZF

Something to fit in here. NZF + $\exists V$ = NF; NZF + $\neg\exists V$ = ZF. Surely there is a connection here with stratified $\in$-induction? NZF + stratified $\in$-induction = ZF! Even parameter-free $\in$-induction?!
This sheds (a bit of) light on an old question, that of whether or not $\neg\exists V$ implies stratifiable parameter-free induction. It does over NZF!

It might be an idea to collect in one place all the facts known about NZF. (It's ZF $\cap$ NF). And a few questions as well, for that matter. And there is the point to be made that it is *not* obvious that we cannot have both ZF $\vdash$ Con(NZF) and NF $\vdash$ Con(NZF). The obvious argument runs: suppose we had both of these, then we would have NZF $\vdash$ Con(NZF) which we can't have beco's NZF is recursively axiomatisable, being the intersection of two recursively axiomatisable theories. The point is that it's far from 100% obvious that we can arithmetize ZF and NF in such a way that the two assertions of Con(NZF) are the same formula in $\mathcal{L}(\in, =)$. I'm guessing, nevertheless, that a single arithmetisation is available, and that the obvious argument works; spelling out the details can do no harm.

A trivial observation: NZF is recursively axiomatisable. NZF is an intersection of two semidecidable sets of formulæ and so is semidecidable. By an observation of Craig's it therefore has a decidable set of axioms. A finite set? No.

Let's prove instead the more general:

Remark 10
Let T_1 and T_2 be recursively axiomatisable theories in the same language, with T_1 finitely axiomatisable and T_2 not finitely axiomatisable.
We also need T_1 to prove at least one thing that is refuted by T_2

Then $T_1 \cap T_2$ is recursively axiomatisable but not finitely axiomatisable.

Proof:

Let $\langle A_i : i \in \mathbb{N}\rangle$ be an axiomatisation of T_2. (We do need the whole of $\mathbb{N}$ beco's it is given that T_2 is not finitely axiomatisable).

Then $\langle T_1 \vee A_i : i \in \mathbb{N}\rangle$ or (for our purposes more usefully) $\langle(\neg T_1) \to A_i : i \in \mathbb{N}\rangle$ is an axiomatisation of $T_1 \cap T_2$, where (overloading) T_1 is a conjunction of all the finitely many axioms of T_1. If $T_1 \cap T_2$ is finitely axiomatisable then it can be axiomatised by finitely many of these axioms. Any conjunction of finitely many of these axioms is logically equivalent to something of the form $(\neg T_1) \to A$ where A is a conjunction of finitely many of those A_i that axiomatised T_2. If $T_1 \cap T_2$ is finitely axiomatisable then, for some such A, $(\neg T_1) \to A$ implies each $(\neg T_1) \to A_i$. Now observe (not a lot of people know this!) that the converse of the logical principle S is a truth table tautology, so – from $((\neg T_1) \to A) \to ((\neg T_1) \to A_i)$ for each i – we can infer $(\neg T_1) \to (A \to A_i)$ for each A_i. By assumption T_1 contains theorems that contradict T_2, so $\neg T_1$ is a theorem of T_2. So we can axiomatise T_2 with the two axioms $\neg T_1$ plus A. So T_2 is finitely axiomatisable. But we know it isn't. So there is no such A. So $T_1 \cap T_2$ was not finitely axiomatisable. ■

There is a rather striking contrapositive way of putting this. Let T_1 and T_2 be theories in the same language, with T_1 and $T_1 \cap T_2$ both finitely axiomatisable. Then T_2 is finitely axiomatisable.

In particular NZF is recursively axiomatisable but not finitely axiomatisable. Notice that we have not used any assumption of Con(NF) in this calculation. After all, if ¬Con(NF) then NZF = ZF, and ZF is not finitely axiomatisable. However we have (of course) used Con(ZF). And we have used that NF and ZF contradict one another: NF ∪ ZF is inconsistent.

My guess is (always assuming Con(NF)) that NZF is extensionality, pairing, sumset, power set, infinity, transitive containment, stratified separation and (full!) collection.

Can we prove this? It would be sufficient to show, in this theory, that each (unstratified) instance of replacement follows from the nonexistence of a universal set. We would also need to show in NZF that if there is a noncantorian set, or if IO fails, or ∃NO, then there is a universal set. That all looks like a tall order. So perhaps there is more to NZF than meets the eye. And NZF + AC = ZFC!

We don't know which (if indeed either) of ZF and NF proves Con(NZF), tho' we do know that they cannot both prove it.

Let us prove something fairly general.

Remark 11
Let T_1 and T_2 be two theories. Then every model of $T_1 \cap T_2$ is either a model of T_1 or a model of T_2.

Proof:
Let $T_1 \vdash \phi_1$ and $T_2 \vdash \phi_2$. Then $T_1 \cap T_2 \vdash \phi_1 \vee \phi_2$. Suppose $\mathfrak{M} \models T_1 \cap T_2$; then $\mathfrak{M} \models \phi_1 \vee \phi_2$. If $\mathfrak{M} \not\models T_1$ then there is a $\phi_1 \in T_1$ such that $\mathfrak{M} \not\models \phi_1$; but then $\mathfrak{M} \models \phi_2$ (any of them) whence $\mathfrak{M} \models T_2$.
■

I was quite alarmed when i discovered this proof, and suspected an error, but it's quite innocent really. After all, T + ϕ and T + $\neg\phi$ are two theories whose union is inconsistent, and every model of their intersection is a model of one or other. So there's no surprise really.

Here's another proof. Suppose $\mathfrak{M} \models T_1 \cap T_2$ but it doesn't satisfy T_1 (so there is $\phi_1 \in T_1$ with $\mathfrak{M} \models \neg\phi_1$) and it doesn't satisfy T_2 (so there is $\phi_2 \in T_2$ with $\mathfrak{M} \models \neg\phi_2$). $T_1 \vdash \phi_1$ and $T_2 \vdash \phi_2$ so both $T_1 \vdash \phi_1 \vee T_2$ and $T_2 \vdash \phi_1 \vee \phi_2$. So $T_1 \cap T_2 \vdash \phi_1 \vee \phi_2$ whence $\mathfrak{M} \models \phi_1 \vee \phi_2$ contradicting assumption.

In particular this justifies the remark above that NZF + AC = ZFC.

Thus, taking T_1 and T_2 to be NF and ZF, NZF + IO axiomatises ZF; NZF + ¬IO axiomatises NF. This makes for a contrast with KF. NZF plus either of $\exists NO$, ¬IO gives NF. As far as i know it's open whether or not either of these things entail the existence of a universal set when added to KF.

Corollary 1
Let T, T', S and S' be theories with T synonymous with T' and S synonymous with S'.

Then $T \cap S$ and $T' \cap S'$ are synonymous (in the "same models" sense).

Proof:

By 11, every model of $T \cap S$ is either a model of T (in which case it can be turned into a model of T') or a model of S (in which case it can be turned into a model of S').

■

Remark 12
Let T_1 and T_2 be two theories such that $T_1 \cup T_2$ is inconsistent;
Let ϕ be a formula such that $T_1 \vdash \phi$ and $T_2 \vdash \neg\phi$.

Then $T_1 \cap T_2 + \phi \vdash T_1$ and $T_1 \cap T_2 + \neg\phi \vdash T_2$.

Proof:

Let ψ be any theorem of T_1. Then $T_2 \vdash \phi \to \psi$ and also $T_1 \vdash \phi \to \psi$, whence $T_1 \cap T_2 \vdash \phi \to \psi$. Thus $T_1 \cap T_2 + \phi \vdash \psi$. But ψ was an arbitrary theorem of T_1. So $T_1 \cap T_2 + \phi$ proves all theorems of T_1. We argue analogously for theorems of T_2.

■

Thus both T_1 and T_2 are finite extensions of $T_1 \cap T_2$.

Surely some connection here with Lyndon's interpolation lemma (which – to my shame – i have only just discovered!)

OK, we took NZF to be NF ∩ ZF. What would have turned out different had we taken it to be NF ∩ ZFC?

Notice that $T \vdash Con(T \cap S)$ iff $T \vdash (Con(T) \vee Con(S))$. The converse of the following is easy, but what of the formula itself (the hard direction)?

$$T \vdash (Con(T) \vee Con(S)) \quad \to \quad T \vdash Con(S)?$$

Try

$$T_0 = T$$

$$T_{n+1} = T_n \cup \{Con(T_n) \vee Con(S)\}$$

$$T_\infty = \bigcup_{i \in \mathbb{N}} T_n$$

WANT:

$T_\infty \vdash Con(T_\infty) \vee Con(S)$ but $T_\infty \not\vdash Con(S)$

which would be a good counterexample.

It seems to me that the *desideratum* $T_\infty \vdash Con(T_\infty) \vee Con(S)$ holds because we can argue in something very elementary (so presumably in T) that in every model of T_∞ either $Con(S)$ holds or $\bigwedge_{i \in \mathbb{N}} Con(T_n)$ holds, which should be enough to show that $Con(T_\infty)$ holds. Therefore $T_\infty \vdash Con(T_\infty) \vee Con(S)$.

Now to persuade ourselves that $T_\infty \not\vdash Con(S)$. If $T_\infty \vdash Con(S)$ then $T_n \vdash Con(S)$ for some n. id est:

$$T \vdash (\bigwedge_{i \leq n} (Con(T_i) \vee Con(S))) \to Con(S)$$

or

$$T \vdash ((\bigwedge_{i \leq n} Con(T_i)) \vee Con(S)) \to Con(S)$$

whence

$$T \vdash \bigwedge_{i \leq n} Con(T_i) \to Con(S)$$

So $T_\infty \models Con(T_\infty) \vee Con(S)$ seems to hold but $T_\infty \models Con(S)$ doesn't – as desired.

Now we can prove the inconsistency of elementary arithmetic

We proved in remark 1 above that NZF is not finitely axiomatisable. Every recursively axiomatisable theory has an independent axiomatisation, so – in particular – NZF has an independent axiomatisation. Consider the theory NZF + $\exists V$. $\exists V$ is not a theorem of NZF so this gives us an infinite independent axiomatisation of NF, by adding $\exists V$ to an independent axiomatisation of NZF. But NF is finitely axiomatisable, and clearly no finitely axiomatisable theory can have an infinite independent axiomatisation. Contradiction

Where is the mistake? Let A be the infinite independent axiomatisation of NZF. Add $\exists V$. The axiomatisation of NF that we thus obtain is not independent. Lots of things in A follow from $\exists V$.

A good thing to think about would be NZF + IO, or NZF + $\exists$NO. No, that's silly. NZF + IO is ZFC, and NZF + $\exists$NO is NF.

1.23.1 A message from Richard Kaye

A good question.

Suppose T is sufficiently strong. (T extends Δ_0 induction + exponentiation will do. $T \supseteq$ Prim rec arithmetic is more than enough.) suppose also that T is consistent, and $T + \neg con(S) \vdash con(T)$. Then, by the assumption that T is strong we have:

1. If σ is any Σ_1 sentence then $T \vdash$ 'T proves σ' (in fact, $T \vdash$ 'Q proves σ', where Q is Robinson's minimal artithmetic containing only the recursive defns of + and .)

2. the second incompleteness theorem can be formalised in T, that is: T proves ' $con(T)$ implies "T does-not-prove $con(T)$" '

Now consider an arbitrary model $\mathfrak{M}$ of T. (This is easier than writing things like T proves '... proves " ... proves ..." ' !) Suppose for the moment that $\mathfrak{M} \models \neg con(S)$. Then by (2) $\mathfrak{M}$ contains a proof from T of $\neg con(S)$, and by simple modification of these nonstandard proofs, together with the standard proof that $T + \neg con(S)$ implies $con(T)$ we have that $\mathfrak{M}$ contains a proof (of nonstandard length) of $con(T)$. But this implies, by 2, that $\mathfrak{M}$ satisfies $\neg con(T)$, for if $\mathfrak{M} \models con(T)$ then it can't have a proof of $con(T)$. Thus $\mathfrak{M} \models \neg con(T)$ and $\neg con(S)$, contradiction, so no such $\mathfrak{M}$ exists so $T \vdash con(S)$.

This argument seems to depend critically on the second incompleteness theorem formalised in the model, so it seems unlikely that the Π_1 disjunction property is possible in general. Actually, its well known that it isn't true in general. We need two facts:

3. For T extending $I\Delta_0$ + exp, as before, T proves the Matijasevic theorem so any Δ_0 formula is equivalent to existential and universal forms. This in turn means that any extension of models of T automatically preserves Δ_0 formulas.

4. There is something called the JOINT EMBEDDING PROPERTY. A theory T has JEP iff for every pair of models $\mathfrak{M}$, $\mathfrak{N}$ of T there is a third model $\mathcal{K}$ of T and embeddings $M \hookrightarrow \mathcal{K}$ and $\mathfrak{N} \hookrightarrow \mathcal{K}$. Plenty of theories have JEP. eg T_p = the theory of fields of a given charateristic p. (You don't have to say anything else, not even that the fields are alg closed.) Some don't, eg the theory of fields. (you cant jointly embed two fields of different characteristic) A well know PRESERVATION theorem says T has JEP iff whenever T proves a disjunction of purely universal sentences then it proves one of them. Unfortunately no theory extending $I\Delta_0$ + exp has JEP. (This is proved either by a neat argument involving Post's simple set, or by a double diagonalization argument, i.e. producing the disjunction explicitly.)

There's a very strange and rather weak theory of arithmetic, called Open induction + normality, which does have JEP. It's the only one we know of: weaker theories tend not to have it, and stronger theories don't either. The rather surpising result that NOI has JEP was proved by Otero recently. Unfortunately it (NOI) is too weak to talk about consistency, or prove the Matijasevic theorem.

Hope this is of interest,
Richard

Is this the place to note the old suggestion that *NF* might be the result of adding $\neg Con(T)$ to some otherwise sensible theory T. Surely it shouldn't be to hard to show that this is nonsense?

Suppose ZF $\vdash$ Con(NF) and NF $\vdash$ $\neg$Con(NF). This is a Believable Scenario. Then, by remark 12, we have NZF + $\neg$Con(NF) = NF. But since Con(NZF) $\to$ Con(NF) we have NZF + $\neg$Con(NZF) = NF.

No, hang on. That doesn't quite work: the inference Con(NZF) $\to$ Con(NF) relies on Con(ZF). Let's look at this closely. In the Believable Scenario we have NZF + $\neg$Con(NF) = NF. So we want NZF $\vdash$ Con(NZF) $\to$ Con(NF). We certainly have NZF $\vdash$ Con(ZF) $\vee$ Con(NF). So it would suffice to have NZF $\vdash$ (what?)

If NF $\vdash$ $\neg$Con(NF), and ZF $\vdash$ Con(NF), then 'Con(NF)' is one of those things that enables you to choose between NF and ZF. So NZF + Con(NF) = ZF.

No: here's what to do. Con(NZF) is a "fork". This is beco's ZF $\vdash$ Con(NF) so certainly ZF $\vdash$ Con(NZF). But NZF $\not\vdash$ Con(NZF) whence NF $\not\vdash$ Con(NZF). Every model of NZF is either a model of ZF or of NF. A model of NZF + $\neg$Con(NZF) cannot be a model of ZF so it must be a model of NF. So NZF + $\neg$Con(NZF) is at least NF. Is it no more? Nothing to say that it can't be...

NZF + Con(NZF) and NZF + $\neg$Con(NZF) must be ZF and NF respectively.

I think that works!

Having another look ...

Suppose we have two theories, T_1 and T_2, with $T_1 \vdash Con(T_2)$ and $T_2 \vdash \neg Con(T_2)$.

Then $T_1 \cap T_2 + Con(T_2) \vdash T_1$ and $T_1 \cap T_2 + \neg Con(T_2) \vdash T_2$.

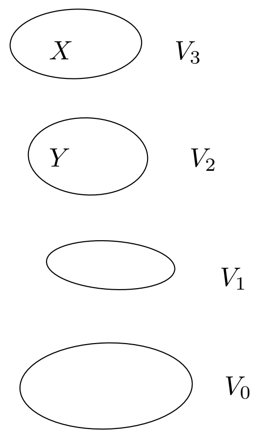
So $Con(T_2)$ is a "fork".

What about $(T_1 \cap T_2) + \neg Con(T_1 \cap T_2)$? Assuming that $T_1 \cap T_2$ knows that it's a subset of T_2, $T_1 \cap T_2$ can infer $\neg Con(T_2)$ from $\neg Con(T_1 \cap T_2)$. So $(T_1 \cap T_2) + \neg Con(T_1 \cap T_2) \vdash T_2$.

This seems quite general. If T_2 is a consistent theory that mistakenly proves its own inconsistency then it is of the form $T + \neg Con(T)$ for some T.

1.24 An Interesting Combinatorial Principle from Randall

Let $X \in V_3$ be a set. Find $Y \in V_2$ such that every permutation of V_0 that fixes Y also fixes X.



At the time two things struck me, and they stick in my mind (what remains of it):
(i) It uses four levels not three;
(ii) it *seems* to say that information about any object at level $n+1$ can be encoded at level n – one level down. That of course is deeply untrue, and that is what makes this principle interesting.

Randall says that if you have AC it's easy. Find a wellordering of $\bigcup\bigcup X$ and think of it as an ordernesting. Then that is the Y you want.

First some notation: any permutation of V_0 acts on the inhabitants of higher levels in an obvious way, and when i write " $\pi(t)$ " where t is something that obviously belongs to one of these other things then it is the obvious action of π that we have in mind.

Let π be a permutation of V_0. The action of π on V_2 will preserve $\subseteq$. Now $\langle Y, \subseteq\rangle$ – being a wellordering – is rigid, so any π that fixes Y pointwise (and therefore setwise) must fix every member of Y. (Here we need $\langle Y, \subseteq\rangle$ to be a wellorder not merely a linear order, co's we need rigidity. It may be worth checking that rigidity is *all* we need.) We want to show that such a π also fixes every member of X. But such a π must fix every member of $\bigcup\bigcup X$ and will therefore fix X.

So it worked by fixing everything in $\bigcup\bigcup X$. So we have a very simple proof of the modified version:

> Let $X \in V_3$ be a set. Find $Y \in V_1$ such that every permutation of V_0 that fixes Y also fixes X.

…with $Y \in V_1$ rather than V_2. But here we want π to fix $\bigcup\bigcup X$ *pointwise* not *setwise*.

1.25 Spectra (well, i've got to call them *something*)

Let $\mathfrak{M}$ be a model of TST, and ϕ a stratifiable expression of the language of set theory. Let the *spectrum* of ϕ (in $\mathfrak{M}$) be the set of $n \in \mathbb{N}$ such that ϕ is true at level n. In pursuit of Con(NF) we want models $\mathfrak{M}$ such that every spectrum is finite or cofinite. It might be an idea to consider what sort of subsets of $\mathbb{N}$ can be spectra. Why do i have the feeling that the Thue-Morse set cannot be a spectrum? Is it possible to arrange that every spectrum is almost-periodic (periodic except at finitely many points)? I'm guessing that it is, and that that is compatible with AC.

The set of spectra of a model of TST (or T$\mathbb{Z}$T, for that matter) forms a boolean algebra. Is it atomic?

Randall says that every set is a spectrum.

Why does he say that?

He says:

It follows from well known results about forcing. In a model of TST, frame a sentence which says "the cardinality of type i is $\aleph_n$ for odd n". This can be made true at any desired set of types by forcing constructions since you can make $(\forall n \in \mathbb{N})(\beth_n = \aleph_{f_n})$ for any strictly increasing $f : \mathbb{N} \to \mathbb{N}$

1.26 Music minus one

For any formula ϕ in $\mathcal{L}(TST)$ we can cook up a formula ϕ^* which says that ϕ holds in the model obtained by removing a single element from level zero. We fix some thing a at level 0; then we replace all occurrences of '$(\exists x_0)\ldots$' by '$(\exists x_0)(x_0 \neq a \wedge \ldots$' and replace all occurrences of '$(\forall x_0)\ldots$' by '$(\forall x_0)(x_0 \neq a \to \ldots$' similarly at higher levels. Then we bind 'a' with a quantifier. It doesn't make any difference which thing we delete, so the quantifier can be whichever of $\exists$ and $\forall$ we find more convenient. Now we need to think about the scheme $\phi \longleftrightarrow \phi^*$. It certainly follows from the assertion that the bottom level is Dedekind-infinite.

We should really show that * (or whatever we end up calling it) commutes with Booleans.

1.27 A Salutory Tale about Stratification, Variables and Recursive Definitions

This has got garbled: sort it out

Alice told me I should write this up.

We always have $x \subseteq \mathcal{P}(\bigcup x)$. Indeed we have $x \subseteq \mathcal{P}^n(\bigcup^n x)$ for every (concrete) n. And these assertions are stratifiable. There is the thought that we might obtain the union $\bigcup_{n \in \mathbb{N}} \mathcal{P}^n(\bigcup^n x)$. Let's call this object $F(x)$ and hope to prove that it always exists. Values of F look a bit like Zermelo cones, which is why they are interesting. $F(x)$ looks like a kind of natural environment for x.

Consider the function

$$f(n, x) = \mathcal{P}^n(\bigcup{}^n x).$$

It looks as if we should be able to define it in NF; after all, for each concrete n, '$x = f(n, x)$' is stratified. So we can, for every concrete n, prove that $(\forall x)(f(n, x)$ exists). Indeed we can even, for each concrete n, prove the sethood of the graph $\{\langle x, y\rangle : y = f(n, x)\}$. We can even prove further that if g is any function that is a set, the function $x \mapsto \mathcal{P}(g(\bigcup x))$ is also a set! What we can't do is prove the same about f with 'n' a variable.

This merits reflection.

So let us try to declare f by recursion on $\mathbb{N}$. Thus

$$f(0, x) =: x;\ f(n+1, x) = \mathcal{P}(f(n, \bigcup x)).$$

That is to say, f is the $\subseteq$-least set of triples extending $\{\langle 0, x, x\rangle : x \in V\}$ and closed under the operation $\langle n, \bigcup x, y\rangle \mapsto \langle n+1, x, \mathcal{P}(y)\rangle$. Observe, however, that this operation we are closing under is not stratified.

$$(\forall y)(a \in y \wedge (\forall x)(x \in y \to (\forall z)(\phi(x, z) \to z \in y))) \qquad \text{(PHI)}$$

and if we want this inductively defined collection to be a set then PHI had better be stratified. But of course it will be stratified only if ϕ is homogeneous. In the recursive declaration of f above ϕ relates $\langle n, \bigcup x, y\rangle$ to $\langle n+1, x, \mathcal{P}(y)\rangle$.

So we can't be sure that the graph of f is a set. Can we be sure that it isn't? Suppose it were. Then we would have the graph of the function $x \mapsto \bigcup_{n\in\mathbb{N}} f(n, x)$. Let's call this function F as above.

My guess is that the graph of F cannot be a set. However I am having more trouble with this than i expected. Randall says that if it is consistent that every transitive set is either V or is hereditarily finite then the graph of F might be a set. That doesn't *quite* work as it stands beco's if F is a set then $\{x : F(x) \neq V\}$ (which is definitely a set) looks as if it might be V_ω... but the point is well-made.

Consider now the function $G(x) = \bigcup\{y : F(y) \subset x \wedge F(y) \neq x\}$. The graph of G is a set, too. Check that G is $\subseteq$-monotone. So by Tarski-Knaster it has a greatest fixed point.

Thinking aloud...

Suppose Y is a fixed point. Then $Y = \bigcup\{X : Y \neq F(X) \subset Y\}$. But $X \subseteq F(X)$ so this is $Y = \bigcup\{F(X) : Y \neq F(X) \subset Y\}$. I don't seem to be reaching a contradiction.

Of course the desired F is a fixpoint for the operation that sends a function H to $\lambda x.\mathcal{P}(H(\bigcup x))$. This is a type-raising operation, and there is a theorem about fixed points for type-raising operations. If we can find x such that x and $\mathrm{op}(x)$ are n-equivalent for some n, then in a permutation model we have a fixpoint.

Is it consistent with NF that there is a function sending each x to $\bigcup_{n\in\mathbb{N}} f(n,x)$? I suspect not, it might give us the collection of all transitive sets.

Should look into this

1.28 A Conjecture about Permutation Models

Presumably the following is true: Whenever Σ is a stratified n-type that is realized by an n-tuple of wellfounded sets then there is a permutation model in which Σ is realized by an n-tuple of illfounded sets.

Let Σ be the n-type realized by all n-tuples of wellfounded sets. That is to say, Σ is the set of all the $\sigma(x_1 \ldots x_n)$ such that NF $\vdash (\forall \vec{x})(WF(\vec{x}) \to \sigma(\vec{x}))$. Can we suppose that no n-tuple of illfounded sets realizes it? What does the type contain? '$(\forall y)(y \in x)$' for one. More generally $x \neq \{y : \phi(y)\}$ for most stratified ϕ with one free variable. So what *was* the correct question?

I've had this thought more than once ... copying this in from another file

Can we characterise sensible *versus* silly illfounded sets? A Quine atom is illfounded for a silly reason, and for every n there is a wellfounded set that is n-similar to it. That is nature's way of trying to tell you that it ought to be wellfounded. I think that is the condition we want. That is to say, you are a silly illfounded set iff you are in the completion of the topology on the wellfounded sets given by the symmetry classes. Let's spell this out a bit. We have a notion of n-equivalence; take the equivalence classes to be the basic closed (we do mean closed, not open...?) sets of a topology. We then complete it, thereby adding lots of illfounded sets. These illfounded sets are all silly, useless illfounded sets, not inhabitants of the attic.

I think i am correct in saying that these are precisely the illfounded sets that can be added to a model of ZF by (Rieger-Bernays) permutation methods. I wonder if NF has a model in which all sets that are illfounded are properly illfounded. I think this would be a consequence of the assertion that if x is not wellfounded then, for some concrete n, $\bigcup^n x = V$.

Having V in your transitive closure is a sufficient condition for not being wellfounded. It's a sufficient condition even for the status of not being, for every n, n-equivalent to a wellfounded set.

Can we find an omitting types model in which ... if for every n, x is n-similar to a wellfounded set then x is actually wellfounded? Call this property $\infty\psi$wf. If all your members are $\infty\psi$wf are you $\infty\psi$wf too?

We can certainly try to omit the 1-type that says that, for each n, there is a welfounded set that is n-similar to x while insisting that $TC(x) \neq V$.

Or, again, by OTT we might perhaps obtain models in which, for all x, if x is, for each n, n-similar to a wellfounded set, then it is itself wellfounded.

Isn't this something to do with the question i consider elsewhere of when every equivalence class of a homogeneous equivalence relation contains a wellfounded set? (We are in ZF, of course)

1.29 Cardinals of high rank imply Con(NF)?

Let θ be a cardinal of very high rank, like *much* bigger than $\aleph(2^{\aleph_0})$. Consider its tree. There is an equivalence relation on cardinals which says $\alpha \sim \beta$ iff $\langle\langle\alpha\rangle\rangle$ and $\langle\langle\beta\rangle\rangle$ are elementarily equivalent. Beco's $\mathcal{T}\kappa$ is so large, this equivalence relation isn't going to be just the identity relation. The equivalence relation give us a quotient of $\mathcal{T}\kappa$, in the sense that the function sending a cardinal to its equivalence class is a graph homomorphism.

What sort of things can happen? We have a concept of *layer* in this tree, and the layers are ordered like the negative integers, with $\{\theta\}$ as the top layer. (Actually we can extend it upward of course. . .) If two cardinals from different layers are equivalent then we get a model of TST + Amb^n for some n, and this we like. If $Th(\langle\langle\alpha\rangle\rangle) = Th(\langle\langle\beta\rangle\rangle)$ then $Th(\langle\langle 2^\alpha\rangle\rangle) = Th(\langle\langle 2^\beta\rangle\rangle)$, and so on up. Eventually the two branches will join, at some cardinal κ, at which point we will have $Th(\langle\langle\kappa\rangle\rangle) = Th(\langle\langle\beth_n(\kappa)\rangle\rangle)$, where n is the numerical difference between the levels. But this theory extends Amb^n.

So suppose we don't.

Let's look closely at the quotient. The equivalence relation is finer than the equivalence relation "belong to the same layer" by assumption. Any two cardinals in the tree that launch elementarily equivalent natural models live at the same level. Is the quotient a wellfounded tree? [need to explain here what the candidate tree strux is] If it isn't then we have an infinite path through it, and that gives us a rather special extension of T$\mathbb{Z}$T, which is a second thing we should consider (might be useful).

So suppose neither of those aces take a trick; what will we be left with? We have a wellfounded tree, but this time it's a tree of theories, not a tree of cardinals, and it is of cardinality at most $2^{\aleph_0}$. Doesn't seem to do anything . . .

1.30 A conversation with Zachiri, Gothenburg 15th april 2015

We know that Zermelo can have models in which every set of infinite-sets-all-of-different-sizes is finite, but all known such models are models of AC. This raises the question: does Zermelo + $\neg AC$ prove that any set that contains infinite sets of all sizes (every infinite set is the same size as a member of it) must be infinite?

Zachiri observes that AxCount$_\leq$ $\vee$ AxCount$_\geq$ implies that every cantorian natural is strongly cantorian. This implies that the cantorian naturals are an initial segment of $\mathbb{N}$, as well as being an elementary substructure. The arithmetic of the cantorian naturals must prove con(TSTI) but for general reasons it an elementary subthingie of the arithmetic of NF: the inclusion embedding from the cantorian naturals into the naturals is elementary for arithmetic.

So what matters is that the fixed points for T should be an initial segment of $\mathbb{N}$.

1.31 An Epimorphism that doesn't split

The *Epimorphism Split* sounds like a dance.

Adam Lewicki wants an example of an epimorphism that doesn't split. My first thought was that there can't be a choice function on the ordinals, but actually that's not obvious. There certainly can't be a choice function that picks wellorderings that are pairwise disjoint.

We could deduce a contradiction from DC if we could show the following:

> *Let $\langle X, R\rangle$ be a wellordering. Then there is a wellordering $\langle Y, S\rangle$ with $X \cap Y = \emptyset$ and $|Y| \geq |X|$.*

Now i think this is correct. Let X be a wellorderable set, and consider the partition of V into X and $V \setminus X$. We would like $|V \setminus X|$ to be $|V|$, so suppose it isn't, but is smaller. But then, by Bernstein's lemma, X and $V \setminus X$ both map onto V. But X is wellorderable, so V, being a surjective image of a wellorderable set, would be wellorderable too. But it isn't.

Randall sez: consider the function that sends every wellordering W with a last element to $\texttt{butlast}(W)$.

Obvious, really.

1.32 NFU and weak compactness

Boise, 2001. Holmes is proving that if one adds to NFU the following: Choice, cantorian sets are stcan, every definable class of scordinals is the intersection of a set of ordinals and the class of scordinals then there is a coding of sets of scordinals as scordinals making the class of scordinals into a model of ZFC + the class ordinal is weakly compact. In fact the two theories are equiconsistent.

Holmes reassures me that on the whole constructions like this can be run in *NF* as well. The domain of the model can be taken to be scordinals (as above) or one can do a relational type construction or even use H_{stcan}.

1.33 Equivalents of AxCount$_\leq$

Consider the relation $Tx < y$ on $\mathbb{N}$. Let's write it 'E'. Now E is wellfounded iff AxCount$_\leq$. Thus AxCount$_\leq$ is equivalent to the assertion that we can do induction for stratified expressions over E.

Now i proved somewhere that AxCount$_\leq$ is equivalent to $\Diamond$(the graph of the comparative-rank quasiorder on V_ω is a set). So perhaps one should be able to prove something directly in the arithmetic of NF+AxCount$_\leq$. Let R be a relation on $\mathbb{N}$ satisfying

$$(\forall n, m \in \mathbb{N})((n = 0 \vee (\forall k\, E\, n)(\exists k'\, E\, m)(k\, R\, k')) \to n\, R\, m)$$

(That is as much as to say that R is a comparative-rank relation for E.) What can we prove about R using E-induction? That it is a wellfounded quasiorder?

(i) Prove by E-induction that every subset of $\mathbb{N}$ has an R-minimal member?
(ii) Prove by E-induction that $(\forall n, m \in \mathbb{N})(n\, R\, m \vee m\, R\, n)$?

(i) looks OK: any set of natural numbers containing 0 has a R-minimal member. Now suppose n to be such that, for each $m\, E\, n$, any set of natural numbers containing m has an R-minimal member. Suppose $n \in X \subseteq \mathbb{N}$. If n is R-minimal we are done. If not, then (by non–R-minimality of n) there is $m\, R\, n$ with $m \in X$ and $\neg(n\, R\, m)$. This last condition gives $(\exists k\, E\, n)(\forall k'\, E\, m)(\neg(k\, R\, k'))$

err......

Write '$x \leq^T y$' for '$Tx \leq y$'. AxCount$_\leq$ implies not only that that the strict part $<^T$ is well-founded, it implies that $\leq^T$ is a well-quasi-order. It is transitive because if $Tn \leq m \wedge Tm \leq k$ then $T^2 n \leq k$ and $k \leq Tk$ so $T^2 n \leq Tk$ and $Tn \leq k$ as desired.

For the condition concerning ω-sequences let $\langle x_i : i \in \mathbb{N}\rangle$ be an ω-sequence of distinct natural numbers. (If they're not all distinct we're home and hosed). By AxCount$_\leq$ it has a $\leq^T$-minimal element, n, say. (*) Let X be the set of elements of $\langle x_i : i \in \mathbb{N}\rangle$ that occur later in the sequence than n does. Suppose there is no $x \in X$ such that $n \leq^T x$. That is to say $(\forall x \in X)(\neg(Tn \leq x))$ which is to say $(\forall x \in X)(x \leq Tn)$. So X must have been finite, so some number appears more than once.

We need the result at *

[presumably there is an analogous result for $(\forall n \in \mathbb{N})(n \geq Tn)$. Indeed an analogous result for any endomorphism]

Is it BQO?

Is there any way one can discuss the tree of bad sequences for this WQO? In ML somehow? There is no reason for it to be a set, but if it is, it is a wellfounded tree. And if it is a proper class, then ML will think it has a rank.

1.34 Building the stratified analogue of L *very very slowly*

A tho'rt prompted by a question of Oren's...does the S hierarchy in NF satisfy anything like condensation?

$$(\forall \alpha)(\forall \mathfrak{M} \prec_{str(\Sigma_1)} S_\alpha)(\exists \beta \leq \alpha)(\mathfrak{M} \simeq S_\beta)?$$

1.34.1 A nugget by Nathan Bowler written up by Thomas Forster

Fast Food/Slow Food

This arose during the regular saturday meeting of the reading group on the literature on hereditarily symmetric sets and related topics, occasioned by the visit of Edoardo Rivello to the Cambridge NF-istes.

The background to this note is that if we construct the stratified analogue of L very slowly, collecting ("banking")[8] very often, then we might end up constructing the whole of L. The point is that every time you bank you are adding a set that is defined only as the closure of a set under stratified operations, and such a definition is typically not stratified – unless the operations are all homogeneous. So *banking adds unstratified information*. Vu said: if there is anything in L that is never going to be constructed at all, however slow we go, then there is probably such a object of very small rank. How about the von Neumann $\mathbb{N}$? I said. Nathan took up the challenge of constructing the von Neumann $\mathbb{N}$ by going slow ...

The first attempt is the stratified Δ_0 function.

$$x, y \mapsto \left\{ \begin{array}{r} x \cup \{y\} \text{ if } |x| = |\iota``y| \\ \\ = \emptyset \text{ o/w} \end{array} \right\} \tag{1.1}$$

The idea is that closing $\{\emptyset\}$ under this will give us the set of von Neumann naturals. But this function isn't Δ_0, so it doesn't work. But the idea is a good one. The next adjustment is due to Vu Dang.

The idea now is to find a stratified Δ_0 function such that closure under it will churn out the bijections we need. The following function will spit out, for each n, a bijection between the set of (von Neumann) naturals below n and the set of singletons of (von Neumann) naturals below n

$$f : x, y \mapsto x \cup \{\langle \pi_1``y, \{\{z : \{z\} \in \pi_2``y\}\}\rangle\}$$

The π_is are the unpairing functions and the angle brackets are Wiener-Kuratowski ordered pairs. This time the definition is indeed Δ_0 – and still stratified.

Let B be the closure of $\{\emptyset\}$ under f. For each $n \in \mathbb{N}$, B contains the bijection $\{\langle i, \{i\}\rangle : i < n\}$ – plus a lot of other rubbish besides (which we don't need to worry about).

$$g : x, y, z \mapsto \left\{ \begin{array}{l} x \cup \{y\} \text{ if } z \text{ is a bijection } x \simeq \iota``y \text{ and neither} \\ x \text{ nor } y \text{ contain any ordered pairs} \\ \\ = \emptyset \text{ o/w} \end{array} \right. \tag{1.2}$$

Then we close B under g. The effect is to add all the von Neumann naturals. Call the result C. We want $(C \setminus B) \cup \{\emptyset\}$. We can do this as long as we have B and C. If B is to be a set in the model then presumably we *bank* B as soon as we make it. This means that the thing we obtain at the next stage by closing

[8] The reference is to *The Weakest Link* where contestants have to *bank* their winnings every now and then. We have to do this too, every time we close under anything, since otherwise the closed set we have just obtained might not be a set of the model.

under g is not the set C we have just described but the closure of $B \cup \{B\}$. We can get round this by modifying g to

$$g : x, y, z \;\mapsto\; \begin{cases} x \cup \{y\} \text{ if } z \text{ is a bijection } x \simeq y \text{ and neither } x \text{ nor } y \\ \text{contain any ordered pairs nor even} \\ \text{anything containing any ordered pairs} \\ \\ = \emptyset \text{ o/w} \end{cases} \tag{1.3}$$

When we close $B \cup \{B\}$ under g we never pick up anything with B inside it because B contains ordered pairs. This means that the closure C of $B \cup \{B\}$ under g contains B and all members of B and all the von Neumann naturals. The von Neumann $\mathbb{N}$ is accordingly obtained as $(C \setminus (B \cup \{B\})) \cup \{\emptyset\}$. However, as Nathan observes, there's actually no need to modify g, since there's no possibility of B bijecting with anything in $B \cup \{B\}$ except itself – it's too big. The point however is well-made: at some point in our construction we have a set B, say. We close it under f_1, then under f_2, then under f_3 and so on. It does make a difference whether or not we bank after each closure. In the above case we wanted to take away everything in some set C that was the closure of a stage under an operation. So we needed S to be a set. It so happens that we could get C as a set *without* banking it but that was down to good luck not good management.

1.35 NFΩ

Here might be a useful chain of theories.... Start with NF0. Add function symbols for its operations, and call the new language $\mathcal{L}^1$. Now consider the theory whose axioms are extensionality + $\Delta_0^{\mathcal{L}^1}$ comprehension. This theory is obtained from NF0 by adding, for each NF0 word W, an axiom giving us the existence of $\{x : W(x, \vec{z}) \in y\}$ where $\vec{z}$ and y are parameters. This theory properly extends NF0, because one of its axioms is the existence, for all y, of $\{x : B(x) \in y\}$, and there are models of NF0 (e.g., the term model) where some values of this function are missing. Other examples are $\{x : \{x\} \in y\}$, and $\{x : \{x\} \cup z \in y\}$. There will be infinitely many of them.

Let us call this theory 'NF(1)' (at least for the moment – we've got to call it *something*!) Now let $\mathcal{L}^2$ be the language obtained from $\mathcal{L}^1$ by adding function letters for all the operations that NF(1) says that the universe is closed under.

Now consider the theory whose axioms are extensionality + $\Delta_0^{\mathcal{L}^2}$ comprehension. This will of course be notated 'NF(2)'. We can keep on doing this, obtaining languages $\mathcal{L}^n$ and theories NFn (extensionality + $\Delta_0^{\mathcal{L}^n}$ comprehension) for each $n \in \mathbb{N}$. Let the union of the languages be $\mathcal{L}^\Omega$ and the corresponding theory 'NFΩ' be extensionality + $\Delta_0^{\mathcal{L}^\Omega}$ comprehension.

Observe that, for each n, NF(n) has a $\Pi_2^{\mathcal{L}^n}$ axiomatisation.

Let us define Ω-formulæ and Ω-terms by a simultaneous recursion.

An **Ω-term** is either a variable or an expression of the form '$\{x : \psi(x, \vec{t})\}$' where ψ is a stratified Ω-formula and the $\vec{t}$ are Ω-terms. An **Ω-formula** is a boolean combination of expressions $t = t'$, $t'' \in t'''$ where t, t', t'' and t''' are Ω-terms.

NFΩ is extensionality plus the existence of $\{x : \phi(x, \vec{z})\}$ where ϕ is a stratified Ω-formula.

How well behaved are these theories? It turns out that NF(1) is consistent and that the term model for NF0 is a model of NF(1). It turns out that NF(2) properly extends NF0 and the term model for NF0 is not a model of NF(2).

First we show that the term model for NF0 is a model of NF(1). We must show that every word of the form $\{x : W_1(x) \in W_2(x)\}$ or $\{x : W_1(x) \in W_2(x)\}$ (where W_1 and W_2 are NF0 words) is equal to an NF0 word. The way to do this is to show that every such word (equation or membership-statement) is equal to a boolean combination of equations and membership-statements between shorter words.

Consider the set abstract $\{x : W_1(x) \in W_2(x)\}$. $W_2(x)$ will be a boolean combination of NF0 words in the generator 'x' with a finite amount of modification by addition or deletion of singletons. So $\{x : W_1(x) \in W_2(x)\}$ will be a boolean combination of things of the form $\{x : W_3(x) \in W_1(x)\}$ and singletons of shorter words, so clearly we have a recursion on our hands.

What about $\{x : W_1(x) = W_2(x)\}$? Both $W_1(x)$ and $W_2(x)$ are boolean combinations of B of shorter terms with a finite amount of modification by addition or deletion of singletons. As before, we can only have $W = W'$ when the things that W is a boolean combination of are pointwise identical with the things that W' is a boolean combination of. So, again, we have reduced it to a finite combination of smaller problems.

Eventually we will have reduced both $\{x : W_1(x) = W_2(x)\}$ and $\{x : W_1(x) \in W_2(x)\}$ to boolean combination of terms of that flavour – which cannot be reduced any further. These bedrock terms are things like $\{x : W_1(x) = W_2(x)\}$ and $\{x : W_1(x) \in W_2(x)\}$ where at least one of $W_1(x)$ and $W_2(x)$ are atomic – and these are taken care of by NF0 words.

But this tells us that the term model for NF0 is in fact a term model for NF(1). Why? Well, any NF(1) word can be thought of as a syntactic tree. We look inside this tree for the lowest occurrences of NF(1) constructors. But – as we have just seen – any such subterm can be replaced with an NF0 term. Thus we can ratchet our way up the syntax tree and eventually end up with an NF0 term.

However, we cannot extend this to NF(2). This because, altho' every NF(1) word (without generators) is equal to (has the same denotation in all models) as an NF0 word, nevertheless an NF(1) word with a generator is not reliably equal to an NF0 word in that generator. This is certainly the case – since $\{x : B(x) \in y\}$ is not an NF0 word in 'x' – and it may matter. Of course $\{x : B(x) \in t\}$ is an NF0 word whenever t is an NF0 term. But that isn't enough. The killer is the NF(2) term $\{x : \{y : B(y) \in x\} \in \{z : \{z\} \in x\}\}$ (also known as $\{x : \{B^{-1}``x \in x\}$). It should be easy to show that this cannot have the same denotation as any NF0 term.

Why might this be interesting? I can think of two reasons. One is that NF is finitely axiomatisable. One dispiriting consequence of this is that any infinite hierarchy of subsystems of NF either reaches NF in finitely many steps or never reaches it at all – usually the former. This system NFΩ is either going to be equal to NF – in which case one of the NF(n) is already equal to NF – or it is strictly weaker, and might offer a stepping stone – in the sense that it might be possible to prove it consistent and also prove NF consistent relative to it. Finally it's a nice theory because in the language with all the function symbols it has a $\forall^*\exists^*$ axiomatisation.

But observe that the theory NF$\exists$ (aka NF$\forall$) also has a $\forall^*\exists^*$ axiomatisation. Simply add a function letter for each axiom and then lots of axioms to tell you what the operations mean, such as $\forall x \forall y(x \in \mathcal{P}(y) \longleftrightarrow x \subseteq y)$ – and all such axioms are $\forall^*\exists^*$. .

1.36 Co-term models

Term models are inductively defined sets: they are manifestations/denotations of the (inductively defined) set of words in a suitable language. There is of course also the *co*-inductively defined set of (co-)words, which are of course infinite . . . streams. What about these coinductively defined analogues?

Is it by omitting types that we prove the existence of such models?

If T is an algebraic theory then it has term models. The interesting cases from our point of view are theories T that, in addition to having axioms that say that the universe is closed under certain operations, have annoying extra axioms such as extensionality whch might prevent the family of T-terms from being a model of T.

Consider NF0. A co-term model of NF0 is a model of NF0 in which every object is a boolean combination of B objects $B(x)$ and singletons $\{y\}$, where the xs and ys are themselves boolean combinations of . . . . But this is first-order isn't it? "$(\forall x)$ there is a finite set of sets and a string of connectives such that . . . " Or, if we don't want quantifiers over finite sets, we can do it by omitting the 1-type Σ^c that says

$$(\forall y)(x \neq B(y)),\ (\forall y)(x \neq V \setminus B(y)),\ (\forall y)(x \neq \{y\}),\ (\forall y)(x \neq V \setminus \{y\}), \ldots \quad (\Sigma^c)$$

Then there is the type Σ^i that one has to omit to get a term model. Anything that realizes Σ^c will realize Σ^i, but there is no reason to expect the converse. So it might be that it is easier to omit Σ^c than it is to omit Σ^i.

For some theories T the theory of co-term models of T is axiomatisable. If T has two operations f and g then the theory of a co-term model of T is just $T + (\forall x)(\exists y)(x = f(y) \vee x = g(y))$, so it's first-order. If T has infinitely many operations f_i then a co-term-model of T is one that omits the type

$$\Sigma_{i \in \mathbb{N}} (\forall y)(x \neq f_i(y)) \quad (\Sigma^c)$$

So:

- for T to have a term model is for T to have a model that omits the type Σ^i;
- for T to have a co-term model is for T to have a model that omits the type Σ^c.

If T locally omits Σ^c then it certainly locally omits Σ^i.

If T has a co-term model then it must have a term model, since the term model is a substructure of the co-term model that is closed under everything under the sun. If the co-term model is a model of T (by satisfying extensionality or whatever) then presumably the term model is too.

T might have a co-term model for trivial reasons. If T has a pair of operations f and g that are inverse then clearly everything is the denotation of the stream $fgfgfg\ldots$. (NF is such a theory, because of ι and $\bigcup$). Is there a nontrivial notion of co-term model to be had for such theories?

So are there theories T with lots of operations that don't have to have inverses, such that T might not have a term model (perhaps beco's of problems like those we have with extensionality in NF) but where T perhaps has a co-term model?

With the term model it is clear what equality is. Not so clear with the co-term model: any bisimulation on the family of streams will do.

What about NF0? The set of NF0 words defines a unique model. This model satisfies every $\forall^*\exists^*$ sentence consistent with NF0. Now consider the co-term model. It's not clear that the set of co-words defines a unique model, nor that that structure has a decidable theory. There may be lots of different ways of turning the set of co-terms into a model.

To get a feel for what is going on, consider a particular theory and a particular co-term model: T$\mathbb{Z}$T0 and its co-term model. What might equality be in this structure? If we have a notation that does not distinguish $x \cup y$ from $y \cup x$ then we have a strict identity that is simply identity of strings. But then there is also a maximal bisimulation. But are these two not exactly the same? So what is $\in$ between these things? There is available to us the same recursion as in the term model case, and the freeness of the constructors will ensure that it usually halts. When might it not? Well, ask whether B^∞ at level n is a member of the B^∞ at level $n+1$. That enquiry never halts. That seems to be about it. The corresponding enquiry about ι^∞ gets the prompt answer 'yes'.

The problem with the co-term model is of course extensionality. I have found myself wondering if the inclusion embedding from the term model into the co-term model is elementary ... and indeed it is. Suppose the co-term model satisfies $(\exists y)\phi(\vec{x}), y)$ where the $\vec{x}$ are from the term model. The parameters $\vec{x}$ are all k-symmetric for k sufficiently large, so think about some type at least k below all the variables in ϕ. Any permutation of this type will fix all the parameters, so we want one that will move the witness y to a denotation of a T$\mathbb{Z}$T0 term. Now, because y is in the co-term model, it can be expressed as some complicated horrendous word in B and ι and the booleans over a lot of generators at level $-k-1$. There are only finitely many of these generators, and there are infinitely many denotations-of-T$\mathbb{Z}$T0-words to swap them with. Let π be one such permutation. It fixes all the $\vec{x}$s and swaps y with a denotation of a T$\mathbb{Z}$T0 word.

(Do we need all permutations of finite support to be setlike in the term model and co-term model? Perhaps we do, but – fortunately – they are!)

What does this rely on? It'll work for any extension of T$\mathbb{Z}$T0 all of whose constructors are type-raising. The other thing we are exploiting is the feature that, for any x in the co-term model and any k, x is k-equivalent to a denotation of a closed term. This reminds me of the condition that cropped up in the attempt Randall and i made to prove the existence of a symmetric model of T$\mathbb{Z}$T: For every x and every k, x is k-equivalent to a symmetric set. So presumably every model in which this is true is elementarily equivalent to a term model. . . ?

Now what about the theory (NFP? . . . NFI. . . ?) which becomes NF when you add the axiom of sumsets? Is it axiomatisable exclusively with extensionality plus axioms giving closure under type-raising operations? If so, does its typed version have a term model/co-term model? Presumably we can do the same trick to show that the term model is an elementary substructure of the co-term model.

I think the same argument will prove that the inclusion embedding

$$V_\omega \hookrightarrow \bigcup\{X : X \subseteq \mathcal{P}_{\aleph_0}(X)\}$$

is elementary for weakly stratified formulæ.

However the same construction will not prove that the inclusion embedding

$$\bigcap\{X : \mathcal{P}_{\aleph_1}(X) \subseteq X\} \hookrightarrow \bigcup\{X : X \subseteq \mathcal{P}_{\aleph_1}(X)\}$$

is elementary for (weakly?) stratified formulæ. We could prove that the inclusion embedding

$$V_\omega \hookrightarrow \bigcup\{X : X \subseteq \mathcal{P}_{\aleph_0}(X)\}$$

is elementary for stratified formulæ because everything in V_ω is symmetric. Sadly not everything in $\bigcap\{X : \mathcal{P}_{\aleph_1}(X) \subseteq X\}$ (aka $H_{\aleph_1}$ or HC) is symmetric. Can we do anything similar to this for HS. . . ? We'd need a model in which, for every set x and infinitely many n, x is n-similar to something in HS.

There does seem to be a general question here. . . if $F : V \to V$ is an operation on sets, for which class Γ of formulæ is the inclusion embedding from the lfp for F into the gfp for F elementary?

Consider the following structure $\mathfrak{M}$ for $\mathcal{L}(\mathrm{T}\mathbb{Z}\mathrm{T})$. Level $-n$ is the set of finite subsets of level $-n-1$. At positive levels, we stipulate that level $n+1$ be the set of almost-symmetric subsets of level n. What is "almost-symmetric"? A set

x at level n is almost symmetric iff there is a finite subset H of level 0 such that every permutation fixing H pointwise will also fix x when it acts n levels up.

Observe that this ensures not only that every set at level n is almost-n-symmetric in the old (FM) sense of almost n-symmetric (when you were almost-n-symmetric iff your support n levels down was finite), it ensures that every set at level n is almost-k-symmetric (in the old sense of almost k-symmetric) for *every* $k > n$! Suppose x is a set at level n. It is almost-n-symmetic, with support H, say. H is finite, and so is $\bigcup^{(k-n)} H$. But then x is almost-k-symmetric, with support $\bigcup^{(k-n)} H$.

This is a key feature, since it was its lack in the earlier attempt by Holmes and Forster that caused that attempt to fail. This structure $\mathfrak{M}$ looks like a Fraenkel-Mostowski model of TST grafted onto a ω^* root-stock where each level is the coinductive object corresponding to V_ω. (What is that object called??)

Now consider a shifting ultraproduct of this structure. (To be precise: for each $i \in \mathbb{N}$, let $\mathfrak{M}^{(i)}$ be the result of relabelling the types of $\mathfrak{M}$ so that level i of $\mathfrak{M}$ is level 0 of $\mathfrak{M}^{(i)}$). Let $\mathcal{U}$ be a nonprincipal ultrafilter on $\mathbb{N}$. Then the *shifting ultraproduct* $\mathfrak{M}^{\mathcal{U}}$ is simply an ultraproduct of the $\mathfrak{M}^{(i)}$.) What happens in it? If x is a element of level 0 of the shifting ultraproduct $\mathfrak{M}^{\mathcal{U}}$ then it is an object whose ith coordinate is an element of $\mathfrak{M}$ that is almost-j-symmetric for every $j \geq i$ – and therefore almost-j-symmetric for all sufficiently large j. So (in $\mathfrak{M}^{\mathcal{U}}$) x *ought* to be almost-j-symmetric for all sufficiently large j. Of course that inference is blocked because "almost-j-symmetric for all sufficiently large j" isn't first-order. However we do get *something*.

$\mathfrak{M}^{\mathcal{U}}$ is part of the way to our goal of a model in which every set is almost-k-symmetric for all sufficiently large k. To obtain such a model we have to omit (at each level) all the 1-types:

$$\{ \text{“}x \text{ is not almost-}k\text{-symmetric”}: k > i\}$$

for every $i \in \mathbb{N}$. The extended omitting types theorem tells us that we can do this if we have a theory T that locally omits all these types. The obvious candidate for such a theory is $Th(\mathfrak{M}^{\mathcal{U}})$. So what we need to establish is that $Th(\mathfrak{M}^{\mathcal{U}})$ locally omits each of these types.[9] Let us write 'T' for '$Th(\mathfrak{M}^{\mathcal{U}})$'.

Suppose ϕ is such that $T \vdash (\forall x)(\phi(x) \to x$ is not k-symmetric for any $k > i)$. Then, for each $k \in \mathbb{N}$, $T \vdash (\forall x)(\phi(x) \to x$ is not k-symmetric). This is first-order, and so must be true in each factor, which is to say, in a large set of the $\mathfrak{M}^{(i)}$. But then nothing in $\mathfrak{M}^{(i)}$ can be ϕ. So nothing in $\mathfrak{M}^{\mathcal{U}}$ can be ϕ either. But this says that T locally omits the 1-type:

{ "x is not almost-k-symmetric": $k > i$}

as desired.

Let's pause to draw breath ... and recycle some letters ...

[9]Parenthetical remark: "$Th(\mathfrak{M})$ locally omits Σ" is not obviously the same as "$\mathfrak{M}$ omits Σ". If "$Th(\mathfrak{M})$ locally omits Σ" then whenever $Th(\mathfrak{M}) \vdash \phi(x) \to \sigma(x)$ for all $\sigma \in \Sigma$ then $Th(\mathfrak{M}) \vdash (\forall x)\neg\phi(x)$. $\mathfrak{M}$ might realise Σ, but whenever ϕ is a property that holds of an x that realises Σ then ϕ also holds of some y that does *not* realise Σ.

Let $\mathfrak{M}$ be a model of TℤT in which every set is almost-k-symmetric for all sufficiently large k. We will show that the substructure of $\mathfrak{M}$ consisting of the symmetric sets of $\mathfrak{M}$ is elementary.

Suppose $\mathfrak{M}$ satisfies $(\exists y)\phi(\vec{x}), y)$ where the parameters $\vec{x}$ are symmetric sets. The parameters $\vec{x}$ are all k-symmetric for some k suitably large, so think about some type at least k levels below all the variables in ϕ. Any permutation of this type will fix all the parameters, so we want one that will move the witness y to a symmetric set. Now y is almost-k-symmetric, and it has finite support. Just find a permutation π that moves everything in the support of y to something symmetric (hereditarily finite will do). π has now moved y to something y' whose support consists of hereditarily finite sets of rank $< j$ for some j. But now any permutation $k + j$ levels down fixes everything in the support of y'. But this means that y' is $k + j$-symmetric. And of course y' is also a witness to $(\exists y)\phi(\vec{x}), y)$ – because the parameters (being $\leq k$-symmetric) are fixed.

seems to be some duplication here

How difficult is it to show that all single transpositions are setlike? [let us reserve 'τ' as a variable to range over single transpositions (a, b).]

I think it is true in the term model for TℤT0 that all transpositions are setlike. Any transposition is certainly 1-setlike, because τ"x is either x, or $(x \cup \{a\}) \setminus \{b\}$, or $(x \cup \{b\}) \setminus \{a\}$, and all these things exist. There is no easy move to be made at the next level up, but if the model we are working in is a term or co-term model then we have other tricks up our sleeve.

To see this trick, start by thinking about what $j^2(\tau)$ does to $B(x)$. If x is a or b it sends it to B of the other one, and o/w $B(x)$ is fixed. So all these values exist. In general, an element of the term model or co-term model is a boolean combination of singletons and principal ultrafilters. For $n > 0$, $j^n(\tau)$ commutes with the boolean operations, so we will be OK if we know how to define $j^n(\tau)$ on singletons and principal ultrafilters.

We conclude that in the term model or any co-term model every permutation of finite support is setlike.

Again, what does this depend on? Will not the same work for term and co-term models of any fragment of TℤT that has type-raising operations only?

Later (9/vi/252) This is easy! A transposition is a set, and so are all its lifts. And TℤT has replacement for functions that are sets. Duh!

The almost-symmetric sets of TZTstandard.tex have the property that they are n-equivalent to symmetric sets, but only for some n, not infinitely many, which is what the above arguments need. They behave a bit like the denotations of co-terms. Perhaps what we want is a model of TℤT containing at each level all possible illfounded hereditarily finite sets. Then we say a set is almost-symmetric if it has a finite family of these things as its support. Observe that a set that is almost-symmetric in this sense is indeed n-equivalent to a symmetric set for infinitely (cofinitely!) many n!!

I think i can show that there is no relation $R \subseteq \iota``V \times V$ which is extensional and "skew–well-founded": $(\forall X \subseteq V)(\exists x \in X)(\forall y \in x)(\langle \neg(\{x\}, y\rangle \in R))$.

Suppose there were such an R. We could then copy it onto a relation S on a moiety of a rather special kind. We want $\bigcup dom(S)$ to be disjoint from $rn(S)$ so that we can do a Rieger-Bernays model with the permutation $\prod_{x \in \bigcup(dom(S))}(x, \bigcup R^{-1}``x)$ which will give us a wellfounded set the same size as a moiety – which we know to be impossible by a theorem of Bowler which says that any wellfounded set is smaller than $\iota^k``V$ for every concrete k.

Details: First split V into two moieties A and B. Further split A into A_1 and A_2. We must set up the copy of R as a relation between singletons of things in A_1 and things in A_2.

This matters because any CO model of NF has an (external) engendering relation on it which is wellfounded and not too far from being extensional. Let me explain. There will be a relation $\mathcal{E}$ such that, for any x, $|\{y : \mathcal{E}^{-1}``\{y\} = \mathcal{E}^{-1}``\{x\}\}|$ is small, being the size of the set of wands. $\mathcal{E}$ thus isn't extensional, but extensionality doesn't fail *badly*.

If $\mathcal{E}$ were extensional it would correspond to an injection from $V \hookrightarrow \iota``V$. This weaker condition says it corresponds to an injection $\hookrightarrow \iota``V$ from not V but a partition of V into countable pieces. Can we generalise Bowler's argument to exclude that?

1.37 Fixed points for type-raising operations

I proved a theorem about this that i need to review. I think the thought ran along the following lines.

Lemma 1
Suppose f is a definable n-stratified inhomogenous function that raises types by 1. Then $x \sim_n y \to f(x) \sim_{n+1} f(y)$.

Proof: '$z = f(x)$' is stratified with z one type higher than x. Suppose further that $x \sim_n y$ beco's $(j^n(\sigma))(x) = y$.

Then we reason:

$$z = f(x)$$

iff

$$j^{n+1}(\sigma)(z) = f(j^n(\sigma)(x)).$$

which is to say, since $z = f(x)$,

$$j^{n+1}(\sigma)(f(x)) = f(j^n(\sigma)(x)).$$

and thence

$$j^{n+1}(\sigma)(f(x)) = f(y).$$

(since $(j^n(\sigma)(x) = y)$.
But this is merely to say that
$f(x) \sim_{n+1} f(y)$ in virtue of σ
Now

$$z = f(j^n(\sigma^{-1}(y)) \longleftrightarrow j^{n+1}(\sigma)(z) = f(y).$$

Now this gives us a strategy for finding fixed points for f in Rieger-Bernays permutation models.

Suppose i want $V^\pi \models (\exists x)(x = f(x))$
This is just

$$(\exists x)(\pi_{n+1}(x) = f(\pi_n(x)))$$

relettering $\pi_n(x)$ as y

$$(\exists y)((j^n\pi)(y) = f(y))$$

So, if we want a permutation model containing a fixed point for an operation f that raises types by 1, it suffices to find a permutation π that sends some y to $f(y)$.

1.38 Recursive APGs

How about getting a model of *i*NF using recursive APGs

A recursive APG is an APG whose domain is the natural numbers and whose graph is a recursive subset of $\mathbb{N} \times \mathbb{N}$. A possible world is a general recursive function. Given two RAPGs A and B a world W believes $A = B$ iff

(i) For every child a of A there is a child b of B such that some value of W is a function that maps a to b. and conversely

(ii) For every child b of B there is a child a of A such that some value of W is a function that maps b to a. (The possibility of the value of W that does the work not being 1-1 takes care of the contraction condition.)

1.39 Almost-symmetric sets again

If we can find, by hook or by crook, a model of T$\mathbb{Z}$T wherein for every x there are infinitely many concrete k such that there is a (concrete) n such that x is almost-k-n-symmetric then we can have a model of T$\mathbb{Z}$T in which every set is symmetric. On that much we are agreed. So can we find such a model?

Let M be a model of T$\mathbb{Z}$T. Let M_0 be the FM model whose bottom level (type 0) is the level 0 of M, built as a substructure of M, so that its atoms are just the elements of M of level 0.

Next let M_1 be that substructure of M_0 obtained by retaining only those atoms of M_0 that are finite-or-cofinite subsets of M_0 and then sticking on the bottom the level -1 of M to obtain a model of TST whose bottom type is labelled '-1'

Next let M_2 be that substructure of M_1 obtained by retaining only those atoms of M_1 that are finite-or-cofinite subsets of M_1 and then sticking on the bottom the level -2 of M to obtain a model of TST whose bottom type is labelled '-2'

and so on. What happens? I think that if you are an element of level k of M_i then you are almost k-symmetric, almost $k+1$-symmentric ... almost $k+i$-symmetric. Now take an ultraproduct of these M_i. This ought to give us a model in which if you are almost k-symmetric you are almost m-symmetric for all $m > k$.

Perhaps we need to be more subtle

If $\mathfrak{M}$ is a model of T$\mathbb{Z}$T and S a notion-of-symmetry, let us say a set x is almost-n-k-symmetric (in the new sense) iff there is a k-sized subset y of the universe n levels down (aka V_{-n}), all of whose members are symmmetric-in-the-sense-of-S, such that x is fixed by all permutation of $V_{-n} \setminus y$. I have just described a way of getting a new notion-of-symmetry from an old one. We want a fixed point for this operation that gives an extensional family of sets. Clearly the operation is monotone wrt $\subseteq$. Ordinary old symmetry is a fixed point, but we don't know that the symmetric sets are extensional. There will be a greatest fixed point ...

1.40 Notes on the seminar of the gang of four

Zachiri asks: do we know of any structures that obey stratified separation and choice but fail at least some of unstratified separation?

In asking this he is lowering his sights slightly from the project to find a model of KF + $\exists NO$! Answer: yes, but infinity fails as well. Work in NFU + $\neg$AxCount$_\leq$ so there is $n \in \mathbb{N}$ with $n < Tn$. Then there is $k \in \mathbb{N}$ with $k > 2^{Tk}$. If we now do the Ackermann permutation we get a set that looks like V_ω, so it's a model of the stratified axioms of ZF but Cantor's theorem fails – since the diagonal set that would prove Cantor's theorem does not exist.

While we are on that subject it seems to be an open question in NF whether the power set of NO is bigger than NO or small or incomparable. It clearly can't be same size. If it can be smaller than we do the following: work in NFU, in a model where P(NO) is smaller than NO, and consider the proper class of hereditarily wellordered sets. It'll be a model for stratified replacement, power set and choice but presumably not unstratified separation. Well, you did ask!

1.40.1 Inductively defined sets

Thierry has a nice observation: let P be a property which is possessed by every transitive set. Then the least fixed point for

$$x \mapsto \text{set of all } P\text{-flavoured subsets of } x \tag{A}$$

is paradoxical.

The point about paradoxical least fixpoints for operations like this (vary P *ad libitum*) is that they seem to be paradoxical iff P is is some sense *unbounded*.

I have been writing up a section on inductive definitions in NF for the handbok article. This suggests to me an axiom for NF:

Let P be a set that misses at least one transitive set. Then the least fixed point for (A) above is a set.

Might this be consistent?

$$(\forall P)(\forall x)(\bigcup x \subseteq x \notin P \to (\exists X)(\forall y)(y \in x \longleftrightarrow (\forall Y)((\forall z)(z \subseteq Y \wedge z \in P \to z \in Y) \to y \in Y)))$$

$$(\forall P)(\forall x)(\bigcup x \subseteq x \notin P \to (\exists X)(\forall y)(y \in x \longleftrightarrow (\forall Y)((\mathcal{P}_P(Y) \subseteq Y) \to y \in Y)))$$

There are transitive sets (V) that are not wellordered, so this will tell us that the set of hereditarily wellordered sets (lfp) is a set. Similarly we get the set of all wellfounded hereditarily cantorian sets, the set of all wellfounded hereditarily strongly cantorian sets

This can now be deleted i think

Thierry,

thank you for your clarification. I think i now understand what the closure of the class of wellfounded sets is, and why. Tell me if i have got this right.

If we think of wellfounded sets inductively (which is the only sensible way to think of them) then a set is wellfounded iff it belongs to every set that contains all its subsets. We call this WF* (becos WF is to be the class of all wellfounded sets) So WF* is the intersection of all sets that extend their own power sets. This ought to be a paradoxical object. However, if you look closely, the proof of the contradiction relies on our ability to perform what Allen Hazen calls *subcision*

$(\forall xy)(\exists z)(z = x \setminus \{y\})$

And subcision fails in GPC. Subcision would give us WF from WF*, and WF is paradoxical!

How do we know that WF* is unique? Might there not be lots of sets WF^* such that $WF^* \setminus \{WF*\} = WF$? No there can't be. Suppose there were, then we could take the intersection of all of them – which would be a set beco's an arbitrary intersection of closed sets is closed – and that intersection would be WF.

This reminds me of something – and it may be pure coincidence. If we look at this a bit more closely it shows not only that there can be at most one WF^* such that $WF^* \setminus \{WF^*\} = WF$; it shows that i cannot have two sets WF_1 and WF_2 such that

$WF_1 \setminus \{WF_2\} = WF_2 \setminus \{WF_1\} = WF$

and so on for larger finite loops. What this reminds me of is a conjecture in NF – the universal-existential conjecture:

There is a model of NF satisfying simultaneously every $\forall\exists$ sentence individually consistent with NF. One thing that appears to be consistent is

$(\forall y_1 y_2)(y_1 \setminus \{y_1\} = y_2 \setminus \{y_2\} \to y_1 = y_2)$

and a similar version for loops

$(\forall y_1 y_2)(y_1 \setminus \{y_2\} = y_2 \setminus \{y_1\} \to y_1 = y_2)$

The nonexistence of Quine atoms is a special case. One reason why counterexamples to these assertions are pathological is that they can violate $\in$-determinacy.

So three things seem to be connected:
(i) the failure of subcision needed to avoid Mirimanoff's paradox in GPC;
(ii) the universal-existential conjecture for NF and
(iii) $\in$-determinacy.......

1.41 Inductive definitions

In ZF we cannot in general define inductively defined sets "top-down" as the intersection of a suitably closed family of sets. This is because we cannot – on the whole – rely on there being a set that contains the founders and is closed under the operations in question. (A good illustration of this is the difficulty we have in proving that the collection of hereditarily countable sets is a set.) We can do it only "bottom-up" by recursion over the ordinals. It doesn't much matter how we implement ordinals, and in principle any sufficiently long wellordering will do. There's the rub: how do we know that there always *is* a sufficiently long wellordering? That's where Hartogs' theorem comes in. It tells us that if a recursive definition crashes, it won't be for shortage of ordinals. In NF the existence of big sets restores the possibility of direct top-down definitions of inductively defined sets: any inductively defined set that can be defined at all can be given direct "top-down" definition. (This is for the gratifyingly simple reason that – whatever your founders and operations – the universal set contains all founders and is closed under all operations, so when we take the intersection of the set of all sets containing the founders and closed under the operations we are not taking the intersection of the empty set.) Thus we obtain the effect of Hartogs' theorem without actually having the theorem itself.

However, altho' such inductive constructions as can be executed at all can be executed in the direct top-down fashion, it is still possible to import ordinals into a description of this activity. Suppose our inductive construction starts from a set X with a stratified definition (so it is $\{x : \phi\}$ for some stratified formula ϕ with one free variable) and we want to obtain the least superset of X closed under some infinitary homogeneous operation. Examples would be: union of countable subsets; or $F(X) := \{y : (\exists f : y \twoheadrightarrow X)(f \text{ is countable-to-one})\}$. The collection of F-stages is the least set containing X, and closed under F and unions of chains. It is of course a set, and it is – for the usual reasons – wellordered by $\subseteq$. Therefore one can associate an ordinal with every F-stage.

(As usual there are several ways of doing it: (i) the set of stages and the set of ordinals are alike wordered so there is a canonical map between them; (ii) each stage bounds an initial segment which has an ordinal for its length. (ii) is guaranteed to work even tho' (i) isn't.)

Now we are in a position to find an echo of the ZF way of doing things. The closure ordinal is in a weak sense well-behaved. It must at least be cantorian. Let f be the map that sends the ordinal α to the αth stage in the construction. f has a stratified definition without parameters, so the expression

$$f(\alpha) = f(\beta) \longleftrightarrow f(T\alpha) = f(T\beta)$$

is stratified (fully stratified: it has no parameters) and can be proved by induction on ordinals. This means that if α is the closure ordinal (that is to say, the least β such that $f(\beta) = f(\beta + 1)$) then so is $T\alpha$.

It would close the circle very nicely if we knew that every closure ordinal of a stratified recursion were strongly cantorian, but i see no proof. Perhaps it's a very strong assumption. It would follow from Henson's axiom CS ("Every wellordered cantorian set is strongly cantorian" and i think NF + CS is as strong as ZF). Is that why Henson thought of it. . . ?

1.42 Hereditarily Strongly Cantorian Sets

Hstcan is the $\subseteq$-smallest class that contains all its strongly cantorian subsets. No reason to suppose it's a set, but if it is, it's wellfounded. I'm not expecting to be able to prove that it isn't a set.

Suppose V_ω exists. Then it contains sets of all finite sizes.

If counting fails, then V_ω contains all its stcan subsets and is therefore a superset of Hstcan, but is not equal to it, and Hstcan would not be a set.

If Hstcan is a set, so is the set of natural numbers that are cardinals of its members, so we can prove the axiom of counting.

If Hstcan is a set then it isn't stcan, so it isn't countable: it will be quite large.

Randall sez V_ω might be Hstcan . . . but in those circumstancs i think neither of them would be sets.

1.43 Extracted Models and Skew-injectivity

Fit this in somewhere.
Think of T$\mathbb{Z}$T as a theory in the one-sorted language $\mathcal{L}(\in, =)$ of Set Theory, so we can use Rieger-Bernays permutation methods.
Work in a model $\mathfrak{M} \models$ T$\mathbb{Z}$T. Equip it with the new membership relation $\in_\tau$ where τ is the product of all transpositions $(\{x\}, \iota``x)$. That is, $x \in_\tau y$ iff $x \in \tau(y)$. This is unproblematic beco's τ fixes each level setwise. $\mathfrak{M}^\tau$ is elementarily equivalent to $\mathfrak{M}$. This is because we can think of this as an RB construction going on in the one-sorted world, and RB constructions preserve stratified expressions. Observe that $\iota : \mathfrak{M} \hookrightarrow_e^{\mathcal{P}} \mathfrak{M}^+$ is a $\mathcal{P}$-embedding. This (i think) shows that T$\mathbb{Z}$Tproves Amb$(\Sigma_1^{\mathcal{P}})$. Maybe we

can get something by iterating and consider the direct limit which (i think) will be a model of T$\mathbb{Z}$T.

This topic has its roots in Rieger-Bernays permutation models. The permutation $\prod(f(x), f``x)$ of all transpositions $(f(x), f``x)$ can give rise to interesting permutation models. A basic condition that makes life easier is for all the transpositions $(f(x), f``x))$ to be disjoint, and this is ensured if f satisfies the condition

Definition 3 *A function f is* **skew-injective** *iff* $(\forall xy)(f(x) = f``y \to x = y)$

For example, the singleton function ι is skew-injective, as follows:
Suppose $\{x\} = \iota``y$. Then, for any z,

$z \in x$ iff
$z \in^2 \{x\}$ iff
$z \in^2 \iota``y$ iff
$z \in \{w\}$ for some $w \in y$ iff
$z = w$ for some $w \in y$ iff
$z \in y$

whence $x = y$ by extensionality.

Of course any $\in$-automorphism is skew-injective ... but then $\in$-automorphisms are plain vanilla injective anyway.

If we declare an $\in$-injection f by recursion on $\in$: $f(x) = f``x$ then f is skew-injective.

Skew-injectivity is an odd-looking property, and one that can only ever crop up in Set Theory, since it asks us to apply a single function f both to arguments and to sets of arguments. One could never find this idea in Analysis, for example.

Skew-injectivity is *prima facie* not stratifiable.

Let's note a few properties of it. Recall that the operation j on functions is defined so that $(jf)(x) = f``x$.

Theorem 3 *The collection of skew-injective functions is closed under composition, j, and right-inverse.*

Proof:
(i) Closed under composition

Suppose f and g are skew-injective and that $f(g(x)) = f``(g``y)$. This gives $g(x) = g``y$ by skew-injectivity of f and then we get $x = y$ by skew-injectivity of g.

(ii) Closed under right-inverse

Suppose f is skew-injective and $f \cdot g = \mathbb{1}$. Suppose x and y are such that $g``x = g(y)$. We wish to infer $x = y$.

$f``g``x = f``g(y)$ but $f \cdot g = \mathbb{1}$ so
$x = f``g(y)$. Again $f \cdot g = \mathbb{1}$ whence

$f \cdot g(x) = f``g(y)$. We use skew-injectivity of f to get
$g(x) = g(y)$. We apply f to both sides:
$f \cdot g(x) = f \cdot g(y)$, Now $f \cdot g = \mathbb{1}$ whence
$x = y$.

(iii) Closed under j.

Suppose f is skew-injective and $\{f``x : x \in X\} = \{f(y) : y \in Y\}$. Then, for every $x \in X$, there is $y \in Y$ with $f``x = f(y)$ which is to say $x = y$. So everything in X is identical to something in Y. Similarly, for every $y \in Y$ there is $x \in X$ with $f(y) = f``x$ whence $x = y$. Thence $X = Y$.

■

I'm not sure what use that was, but it's quite cute.

Remark 13 *If f is a skew-injective function that is surjective then it is actually an $\in$-automorphism.*

Proof:
Surjectivity of f tells us that, for all x, there must be z such that $f``x = f(z)$. But then $z = x$ by skew-injectivity of $f$. So $f(x) = f``x$. I had been blithely assuming that establishes that f is an $\in$-automorphism but my student August Liu says: "it's obvious that $x \in y$ implies $f(x) \in f(y)$, but what about the other direction??" Suppose $f(x) \in f(y) = \{f(z) : z \in y\}$. That doesn't tell us that $x \in y$ unless f is injective! After all we might have $x \notin y$ but $f(x) = f(z)$ for some $z \in y$. To my shame this possibility had never occurred to me!! Fortunately surjectivity of f plus skew-injectivity does imply injectivity as we will now show. Suppose $f(x) = f(y)$. Then $f``\{x\} = f``\{y\}$ and both of these must be $f(z)$ for some z. This gives $\{x\} = z$ and $\{y\} = z$ so $\{x\} = \{y\}$ and $x = y$. So we are home and hosed. Thank you August! ■

Does skew-injectivity imply injectivity, without use of surjectivity? My guess is: not. It shouldn't be hard to find a counterexample. Find one!!

Skew-injectivity of f is useful because of the permutation

$$\pi := \prod_{x \in A} (f(x), f``x)$$

which is well-defined when f is injective but is particularly interesting when f is skew-injective. What is A here? It could be anything – might be V.

Anyway, take f to be ι (the singleton function). What happens in V^π? Not hard to see that π must swap $\emptyset$ and $\{\emptyset\}$ so it adds a Quine atom. I got quite excited for a while beco's if X is a transitive set then $\iota``X$ is a transitive set in V^π but that doesn't really matter. Of much more importance – it seems to me – is the following observation.

Remember that there is always the possibility of a $\mathcal{P}$-embedding from V into V^σ whenever σ is a permutation. There is an obvious recursion:

$$i(x) := \sigma^{-1}(i``x) \tag{1.4}$$

and if V is actually wellfounded this is a legitimate definition. For our transposition π above, it turns out to be easy to prove that the injection i is precisely the singleton function. Presumably in general it is going to be precisely f. This struck me. Does this remind you of anything? It reminded me of extracted models of the kind that produce atoms. Define a new membership relation on V by saying $x \in_{new} y$ iff $y = \{z\}$ and $x \in z$. Anything not a singleton is an urelement.

We seem to be doing something very similar here, the difference being that the things that aren't copies of old sets become illfounded sets rather than urelemente. We don't throw *all* their structure away, just some.

It occurs to me to wonder if one can reconstrue in the same way the extracted models that one uses to get models of NFU Jensen-Boffa style. We probably have to be quite careful how we do it, and we should start with a simple case. Another thing we have to do is reconstrue type-theory as a one-sorted theory of sets with an I-am-the-same-type-as-you relation definable in terms of $\in$.

In this setting applying the permutation π above should correspond somehow to extracting every second type.

Ah! Here is a cute fact.

Remark 14
Suppose f is skew-injective and let π be $\prod(f(x), f``x)$.
Then V^π is an end-extension of V.

Proof:
$x \in y$ iff $f(x) \in f``y$ iff $f(x) \in_\pi f(y)$.

Clearly the range of f is a transitive subset of V^π.

So every model of NF has a proper $\mathcal{P}$-extension which satisfies the same stratifiable formulæ. Actually this is an old result. It's in the monograph, theorem 101 or thereabouts.

This should be turned into an exercise. What about $\{x\} = \{\iota``z : z \in y\}$ ...? Actually i think it should be $\iota``x = \{\iota``z : z \in y\}$ ....

Suppose $\iota``x = \{\iota``z : z \in y\}$ and $z \in x$. Then $\{z\} \in \iota``x = \{\iota``z : z \in y\}$. So $\{z\} = \iota``w$ for some $w \in y$, giving $z = w$ by the foregoing, and $z \in y$. Better check that it's an iff.

But what in general do we want to say about two functions f and g satisfying $(\forall xy)(f(x) = g(y) \to x = y)$? A symmetrical binary relation... let's write it with an 'R'. $R(f, f) \to f$ is injective. Do other conditions on f get captured neatly by R?

Suppose $f : A \to A$ and $g : A \to A$. Then we have a two-generator boolean algebra, with $f``A$ and $g``A$. Look at $f``A \cap g``A$. The preimages under f and g are the same, and they constitute a region $A' \subseteq A$ such that $f \restriction A' = g \restriction A'$ are injective.

To answer a question of Zachiri's. Let π be $\prod_{x \in V}(\{x\}, \iota``x)$. We claim that, in V^π, the old $\{V\}$ has become a set identical to its own power set, and that (seen from outside) it is an isomorphic copy of the home model. In order not to confuse ourselves, let's name this object $\mathfrak{v}$.

Observe $V^\pi \models x \in y \in \mathfrak{v}$ iff
$V \models x \in \pi(y)$ and $y \in \pi(\mathfrak{v})$ iff
$x \in y \in V$.

Now we must check that V^π believes that everything in $\mathfrak{v}$ is the same size as a set of singletons. Now (for any π, actually)
$V^\pi \models$ "x is the same size as a set of singletons" iff
V believes that $\pi_2(x)$ is the same size as a set of singletons.
Now $\pi_2(x) = \pi``(\pi(x))$. If $V^\pi \models x \in \mathfrak{v}$ then $x$ is a singleton, so $\pi(x)$ is a set of singletons, so $\pi``\pi(x)$ is the same size as a set of singletons, namely $\pi(x)$!

Now i think that – in V^π – $\mathfrak{v}$ contains all the wellfounded sets. (That is to say: the $\mathcal{P}$-extension adds no new wellfounded sets). So we want to prove by $\in$-induction on the wellfounded sets of V^π that they are all in $\mathfrak{v}$. So we want V^π to believe that $\mathfrak{v}$ contains all its subsets. But of course it does. Duh!

1.44 Yablo's Paradox in NF

I have just discovered a wonderful connection between Yablo's paradox and wellfounded sets and permutation models in NF.

Suppose the largest fixed point for $\lambda x.(\{\emptyset\} \cup \iota``x)$ exists. This is the collection of all those x such that every nonempty thing in $TC(x)$ is a singleton. Let's call it H. Now let

$$\pi := \prod_{x \in H}(x, V \setminus x)$$

(Actually you don't have to swap x with $V \setminus x$: anything large and distant will do.) What happens in V^π? Suppose $\langle x_n : n \in \mathbb{N}\rangle$ were a descending $\in$-sequence of singletons-in-the-sense-of-V^π, so that $\pi(x_n) = \{x_{n+1}\}$ for all n. We derive a contradiction from this assumption.

The contradiction we obtain is a version of Yablo's paradox: we ask whether or not each x_i is fixed by π. π swaps with its complement everything that is a singleton^n for every n. Also, if $x \in H$ then $\pi(x)$ is a singleton, and in this sequence $\pi(x_n) = \{x_{n+1}\}$.

Suppose x_k is moved. Then one of x_k and $\pi(x_k)$ is a singleton$^\infty$ and since $\pi(x_k)$ is known to be a singleton (it is actually $\{\pi(x_{k+1})\}$), it must be $\pi(x_k)$ that is a singleton$^\infty$. But then x_{k+1} is a singleton$^\infty$ and is therefore moved, and moved to $\pi(x_{k+2})$ which is a singleton and is the complement of x_{k+2}. This is impossible: we cannot have two singletons which are complements! So x_k wasn't moved; k was arbitrary, so they are all fixed. But if they are all fixed, x_1 is a singleton$^\infty$ and must be moved.

I think this means that in the new model the only things whose transitive closure consists entirely of singletons are the Zermelo naturals. Of course it doesn't prove that the Zermelo naturals is a set, but it's good for a laugh. Feel free to make any use of it you like.

Now let's think about how to generalise this. Let S be a 1-stratified property (like being finite, or a singleton or something like that) with the feature that we can't have both $S(x)$ and $S(V \setminus x)$. Suppose further that the set $H := \{x : (\forall y \in TC(\{x\}))S(y)\}$ exists.

Let π be the permutation

$$\prod_{x \in H} (x, V \setminus x).$$

Notice that every set that is moved is either a thing in H or the complement of a thing in H, and we can always tell which.

I claim that, in V^π, every set of things that are S must have an $\in$-minimal element.

Suppose not, and let X be a counterexample. Since S is 1-stratifiable, $V^\pi \models S(x)$ iff $S(\pi(x))$. Let x be an arbitrary element of X. We ask: "is x moved?". Suppose it were. We know that $S(\pi(x))$ so $\pi(x)$ cannot be the complement of a thing in H so it must be in H. So any x' believed by V^π to be in x is also in H, and is therefore moved by π. Moved to what? Moved to $\pi(x')$ which we know is S, beco's S is 1-strat. But then x' and $\pi(x')$ are complements and both are S. This isn't possible.

So x is fixed. Now x was arbitrary, so everything in X is fixed. What we want to do now is to argue that any given $x \in X$ must now be moved beco's everything in its transitive closure is fixed. But this doesn't work: all we know is that everything in $\{y \in TC(\{x\}) : S(y)\}$ is fixed, and that's not enuff to place x in H.

So on reflection perhaps the Yablo angle is a red herring. Can't we kill off all singletons$^\infty$ by swapping every singleton^2 with its complement?

1.45 A Puzzle of Randall's

Find a permutation model containing, for each strongly cantorian cardinal α, a set of Quine atoms of size α. Beco's of the analogy with the sentence IO (that says that every set is the same size as a set of singletons) and the fact that it's due to Holmes i shall call it 'HO', thus:

Every strongly cantorian set is the size of a set of Quine atoms HO

One thinks immediately of Henson's permutation

$$\prod_{\alpha \in On} (T\alpha, \{\alpha\}).$$

This gives a permutation model in which every old strongly cantorian ordinal has become a Quine atom, and in which every Quine atom arises from a strongly cantorian ordinal. The significance of the Henson permutation in this context is that it gives us a model in which every *wellordered* strongly cantorian set is the same size as a set of Quine atoms, whereas what we are after is the same assertion with the 'wellordered' dropped. Perhaps a similar idea will give us the stronger result we want ...?

Think: $\iota``V$ is NO, $\{x\} \mapsto \{\iota``x\}$ is T. So let π be

$$\prod_{\{x\}\in\iota``V}(\{\{x\}\}, \{\iota``x\})$$

The trouble is: this analogue of the T function doesn't have enuff fixed points. As Randall says, this permutation turns any set of Quine atoms into a Quine atom. What one really wants is a kind of T operation on a set larger than any strongly cantorian set. One can do this to BF or even the set of all set pictures. However there are deep reasons why one cannot do it to V.

One would need a set X larger than any strongly cantorian set, together with a stratified but inhomogeneous injective function $f : X \to X$ (That is to say, the graph of $f \cdot \iota$ is a set) such that f has a lot of fixed points.

The **Henson permutation** for D_n is the product of all transpositions $(\{X\}, T(X))$ for $X \in D_n$. The question now is: is there an n such that every strongly cantorian set is size of a set of T-fixed members of D_n?

There is a surjection from D_{n+1} to D_n and this surjection commutes with T, so it sends T-fixed things to T-fixed things. Thus the chances of a successful search improve as n gets bigger.

Holmes and i both feel that this is the only hope of finding a permutation that makes his proposition true.

Further observations.

- If AxCount fails then the Henson permutation makes Holmes' formula true;
- There doesn't seem to be any obvious objection to the assertion that there is a function defined on V which, to every stcan set x, assigns a set of singletons the same size as x.

Of course, the natural thing to consider is not HO but $\Diamond$HO.

3/vii/06

1.46 Part IV Set Theory

Spend a lot of time explaining stratification and explaining how to compute sizes of noncantorian sets.

Then talk about cantorian and strongly cantorian sets and subversion of stratification. Tell them to read relaxing.tex

It is important not to think of this as a pathology of NF, and accordingly as a good reason for eschewing NF. The correct point to take away from this is that we have here an important fact about the nature of syntax and the type distinctions that arise from it. There is a moral here for typing systems everywhere.

The hard part is to fully understand stratification. There is an easy rule of thumb with formulæ that are in primitive notation, for one can just ask oneself whether the formula could become a wff of type theory by adding type indices. It's harder when one has formulæ no longer in primitive notation, and the reader encounters these difficulties very early on, since the ordered pair is not a set-theoretic notion. How does one determine whether or not a formula is stratified when it contains subformulæ like $f(x) = y$? The technical/notational difficulty here lands on top of – as so often – a conceptual difficulty. The answer is that of course one has to fix an implementation of ordered pair and stick to it. Does that mean that – for formulæ involving ordered pairs – whether or not the given formula is stratified depends on how one implements ordered pairs? The answer is 'yes' but the situation is not as grave as this suggests, and this is for a logically deep reason that I want you to understand. Let us consider again the formula $x = f(y)$. This is of course a molecular formula, and how we stratify it will depend on what formula it turns out to be in primitive notation once we have settled on an imp[lementation of ordered pairs. If we use Wiener-Kuratowski ordered pairs then the formula we abbreviate to $x = f(y)$ is stratified with x and y having the same type, and that type is three types lower than the type of f. If we use Quine ordered pairs then the formula we abbreviate to $x = f(y)$ is stratified with x and y having the same type, and that type is one type lower than the type of f. There are yet other implementations of ordered pair under which the formula we abbreviate to $x = f(y)$ is stratified with x and y having the same type, and that type is two or possibly more types lower than the type of f.

The point is that our choice among the possible implementations will affect the difference in level between x (and y) and f but will not change the formula from a stratified one to an unstratified one. This is subject to two important provisos:

1. we must restrict ourselves to ordered pair implementations that ensure that in $x = \langle y, z\rangle$ y and z are given the same type.

2. We do not admit self-application: $(f(f))$.

Thes two provisos are of course related. The second will seem reasonable to anyone who thinks that mathematics is strongly typed. (The typing system in NF interacts quite well with the endogenous strong typing system of mathematics.) If we consider expressions like $x = \langle x, y\rangle$ we see that their truth-value depends on how we implement ordered pairs. There is a noncontroversial sense (entirely transparent in the theoretical CS tradition) in which expressions of this kind are not part of mathematics – in contrast to expressions like $x = f(y)$

which are. The only formulæ whose stratification status are implementation sensitive in this way are formulæ that are not in this sense part of mathematics.

The second one is a bit harder to understand: why should we not have an implementation that compels y and z to be given different types in a stratification of $x = \langle y, z \rangle$ – or even make the whole formula unstratified?

H I A T U S

If we make $x = \langle y, z \rangle$ into something unstratified then we cannot be sure that $X \times Y$ exists, nor that compositions of relations (that are sets) are sets; converses of relations might fail to exist; and we will not really be able to do any mathematics. After all, $X \times Y$ is $\{z : (\exists x \in X)(\exists y \in Y)(z = \langle x, y \rangle)\}$ and if $z = \langle x, y \rangle$ is not stratified then the set abstraction expression might not denote a set.

However, even if we muck things up only to the extent of allowing $x = \langle y, z \rangle$ to be stratified with y and z of different types then we will find not only that some compositions of relations (that are sets) are not sets but also that for some big sets X (such as $X := V$) that the identity function 1_X is not a set. Let's look into this last point a bit more closely. Suppose "$x = \langle y, z \rangle$" is stratified but with y and z being given different types. Then $X \times Y$ is $\{z : (\exists x \in X)(\exists y \in Y)(z = \langle x, y \rangle)\}$ which this time is stratified, so $X \times Y$ is a set. However if $R \subseteq X \times Y$ and $S \subseteq Y \times Z$ then $R \circ S$ is $\{z : (\exists x \in X)(\exists y \in Y)(\exists z \in Z)(\langle x, y \rangle \in R \land \langle y, z \rangle \in S \land z = \langle x, z \rangle)\}$

This is not stratified. If the difference between the types of the two components of an ordered pair is n, then x and y have types differing by n, and y and z too have types differing by n, and x and z have types differing by n!

The problem with 1_X arises because $(\exists x \in X)(y = \langle x, x \rangle)$ is not stratified, so its extension is not certain to be a set. By the same token no permutation of a set can be relied upon to be a set. The (graph of the) relation of equipollence might fail to be reflexive, or symmetrical, or transitive.

The conclusion is that if we want our implementation of mathematical concepts into set theory to be tractable from the NF point of view, then we want a pairing/unpairing function that interprets $x = \langle y, z \rangle$ as a stratified formula with y and z having the same type. One such ordered pair is the Wiener-Kuratowski ordered pair that we all know and love. In fact in NF we usually use the Quine ordered pair which i will now explain.

Does the difference between Quine pairs and W-K pairs matter? Much less than you might think. In some deep sense it doesn't matter at all. Let me explain.

[discussion of Cantor's theorem here: the problem is caused by the fact that the argument and the values of the surjection are of different types. That cannot be cured by changing from W-K to Quine or Quine to W-K.]

If your mathematics is strongly typed, and all your mathematical operations are implemented by stratified operations on sets, then everything is OK.

START REWRITING HERE

There are various standard definitions of ordered pair, and they are all legitimate in NF, and all satisfactory in the sense that they are "level" or *homogeneous*. All of them make the formula "$\langle x, y \rangle = z$" stratified and give the variables x and y the same type; z takes a higher type in most cases (never lower). *How* much higher depends on the version of ordered pair being used, but there are very few formulæ that come out stratified on one version of ordered pair but unstratified on another, and they are all pathological in ways reminscent of the paradoxes. The best way to illustrate this is by considering ordinals (= isomorphism classes of wellorderings) in NF. For any ordinal α the order type of the set (and it is a set) of the ordinals below α is wellordered. In ZF one can prove that the wellordering of the ordinals below α is of length α. In NF one cannot prove this equation for arbitrary α since the formula in the set abstract whose extension is the graph of the isomorphism is not stratified for any implementation of ordered pair. Now any wellordering R of a set A to length α gives rise to a wellordering of $\{\{\{a\}\} : a \in A\}$, and if instead one tries to prove (in NF) that the ordinals below α are isomorphic to the wellordering of length α decorated with curly brackets, one finds that the very assertion that there is an isomorphism between these two wellorderings comes out stratified or unstratified depending on one's choice of implementation of ordered pair! This is because, in some sense, the applications of the pairing function are *two* deep in wellordering of the ordinals below α, but only *one* deep in the wellordering of the set of double singletons. If we use Quine ordered pairs, the assertion is stratified – and provable. If one uses Wiener-Kuratowski ordered pairs then the assertion is unstratified and refutable. However if one uses Wiener-Kuratowki ordered pairs there is instead the assertion that the ordinals below α are isomorphic to the obvious wellordering of $\{\{\{\{\{a\}\}\}\} : a \in A\}$, which comes out stratified (and provable). In general for each implementation of ordered pair there is a depth of nesting of curly brackets which will make a version of this equality come out stratified and true. This does not work with deviant implementations of ordered pair under which "$\langle x, y\rangle = z$" is unstratified or even with those which are stratified but give the variables x and y different types. Use of such implementations of ordered pairs result in certain sets not being the same size as themelves!

Perhaps a concrete example would help. Let us try to prove Cantor's theorem. The key step in showing there is no surjection $f : X \twoheadrightarrow \mathcal{P}(X)$ by *reductio ad absurdum* is the construction of the diagonal set $\{x \in X : x \notin f(x)\}$. The proof relies on this object being a set, which it will be a set if "$x \in X \wedge x \notin f(x) \wedge f : X \to \mathcal{P}(X)$" is stratified. This in turn depends on "$(\exists y)(y \in \mathcal{P}(X) \wedge \langle y, x\rangle \in f \wedge f : X \to \mathcal{P}(X))$" being stratified. And it *isn't* stratified, because "$\langle y, x\rangle \in f$" compels '$x$' and '$y$' to be given the same type, while "$f : X \to \mathcal{P}(X)$" will compel 'y' to be given a type one higher than 'x'. This is because we have subformulæ '$x \in X$' and '$y \subseteq x$'. Notice that we can draw this melancholy conclusion without knowing whether the type of 'f' is one higher than that type of its argument, or two, or three .... We cannot prove Cantor's theorem.

However if we try instead to prove that $\{\{x\} : x \in X\}$ is not the same size as $\mathcal{P}(X)$ we find that the diagonal set is defined by a stratified condition and exists, so the proof succeeds. This tells us that we cannot prove that $|X| = |\{\{x\} : x \in X\}|$ for arbitrary X: graphs of restrictions of the singleton function tend not to exist. (If they did, we would be able to prove Cantor's theorem in full generality.) This gives rise to an endomorphism T on cardinals, where $T|X| := |\{\{x\} : x \in X\}|$. T misbehaves in connection with the sets that in NF studies we call **big** (as opposed to *large*, as in *large cardinals* (in ZF)). These are the collections like the universal set, and the set of all cardinals and the set of all ordinals: collections denoted by expressions which in ZF-like theories will pick out proper classes. If $|X| = |\{\{x\} : x \in X\}|$ we say that X is **cantorian**. If the singleton function restricted to X exists, we say that X is **strongly cantorian**. Sets whose sizes are concrete natural numbers are strongly cantorian. $\mathbb{N}$ (the set of Frege natural numbers) is cantorian, but the assertion that it is strongly cantorian implies the consistency of NF.

1.47 Weakly stratified

To explain weakly stratified we have to think of stratifications as defined on *occurrences* of variables not on variables. Something is weakly stratified if there is a stratification that gives all occurrences of each bound variable the same type. Two occurrences of a free variable may be given two different types. If a variable has only one occurrence then it can never be responsible for the failure of a stratification: each occurrence can be connected to only one other occurrence of one other variable. So what happens if we have three-placed predicates???

If we write an $\in$-restricted-to-small-sets-is-wellfounded condition into the definition of small we find that $\iota``V$ is not small: $\iota``V \in \{\iota``V\} \in \iota``V$. Perhaps the correct notion of smallness is being the size of a set of singleton^n for every n

partiii2006: get straight the definition of extracted model: use Barnaby's trick:

We start by thinking of the old $\in$-relation as a single one-sorted global relation in terms of which one can define the types.

Have an axiom to say that the relation $\texttt{sametype}(y_1, y_2)$ defined by $(\exists x)(y_1 \in x \wedge y_2 \in x)$ is an equivalence relation. (This is universal-existential, for what it's worth.) Then there is a relation $S(x, y)$ which says that x is one type lower than y: $(\exists z_1, z_2)(x \in z_1 \in z_2 \wedge y \in z_2)$

Extensionality now says

$(\forall x_1 x_2)(\texttt{sametype}(x_1, x_2) \to (x_1 = x_2 \longleftrightarrow (\forall y)(y \in x_1 \longleftrightarrow y \in x_2)))$

We need the $\texttt{sametype}$ clause in lest we make empty sets at different types identical. We could use the other version of extensionality

$$(\forall x_1 x_2)(x_1 = x_2 \longleftrightarrow (\forall y)(x_1 \in y \longleftrightarrow x_2 \in y))$$

but this might upset some purists since it relies on the existence of singletons.

We can now set up an axiom scheme of comprehension. Let ϕ be a stratified formula with k variables to wit: n bound variables $z_1 \ldots z_n$, one free variable x_n and remaining free variables $y_{n+1} \ldots y_k$. Suppose further that the variable with subscript j has type $\sigma(j)$ in ϕ. Then the following is an axiom

$(\forall x_1 \ldots x_n)(A(x_1 \ldots x_n) \to$

(Where A is the conjunction of all the true assertions about the type relations between the various x, assertible using S)

$(\exists y)(\forall z)(S(z,y) \to (z \in y \longleftrightarrow \ldots$

and now comes the hard bit: we have to restrict the variables to their types, in order to make sense when we assert existence axioms like complement etc.

Think of the new $\in$-relation as one-sorted: global. $x \in_{extract} y$ iff $\iota^k(x) \in y$ where y is $k+1$ types higher than x

1.48 Cooking up a nontrivial congruence relation for $\in$

Ain't none.

If $x \sim x'$ then $x \in \{x\} \to x' \in \{x\}$ so $x = x'$

But that's the wrong definition. What we can sensibly ask for is a relation $\sim$ such that if $x \sim x'$ and $x \in y$ then there is $y' \sim y$ with $x' \in y'$. And the answer to this – on quite weak assumptions – is 'yes'. Cycles of $\in$-automorphisms are equivalence classes for equivalence relations like this.

1.49 Modal equivalence classes

Let us say that ϕ and ψ are $\Box$-equivalent if $\Box\phi \longleftrightarrow \Box\psi$ and $\Diamond$-equivalent if $\Diamond\psi \longleftrightarrow \Diamond\phi$. It's not clear to me that these two equivalence relations are the same, tho' they look as if they should be.

Can we prove any theorems like: Let Γ be a class of formulæ (a quantifer class or something like that) then Every $\Diamond$-equivalence class contains a member of Γ..?

1.50 Körner Functions

For X an initial segment of the ordinals, a **Körner function** on X is a function $f : X \to On$, such that $(\forall x \in X)(x \leq f(Tx))$. Friederike Körner and I realised independently at about the same time that Körner functions $\mathbb{N} \to \mathbb{N}$ were the gadget needed to refine Boffa permutations to obtain models of NF in which $\in$ restricted to finite sets is wellfounded. Friederike used Henson-style Ehrenfeucht-Mostowski models to show that for any consistent stratified extension S of NF, if S has models at all, then it has models with Körner functions for $\mathbb{N}$. (That's why I call them "Körner functions".) The fact that one can find models containing Körner functions for $\mathbb{N}$ *without* strong assumptions means that "$\in$ restricted to finite sets is wellfounded" is not strong. It *might just* mean that the existence of an infinite wellfounded set is not strong either.

Friederike's original model had a special kind of natural number – which i call a **Körner number** – which is a natural number k such that, for all $k' > k$, $k' < Tk'$. This gives a Körner function immediately ("add k"!) and this Körner function is inflationary and monotone increasing, but sadly it does not commute with T.

We will want to show that if there any Körner functions at all then there are Körner functions that are strictly increasing.

Sse f is a Körner function. Consider $h(0) = f(0)$; $h(n+1) = \mathtt{max}\{f(n+1), h(n)+1\}$. We want h to commute with T as long as f does.

$$h(Tn) = \mathtt{max}\{f(Tn), Th(n-1)+1\} = T\mathtt{max}\{f(n), h(n-1)+1\}.$$

The "rounding up" operator is idempotent.

We will also need:

If f is a Körner function then so is

$\lambda n.\mathtt{max}\{n, f(n)\}$.

...and it commutes with T if f does. That is to say, if $Tf(n) = f(Tn)$ then $T\mathtt{max}\{n, f(n)\} = \mathtt{max}\{Tn, Tf(n)\}$. And that's true

So if there is a Körner function then there is one that dominates the identity function.

■

Remark 15 *If there is Körner function on X* that commutes with T *then $AxCount_{\leq}$ holds.*

Proof:

Let f be a Körner function $\mathbb{N} \to \mathbb{N}$ that commutes with T.

We then infer that there is one that is strictly increasing.

Suppose we have two functions g_1 and g_2 both obeying $g(n) = f^n(0)$.

Then we prove by induction on 'n' that $Tg_2(n) = g_1(Tn)$.

Suppose true for n. Then

$$Tg_2(n+1) = f(Tg_2(n)) = f(g_1(Tn)) = g_1(Tn+1)$$

Now suppose *per impossibile* we have $Tn < n$ for some n. Consider $g(Tn)$ and $g(n)$.

We have

$$g(n) \leq f(T(g(n))) = f(g(Tn)) = g(Tn+1)$$

whence $n \leq Tn + 1$ contradicting $Tn < n$.

■

Notice that the existence of a Körner function on NO doesn't obviously imply the existence of a Körner function on the naturals: if f is a Körner function on the ordinals its restriction to the naturals might not be a Körner function on the naturals!

There can be no Körner function $f : NO \to NO$ that commutes with T. Suppose there were, and reason, à la Henson, about $\phi(\alpha, f)$. We argue that the least α such that $\phi(\alpha, f)$ is finite must be cantorian and we then find that $|\phi(\alpha, f)|$ is both odd and even. ■

(i) The existence of a Körner function: $f : \mathbb{N} \to \mathbb{N}$ such that $n \leq f(Tn)$ fits in nicely here. I think we will need to consider its extension to the ordinals. What about a function $f : On \to On$ such that $\alpha \leq f(T\alpha)$? It's not obviously impossible. It clearly implies that $cf(\Omega)$ is cantorian (and so Ω is not regular, contradicting AC_2). If there is such an f, set $g(\alpha) := \sum\limits_{\beta<\alpha} f(\beta) + 1$. Then g has the same nice property and is both cts and inflationary. Of course there can't be such a function which commutes with T, since presumably the least thing moved would be a disaster, or the least thing that cannot have g applied to it infinitely often and so on.

We might have to consider the extension of Körner functions to BF. It seems to me that this should have modal consequences. I mean: what happens in the model given by the Ackermann permutation?

(later, march 2022)

The time may now be ripe to consider again the functions that Ward Henson writes about in *Type-raising operations in Quine's NF*, co's they look rather like Körner functions.

Time to take this section by the scruff of the neck!

1.51 $\in$-games

There is this paradox, that Isaac calls 'Forster's paradox', to the effect that `I` and `II` cannot be sets. In what sense can $\in$-determinacy hold in a model of NF? There is a result in the book that sez that V can be the disjoint union of X and Y where $X = \mathcal{P}(Y)$ and $Y = \flat(X)$ but that's a red herring, beco's by the preceding result, in those circumstances, they can't be `I` and `II`!

But can there be a global nondeterministic winning strategy for `I`? This would be a relation E, say, such that $\{\langle\{x\}, y\rangle : x\,E\,y\}$ is a set, and $x\,E\,y \to x \in y$, the idea being that $x\,E\,y$ if $x \in y$ and `I` has no winning strategy in G_y or x is a member of $y \cap \mathtt{II}$ of minimal rank if there is such a thing. That way all `I` has to do, on being confronted with y, is to reach for any x such that $x\,E\,y$. E must not only be wellfounded but must satisfy the extra condition that for any x, either every descending E-chain starting at x is even, or every descending E-chain starting at x is odd. This condition, being "even" or "odd" is not stratified, so there is no obvious lapse into paradox. Noteworthy that the existence of E as a set does not allow us to define a rank relation, any more than the existence of H_κ implies the existence of Π_κ.

So is there a permutation model containing such a set of ordered pairs??

"There is a set $E \subseteq \{\langle\{x\}, y\rangle : x \in y\}$ such that, E^{-1}"$\{x\}$ isn't empty unless x is, and for every x either every descending E-chain starting at x is even or every descending E-chain starting at x is odd."

1.52 Stuff to fit in

J_0 is paradoxical (in Wagon's sense). Any two countable sets are J_0-equidecomposable with one piece, or – simpler – J_0 equivalent. So we can find disjoint sets A and B, and $\sigma, \tau \in J_0$ such that $A = \sigma``A$ and $A \cup B = \tau``A$. All we need was a couple of disjoint countable sets. By the same token, all we need to show J_n paradoxical is a couple of disjoint set of singletons^n.

It still isn't clear to me whether or not AxCount$_\leq$ implies the analogue for ctbl ordinals. What is clear to me is that if i wish to get to the bottom of this i will have to *really* understand the theory of ordinal notations. If you are interested in this you may wish to take up my suggestion that the way in is to consider why AxCount$_\leq$ implies that $\alpha \leq T\alpha$ for all ordinals below ϵ_0, for example. It's beco's we have a system of notation for the ordinals below ϵ_0 that makes each such ordinal a finite object – in the sense that there is a bijection between them and $\mathbb{N}$ that commutes with T. In the standard treatment in the literature this is just the condition that the bijection be definable. Now there is a theorem of Diana Schmidt that says that for each ctbl alpha there is a 'nice' system of notation for the ordinals below alpha. If the proof is 'nice' enuff then presumably one can recover a proof that the notation system respects T. But i think this is going to be hard. Worth getting to the bottom of tho'....

Find a model for *i*NF in the recursive functions.

1.53 Chores and Open problems

NF0 $\subseteq$ NF_3. (but surely this is obvious..?)
NF0 $\subseteq$ *NF*P $\subseteq$ *NF*I.

Holmes sez NF_3 + *NF*P = *NF*, beco's NF_3 has unions (which is what we have to add to *NF*P to get *NF*) and *NF*P has a type-level ordered pair (which is what we have to add to NF_3 to get *NF*). Note $NF_3 \not\subseteq NFI$ beco's $\bigcup x$ is in NF_3.

NF$\forall$ $\subseteq$ NF_3.

Holmes' axiom of small ordinals: for any property ϕ of ordinals whatever, there is a *set* X such that the class of all α such that $(\phi(\alpha) \wedge \alpha = T\alpha) = X \cap$ the class of all α such that $\phi(\alpha)$.

(Explain how this is like P = NP)

In conjunction with large ordinals we can show that there is a canonical set that will do. let ϕ be any property. It is shadowed by a set, C. C and $T^{-1}``C$ both shadow it. If they are the same, we're done. If not, consider the smallest element of $C$ `XOR` $T^{-1}``C$. It is above $T^n`\Omega$ for some n, so grab $T^{-n}``(C$ `XOR` $T^{-1}``C)$. Gulp. This is closed under T and T^{-1}.

Randall next sez: call sets which "commute with T" *natural sets*. Then postulate that every property of natural sets (of ordinals) is coded by a set.

Randall is also concerned about what he calls the "downward cofinality" of the noncantorian ordinals. How long can a descending class of noncantorian ordinals be? A natural axiom to consider is one that sez that, if you are a noncantorian ordinal, then, for some n, $T^n\Omega$ is below you. This is something one can approach with omitting types....

1. Randall asks: how about a pairing function that raises types by one in NF_3? Does it give *NF*? (You can't use his clever pair beco's – since it looks inside the components – it uses too many types) Add a primitive pairing relation.

2. Is there a $\forall^*\exists^*$ version of the axiom of infinity? (see Parlamento and Policriti JSL **56** dec 91 pp 1230–1235; see also Marko Djordjevic JSL **68** (2004) pp 329–339)

3. $NF \vdash \Diamond\neg\exists V_\omega$? If we express AxInf in Zermelo in the form "there is an infinite set" then we cannot prove the existence of V_ω or indeed any particular infinite set.

4. $NF \vdash Con(TSTI_\omega)$? $NF \vdash Con(TSTI_{\omega^*})$? Do either of these follow from $\mathrm{AxCount}_\leq$?

5. Is NF_3 as strong as TST? Holmes thinks so. He adds that NF_3I is much weaker than $TSTI$? Perhaps Pabion's result is relevant here: NF_3I = second-order arithmetic.

6. Once you've solved the universal-existential question for T$\mathbb{Z}$T do it for T$\mathbb{Z}$Tλ.[10]

7. Are the $f \in \mathbb{N}^\mathbb{N}$ that commute with T cofinal in the partial order under dominance?

8. $\mathrm{AxCount}_\leq \to (\forall\alpha < \omega_1)(\alpha \leq T\alpha)$?

9. If Φ is a sentence in arithmetic-with-T that is true of the identity but not provable in arithmetic-with-T is there an Ehrenfeucht-Mostowski model in which it fails?

10. André's question. $(\exists n \in \mathbb{N})(n \neq Tn \wedge (\forall m < n)(m \leq Tm))$

11. What holds in the constructible model of KF?

12. Understand Orey's proof well enough to know whether or not $\mathrm{AxCount}_\leq$ suffices to prove Con(*NF*).

13. Takahashi's proof that every $\Sigma^{\mathcal{P}}_n$ formulae is in Σ^{Levy}_{n+1}. Does it really need foundation?

[10]The obvious comprehension axiom for T$\mathbb{Z}$Tλ is

$$(\bigwedge\alpha)(\bigwedge\beta)((\forall x_\alpha)(\exists! y_\beta)(\Phi(x_\alpha, y_\beta)) \to (\exists f_{\alpha\to\beta})((\forall x_\alpha)\Phi(x_\alpha, f`x_\alpha)))$$

. . . with parameters of course!

14. Can there be $f : NO \to NO$ with $(\forall \alpha)(\alpha \leq f`T\alpha)$?

    Well, if there is, then AC_{wo} fails beco's $cf(\Omega)$ must be cantorian. If there is such an f then there must be one that is cts and inflationary i think. Set $g(\alpha) := \sum_{\beta<\alpha} f(\beta) + 1$.

15. is there a bijection $V \longleftrightarrow V^V$ that enables us to interpret the λ-calculus in *NF*?

16. Aczel's point about $V \sim V \to V$ being possible constructively.....

17. Is the theory of wellfounded sets in *NF* invariant?

    Holmes has this permutation that kills off infinite transitive wellfounded sets, whatever model you start in. This means that if $\mathrm{AxCount}_{\leq}$, for example, then the theory of wellfounded sets is not invariant, since possibly there are infinite transitive wellfounded sets and possibly there aren't.

18. Is there always a permutation model of Forti-Honsell Antifoundation?

    It says:

    $$(\forall X)(\forall g : X \to \mathcal{P}(X))(\exists! Y, f)(f``X = Y \wedge f = (j`f) \circ g)$$

    To find out what $\Diamond$ of this is, reletter the failures of stratification.

    $$(\forall X, Z)(\forall g : X \to \mathcal{P}(Z))(\exists! Y, f, h)(f``X = Y \wedge h = (j`f) \circ g)$$

    $$(\forall X, Z)(\forall g : \pi_{n+1}`X \to \mathcal{P}(\pi_n`Z))(\exists! Y, f, h)(f``X = Y \wedge \pi_{n+1} h = (j`(\pi_{n+1} f)) \circ \pi_{n+2}`g)$$

    $$(\forall X)(\forall g : j^{n+1}`X \to \mathcal{P}(X))(\exists! Y, f)(f``X = Y \wedge f^{j^n`\pi} = (j`f) \circ g)$$

Each ordinal α has a unique representation in the form $2^{\alpha_1} + 2^{\alpha_2} + \ldots$ with that α_i strictly decreasing. Consider α as the set $\{\alpha_1, \alpha_2 \ldots\}$. Then $\omega = \{\omega\}$ is non-well-founded, but we only get non-well-founded sets of particular form. If we only consider ordinals less than ϵ_0 then we only get one autosingleton. Which part of Aczel's AFA holds in this case?
Maurice.

Proofs by *reductio* where the *absurdus* is an allegation that all ordinals can be embedded in the propositum. (!) Specker (and Conway's generalisation).

Diag(y, x) sez y is a formula with one free variable and x is the result of substituting the gnumber of y in y.

We want things like: diag$(y, x) \wedge \phi(x)$. This is a formula that sez of itself that it is ϕ. Now i want a three-place relation.

$S(A, B, C)$: A is the result of substituting for the free variable in B the numeral of the gnumber of C.

(why don't i know whether or not to insert the words "numeral of" before "gnumber"??)

So once we start thinking "permutation models" we get

$S(A, B, C)^\pi$: A is the result of substituting for the free variable in B the numeral of π of the gnumber of C.

So there is an operation `splat` such that

$(\forall ABC)(S(A, B, C)^\pi \longleftrightarrow S((\texttt{splat}`\pi)`A, B, C))$.

to be continued

> Is there anything to be said for adopting an axiom scheme that says that for any set x and any finite family of stratified (but possibly inhomogeneous) $\Delta_0^{\mathcal{P}}$ operations $\vec{f}$ the f-closure of x is a set? What we've just shown is that AxCount$_\leq$ is equivalent to the special case where x is $\{\emptyset\}$.
>
> We would use this, starting with $\{V\}$ and setting $\vec{f}$ to be the stratrud operations, to get lots of models of *NF*. Let us write $Sr(x)$ for the stratrud closure of x. (It might be an idea to pause and check that $Sr(\{V\})$ does not contain a Quine atom, or $H_{\aleph_0}$ by showing that if a is a Quine atom then $V \setminus \{a\}$ contains V and is stratrud closed. Ditto $V \setminus \{H_{\aleph_0}\}$. It might also be an idea to check that $Sr(\{V\}) \notin Sr(\{V\})$.) Another question: if $x = Sr(x)$ does x contain all constructible sets?
>
> Then we consider the inductively defined class containing $\{V\}$ and closed under $\lambda x.Sr(x \cup \{x\})$. Consider the wellfounded part of its sumclass. That is L.
>
> The attraction of this is that it draws our attention to a new kind of submodel. Submodels which preserve complementation are not transitive. Another way to put it: any model has a universal set. Does this universal set have to be the same as the domain of the model? No, of course not. Another detail to check: does respecting complementation ensure that inclusion is a 1-embedding? One needs B as one of the operations but relativised B is stratrud ...

(18.viii.97)

Actually that isn't *quite* what we want. We want the intersection of all stratrud-closed sets containing V *that also contain wellfounded sets of arbitrarily high rank.* – because there doesn't seem to be any reason to believe that there can't be a countable set with the first condition and we want something that has the effect achieved in the ZF case by requiring the sets concerned to contain all Von Neumann ordinals. We haven't got a rank function that is a set, but it doen't matter, because (see section **??** remark 42) all the various rank relations that we might have all agree on wellfounded sets. Perhaps we could replace "*that also contain wellfounded sets of arbitrarily high rank.*" with *that meets every ordinal containing a wellordering of a wellfounded set.*" Are these perhaps equivalent? They are both attempts at saying "contains all Von Neumann ordinals" which is emphatically *not* what we *really* mean beco's they might stop at ω.

Can we define L as the intersection of all rud-closed sets X such that $(\forall\alpha)(X \cap V_\alpha \in X)$?

$\Diamond\exists V_\omega$ *ought* to be equivalent to an assertion in arithmetic-with-T ... but which? The search from obscure bits of unstratified arithmetic reminds me of the (at times) acrimonious exchange between Richard K and me about the unstratified version of Paris-Harrington.

A subset of $\mathbb{N}$ is relatively large (or '0-large' for short) if its size is bigger than its smallest member. Thereafter x is $n+1$-large iff x minus its bottom element is n-large. Now let f be a slowly increasing function $\mathbb{N} \to \mathbb{N}$. We say x is f-large iff x is $f(|x|)$-large. Does this give a version of P-H?

1.53.1 An axiom for H?

For any property ϕ, let $H_\phi = \bigcap\{x : \mathcal{P}_\phi(x) \subseteq x\}$. Easy to show that $H_\phi \notin H_\phi$ beco's $\neg\phi(H_\phi)$. So H_ϕ can be taken as a generic example of something that is not ϕ? Tasty! Let's see what goes wrong. If ϕ is self-identity then we get WF, which cannot be a set, so this is only going to work if there are some things that aren't ϕ. It won't work if ϕ is transitivity beco's that way we get the von Neumann ordinals. So we chuck out unstratified properties as well. But then we have things like being hereditarily not equal to V (and in ZF we'd have a problem with 'wellordered') or we express it in terms of sets. So how about:

$WF \not\subseteq x \to H_x$ is a set?

This implies for example that if there are any infinite wellfounded sets then V_ω exists. Unlikely to be a theorem of *NF* but not obviously terribly strong. Is it related to assertions of the kind $WF \prec_\Gamma V$?

I once had an axiom that said for each Φ either H_Φ is a set or it is WF. This doesn't work beco's H_{trans} is paradoxical but not equal to WF. Presumably we have to restrict it to Φ that are downward-closed.

1.53.2 A message from Holmes on reflection

The idea is to redefine $x \in y$ as $Tx \in y$ (where the older $\in$ is the natural relation on isomorphism classes of digraphs). But this does not work out exactly as one would wish. The definition which works is to define $x \in y$ (new sense) as $Tx \in y$ and for all $z \in y, T^{-1}z$ exists. This gives a fine interpretation of NFU!

To get an interpretation of NF, you need a class of isomorphism types such that all "elements" are images under T and which has adequate comprehension properties. Even in NF, I haven't been able to define such a class; in fact, there is no reason to expect that one could, since an interpretation of NFU constructed in this way will generally satisfy the Axiom of Endomorphism, which is false in NF.

–Randall

But a suitable version of NFU will reflect itself exactly in this way! –Randall

Even if $H_{\aleph_0}$ exists there is no guarantee that we can define functions on it by $\in$-recursion. However we can try the following. Start with the branching quantifier formula that says that there is a function of the sort you want, and then look at the approximants.

A good place to start would be with the formula that says that $f`x =$ sup $T2^{f`y}$ for $y \in x$, or perhaps the formula that says there is a homomorphism from $\langle FIN, \in\rangle$ to $\langle \mathbb{N}, <^T\rangle$

This is

$$A : \quad {}^{(\forall y_1 \in FIN)(\exists n_1)}_{(\forall y_2 \in FIN)(\exists x_2)}(y_1 \in y_2 \to Tn_1 < n_2 \ \wedge \ y_1 = y_2 \to n_1 = n_2)$$

One also immediately thinks of branching-quantifier formulæ saying that $<^T$ is wellfounded. (or rather, that there is a homomorphism from $\langle \mathbb{N}, <^T\rangle$ to $\langle \mathbb{N}, <\rangle$.) This is

$${}^{(\forall m_1)(\exists n_1)}_{(\forall m_2)(\exists n_2)}(Tm_1 < m_2 \to n_1 < n_2 \wedge m_1 = m_2 \to n_1 = n_2)$$

But even the *first* approximant implies $\mathrm{AxCount}_{\leq}$.

There is also the formula stating that there is a homomorphism in the opposite direction:

$${}^{(\forall m_1)(\exists n_1)}_{(\forall m_2)(\exists n_2)}(m_1 < m_2 \to Tn_1 < n_2 \wedge m_1 = m_2 \to n_1 = n_2)$$

. . . which is presumably true. But what is the difference between A and

$$A' : {}^{(\forall y_1 \in FIN)(\exists n_1 \in \mathbb{N})}_{(\forall y_2 \in FIN)(\exists x_2 \in \mathbb{N})} (y_1 \in y_2 \to n_1 < n_2 \wedge y_1 = y_2 \to n_1 = n_2)$$

Isn't this going to show something quite general? Namely that assuming that a structure is wellfounded is no stronger than assuming that it lacks loops.

1.54 A message from Isaac

[A] Big Sur is real - it is on the cliffs overlooking the Pacific Ocean, about 200 miles south of San Francisco. Its unique character is something like this: Rapidly changing conditions and views, but it (almost) always looks like something out of a Chinese landscape painting. Big Sur is associated with Henry Miller and

Robinson Jeffers, who both lived there. Also the Esalen Institute, a cutting-edge humanistic/peak-experience academically-oriented psychological institute there. Steep cliffs, deep forests, difficult access, unspoiled.

[B] I looked up 'burble' in the OED: It is a verb which means to confuse, confound (to *paradox* ?). I wonder how to use it?

I have a lot of thoughts relating to your paradox and game, here are my current rough ideas (I am forwarding these thoughts to you as-is because I suspect you can think through some of them very rapidly, whereas it might take me several months; also I surmise that some feedback may be useful to you before I got to Big Sur. When I return, I will attempt to write up some thoughts in a more thorough fashion)

[C] Regarding time limits in your game: Let's suppose that for all x, Player I has a winning strategy. Then for all x, we can associate a "rank" for x. E.g., 0 has the lowest rank, all well-founded sets have the standard rank, the rank of a,b is one higher than the rank of either member, and so on.

The "rank" is a measure of how fast Player I can win - that's what I meant about time limit.

[D] Is the following true?

(NF is consistent) $\rightarrow$ (NF + Player-I-always-wins) is consistent

[E] (Straight off, I think that Player-I-always-wins is a truth about sets)

In Malitz set theory, Player I always wins. In NF, this is not the case (e.g. suppose the game begins with V, and V minus its own singeleton.)

On account of this, you could say that in Malitz set theory, all sets are semi-well-founded.

[G] Items I am thinking about:

Hypothesis [D] above.

Is there some variation of the Malitz Game for which it is consistent that Player I always wins in NF?

The Malitz Game is nice because it leads to a characterization of all sets as being semi-well-founded, it provides simple ways to build models. Is there a variation of the Malitz Game or Forster's game that allows similar stuff for NF?!?

1.55 A message from Adrian

Let M be the model obtained as follows. Put $t = \{0, \{0\}, \{\{0\}\}, ...\}$. Notice that t is transitive. Set $M(0) = t$, $M(n+1) = Power(M(n))$ and let $M = \bigcup_{n<\omega} M(n)$. Then ω is not a member of M: that follows from our first **Lemma** $x \cap \omega = n$ implies $Power(x) \cap \omega = n+1$ which is readily proved by induction on n and since $t \cap \omega = 2$ has the **Corollary** $Power^k(t) \cap \omega = k+2$ M is a model of the rest of Zermelo (the set of $M(n)$'s is fruitful in the sense of my paper except for 1.0.1), and t is a member of M, and is Dedekind infinite under the map $z \mapsto \{z\}$. *[amusing question: does this model contain a relation on t which well-orders it in order type ω ? actually it does, but can you prove in our* *weakened Zermelo, that there is a dedekind infinite set which is well-orderable ? If you're desperate, start from the assumption that there is a set z such that $0 \in z$ and whenever $y \in z$ then $\{y\} \in z$.*

Bonus marks if you do NOT USE the power set operation.]

On the other hand, if you start from $N(0) = \omega$ and then iterate the power set operation ω times, you get a model of Zermelo containing ω but not containing t. Call it N.

Theorem $N \cap M = HF$.

Define $z(0) = 0$, $z(n+1) = \{z(n)\}$, so $t = \{z(n) \mid n \in \omega\}$.
Define $s(n) = \{z(m) \mid m < n\}$.

Lemma $0 = s(0)$, $1 = s(1)$, $2 = s(2)$.
there it stops, baby.

Lemma $\omega \cap t = 2 = s(2)$. **Lemma** $x \cap t = s(n)$ implies $Power(x) \cap t = s(n+1)$. **Corollary** $Power^k(\omega) \cap t = s(k+2)$. *Proof of the theorem:* Suppose $x \in Power^k(t) \cap Power^m(\omega)$. We show that $x \in HF$. Case 1: $k \geq m$: then $\bigcup^m x \subseteq Power^{k-m}(t) \cap \omega = k - m + 2$

Case 2: $k < m$: then $\bigcup^k x \subseteq t \cap Power^{m-k}(\omega) = s(m-k+2)$. In either case, $\bigcup^j x$ for some finite j is a subset of a hereditarily finite set, and therefore x is hereditarily finite. $\dashv$

1.56 Does *NF*+ AxCount$_\leq$ prove Con(*NF*)?

Since *NF*+AxCount$_\leq$ proves Con(Zermelo) and various people have conjectured that *NF* is no stronger than Zermelo, we would expect that *NF*+AxCount$_\leq$ proves Con(*NF*).

Actually *NF*+AxCount$_\leq$ proves Con(Zermelo) by a pretty roundabout route (You have to prove that there is a wellfounded extensional relation of rank ω_ω with no holes, and you infer this from the existence of sets of size $\aleph_\omega$) so we shouldn't be too discouraged by the apparent difficulty of proving that *NF*+AxCount$_\leq$ proves Con(*NF*). See Roland: *NF* et l'axiome d'universalité: jaune n)

Anyway, if we are to show that *NF*+AxCount$_\leq$ proves Con(*NF*) the obvious thing to do is to try to recreate in *NF*+ AxCount$_\leq$ Orey's demonstration that *NF*+ Axiom of counting$\vdash Con$(*NF*).

OK, let's have an Orey model with four types. That is to say $T_0 = \iota^3``V$; $T_1 = \iota^2``V$; $T_2 = \iota``V$ and $T_3 = V$. Also $\in_2$ ($\in$ between types 2 and 3) is $\subseteq$, $\in_1$ ($\in$ between types 1 and 2) is $\mathtt{RUSC}(\subseteq)$, $\in_0$ ($\in$ between types 0 and 1) is $\mathtt{RUSC}^2(\subseteq)$. Let the variables of bottom type be a with subscripts. Then b for type 1 and so on.

We might be interested in assignment functions f that commute with T in the sense that

$$\begin{array}{l}(\forall n)(\forall x)(f(\ulcorner d_n \urcorner) = x \to (f(\ulcorner c_{Tn} \urcorner) = \{x\}))\wedge \\ (\forall n)(\forall x)(f(\ulcorner c_n \urcorner) = x \to (f(\ulcorner b_{Tn} \urcorner) = \{x\}))\wedge \\ (\forall n)(\forall x)(f(\ulcorner b_n \urcorner) = x \to (f(\ulcorner a_{Tn} \urcorner) = \{x\}))\end{array}$$

but this condition is clearly unstratified. The right thing to do is to look for some relation on which to do induction that is wellfounded only if AxCount$_\leq$ holds.

There is a pretty obvious tro on assignment functions. If f sends 'a_n' to x, f^* must send '$b_{T^{-1}n}$' to ι^{-1}'x; if f sends 'b_n' to x, f^* must send '$c_{T^{-1}n}$' to ι^{-1}'x; if f sends 'c_n' to x, f^* must send '$d_{T^{-1}n}$' to ι^{-1}'x.

Slight worry about this: f^* contains less information than f beco's it says nothing about what happens to a variables.

To recap. Type 0 is ι^3"$V \times \{0\}$; type 1 is ι^2"$V \times \{1\}$; type 2 is ι"$V \times \{2\}$; type 3 is $V \times \{3\}$. The tro τ is $\lambda x.\langle \iota^{-1}\mathtt{fst}(x), \mathtt{snd}(x) + 1\rangle$.

Notice that the + operation on formulæ must commute with T if we are to stay sane, but it will commute if the gnumbering is natural and recursive

If f is an assignment function defined on variables of type 0, 1 and 2, then $\mathtt{caf}(f)$ (Orey's notation) is the function that, on being given a variable of types 1 2 or 3, with gnumber n, shunts it down one type (remember + and its inverse are homogeneous operations!), applies T to it (presumably it doesn't matter in which order it does these two things since + commutes with T) applies f to the resulting variable to obtain $\langle x, k\rangle$ (where k is 0, 1 or 2) and returns $\tau(\langle x, k\rangle)$ which is to say $\langle \iota^{-1}(x), k + 1\rangle$

$$\mathtt{caf}(f) = \lambda n.\tau(f(T(n^-)))$$

The idea is that $\mathtt{caf}$ is a bijection between the assignment functions for types 0, 1 and 2 and the assignment functions for types 1,2 and 3. A Pétry diagram will show that $\mathtt{caf}(g) = f$ is stratified but inhomogeneous: 'g' is one type higher than 'f'.

Is it obvious that f satisfies Tn iff $\mathtt{caf}(f)$ satisfies n^+? Is this immediate or something very hard that we have to prove? ("yes, gentlemen, it is obvious") and we prove it by structural induction on formulæ

I think the hard thing to prove is that that f satisfies n iff it satisfies Tn. (Remember that Tn and n talk about the same types). So perhaps what we should be trying to prove is that n is true (satisfied by all assignment functions) iff Tn is true. Any chance of proving this by induction on the funny wellfounded relation in $\mathbb{N}$? It's not looking hopeful: There doesn't seem to be any reason why AxCount$_\leq$ should be any more useful than AxCount$_\geq$.

This may be the place to think about André's axiom scheme. Also Friederike's axiom about fast-growing functions.

Something worth bearing in mind is that AxCount$_\leq$ is strong only when there are big sets around: Mac is equiconsistent with *KF*. So we must make use of big sets.

1.57 Weak Stratification – conversations with Albert

Albert and I are going over Michael's latest thoughts on Cnumbers in NFU, and thinking about the significance of there being lots of empty sets.

[This is northern autumn 2019, in Cambridge]

I have never worried about this until now, but Albert has pointed out to me there is a problem.

We work with NFU. It has lots of atoms, empty sets. We want to make life easier for ourselves by designating *one* of the empty sets as **the** empty set. I had always supposed that this was completely unproblematic – just expand the language by adding a new constant symbol. We have a new notion of stratification, according to which the new constant symbol can be given any type – indeed *multiple* types in any stratification. The obvious question is: is this extension conservative? Do we get any new theorems in the old language? I had always assumed that the answer is 'no' and it seemed so obvious that i had never bothered to check it. However on reflection it seems a great deal less obvious. Is it even true!?

One way of phrasing this question occurs to me (I'm not sure if it is exactly the same question but it's pretty similar). Take two copies of a model of NFU and expand them by decorating in each an empty set as *the* empty set. Are these two structures elementarily equivalent? This sounds *very* like an old question about whether the atoms in a model of NFU can be indiscernible. That looks like a strong assumption (Holmes has shown that most methods of producing models of NFU produce models in which the atoms are very much discernible!)

So perhaps we should be considering the following. Think of NFU Expand $\mathcal{L}(\in, =)$ by adding a symbol for an empty set. Allow stratified comprehension for formulæ containing the new symbol. Look at the $\mathcal{L}(\in, =)$ theorems provable in the new theory. Is this NFU? Presumably. However, now ask what sounds like the same question in a different way. Fix a model $\mathfrak{M} \models$ NFU. Expand it by decorating one of the empty sets to obtain a structure for the new language. Are all structures thus obtained elementarily equivalent? Presumably this is a nontrivial assumption.

Albert is contrasting this with a situation that he calls 'parametric interpretation' where (in this case) you reserve a variable –'λ', say – to point to the empty set, but it remains a variable. Under this scheme everything in the range of this ("parametric") interpretation has a free variable in it. This means that the axiom giving the existence of the von Neumann ordinal 2 is no longer stratified:

$(\exists x)(\forall y)(y \in x \longleftrightarrow y = \lambda \vee (\forall z)(z \in y \longleftrightarrow y = \lambda))$

tho' it is of course still *weakly* stratified.

Albert is saying that it is not obvious that NFU interprets NFU* (the theory in the new language) [Later: i am no longer sure what are the formulæ of the new theory: are they the literal translations using λ in this way, open formulaæ? Or are they things of the form $(\forall\lambda)((\forall x)(x \notin \lambda) \to \phi)$? I think that must be what is meant...see below]

Is this extension conservative?

The answer is yes! And for well-understood general reasons that i have never thought about, to my shame.

Suppose $(\exists y)(\forall z)(z \in y \longleftrightarrow \phi(z))$ is a comprehension axiom under the new dispensation, with lots of occurrences of '$\emptyset$', possibly at lots of different types. Replace every occurrence of '$\emptyset$' with an occurrence of a new variable 'λ'. We now need the axiom

$$(\forall \lambda)(\exists y)(\forall z)(z \in y \longleftrightarrow \phi(z, \lambda))$$

but this is weakly stratified and therefore is an axiom!

Let's try to place this is a general context. Names not just for the empty set...!

Suppose NF(U)$\vdash$ $(\exists x)(\phi(x))$. We expand the language by adding a constant symbol 'p' and extend NF(U) by adding an axiom $\phi(p)$. We modify our definition of stratification for the new language by ruling that different occurrences of 'p' in a formula may be given different types in a stratification. This gives us new comprehension axioms. We want the new theory to be a conservative extension of the old.

We rely on two facts.

(i) The comprehension axioms of NF(U) allow parameters;
(ii) the eigenformula in a comprehension axiom of NF(U) is required merely to be *weakly* stratified and is not required to be stratified.

Take a new comprehension axiom:

$$(\forall \vec{u})(\exists x)(\forall y)(y \in x \longleftrightarrow \psi(\vec{u}, y, p))$$

where ψ is weakly stratified in the new sense, where every occurrence of 'p' can be given any type in a stratificiation. Replace every occurence of 'p' by a new variable 'w' and bind the new variables to obtain

$$(\forall w)(\forall \vec{u})(\exists x)(\forall y)(y \in x \longleftrightarrow \psi(\vec{u}, y, w))$$

which is in the old language and weakly stratified, and is therefore an axiom.

Now we instantiate the '$\forall w$' to some a such that $\phi(a)$ and we infer

$$(\forall \vec{u})(\exists x)(\forall y)(y \in x \longleftrightarrow \psi(\vec{u}, y, a))$$

which is now a theorem of NF(U) and is an alphabetic variant of the suspect new comprehension axiom.

Hmm .... That's clearly true and important, but it's not yet a proof of conservativeness.

Suppose we have, in the new theory, a proof of a formula A that does not mention 'p'. It uses various axioms like

$(\forall \vec{u})(\exists x)(\forall y)(y \in x \longleftrightarrow \psi(\vec{u}, y, p))$ and $\phi(p)$. It seems pretty clear that we can manipulate this proof into a proof of A in the old theory that uses $(\forall w)(\forall \vec{u})(\exists x)(\forall y)(y \in x \longleftrightarrow \psi(\vec{u}, y, w))$, but it might be instructive to supply the details.

■

1.57.1 Afterthoughts

This should work comfortably in general. Let T be a theory that proves $(\exists x)\phi(x)$. Extend $\mathcal{L}(T)$ by adding a constant symbol 'p' and extend T by adding an axiom $\phi(p)$, thus obtaining a new theory T'. Let Ψ be an expression, not containing 'p', which has a T'-proof. This proof will look like

$$\begin{array}{c} \phi(p) \\ \vdots \\ \Psi \end{array}$$

We can modify this to

$$\dfrac{[\phi(p)] \quad \begin{array}{c}\vdots \\ (\exists x)\phi(x)\end{array}}{\begin{array}{c}\vdots \\ \Psi\end{array}}$$

where the '$\phi(p)$' is the discharged assumption of an $\exists$-elimination. If we do this to all occurrences of '$\phi(p)$' we obtain a T-proof.

So this worry about parameter-freeness is a red herring. Unless the availability of parameters does more than prove conservativeness, that is. For example, we know that if we add to $\mathcal{L}(\in, =)$ a symbol '$\emptyset$' and add to any theory T that proves $(\exists y)(\forall x)(x \notin y)$ an axiom $(\forall x)(x \notin \emptyset)$, then $T \cup \{(\forall x)(x \notin \emptyset)\}$ is a conservative extension of T. But now let's think of NFU as a theory in the pure language of set theory – no constant symbols. Take a model – any model – of this version of NFU, and make two copies of it. Expand the theory by adding a name for the empty set, and expand the two models by baptising, in each, one of the atoms as THE empty set. Different atoms of course. Are these two structures elementarily equivalent? That doesn't obviously follow. But perhaps the extra control given by the parameters might help.

1.57.2 More Afterthoughts

Albert has a thing that he calls a *profile* of a formula.

This idea of parametric interpretation must be something to do with cylindrification.

1.58 Smash for Albert

Albert: this is how i encountered the smash function in NF – always in connection with infinite cardinals.

We don't expect to be able to define $\alpha * \beta$ for arbitrary cardinals α and β. However we would expect to be able to do it if α is 2^γ and β is 2^δ for some γ and δ.

We want to define $2^\alpha * 2^\beta$ to be $2^{\alpha\cdot\beta}$. (D)

Prima facie we have a problem because there might be lots of cardinals δ such that $2^\delta = 2^\alpha$ so it might matter which of the δs we put into the *definiens* of (D). But it doesn't! Suppose $2^\delta = 2^\alpha$. Then

$$2^{\delta\cdot\beta} = (2^\delta)^\beta = (2^\alpha)^\beta = 2^\alpha * 2^\beta.$$

My reason for interest in this operation (and its higher congenenors) was this.

Consider a sequence of cardinals $\alpha_0, \alpha_1, \alpha_2 \ldots$ where $\alpha_{i+1} = 2^{\alpha_i}$, and α_0 is Dedekind-infinite. The αs get better behaved as the subscripts get bigger.

We have:
$\alpha_0 + 1 = \alpha_0$;
$\alpha_1 + \alpha_1 = \alpha_1$;
$\alpha_2 \cdot \alpha_2 = \alpha_2$;
and then
$\alpha_3 * \alpha_3 = \alpha_3$.
(This last is beco's $\alpha_3 * \alpha_3 = 2^{\alpha_2\cdot\alpha_2} = 2^{\alpha_2} = \alpha_3$.)
and so on, getting nicer and nicer equations (using operations that we have no notations for!). We even get

$$(\alpha_3)^{\alpha_2} = (2^{\alpha_2})^{\alpha_2} = 2^{\alpha_2\cdot\alpha_2} = 2^{\alpha_2} = \alpha_3.$$

Why might this matter? The point is that, in NF (+ Counting), there are cardinals ($|V|$ is one) that, for every (concrete) $n \in \mathbb{N}$, can be seen as α_n in a sequence like the above. The hope is that this will enforce on these cardinals good behaviour of the kind that will contradict the known refutation of AC for large cardinals.

There may be a crunch point to be found along these lines, but i've never found one.

1.59 A new refutation of AC in NF(?)

GC is the principle that i call "Group Choice" since it is the version of AC that is need to prove that, in a full symmetric group, permutations of the same cycle type (sometimes called *conformal*) are conjugate. So GC is the principle that every set of countable sets has a selection function. This is *not* usual countable choice, which says that every countable family of sets has a choice function.

The proof is made of several jigsaw pieces, some of them quite old.

If there is an antimorphism then AC_2 fails.
All cycles of an antimorphism are even or infinite;
If we have AC for set of finite sets then any two permutations of the same cycle type are conjugate;
If we can find a permutation τ such that τ and $j\tau \cdot c$ (c is complementation) are conjugate then there is a permutation model containing an antimorphism.

The missing piece, which i have only just computed, is the relation between the cycle types of $j\tau$ and $j\tau \cdot c$. The cycle type of $j\tau$ constrains the cycle type of $j\tau \cdot c$ very closely. What we are after is a permutation τ such that τ and $j\tau \cdot c$ are conjugate.

First we consider even cycles in $j\tau$. We consider them in pairs, in that – for any x – we consider the cycle of x and the cycle of $c(x)$ together. These two cycles might be the same, of course, and in those circumstances there is nothing to do.

[oops – what happens if there is a single $j\tau$-cycle $\{x, c(x)\}\ldots$? For the moment let's suppose that no $j\tau$-cycle contains both x and $c(x)$.]

Let x be a member of a $2n$ cycle under $j\tau$. Then $c(n)$ belongs to a $2n$ cycle, and these two $2n$-cycles are conjugated by c. Colour all the elements of the cycle of x red and all the elements of the cycle of $c(x)$ blue. Then these $4n$ inhabitants of these two $j\tau$ cycles belong to two $j\tau \cdot c$ cycles; and both these two $j\tau \cdot c$ are of course of size $2n$ and they consist of alternating red and blue elements. Thus a pair of even $j\tau$-cycles (and all such even cycles come to us in pairs as indicated above) gives rise to a pair of even cycles in $j\tau \cdot c$. No other brace of cycles is involved in this construction at all. The treatment of infinite cycles is similar.

Thus if all cycles in $j\tau$ were even or infinite then $j\tau$ and $j\tau \cdot c$ would have the same cycle type. So we need to consider odd cycles in $j\tau$.

Odd cycles, indeed, come in braces[11] in the same way even permutations do: the $(2n+1)$-cycle containing x and the $(2n+1)$-cycle containing $c(x)$. As before, these two cycles are conjugated by c. As before, we colour everything in one cycle red and everything in the other cycle blue. Then there is a single $j\tau \cdot c$-cycle of size $4n+2$ wherein the blue and red points alternate.

So *as long as no $j\tau$-cycle contains both x and $c(x)$* we can conclude that $j\tau \cdot c$ has no odd cycles. The idea is now that $j\tau \cdot c$ and τ have the same cycle type and will therefore be conjugate by GC and we will obtain an antimorphism.

For this to work we need τ to have no odd cycles, and $j\tau$ must have the largest possible number of $2n$-cycles for each n, so that when we add new even cycles by stitching together the odd cycles in $j\tau$ we do not increase the number of $2n$-cycles and thereby preclude conjugacy with τ.

OK. What happens if some $j\tau$-cycle contains both x and $c(x)$ for some x? Observe that if $\tau^k``x = c(x)$ then $\tau^k``c(x) = x$ so the cycle containing x and $c(x)$ is of size $2k$ and is even. So don't have to worry about the possibility of odd cycles ever containing x and $c(x)$.

[11] Is this word too old-fashioned? Brace of pistols, of partridges. . . ?

For example if there is $x = \tau``(V \setminus x)$ we are in big trouble, so that cannot be allowed to happen. Unfortunately if all τ-cycles are even then AC_2 will produce such an x.

But we might be able to recover something. Consider the family of partitions of V into pairs *that lack transversals*. Think of such a partition as an involution τ. Then $j\tau \cdot c$ is another partition of V into pairs. Does it lack transversals? The mirage on the horizon is the thought of a Bowler-maximal partition of this kind. The set of such partitions/involutions is upward-closed in Bowler's order.

Need to check whether or not this operation is monotone...

Sse $\tau \leq \sigma$ in virtue of $f : V \hookrightarrow V$. Thus $\{\{f(x), f(y)\} : \{x, y\} \in \tau\} \subseteq \sigma$. We want $\{\{x, j\tau \cdot c(x)\} : x \subseteq V\} \leq \{\{x, j\sigma \cdot c(x)\} : x \subseteq V\}$ in virtue of (presumably!) jf.

So we want

$$\{\{f``x, f``(V \setminus \tau``x)\} : x \subseteq V\} \ \subseteq \ \{\{x, V \setminus \sigma``x\} : x \subseteq V\}$$

So we want

$$f``(V \setminus \tau``x) = x, V \setminus \sigma``f``x$$

and there is no way that is going to be true. So the operation is not monotone. This has got garbled

But there still remains the question of whether or not there is a Bowler-maximal partition into pairs that lacks a transversal. Is there a candidate? Consider the set $\{x : x \neq c``x\}$. It splits naturally into pairs $\{x, c``x\}$. It would be nice if this was somehow a partition-into-pairs that was most likely to lack a transversal. But! This is an immediate consequence of Nathan's demonstration that jc is a universal involution.

This could enable us to constrain the complexity of AC_2. AC_2 holds iff there is a choice function on $\{\{x, c``x\} : x \in V\}$

$(\exists T)(\forall x)(x \neq c``x \to x \in T \longleftrightarrow c``x \notin T)$

$c(y) \in x$ is ... both $(\forall w)(w = c(y) \to w \in x)$ and $(\exists w)(w = c(y) \wedge w \in x)$ and $w = c(y)$ is $\forall$ so $c(y) \in x$ is ... both $\forall\exists$ and $\exists\forall$

Now $x \neq c``x$ is $(\exists y)(c(y) \notin x)$ which is $\exists\forall$

How many quantifiers? I think it's going to be four whatever happens.

Anyway! The fact that jc is Bowler-maximal ("universal") means that its restriction to the set of things not fixed by jc can be copied over to V to give us a maximal ("universal") involution without either fixed points or transversals. This is beco's $\{x : x \neq c``x\}$ – and, for that matter – $\{x : x = c``x\}$ is of size $|V|$). Let's call this involution υ (to recall universal). Then $j\upsilon \cdot c$ is an involution without fixed points. It remains to show that it lacks transversals and is maximal. That sounds possible, but it would involve a great deal of computation beco's the definition of υ is so convoluted.

This is progress of a sort. Bowler's work shows that there is a definable partition of V into pairs with the property that if it has a choice function then all sets of pairs have choice functions.

1.60 Someone should write this up

We know what the TST_n are. How strong are they? It seems that (and the original text on this seems to be [3]: "Some formal relative consistency proofs. Journal of Symbolic Logic **18**, pp. 136–144.) that TST_{n+1} – or perhaps TST_{n+2} – proves the consistency of TST_n. Truth definitions. I am a bit worried about this. Plain vanilla TST (thought of as TST_ω in this setting) is equiconsistent with PA. How can we fit in infinitely many theories between TST_2 and TST_ω? Perhaps we need infinity for these truth definitions to work? I have never thought about the details of a consistency proof for TST_n in TST_k with $k >> n$. I want to get straight the role of AxInf in this.

There is a related issue in need of clarification, not least because the similarities can cause confusion (they confused me all right!). We can restrict any TST_n by restricting the degree of impredicativity allowed in the comprehension scheme. Randall has persuaded me that this hierarchy collapses, and the reason is as follows. *For any ϕ whatever* existence of $\{\iota^k(x) : \phi(x)\}$ is a predicative axiom for k suff large. Then one repeatedly applies the axiom of sumset. This is clearly a key fact.

The task of writing this up is one i should public-spiritedly take up. I am less busy than the two of you and i am in lockdown! I must say i am not looking forward to processing McNaughton. The notation is ∼70 years old and and it's not an easy read.

Any suggestions welcome.

1.61 A Contribution from Isaac Malitz

```
From malitzi@logic-handle.com Mon Aug 11 19:54:43 1997
Received: by emu.dpmms.cam.ac.uk (UK-Smail 3.1.25.1/1); Mon, 11 Aug 97 19:54 BST
Received: from ISAAC by mail.pronex.com (NTMail 3.01.03) id ua019338; Mon, 11 Aug 1997 12:02:24 -0700
X-Sender: malitzi@mail.pronex.com (Unverified)
X-Mailer: Windows Eudora Pro Version 2.1.2
Mime-Version: 1.0
Content-Type: text/plain; charset="us-ascii"
To: t.forster@pmms.cam.ac.uk
From: Isaac Malitz <malitzi@logic-handle.com>
Subject: Axiom of pseudofoundation
Date: Mon, 11 Aug 1997 12:02:24 -0700
Message-Id: <19022490000576@mail.pronex.com>
Status: RO
```

This is in response to an issue raised in your talk at the NF conference.

You were looking for some kind of "axiom of foundation" suitable for NF.

In what follows, I will describe two axioms of pseudofoundation; I suspect that the second one is suitable.

Both of these axioms are characterized by means of games. The first one will look familiar, the second one is a variation on the first.

1. Extensionality Game 1

"All there is to know about a set is its members."

This game is played by two players, Eve and Adam. The game has potentially an infinite number of stages STAGE0, STAGE1, ...

STAGE0 begins with two distinct sets. The objective of Eve is to "demonstrate" that these two sets are distinct (in a finite number of stages). The objective of Adam is frustrate Eve's efforts by causing the game to go on without end.

The game is played as follows: At each stage,there are two sets (the "sets-for-that-stage"). At each stage, Eve picks a member of one of the two sets-for-that-stage; the set picked by Eve is known as "Eve's set". Then Adam picks a member of the other of the two sets-for-that-stage; the set picked by Adam is known as "Adam's set". It is required that Eve's set be a member of exactly one of the two sets-for-that-stage; it is required that Adam's set be distinct from Eve's set.

The game begins with two distinct sets at STAGE0. If the game reaches a stage where Adam is unable to respond, then Eve wins. If the game goes on forever, then Adam wins.

(Intuitively: At each stage, Eve is saying "I can demonstrate that the two sets-for-this-stage are distinct. Specifically, I am picking a set EVEn that is a member of one but not the other". Adam responds "Well, there is a set ADAMn that is a member of the other set-for-this-stage; demonstrate to me that EVEn and ADAMn are distinct")

COMMENTS: If the game begins with two distinct well-founded sets, then Eve wins. If the game begins with two distinct non-well-founded sets, Eve can still win, provided that there are appropriate distinct well-founded sets embedded in the respective epsilon trees.

If the game begins with V and $V \setminus \{V\}$, Adam wins: At each stage, Eve is forced to select V; a winning strategy for Adam is to select $V \setminus \{V\}$ at each stage. Intuitively, there should be a variation of Extensionality Game 1 that allows Eve to win in this circumstance.

2. Extensionality Game 2

"Two sets can be distinguished by means of different members *or* different non-members".

Game is same as Extensionality Game 1, except that at each stage, Eve may (optionally) pick a set which is *not* a member of (exactly) one of the two sets-for-that-stage. If Eve does this, then Adam is required to pick a non-member of the other set-for-that-stage which is distinct from Eve's set.

COMMENT: If the game begins with V and $V \setminus \{V\}$, then Eve wins: At STAGE 0, she picks a non-memb

1.61.1 Another game

Consider also the game H_x played as follows. If x is empty, `II` loses. Otherwise `I` picks $x' \in x$ and they play $H_{x'}$, swapping rôles. Thus `I` wins iff the game ever comes to an end.

Let `A` be the collection of sets Won by `I`, and `B` the collection of sets Won by `II`. If even one member of x is a subset of `A` then for his first move `I` can pick that element, and then, whatever member x'' of it `II` chooses, the result is a game for which `I` has a winning strategy. Thus $Ƅ(\mathcal{P}(\mathtt{A}) \subseteq \mathtt{A}$. Similarly, if every member of x contains a member of `B` then whatever `I` does on his first move, `II` can put him into a game $H_{x'}$ with $x' \in^2 x$ for which she has a winning strategy, so $\mathcal{P}(Ƅ(\mathtt{B})) \subseteq \mathtt{B}$. Indeed, that is the only way `II` can win, by living on to fight another day, so in fact we have $\mathcal{P}(Ƅ(\mathtt{B})) = \mathtt{B}$. But wait! We don't mean "power set of" $Ƅ(\mathtt{B}) = \mathtt{B}$! we mean "set of nonempty subsets of" $Ƅ(\mathtt{B}) = \mathtt{B}$! Without this we would have concluded that in this games `II` can Win any set x for which she could have won G_x. This is obviously wrong, because `II` Wins $G_{\{\Lambda\}}$ but is doomed to lose $H_{\{\Lambda\}}$ whatever `I` does: `II` cannot win H_x if x is wellfounded.

1.62 Non-principal ultrafilters

See the discussion after conjecture 9. The assertion that there is a nonprincipal ultrafilter on V is $\Sigma_1^{\mathcal{P}}$ (that is to say, simple).

Are ultrafilters extensional?

Are there any symmetric non-principal ultrafilters?

Any $\mathcal{U}$ on $\{x : x \text{ is } (n-2)\text{-symmetric}\}$ is n-symmetric and extends to a $\mathcal{U}$ on V.

1.63 A pretty picture

Recursive models	Decidability		Axiomatisability
	Is NF Γ-complete	Is ΓNF recursive?	$NF = \Gamma NF$?
$NF0$ yes $NF\forall_1$ yes	$\exists_1$ yes		$\exists_2$ No $\Pi_2^{\mathcal{P}}$ No
$NF\forall^1$ yes?	$str(\forall_2)$ yes? $str(\forall_3)$ No?		
	$\forall_2$ No $str(\exists_3^+)$ No	$str(\exists_3^+)$ No	$\Sigma_2^{\mathcal{P}}$ yes $\forall_4^+$ yes $str(\forall_4 \cup \exists_3^+)$ yes

1.64 Typical ergodicity

Most of this stuff has been moved to TℤTstuff.tex. However there does remain one thing...

Another kind of weak ambiguity that could be confused with Typical Ergodicity (well, *I* confused it) is that exhibited by a model $\mathfrak{M}$ (of TℤT) where, for each closed formula ϕ, there is $n \in \mathbb{N}$ such that $\mathfrak{M} \models$ the scheme $\phi \longleftrightarrow \phi^n$ over all levels. Are there models of TℤT in which for every ϕ there is an n such that ...? Presumably yes (beco's we believe NF to be consistent) but is "for every ϕ there is n ..." weak enough to not refute AC? What happens to $\mathrm{T\mathbb{Z}T} + AC$ if it does? Think about the tree of lists of pairs $\langle \phi, n \rangle$ meaning the scheme $\phi \longleftrightarrow \phi^n$ over all levels. Ordered by reverse end-extension of course. If the grand scheme is inconsistent then the set of consistent lists is a wellfounded fragment of the tree and every list in it has a rank. The lower the rank the stronger the theory(!!?!)

At some point i must work out whether Van der Waerden's theorem has anything to say about Amb^n.

Is omitting types a weapon for this..?

1.65 Some tho'rts about the theory of wellfounded sets in NF

The more i think about it, the odder the proof of AxInf looks. We have the principle that The Attic tells us nothing about wellfounded sets. So The Attic isn't going to tell us that there are infinite wellfounded sets. But it does tell us that there are infinite (small) sets.

In Randall's models the power set of a wellorderable set is wellorderable. So the set of wellorderable sets is a fat set, so every wellfounded set is wellorderable.

It obviously contains pairing, power set, sumset and extensionality. Also (for what it's worth) stratified $\Delta_0^{\mathcal{P}}$ separation. That's KF – which is a neat fit. V is a $\mathcal{P}$-extension of WF so this theory contains every $\Pi_1^{\mathcal{P}}$ theorem of NF.

Does TCo hold in WF?

One hopes to be able to say that WF has a nice stratified theory. The problem is that even if ϕ is stratified, ϕ^{WF} typically won't be. Thus one can't show that WF obeys stratified replacement, despite the following line of chat. Suppose x is wellfounded, and f is a function that takes wellfounded sets to wellfounded sets such that '$y = f(x)$' is stratified. Let X be a wellfounded set; $f``X$ exists by stratified replacement in NF. It is a set of wellfounded sets and is therefore a wellfounded set. However this doesn't mean that WF satisfies stratified replacement. The point is that '$y = f(x)$' being stratified doesn't ensure that there is a stratified g such that $y = f(x)$ is equivalent to $y = g(x)$ where f is g interpreted inside WF.

So what in fact happens is that WF satisfies stratified $\Delta_0^{\mathcal{P}}$ replacement. This is beco's V is a $\mathcal{P}$-extension of WF: the inclusion embedding $WF \hookrightarrow V$ is elementary for $\Delta_0^{\mathcal{P}}$ formulæ.

Notice that this argument does not support stratified $\Delta_0^{\mathcal{P}}$ collection. Suppose 'F' is stratified, and for every wellfounded x there is a wellfounded y such that $F(x, y)$. (This is the antecedent of an instance of stratified collection). Let X be a wellfounded set. By stratified collection in NF there is now a collection C such that $(\forall x \in X)(\exists y \in C)F(x, y)$. The trouble now is that there is no reason to suppose we can exhibit such a C which is wellfounded. The intersection of C with the class of wellfounded sets would do but of course there is no reason for it to be a set. Subject to the same caveat as stratified replacement. Requiring F to be str$\Delta_0^{\mathcal{P}}$ doesn't help.

Naturally WF obeys the axiom of regularity. However, inferring $\in$-induction needs a certain amount of comprehension. Suppose NF proves $(\forall x)((\forall y \in$

$x)\phi(y) \to \phi(x))$. If $\phi \in \Delta_1^{\mathcal{P}}$ then this formula is $\Pi_1^{\mathcal{P}}$ and generalises downwards to WF. Furthermore $\{x : \phi(x)\}$ is a fat set and contains all of WF. For this to imply that WF believes $(\forall x)\phi(x)$ we need ϕ to be $\Pi_1^{\mathcal{P}}$.

It still seems to be open whether or not the stratified part of this theory is invariant. I think it is clear that the unstratified part might not be invariant. Holmes' clever permutation can kill off all infinite transitive subsets of V_ω, and it is certainly consistent with NF + AxCount$_\leq$ that there should be an infinite transitive subset of V_ω – for example V_ω itself. However we can use the fact that V is a $\mathcal{P}$-extension of WF to show that the str$\Delta_1^{\mathcal{P}}$ fragment is invariant. Suppose WF $\models \phi$, where $\phi \in$ str$\Delta_1^{\mathcal{P}}$. ϕ generalises upwards and so holds in V. Let π be a permutation. We have ϕ^π and we now want to say that ϕ^π will generalise downwards co's it's $\Delta_1^{\mathcal{P}}$. But is it really $\Delta_1^{\mathcal{P}}$?

The inclusion embedding WF $\hookrightarrow_e^{\mathcal{P}} V$ is not elementary for $\Sigma_1^{\mathcal{P}}$-espressions. Not even elementary for *stratified* $\Sigma_1^{\mathcal{P}}$ expressions: the existence of an infinite set is $\Sigma_1^{\mathcal{P}}$ and is a theorem of NF but we don't know how to prove the existence of an infinite wellfounded set. Indeed we don't even know how to prove it consistent relative to NF that there should be an infinite wellfounded set – tho' it's clearly consistent relative to NFC. In fact i show in [5] that every model of NF + AxCount$_\leq$ has a permutation model containing the graph of the rank prewellordering on V_ω. Holmes' clever permutation shows that this theory does not prove the existence of any infinite transitive subset of V_ω.

Also there are $\Sigma_1^{\mathcal{P}}$ things that say there are ridiculously big sets, whereas all wellfounded sets are small. Also of course there is a $\Sigma_1^{\mathcal{P}}$ sentence saying there is a self-membered set, and that of course can't be true in WF. So perhaps one has to restrict oneself to stratified expressions.

Does WF believe IO? NF proves that every wellfounded set is the same size as a set of singletons but there is no evident way to prove it's the same size as a *wellfounded* set of singletons.

The situation with TCo is similar. Every wellfounded set has a transitive superset; trouble is, there's no obvious way to find a *wellfounded* transitive superset.

Collection: it's the same story.

1.66 Second Edition of Rosser

Just (21/viii/23) got my very own copy of the second edition of Rosser, Logic for Mathematicians 2nd edn. I needed it in order to check a bit of history.

Specker published a proof of $\neg$AC in NF, in 1953. He had earlier discovered a proof of AxInf in NF, but improved this to a refutation of AC. He published the refutation of AC and obtained the proof of AxInf as a corollary. He never published the proof of AxInf, and i never thought to ask him what his proof was. I think all NFistes worked out proofs of this fact for themselves. (It's rather like finite axiomatisations of NF: every home should have one, and indeed you should build your own. Any NFiste worth their salt has done so.) I discovered my proof when i was writing my Ph.D. thesis on NF in the mid-70's, and when i started writing the history for the latest edition of the monograph it occurred to me to wonder whether "my" proof of AxInf was really mine or whether someone had shown it me ... in particular whether i had read it in the second edition of Rosser, Logic for Mathematicians. (It can't've been the first edition – which of course i *had* read – beco's that came out before Specker's work became known). Now that i have a copy of the second edition i can confirm that i did not learn this proof from Rosser – the second edition is later than my thesis! We may never know for sure exactly what Specker's proof was, tho' the best guess must be that it is the same as Rosser's proof – which turns out to be the same as mine. Rosser does not attribute his proof to Specker. One would have expected them to compare notes, so it's a bit odd that the matter is so unclear. It may be that Rosser – on seeing Specker's refutation of AC – found his own proof of AxInf. We've all done things like that, it being so much easier to prove something when you know what it is and that it's true.

The new idea, crucial to the proof, is that of a *cardinal tree*. It's there in Rosser, tho' he does not identify it nor develop it beyond the bare minimum needed for the proof. Holmes always calls them *Specker trees* on the (not unreasonable) grounds that: since every proof of AxInf uses them, Specker's original proof must have used them, so it must've been Specker who invented them. The reasoning is flawless, but i had fondly hoped that i would get a pat on the back for being the first person to show them to the public (in my thesis), not least because it was i who proved the fundamental theorem that these trees are wellfounded. (Making a song-and-dance about this fact was one of the things Boffa – one of my examiners – wanted me to do in my resubmission) Small fry like me don't get many chances to have things named after us. My late zoologist friend John Leader once named a midge after me – even tho' i can hardly claim that it is all my own work – and perhaps that's all i'm going to get.

1.67 Sergei T's "strictly impredicative" TST

$(\exists y)(\forall x)(x \in y \longleftrightarrow \phi(x))$

where all bound variables in ϕ are of type $\geq$ type(x). "can't look inside x". This is stronger than it looks because one is allowed parameters. For example this axiom, giving $\bigcup z$, is allowed:

$(\exists y)(\forall x)(x \in y \longleftrightarrow (\exists w)(x \in w \in z))$

He also wants Frege cardinals. I think (and so does Randall) that a Church-Oswald model of NF_2 is a model of this theory.

Chapter 2

ML

I've stumbled into some interesting questions about ML recently, as a result of deciding to write a tutorial about the Sad History Of the Inconsistency Of The First Edition of ML.

Axiom 1 *Every separative poset has a generic filter.*

Years ago (*years* ago, like early 1970s) i thought i had a proof in ML of the existence of a proper class that totally ordered the universe. I even wrote to Adrian about it. (I found my letter to him years later in a pile of papers he left in G20c). The thing that caught my attention was the least fixed point for the +-operation on posets (as in WQO theory). Of course this least fixed point has an unstratified definition, so you're not going to get it in NF, and even if you get it in ML it's no help co's there's no reason to suppose it's a toset. In fact it's pretty clear that it isn't. My ideas from that time survive in material below (coroll 7) on totally-ordering the term model for NF0.

But Randall's point about starting with a countable model of NF and adjoining all subsets as classes does the trick. If i have a countable model $\mathfrak{M}$ of NF i can always obtain a (countable!) generic subset for any (countable!) separative poset by exploiting the (external) wellordering of $\mathfrak{M}$ – simply wellorder the dense subsets in order type ω and add an element of each dense subset one by one. So any model of ML obtained in this way from a countable model of NF satisfies "Every separative poset has a generic filter". Easy *peasy*.

2.1 Refuting a weak version of Choice in MLU

Consider the following four assertions of *prima facie* increasing strength
(i) $\exists$ class-ordering of V s.t. every nonempty subset has a bottom element.
(ii) $\exists$ class-ordering of V s.t. every nonempty subclass has a bottom element.
(iii) $\exists$ set-ordering of V s.t. every nonempty subset has a bottom element.
(iv) $\exists$ set-ordering of V s.t. every nonempty subclass has a bottom element.

They get stronger as you go down the list: (i) $\to$ (ii) $\to$ (iii) $\to$ (iv).
(i) and (ii) are easily consistent because of Skolem-Löwenheim.;
(iii) is refuted by Specker and it's hard;
(iv) is quite easy to refute.

We have known since Specker 1953 that there cannot be a set $<$ of ordered pairs that totally orders the universe in such a way that every nonempty set has a $<$-minimal element. The proof is short enough to be formally verified but it's still rather mysterious. And it cannot be conducted in NFU. It now seems that, in contrast, (iv) has a very natural refutation, and one that doesn't use the nonexistence of *urelemente*, so it can be conducted in (what one might call) MLU.

THEOREM 4 *(MLU)*
There is no set $<$ of ordered pairs that strictly totally orders V in such a way that every nonempty class has a $<$-minimum element.

Proof:
Suppose there were such a total order, $<$.

Consider the class $\widehat{x}(\neg\mathtt{stcan}(\{y : y < x\}))$. We want it to be empty, so suppose it isn't.

All the initial segments of $<$ are sets, since $<$ is a set. We want all initial segments to be strongly cantorian. Suppose not. Let x be $<$-minimal such that the initial segment $\{y : y < x\}$ is not strongly cantorian. But then that initial segment is a wellordering all of whose proper initial segments are strongly cantorian so it, too, is strongly cantorian by remark 2, contradicting assumption.

So all initial segments are strongly cantorian, so in particular V is strongly cantorian – which of course is not the case.

■

> Notice how we had to exploit the fact that these initial segments are wellorderings. We can't straightforwardly prove that a nested union of strongly cantorian sets is strongly cantorian.

Randall makes the point that this is merely a restatement of Rosser-and-Wang. [11].

If you relate it to [12] then you are struck by the fact that [12] needs extensionaity whereas our result here doesn't. But then perhaps the fact that extensionality is not used isn't really such a big deal: Rosser-Wang applies to NFU too, tho' they didn't spell that out in 1950 co's they didn't know about NFU then!

So perhaps the correct take on theorem 4 is that it's an internalisation in MLU of Rosser-Wang [11] and serves to remind us that the phenomenon they were interested in occurs in NFU as well as in NF – a point they were not in a position to make in 1950 but one which modern readers should take on board.

Chapter 3

Is NF stratified-tight?

I am having difficulty with the idea that a *theory* might be tight. In my idiolect, the only non-literal meaning 'tight' has is *drunk* as in

Cockney bus conductor, **as bus is starting, to standing female passenger**	'old tight, Lady!
Standing female passenger (indignantly)	'Ooo are you a-callin' of an old tight lady?!

(recounted to me by my great-aunt Poppy, a Londoner)

A theory is *tight* iff any two synonymous extensions of it are identical. In saying that two theories are *synonymous* I mean that any model of either can be turned into a model of the other in a definable way, and the two transformations are mutually inverse up to logical equivalence. Boolean rings/boolean algebras; partial orders/strict partial orders, that kind of thing. I think this is also called *bi-interpretability*.

Situations where two models of a theory T have the same carrier set are familiar to us from the (admittedly rather special) situation of Rieger-Bernays permutation models in set theory. They were first dreamt up to prove the independence of the axiom of foundation from ZF(C), but the bulk of the applications have come in an NF context. This is beco's the R-B construction preserves stratifiable formulæ and is therefore a very natural device to use on models of NF.

If τ is a definable permutation and has the further property that in V^τ there is a definable "return" permutation σ such that $(V^\tau)^\sigma$ is isomorphic to V then Th(V) and Th(V^τ) are synonymous, but may not be identical. If we start with a model containing no Quine atoms and let τ be the transposition $(\emptyset, \{\emptyset\})$ then there is such a definable "return" permutation σ and we have precisely the situation described[1] so NF is not tight. However the two theories disagree only on unstratified expressions, so – altho' this is a counterexample to NF being tight – it's not a counterexample to NF being what one might call *stratified-tight*. NF is *in fact* stratified-tight, as we shall soon show. In fact every extension of NF is stratified-tight.

However, before we can give the proof, we need the rather recondite model-theoretic device of *stratimorphism*, which we will now define.

Any structure $\mathfrak{M} = \langle M, \in\rangle$ for $\mathcal{L}(\in, =)$ can give rise to a structure for $\mathcal{L}(TST)$ by the simple device of making multiple copies of it of the form $M \times \{i\}$ for each $i \in \mathbb{N}$, and defining a membership relation on the resulting $\mathcal{L}(TST)$-structure by declaring – for each n – that $\langle x, n\rangle \in \langle y, n+1\rangle$ iff $\mathfrak{M} \models x \in y$. We then say that two $\mathcal{L}(\in, =)$-structures $\mathfrak{M}_1$ and $\mathfrak{M}_2$ for are **stratimorphic** if the two $\mathcal{L}(TST)$-structures obtained as above are isomorphic. Stratimorphic structures agree on their stratifiable formulæ. Stratimorphism is related to elementary-equivalence-for-stratifiable-formulæ rather the way in which isomorphism is related to elementary equivalence, and the reader can probably guess the statement of an analogue of a theorem of Keisler's: $\mathfrak{M}_1$ and $\mathfrak{M}_2$ agree on stratifiable sentences iff they have stratimorphic ultrapowers. We don't need it, so we won't prove it. The thing we *do* need – namely that any two stratimorphic $\mathcal{L}(\in, =)$ structures satisfy the same stratifiable formulæ – is obvious.

We are now in a position to state and prove

THEOREM 5 *Let T be an extension of SF. Suppose $\mathfrak{M}_1 = \langle V, \in_1\rangle$ and $\mathfrak{M}_2 = \langle V, \in_2\rangle$ are two models of SF with the same carrier set, and that their theories are synonymous, in the sense that $x \in_1 y$ is equivalent to a stratifiable formula $E_1(x, y)$ in $\mathcal{L}(\in_2, =)$ and $x \in_2 y$ is equivalent to a stratifiable formula $E_2(x, y)$ in $\mathcal{L}(\in_1, =)$.*

Then $\langle V, \in_1\rangle$ and $\langle V, \in_2\rangle$ satisfy the same stratifiable sentences.

(For the moment I know how to prove it only when E_1 and E_2 are stratifiable, but I suspect it is true anyway. The proof of the stratified version runs as follows.)

Proof:

Let $\mathfrak{M}_1 = \langle V, \in_1\rangle$ and $\mathfrak{M}_2 = \langle V, \in_2\rangle$ be as in the statement of the theorem. We shall show them to be stratimorphic, so we need a family $\langle f_i : i \in \mathbb{N}\rangle$ of permutations of V satisfying, for each $n \in \mathbb{N}$, $(\forall x, y)(x \in_1 y \longleftrightarrow f_n(x) \in_2 f_{n+1}(y))$.

Naturally f_0 – the bijection between the two 0th levels – is the identity. For the recursion to succeed it is important – for set-existence reasons – that the f_i should have definitions that are stratified. What about f_1? To what must the stratimorphism send an element x_1 of level 1 of $\mathfrak{M}_1$? It has a handful of members-in-the-sense-of-$\in_1$. We must send it to that element of $\mathfrak{M}_2$ that has precisely those members ... in the sense of $\in_2$. But this is easy. By assumption '$y \in_1 x$' is a stratifiable expression of $\mathcal{L}(\in_2, =)$, and so its extension is a set by

[1]It is possible to write this out in exact detail – and that would be a good thing to do – but there is no call for it here and now.

stratifiable comprehension in $\langle V, \in_2\rangle$, and by the enhancement (the $\exists!$ quantifier instead of the $\exists$ quantifier) it is unique. Higher levels are analogous. Notice that we need $\in_1$ to be equivalent to a stratified expression of $\mathcal{L}(\in_2, =)$,

■

COROLLARY 2 *Every theory extending NF is stratified-tight.*

However this exploited the fact that '$y \in_1 x$' is a *stratifiable* expression of $\mathcal{L}(\in_2, =)$ and '$y \in_2 x$' is a stratifiable expression of $\mathcal{L}(\in_1, =)$. What happens if we drop this assumption? Do we gain any extra generality? It seems highly implausible that there should be an unstratified expression $E_1(x, y)$ in $\mathcal{L}(\in, =)$ such that NF proves that $\langle V, E_1\rangle \models NF$. This would require that any weakly stratified formula $\phi(x, \vec{z})$ when rewritten with E instead of $\in$ should be sufficiently well-behaved for its extension to be a set.

Let's pursue this. If there is even one such expression there will be lots, co's we can compose such a relation with any permutation to get another. Here's another thing that might be helpful. If E is such an expression then $\neg E$ is another so we could start by asking for the E of minimal logical complexity and then assuming that, once it's in PNF, the leading quantifier is existential; or (if we prefer) that it is universal.

Well, we still have that the two theories are synonymous, which is to say that if we rewrite '$E_1(x, y)$' by replacing all occurrences of '$\in_2$' in it by 'E_1' then the result is an expression of $\mathcal{L}(\in_1, =)$ which is equivalent to '$x \in_1 y$'. The hope is that this fact alone will compel $E_1(x, y)$ and $E_2(x, y)$ to both be stratified.

One might be able to show that E_1 and E_2 are each equivalent to a stratifiable formul *with a parameter*... possibly something as banal as $\langle x, y\rangle \in E$, so the the graph of E_i is a set

Some thoughts:

I hope to be able to sort out the business of the interpretations being stratified in the fullness of time.

For the moment let's turn our attention to tightness in general, to other tight theories. I have the feeling that tightness is something to do with second-order categoricity.

Must $\mathfrak{M}_1$ and $\mathfrak{M}_2$ agree on invariant sentences too?

Presumably every (stratified) extension of a (stratified-)tight theory is (stratified-)tight? Every invariant extension of an (invariant)-tight theory is (invariant-)tight?

Failures of stratification are located at edges not vertices

But perhaps the real point is not that NF is stratified-tight, but that it is not tight, and o invariant extension of it can be tight.

3.1 Correspondence between tf and Ali Enayat

3.1.1 tf to Ali Enayat

On Fri, May 7, 2021 at 9:10 PM Thomas Forster ¡tf@dpmms.cam.ac.uk¿ wrote:

Dear Ali,

I hope you will forgive me asking questions that seem rather vague, but i hope at least that they will be easy to answer.

Many years ago Richard Kaye said to me that no-one would ever find a Church-Oswald style proof of Con(NF). I now understand Church-Oswald construction better than i did then, and Tim Button has now confirmed my long-held suspicion that the basic version of CUS is synonymous with ZF. I now feel very strongly that this is nothing more than a standard effect of the CO construction, and theories with models obtained by CO constructions from models of a theory T will be synonymous with T. I hope that someone will prove an omnibus lemma to this effect. In the light of this, i am reading Kaye's conjecture to be that NF is not synonymous with any theory of wellfounded sets. This is - or would be - significant, since it gives flesh to the idea that the conception of set behind NF really is different from the conception of set behind ZF.

It now seems to me that tightness might be a way of proving this conjecture. It is easy to see that no stratified extension of NF can be tight (even tho' many of them will probably be stratified-tight). This is beco's NF + "there is a unique Quine atom" and NF + "there are no Quine atoms" are synonymous but distinct theories. And the same goes for any stratified extension of NF, since all we use is the Rieger-Bernays permutation construction. Ali assures me that tightness is preserved by synonymy. So no stratified extension of NF is synonymous with any tight theory.

So my question is: is there an omnibus theorem/lemma of some kind that shows that lots of theories-of-wellfounded sets are tight..? Is the axiom of foundation helpful in proving tightness? Any such omnibus lemma would prove a version of Kaye's conjecture.

Any thoughts?

3.1.2 Ali Enayat to tf

On May 8 2021, Ali Enayat wrote:

Hello Thomas,

You asked:

"So my question is: is there an omnibus theorem/lemma of some kind that shows that lots of theories-of-wellfounded sets are tight..? Is the axiom of foundation helpful in proving tightness? Any such omnibus lemma would prove a version of Kaye's conjecture."

The best result I know of is that ZF (and all its extensions) are tight, and I conjectured at the end of my paper that no proper subtheory of ZF is tight, because the proof of tightness of ZF (I am saying *the* proof, since all known proofs are minor variations of each other) uses all of the axioms (including foundation) of ZF to succeed. So your conjecture that NF is not synonymous with some theory of well-founded sets is a special case of my conjecture that no proper subtheory of ZF is tight, since NF is finitely axiomatizable, and finite axiomatizability is preserved by bi-interpretations (and in particular by synonymies). Indeed, I do not know of any finitely axiomatizable tight theory, and suspect that there are none, at least if they are also sequential, i.e., have a coding device for handling finite (in the sense of the theory) sequences of objects.

An interesting proper subtheory of ZF that we do not have a proof of failure of tightness is Zermelo + Ranks (where Ranks says that the universe can be written as the union of sets V_α, as α ranges in the ordinals). What's attractive about Z + Ranks is that its second order counterpart is categorical, in the sense that models of its second order counterpart are of the form V_α, for some limit ordinal α (this was proved by Uzquiano, in a paper in the Bulletin of Symbolic Logic, 1999).Vol. 5, No. 3, Sep., 1999 "Models of Second-Order Zermelo Set Theory" (pp. 289-302) Gabriel Uzquiano `https://doi.org/10.2307/421182` `https://www.jstor.org/stable/421182`

One more note: Hamkins and Freire in their recent JSL paper show that Zermelo set theory is not tight (using Mathias' technology of building models of Zermelo set theory), but their models of Zermelo do not satisfy Ranks. They also show that ZFC \{power set} is not tight. I had observed, in my paper, that ZF \{Foundation} and ZF \{Extensionality} are not tight.

All the best,

Ali

3.1.3 tf to Ali Enayat

Ali,

Thanks v much for prompt and informative reply. We do seem to be getting somewhere. At the very least we have the special case:

No stratified (indeed: no *invariant*) extension of NF is synonymous with any extension of ZF.

which certainly has the flavour we want. I shall read your email v carefully before i shoot my mouth off again. As i say, we seem to be getting somewhere!

v best wishes

Thomas

On reflection perhaps the connection with Kaye's conjecture is not so close after all. Yes, CO constructions give you synonymy results but they give you synonymy results with systems that have *Beschränkheitsaxiome* (think about the appearance of ¬Inf in Kaye-Wong) and these *Beschränkheitsaxiome* are likely to be unstratified.

Facts that may be connected. No stratified (indeed invariant) extension of NF proves Counting. That might mean that no stratified extension of NF is as strong as ZF. Tho' NF + Counting is invariant. Also there may be a connection between Ali's "ranks" axiom and the fact that $\text{AxCount}_{\leq}$ is equivalent to a version of "ranks" being true in a permutation model.

It now seems to me that the point about the CO construction is that when T is a theory with a CO model then T and $\{\phi : T \vdash \phi^{WF}\}$ are synonymous. This is beco's the wellfounded part of the CO model is an isomorphic copy of the original wellfounded structure. It raises the question: what is the relation between NF and the theory of wellfounded sets in NF? Are they synonymous? Presumably not, since NF proves infinity, and the theory of wellfounded sets in NF does not, and (presumably?) no theory that proves infinity can be synonymous with one that doesn't. . . ? So that's a straw in the wind.

Perhaps worth spelling out why. It is consistent with NF that there should be a natural number n with $2^{Tn} < n$. Any model with such an n has a permutation model with a finite fat set, and in any such model every wellfounded set is finite. So the wellfounded sets in this model do not model infinity (might be an idea to spell this out in gory detail!) So $\{\phi : NF \vdash \phi^{WF}\}$ does not prove infinity, and therefore cannot be synonymous with NF. This argument doesn't work for arbitrary invariant extensions of NF, since it fails for NF + Counting. But the conclusion might be true for other reasons. A straw in the wind, as i say.

Ali says that every theory synonymous with a tight theory is tight; ZF is tight, CUS is synonymous with ZF, so CUS is tight. So CUS + "$\exists!$ Quine atom" and CUS + "$\neg\exists$ Quine atom" cannot be synonymous. So the R-B permutation doesn't work for CUS. It would be nice to spell out how this failure happens. This is very worrying! It looks on the face of it as tho' R-B permutations work v well for CUS, so that CUS + "$\exists!$ Quine atom" and CUS + "$\neg\exists$ Quine atom" are synonymous!!

But it's OK! The version of CUS that Button has proved synonymous with ZF asserts that there are no Quine atoms.

I now feel that there is a new way of thinking of the special nature of CO constructions. The theory of a CO model is synonymous with the theory of its wellfounded part. But there are other fragments one could use instead of 'wellfounded'. There is the theory of the BFEXTS, what Adrian calls its *lune*. Time to look again (in an NF context) at the theory of the relational types of extensional APGs. Does it obey extensionality?

3.2 Other tight Theories

3.2.1 PA

Albert and Harvey have shown that PA is tight. Let me see if i can reconstruct how they did it. My guess is that it starts with the usual story about why are not any two models of PA isomorphic? Suppose $\mathfrak{M}_1$ and $\mathfrak{M}_2$ are two models of PA. I define an injection f from $\mathfrak{M}_1$ into $\mathfrak{M}_2$ by sending the zero 0_1 of $\mathfrak{M}_1$ to the zero 0_2 of $\mathfrak{M}_2$, after which i procede by recursion inside $\mathfrak{M}_1$. To what does f send $S_1(x)$ (the successor of x in the sense of $\mathfrak{M}_1$)? Well, obviously to S_2 of whatever f sent x to. Why does this not wrap things up completely, and show that my recursively defined map f is total? Because we have yet to prove (by induction on $\mathfrak{M}_1$) that f is total. Why is this not completely straightforward? Because the thing we are trying to prove isn't couched entirely in $\mathcal{L}(\mathfrak{M}_1)$; it makes reference to S_2, a gadget to which $\mathfrak{M}_1$ has no access and can't prove inductions about. The assertion "If $f(x)$ is defined so is $f(S_1(x))$" is not an assertion inside $\mathcal{L}(\in_1, =)$. However! if each of $\mathfrak{M}_1$ and $\mathfrak{M}_2$ are definable in terms of the other then $\mathfrak{M}_1$ *does* have access to S_2 and the recursion succeeds.

Thus if the operations of $\mathfrak{M}_2$ can be defined in $\mathfrak{M}_1$ then the bijection we are trying to define is defined on the whole of $\mathfrak{M}_1$. And the other way round too: if the operations of $\mathfrak{M}_1$ can be defined in $\mathfrak{M}_2$ then the bijection we are trying to define coming from the other direction is defined similarly on the whole of $\mathfrak{M}_2$.

3.2.2 ZF

It occurs to me that the proof that ZF is tight must procede along the same lines. If you have two models of ZF with the same carrier set and their theories are synonymous then you define an isomorphism between them by $\in$-recursion. Suppose $\langle V, \in_1\rangle$ and $\langle V, \in_2\rangle$ are two models of ZF with the same carrier set where each of $\in_1$ and $\in_2$ is definable in terms of the other. Then we define an isomorphism f between the two structures by $\in$-recursion. What is $f(x)$ to be? Obviously it must be that y such that $(\forall z)(z \in_1 x \longleftrightarrow f(z) \in_2 y)$ ... which had better be a set by replacement. Why is it a set?

Look at the material in COmodels.tex and axiomsofsettheory.tex

3.2.3 CUS

And similarly also for the basic version of Church's Universal Set theory – the version of CUS without j-cardinals but instead with the *Beschränkheitsaxiom* $(\forall x)(\mathtt{low}(x) \vee \mathtt{low}(V \setminus x))$. This is the system that Tim Button calls BLT. The proof involves a wellfounded recursion, just as the proofs for ZF and PA do. In this case the relation on which the recursion is done is the wellfounded relation $x\,\mathcal{E}\,y$ defined by $x\,\mathcal{E}\,y$ iff $(x \in y \longleftrightarrow \mathtt{low}(y)$. Or – rather – on the *two* E relations, one for each model. This is another notch on the cane where i am collecting proofs by induction on this E relation.

The tightness of BLT follows from a combination of the tightness of ZF, the synonymy of BLT and ZF, and the fact that synonymy preserves tightness; however it is possible to give a direct proof.

3.2.4 Other stuff to fit in

Every permutation model of a wellfounded model of ZF is an end-extension.

Can that be used to prove Kaye's Conjecture that NF has no CO-models (which i take to mean that NF is not synonymous with any theory of wellfounded sets

3.3 Do Tight theories form a Filter?

Supose T is tight, and $S \supseteq T$. Suppose $T + \phi$ and $T + \psi$ are synonymous. Hmm perhaps not.

Suppose T_1 and T_2 are tight, and that $(T_1 \cap T_2) \cup \{\psi\}$ and $(T_1 \cap T_2) \cup \{\phi\}$ are synonymous, in the sense that any model of one can be turned in to a model of the other by internally definable means. We want to show that $(T_1 \cap T_2) \cup \{\psi\}$ and $(T_1 \cap T_2) \cup \{\phi\}$ are the same theory. Now every model of $T_1 \cap T_2$ is either a model of T_1 or a model of T_2. So every model of $(T_1 \cap T_2) \cup \{\phi\}$ is either a model of $T_1 \cup \{\phi\}$ or a model of $T_2 \cup \{\phi\}$. We want to show that $T_1 \cup \{\psi\}$ and $T_1 \cup \{\phi\}$ are synonymous, as are $T_2 \cup \{\psi\}$ and $T_2 \cup \{\phi\}$. So, take a model of $T_1 \cup \{\phi\}$ and do to it our magic that turns models of $(T_1 \cap T_2) \cup \{phi\}$ into models of $(T_1 \cap T_2) \cup \{\psi\}$. (The model of $T_1 \cup \{\phi\}$ is certainly a model of $(T_1 \cap T_2) \cup \{\phi\}$). What we want is to get a model of $T_1 \cup \{\psi\}$. However there is nothing to say that we don't instead get a model of $T_2 \cup \{\psi\}$. That would obey the letter of the law.

Ali Enayat sez that the intersection of two tight theories need not be tight.

Tennenbaum's theorem has something to say about two models of PA inhabiting the one carrier set. There might be something useful one could say about the connection with tightness.

So, picking up on that.. the stratified-tightness of NF would correspond to the fact that higher-order TST is categorical in every power.... That is to say: for each cardinal κ there is a unique model of TST whose base level is of power κ and where the power set operation is honest. (Randall and i have always called these the *natural models* of TST)

But perhaps that is too easy, and there is *less* to it than meets the eye. But it is a very good fit.

3.4 Synonymy

One thing to notice:

I'm not sure if TZT is finax in the one-sorted language (if true, it would be beco's NF = NF_4) but i do know that TZT + AC is finitely axiomatisable: take the one-sorted theory whose models are models of TZT or $\mathrm{TC}_n\mathrm{T}$ or disjoint unions thereof (which is finitely axiomatisable) and add AC. AC is false in all the models of $\mathrm{TC}_n\mathrm{T}$ so only the models of TZT are left.

The other thing to notice is that TZT is mutually interpretable with the theory TTTU with Z-many levels. This is beco's what you (for the moment!) call the 'lateral' membership relations are definable in the model of TZT.

These two things get connected beco's ... isn't there a theorem of Friedman and Visser or Enayat that says that finite axiomatisablity

Is the intersection of tight theories tight? Ali sez no.

Presumably one can prove a synonymy result for any two permutations in $C_{J_0}(J_1)$. Write $\in_1$ and $\in_2$ for the two membership relations. We have

$$x \in_2 y \text{ iff } x \in_1 y \longleftrightarrow A(y)$$

where A is some formula with y free at level 1. And of course all '$\in$'s in A are $\in_1$.

How do we define $\in_1$ in terms of $\in_2$? Well, it's going to be

$$x \in_1 y \text{ iff } x \in_2 y \longleftrightarrow B(y)$$

where B is some formula with y free at level 1. And of course all $\in$s in A are $\in_2$.

Substituting $x \in_1 y \longleftrightarrow A(y)$ for $x \in_2 y$ in the expansion of $x \in_1 y$ we get $A(y) \longleftrightarrow B(y)$ More detail please

Albert says that PA is not mutually interpretable with any finitely axiomatisable theory. Therefore (for example) the arithmetic of NFU + *i*NF must be MORE than PA.

Is the collection of Cantorian cardinals closed under EXP? Yes.

Is the collection of cantorian ordinals (or scordinals) closed under DT? Presumably, but spell it out.

Albert says that GB proves Con(ZF) in the sense that there is a definable cut in the naturals of GB in whose arithmetic one can prove Con(ZF). One wants to compare this situation with Morse-Kelley and Quine's ML (*vis-a-vis* NF).

Chapter 4

Permutation Models

Prove that for every concrete k there is a permutation that is k-setlike but not $(k+1)$-setlike.

Nathan says look at the modal logic of definable permutation models. It satisfies transitivity (and reflexivity - obviously) but doesn't – on the face of it – satisfy symmetry. We know of definable permutations whose return permutation is not definable. However for every definable permutation σ in $\mathfrak{M}$ there might be a definable τ in $\mathfrak{M}^\sigma$ s.t. $(\mathfrak{M}^\sigma)^\tau$ is elementarily equivalent to $\mathfrak{M}$.

In any case the group of deinable permutations in any one model is a normal subgroup of the group of internal permutations – at least if we take definable to mean "fixed by everything in J_n for some n. Of course in principle there might be a permutation definable by an unstratified expression.

> It may be worth emphasising the entirely banal point that in the expression ϕ^σ there are no occurrences of 'σ' to the left of an $\in$. This means that we are in with a chance of making sense of reading as "for all *setlike* permutations..." and working in ML – and still having the possibility of iterating it. To do that we would have to make sense of the idea of applying a setlike (proper class) permutation to a setlike (proper class) permutation.

There are two ways of obtaining a new structure from $\mathfrak{M} = \langle M, \in \rangle$ by means of a permutation σ of M.

On the left: $\sigma(x) \in y$;

On the right: $x \in \sigma(y)$.

(R): Tweaking on the right ($x \in \pi(y)$) preserves stratified expressions as long as π is setlike;

(L): tweaking on the left ($\pi(x) \in y$) preserves stratified expressions as long as π^{-1} is setlike.

(R) is standard. I don't think (L) has been remarked on before but the proof is similar and straightforward. It relies on rewriting so that every occurrence of any one variable has the same prefix. We illustrate:

$$f(z) \in x \wedge f(x) \in y \wedge f(y) \in w$$

becomes

$$f(z) \in x \wedge x \in (jf)^{-1}(y) \wedge (jf)^{-1}(y) \in (j^2(f^{-1})) \cdot (jf)^{-1}(w)$$

or something like that (check the last term just to be safe). It's parallel to tweaking on the right. But here you need f^{-1} to be setlike (rather than f). The terms one obtains through this rewriting are the exact analogues of the terms one obtains by rewriting with tweaking on the right.

Observe that

$$f(x) \in y \quad \text{iff} \quad x \in (j(f^{-1}))(y)$$

So tweaking on the left is a special case of tweaking on the right: tweaking by a permutation on the left is the same as tweaking by a permutation that is j of something on the right, i.e. a special case of tweaking-on-the-right.. But is tweaking-on-the-right any more general? I think not, for consider:

$$x \in f(y) \quad \text{iff} \quad f(x) \in j(f) \cdot f(y)$$

But this is as much as to say that f is an isomorphism between the model you get by tweaking-on-the-right with f and the model you get by tweaking-on-the-right with $j(f)$. (This has been known for some time)

So anything you can do by tweaking on the left you can do by tweaking on the right with j of something, and tweaking-on-the-right with $j\sigma$ is the same as tweaking-on-the-right with σ. So anything you can do by tweaking on the left you can do by tweaking on the right (and *vice versa*).

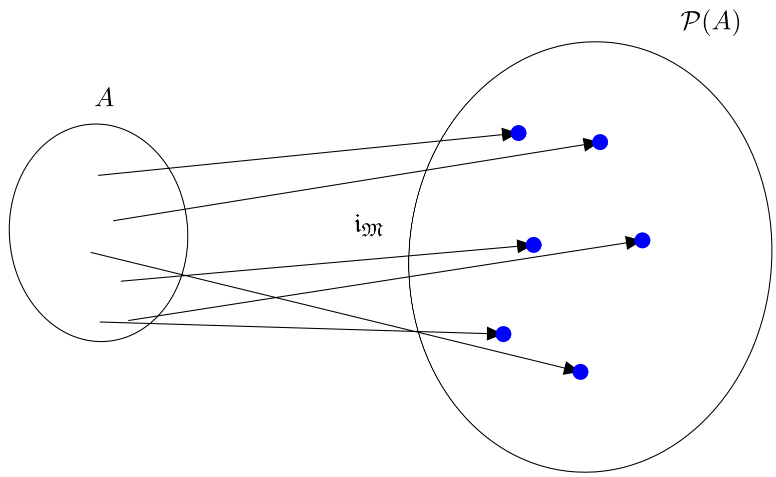
In what follows we will present everything in terms of tweaking-on-the-right. There will be no loss of generality.

Let τ be $(V, \emptyset, \{\emptyset\})$. Suppose V harbours no Quine atoms. Then V^τ has a unique Quine atom but $V^{\tau^{-1}}$ has no Quine atoms. So τ and τ^{-1} are not skew-conjugate.

4.1 The di Giorgi Picture

Work in your favourite formal metatheory (ZF(C) ... whatever). We will use the Di Giorgi picture of models of set theory. A model of Set theory is of course a carrier set (always infinite in this context) A (for Atoms), decorated with a binary relation to obtain a structure $\mathfrak{M} = \langle A, \in \rangle \models \text{NF}$. What is distinctive about the Di Giorgi picture is that it thinks of the binary relation $\in$ of the model as arising from an injection $\mathfrak{i} : A \hookrightarrow \mathcal{P}(A)$. The binary relation that decorates A is $\{\langle a, b\rangle : a \in \mathfrak{i}(b)\}$. (The '$\in$' here is of course the membership relation of our Favourite Formal Metatheory, and the $\mathcal{P}$ is the power set in the sense of our Favourite Formal Metatheory.) Either way we call the resulting model '$\mathfrak{M}$'. Perhaps we should write the injection with a subscript: $\mathfrak{i}_\mathfrak{M}$ ...? Or attach the subscript to the letter '$\mathfrak{M}$' since the injection determines the model.

I think the most satisfactory practice is to denote the model corresponding to an injection $\mathfrak{i}$ as '$\mathfrak{M}$' and to denote the injection corresponding to a model $\mathfrak{M}$ as '$\mathfrak{i}$', so that we regard '$\mathfrak{i}$' and '$\mathfrak{M}$' as reserved letters. We can scatter subscripts about to dissolve ambiguities *ad lib.*.



It is long-standing practice to use lower case *Greek* letters to denote permutations of A that are sets of the models $\mathfrak{M}$. Of late Nathan has had the habit of using lower-case Roman letters to denote elements of Symm(A), and equipping those letters with superscripts, so that '$s^{\mathfrak{M}}$' denotes that object in $\mathfrak{M}$ (aka element of A) such that $\mathfrak{i}(s^{\mathfrak{M}})$ is the graph of the permutation s of A, and i can well believe that we will need that usage too. Accordingly i shall systematise notation by using lower case Roman letters 'p' 's' 't'... for arbitrary permutations of A, and the corresponding lower-case Greek letters ('π', 'σ', 'τ' ...) for permutations that are sets of $\mathfrak{M}$. Notice that when we write 'arbitrary permutation of A' we always mean a permutation of A that is a set from the point of view of the FFM – our Favourite Formal Metatheory.

I am going to reserve the letter '$\mathfrak{i}$' (decorated from time to time with suitable subscripts) to range over injections $A \hookrightarrow \mathcal{P}(A)$. And of course we reserve the letter 'A' to denote the set of atoms.

There is a natural action of Symm(A) on $A \hookrightarrow \mathcal{P}(A)$, the set of injections from A into $\mathcal{P}(A)$. A permutation $s \in$ Symm(A) sends $\mathfrak{i}$ to $(js)^{-1} \cdot \mathfrak{i} \cdot s$. It is natural to describe $(js)^{-1} \cdot \mathfrak{i} \cdot s$ and $\mathfrak{i}$ as *conjugate*.

REMARK 16 *Conjugate injections give rise to isomorphic di Giorgi models.*

$$x \in (js)^{-1} \cdot \mathfrak{i} \cdot s(y) \text{ iff } s(x) \in \mathfrak{i}(s(y)).$$

which says that s is an isomorphism between $\mathfrak{M}^{(js)^{-1} \cdot \mathfrak{i} \cdot s(y)}$ and $\mathfrak{M}^{\mathfrak{i}}$.

■

Notice that this doesn't rely on s being $\mathfrak{i}$-setlike.

We haven't defined 'se yet

This means that in what follows when we talk about (di Giorgi) models and injections we are speaking of them only up to conjugacy.

4.1.1 Internal Permutations

When $\mathfrak{M}$ is a model arising from an injection $\mathfrak{i} : A \hookrightarrow \mathcal{P}(A)$ in this style, and s is a permutation of A, there is the possibility that (the graph of) s is coded inside $\mathfrak{M}$. In these circumstances we say that s is *internal* and we write '$\mathfrak{M}^s$' (or perhaps 'σ') for the member of A that $\mathfrak{M}$ believes to be the graph[1] of s.

The class of internal permutations is of interest to us for various obvious reasons. Also of interest is the larger class of *setlike* permutations (of/for $\mathfrak{M}$), which (or: whose graphs) might not be sets of $\mathfrak{M}$ but which nevertheless behave rather like sets of $\mathfrak{M}$. They are the subject of the section which now follows.

4.2 Setlike Permutations

There are two sections both called 'Setlike Permutations

'

The blue dots inhabiting the ellipse on the right in the picture above are subsets of A that are values of the injection $\mathfrak{i}_{\mathfrak{M}} : A \hookrightarrow \mathcal{P}(A)$. Any permutation p of A lifts in an obvious way to a permutation $j(p)$ of $\mathcal{P}(A)$. Whenever we have an injection $\mathfrak{i}$ in mind we are interested in permutations p such that jp permutes $\mathfrak{i}``A$.

The reason for this is to capture the property of permutations σ that enable us to prove the lemma of Henson's to the effect that when ϕ is a stratifiable formula then $\phi^\sigma \longleftrightarrow \phi$. The key step is the equivalence of $(\exists x)\phi(x)$ with $(\exists x)\phi(j^n(\sigma)(x))$, and for this we need $j^n(\sigma)$ to be a permutation of V.

Let p be such a permutation; [This assumption is not essential to what follows but the development is much better motivated when it holds.] Then jp is defined on the whole of $\mathfrak{i}``A$ and its restriction is therefore a permutation of $\mathfrak{i}``A$. This means it can be copied downstairs to a permutation $\mathfrak{i}^{-1} \cdot jp \cdot \mathfrak{i}$ of A. [Notice that here we are using a *Roman* letter 'p' because we are not assuming that the permutation in play is a set of $\mathfrak{M}$.]

Definition 4
When $\mathfrak{i}^{-1} \cdot jp \cdot \mathfrak{i}$ is total we will call it the **derivative** *of p, and write it '$\mathfrak{D}_\mathfrak{i}(p)$'. It, in turn, might have a derivative (and it might not, of course). If the nth derivative exists (= is total) and is a permutation we say that p is n-***setlike**$^\mathfrak{i}$*;*

If all derivatives exist we say p is **setlike**$^\mathfrak{i}$.

We will omit the subscript when $\mathfrak{i}$ is clear from context.

From the point of view of the model $\mathfrak{M}$ arising from $\mathfrak{i}$ the derivative of an internal permutation σ is (the permutation which it believes to be) $j\sigma$.

'Setlike' is a weaker condition on a permutation than 'internal'. Every $\in$-automorphism of a model of any set theory (and not just NF) must be setlike,

[1] Thank you, Nathan, for this notation!

but it is easy (think: Ehrenfeucht-Mostowski) to cook up models with external automorphisms that are not internal (sets of the model). Of course with second-order theories such as second-order Zermelo every setlike permutation is locally a set, in the sense that its restriction to any set is a set. I might provide a proof of this fact (tho' it could be left as an exercise for the reader) but in any case our chief concern here is with di Giorgi models of NF rather than of theories of wellfounded sets.

However there remains the question of whether or not there can be *definable* setlike permutations that are not internal.

It is easy to see that the function $\mathfrak{D}_\mathfrak{i}$ sending a permutation p to its derivative is injective, even if it is merely partial. Any fixed point for $\mathfrak{D}_\mathfrak{i}$ is $\mathfrak{i}$-setlike of course, but it is more than that: it is an automorphism of the model arising from $\mathfrak{i}$. Indeed the converse is true too. A permutation of A is an $\in$-automorphism of $\mathfrak{M}$ iff it is a fixed point for $\mathcal{D}_\mathfrak{i}$. [It occurs to me to wonder about permutations p such that, for all n, p is $\mathfrak{D}_\mathfrak{i}^n$ of something. Does such a p have a derivative? However that is for later; see the second appendix to this chapter, section 4.9]

REMARK 17 $\mathcal{D}_\mathfrak{i}$ *is a group homomorphism.*

Proof:

It clearly fixes $\mathbb{1}$; for multiplication we compute

$$
\begin{array}{ll}
\mathcal{D}_\mathfrak{i}(s)\cdot\mathcal{D}_\mathfrak{i}(t) & = \\
\mathfrak{i}^{-1}\cdot js\cdot\mathfrak{i}\cdot\mathfrak{i}^{-1}\cdot jt\cdot\mathfrak{i} & = \\
\mathfrak{i}^{-1}\cdot js\cdot jt\cdot\mathfrak{i} & = \\
\mathfrak{i}^{-1}\cdot j(s\cdot t)\cdot\mathfrak{i} & = \\
\mathcal{D}_\mathfrak{i}(s\cdot t) &
\end{array}
$$

This works beco's if js and jt are in the setwise stabiliser of $\mathfrak{i}``A$ then so is $j(s\cdot t)$ because the setwise stabiliser is (of course) closed under composition, and j is a group homomorphism

Inverse is similar:

$\mathcal{D}_\mathfrak{i}(s^{-1}) = \mathfrak{i}^{-1}\cdot j(s^{-1})\cdot\mathfrak{i}$ and that's OK beco's s is in the setwise stabiliser and that is a group (closed under inverse) so the RHS of the equation is defined.

■

Notice that we are not claiming that $\mathcal{D}$ is a homomorphism defined on everything in $\mathfrak{Internal}^{\mathfrak{M}}$, let alone Symm$(A)$! However it is a homomorphism from the group $\mathfrak{Setlike}_1^\mathfrak{i}$ of permutations that are 1-setlike from the point of view of $\mathfrak{M}$ (or $\mathfrak{i}$). And at this stage I am not proposing to reserve any special font for variables ranging over setlike permutations.

We will be interested in the group Symm(A) of all permutations of A, but also (and mainly) in two subgroups of it, both related to the model $\mathfrak{M}$. One group is the group of setlike permutations as defined above – the set of permutations s for which $\mathcal{D}_\mathfrak{i}$ is defined. The other is the (presumably much smaller) group of those permutations of A that are encoded as sets of the model $\mathfrak{M}$. Let us call these two groups $\mathfrak{Setlike}^\mathfrak{i}$ and $\mathfrak{Internal}^\mathfrak{i}$. (It would probably make as much sense to have a '$\mathfrak{M}$' superscript as to have the $\mathfrak{i}$ superscript – which suggests that the notation is not optimal – but we will in any case omit the superscript when $\mathfrak{i}$ and $\mathfrak{M}$ are clear from context). Recall that $\mathfrak{M}$ is a model of NF, and the axioms of NF promise us that the collection of all permutations of V is a set (a *group* indeed); and there are plenty of definable set abstracts that define permutations, so this definition is not vacuous.

Recall at this juncture Henson's subscript notation σ_n, which we can invoke when σ is $\mathfrak{i}$-setlike:

$$s_1 = s; \quad s_{i+1} = \mathcal{D}_{\mathfrak{i}}(s_i) \cdot s.$$

A bit of housekeeping. . . .
s is $n+1$-setlike wrt $\mathfrak{i}$ iff $\mathcal{D}^n_{\mathfrak{i}}(s)$ is 1-setlike wrt $\mathfrak{i}$.
or do i mean "$\mathcal{D}_{\mathfrak{i}}(s)$ is n-setlike wrt $\mathfrak{i}$". . . ?
(Or both!)
best write this out.

We are going to need a notation for the groups of permutations that are n-setlike wrt $\mathfrak{M}$. Shall we write "$\mathfrak{Setlike}^{\mathfrak{i}}_n$" for this group? Could also write "$\mathfrak{Setlike}^{\mathfrak{M}}_n$" for this group?

We note that

Lemma 2 *$\mathfrak{Internal} \lhd \mathfrak{Setlike}$.*

Proof:

NF proves that the image of a set in a function is a set so, for any $\mathfrak{M} \models$ NF, if (the graph of) s is a set of $\mathfrak{M}$ then s is clearly setlike. So $\mathfrak{Internal}^{\mathfrak{M}}$ is certainly a subgroup of $\mathfrak{Setlike}^{\mathfrak{M}}$; it remains to be shown that it is a *normal* subgroup.

Let s be an $\mathfrak{M}$-setlike permutation and τ a permutation-internal-to-$\mathfrak{M}$. We want $s\tau s^{-1}$ to be a permutation internal to $\mathfrak{M}$. Since s is setlike we know that $s``\tau$ is a set of $\mathfrak{M}$. Is this any good? It isn't quite what we want, but it is a step in the right direction. If s is setlike, so is $j^n(s)$ for any n. So $j^n(s)$ of (the graph of) τ is a set; now – for suitable n depending on our choice of pairing function – $j^n(s)$ of (the graph of) τ is τ^s. This gives us the normality we seek. ■

Need to rephrase this using the $\mathcal{D}$ derivative notation

Actually we want to say
$\mathfrak{Internal}^{\mathfrak{M}} \lhd \mathfrak{Setlike}^{\mathfrak{M}}_n$ for each concrete n.

Well, *sufficiently large* concrete n. I think $n > 1$. I think my example of a permutation that is not 1-setlike illustrates this. τ swaps every wellfounded set with its complement and fixes everything else. I claim $\tau \cdot jc \cdot \tau^{-1}$ is not a set. I think the collection of fixed points for $\tau \cdot jc \cdot \tau^{-1}$ is not a set. What is $\tau \cdot jc \cdot \tau^{-1}(x)$? If x is wellfounded or the complement of a wellfounded set then $\tau(x) = x$ so $\tau \cdot jc \cdot \tau^{-1}(x) = \tau \cdot jc(x)$. Now beco's x is either wellfounded or the complement of a wellfounded set it follows that $jc(x)$ is neither a wellfounded set nor the complement of a wellfounded set, so it is swapped with its complement by τ, so we get $c \cdot jc(x)$. OTOH if x is neither a wellfounded set nor the complement of a wellfounded set then τ moves it to $V \setminus x$. So $\tau \cdot jc \cdot \tau^{-1}(x) = \tau \cdot jc \cdot c(x)$ But what is the RHS of this equation? Eurgh.

If x is not wellfounded then it has a nonwellfounded member. If $V \setminus x$ is not wellfounded then it, too, has a non-wellfounded member. So if x is neither a wellfounded set nor the complement of a wellfounded set it contains some but not all illfounded sets.

Now if x is neither a wellfounded set nor the complement of a wellfounded set the same goes for its complement. τ swaps such things with their complements, but the complement of such a thing is another such thing.

Lemma 2 actually shows that $\mathfrak{Internal}^{\mathfrak{i}}$ is a normal subgroup of all of the $\mathfrak{Setlike}^{\mathfrak{i}}_n$ and that later groups are normal subgroups of earlier subgroups, but that none of them are normal subgroups of $\mathrm{Symm}(A)$. For a start, $\mathfrak{Setlike}^{\mathfrak{i}}_1$ is the setwise stabiliser of $\mathfrak{i}``A$ which is of course not a normal subgroup of $\mathrm{Symm}(A)$.

Sort this out

REMARK 18 *For every injection $\mathfrak{i} : A \hookrightarrow \mathcal{P}(A)$, the set $\mathfrak{Setlike}^{\mathfrak{i}}$ of permutations of A that are $\mathfrak{i}$-setlike form a group.*

Proof:

We will prove by induction on 'n' that, for all $\mathfrak{i}$, the set of permutations that are n-setlike wrt $\mathfrak{i}$ is a group, and later members of the sequence are subgroups of earlier members. The set $\mathfrak{Setlike}^{\mathfrak{i}}$ of permutations of A that are $\mathfrak{i}$-setlike is the intersection of this nested sequence of groups and is therefore a group.

Base case: $n = 1$

For all $\mathfrak{i}$, the set of permutations that are 1-setlike wrt $\mathfrak{i}$ is – by definition – the setwise stabiliser of $\mathfrak{i}``A$ which is of course a group.

Induction step

Suppose the collection of permutations that are n-setlike wrt $\mathfrak{i}$ is a group.

Closed under inverse

First we show that the collection of permutations that are $(n+1)$-setlike wrt $\mathfrak{i}$ is closed under inverse.

$$\begin{array}{ll} & s \text{ is } (n+1)\text{-setlike wrt } \mathfrak{i} \\ \text{iff} & j(\mathfrak{i}^{-1} \cdot j(\mathcal{D}^n_{\mathfrak{i}}(s)) \cdot \mathfrak{i}) \text{ is in the setwise stabiliser of } \mathfrak{i}``A \\ \text{iff} & (j(\mathfrak{i}^{-1} \cdot j(\mathcal{D}^n_{\mathfrak{i}}(s)) \cdot \mathfrak{i}))^{-1} \text{ is in the setwise stabiliser of } \mathfrak{i}``A. \end{array}$$

Now

$$\begin{array}{l} j(\mathfrak{i}^{-1} \cdot j(\mathcal{D}^n_{\mathfrak{i}}(s)) \cdot \mathfrak{i})^{-1} \quad = \\ j(\mathfrak{i}^{-1} \cdot (j(\mathcal{D}^n_{\mathfrak{i}}(s)))^{-1} \cdot \mathfrak{i}) = \\ j(\mathfrak{i}^{-1} \cdot j((\mathcal{D}^n_{\mathfrak{i}}(s))^{-1}) \cdot \mathfrak{i}) = \\ j(\mathfrak{i}^{-1} \cdot j((\mathcal{D}^n_{\mathfrak{i}}(s^{-1}))) \cdot \mathfrak{i}) \end{array}$$

which is therefore in the setwise stabiliser of $\mathfrak{i}``A$, which is to say that $(\mathcal{D}^n_{\mathfrak{i}}(s))^{-1}$ is 1-setlike, which is to say that s^{-1} is $(n+1)$-setlike.

Closed under composition

s and t are both $(n+1)$-setlike wrt $\mathfrak{i}$ iff $j(\mathfrak{i}^{-1} \cdot j(\mathcal{D}^n_{\mathfrak{i}}(s)))$ and $j(\mathfrak{i}^{-1} \cdot j(\mathcal{D}^n_{\mathfrak{i}}(t)))$ are both in the setwise stabiliser of $\mathfrak{i}``A$.

So: assume that they are, and deduce that $s \cdot t$ is in the setwise stabiliser of $\mathfrak{i}``A$. We get

$$j(\mathfrak{i}^{-1} \cdot j(\mathcal{D}^n_{\mathfrak{i}}(s)) \cdot \mathfrak{i}) \cdot j(\mathfrak{i}^{-1} \cdot j(\mathcal{D}^n_{\mathfrak{i}}(t)) \cdot \mathfrak{i})$$

is in the setwise stabiliser of $\mathfrak{i}``A$, since the stabiliser is closed under composition.

The displayed expression rearranges to

$$j(\mathfrak{i}^{-1} \cdot j(\mathcal{D}^n_{\mathfrak{i}}(s)) \cdot \mathfrak{i} \cdot \mathfrak{i}^{-1} \cdot j(\mathcal{D}^n_{\mathfrak{i}}(t)) \cdot \mathfrak{i})$$

then to

$$j(\mathfrak{i}^{-1} \cdot j(\mathcal{D}^n_{\mathfrak{i}}(s)) \cdot j(\mathcal{D}^n_{\mathfrak{i}}(t)) \cdot \mathfrak{i})$$

and finally

$$j(\mathfrak{i}^{-1} \cdot j((\mathcal{D}^n_{\mathfrak{i}}(s \cdot t)) \cdot \mathfrak{i}))$$

and this last object is in the setwise stabiliser of $\mathfrak{i}``A$ ... which is to say that $s \cdot t$ is $(n+1)$-setlike wrt $\mathfrak{i}$ as desired.

■

This still needs to be thoroughly checked and titivated

We have used nothing beyond the facts that $\mathcal{D}_{\mathfrak{i}}$ and j are group homomorphisms. In particular we have not assumed that $\mathfrak{M} \models$ NF – even tho' that is the motivation for this investigation. In fact we have made no assumptions about the theory of the model $\mathfrak{M}$ at all: all this is going on in complete generality in our Favourite Formal Metatheory. That will change when we start proving theorems about the group $\mathfrak{Internal}$ of permutations internal to $\mathfrak{M}$. Then we will be assuming that $\mathfrak{M} \models$ NF or something similar. After all, if a permutation of the carrier set of a model of a set theory is a set of the model then we clearly aren't in Kansas any more i mean ZF.

Interestingly I know nothing about the cardinality of these groups. The cardinality of $\mathfrak{Internal}$ is of course bounded by $|A|$, and – reasoning inside $\mathfrak{M}$ – it's not hard to see that $|\mathfrak{Internal}| = |V|$. At least if $\mathfrak{M} \models NF$! As far as $|\mathfrak{Setlike}|$ goes, that's anyone's guess.

In checking whether or not a permutation σ is setlike for an injection $\mathfrak{i}$ we ask only about the *range* $\mathfrak{i}``A$ of $\mathfrak{i}$. Thus, for any s at all, since $\mathfrak{i}$ and $\mathfrak{i} \cdot s$ have the same range they see the same 1-setlike permutations. Unfortunately this seems not to establish that they see the same setlike permutations. However i think all the groups of permutations involved are conjugate copies of one another. We'd better check that...

From an injection $\mathfrak{i} : A \hookrightarrow \mathcal{P}(A)$ we can obtain a sequence of injections $\mathfrak{i}_n : A \hookrightarrow \mathcal{P}^n(A)$ using j in the obvious way. One gets a definition precisely analogous to Henson's. Do we get:

"s is n-setlike wrt $\mathfrak{M}$ iff $j^n s$ is in the setwise stabiliser of $\mathfrak{i}_n$"A "? Should prove this!

It is standard in NF studies that members of $\mathfrak{Setlike}$ give rise to Rieger-Bernays permutation models. If $\mathfrak{M} \models$ NF, and $s \in \mathfrak{Setlike}^{\mathfrak{M}}$, then there is a model notated '$\mathfrak{M}^s$' which is also a model of NF and which agrees with $\mathfrak{M}$ on stratifiable sentences.

Given any model $\mathfrak{M}^s$ obtained in this way we can form two groups $\mathfrak{Internal}^s$ and $\mathfrak{Setlike}^s$ in the same way as $\mathfrak{Internal}$ and $\mathfrak{Setlike}$ arise from $\mathfrak{M}$. This gives rise to a family of four questions:

> "For $\mathfrak{M} \models NF$, and s a (setlike/internal) permutation of $\mathfrak{M}$, does $\mathfrak{M}^s$ have the same (setlike/internal) permutations as $\mathfrak{M}$?"

Life would be very simple if the answers to all four were 'yes'!

Lemma 3 *Fix* $\mathfrak{i}$*; then* $(\forall s \in \mathfrak{Internal})(\mathfrak{Internal}^s = \mathfrak{Internal})$.

Proof:

Let s be a permutation of A, and π a permutation of A that is a set of M. We will show that s is a set of $\mathfrak{M}$ iff it is a set of $\mathfrak{M}^\pi$. The key new idea is to expand the language by giving a name to every atom in a. Suppose s is a set of $\mathfrak{M}$. By Henson's lemma $\mathfrak{M}^\pi \models$ "s is a permutation of V and $s(a) = b$" iff $\mathfrak{M} \models$ "$\pi_n(s)$ is a permutation of V and $\pi_n(s)(a) = b$". Now every element of A is named in both $\mathfrak{M}$ and $\mathfrak{M}^\pi$, so what this is telling us is that $\pi_n(s)$ is that element of $\mathfrak{M}^\pi$ which codes the graph of s. In fact π_n is a partial map $A \rightharpoonup A$ which, for any internal permutation s, sends the atom coding s in $\mathfrak{M}$ to the atom coding s in V^π. Since π_n is a permutation it has an inverse, and gives a bijection between the atoms representing internal permutations in $\mathfrak{M}$ and the atoms representing the same permutations in $\mathfrak{M}^\pi$.

■

This proof exploits the fact that π is a set of $\mathfrak{M}$, so it cannot be straightforwardly repurposed to prove any of the other. However, i think the following is safe:

Remark 19 *Fix* $\mathfrak{i}$*; then* $(\forall s \in \mathfrak{Setlike})(\mathfrak{Setlike}^s = \mathfrak{Setlike})$.

Proof:

Let $\mathfrak{M}$ be a model of NF, and s, t two permutations of A that are setlike for $\mathfrak{M}$. We want to show that t is setlike for $\mathfrak{M}^s$. Let x be a set of $\mathfrak{M}^s$; we want t"x to be a set of $\mathfrak{M}^s$. Now t"x (in the sense of $\mathfrak{M}^s$) is $\{t(y) : y \in s(x)\}$, and this is of course a set of $\mathfrak{M}^s$, being t"$(s(x))$, since t is 1-setlike for $\mathfrak{M}$. So t is 1-setlike for $\mathfrak{M}^s$. n-setlike is analogous.

■

Actually we have to be careful here... all this shows is that if t is setlike for $\mathfrak{M}$ then it is setlike for $\mathfrak{M}^s$ as long as s is setlike for $\mathfrak{M}$. It doesn't show the converse (and thereby provide a 'yes' answer). So in principle permutation models could acquire new setlike permutations, unlikely tho' that probably sounds. We should plug that gap. Of course we could plug it by showing that setlike permutations can be undone.... That is to say, we desire a proof that relation (ii) of the next section should be symmetrical.

4.3 Permutation Models

We have several relations that might hold between two models $\mathfrak{M}$ and $\mathfrak{N}$...

(i) $\mathfrak{N} = \mathfrak{M}^\sigma$ for σ an internal permutation of $\mathfrak{M}$;
(ii) $\mathfrak{N} = \mathfrak{M}^\sigma$ for σ a setlike permutation of $\mathfrak{M}$;

(i)$'$ $\mathfrak{N} = \mathfrak{M}^\sigma$ for σ a definable internal permutation of $\mathfrak{M}$;
(ii)$'$ $\mathfrak{N} = \mathfrak{M}^\sigma$ for σ a definable setlike permutation of $\mathfrak{M}$.

These relations are all going to crop up as accessibility relations for Kripke structures for families of di Giorgi models. We have to check which of them are equivalence relations and which aren't. They are all reflexive. I think we now know that (i) is a equivalence relation and that (ii)$'$ is a quasiorder but not an equivalence relation. We will prove both these facts below. It is clear that (i)$'$ is transitive for at least some definitions of definable, for example the definition that says that σ is definable iff "$x \in \sigma(y)$" is equivalent to a suitable formula in $\mathcal{L}(\in, =)$. However, as Nathan has showed, if we take 'definable' to mean "fixed by all internal automorphisms", then it isn't symmetrical. That is theorem 7 below.

Theorem 6
The relation – (i) above – that holds between two models $\mathfrak{M}$ and $\mathfrak{N}$ of NF when $\mathfrak{N} = \mathfrak{M}^\sigma$ for σ an internal permutation of $\mathfrak{M}$ is an equivalence relation.

Proof:
It's obviously reflexive. We need to check transitivity and symmetry.

Transitivity

$$(\forall \sigma, \pi)(\exists \tau, \mu)(\mu : V^\tau \simeq (V^\sigma)^\pi)$$

$$\text{so } \mu(x) \in \tau \cdot \mu(y) \text{ iff } (x \in \sigma(y))^\pi$$
$$\text{so } \mu(x) \in \tau \cdot \mu(y) \text{ iff } \pi_n(x) \in (\pi_{n+2}(\sigma)(\pi_{n+1}(y))$$

Now reletter '$\pi_n(x)$' as 'x' and '$\pi_n(y)$' as 'y' getting

$$\text{so } \mu \cdot (\pi_n)^{-1}(x) \in \tau \cdot \mu \cdot (\pi_n)^{-1}(y)) \text{ iff } x \in (\pi_{n+2}(\sigma)((j^{n+1}\pi)(y))$$

Doctor the LHS to

$x \in j(\mu \cdot (\pi_n)^{-1})^{-1} \cdot \tau \cdot \mu \cdot (\pi_n)^{-1}(y))$
$x \in j(\pi_n) \cdot (j\mu)^{-1} \cdot \tau \cdot \mu \cdot (\pi_n)^{-1}(y))$

giving

$$j(\pi_n) \cdot (j\mu)^{-1} \cdot \tau \cdot \mu \cdot (\pi_n)^{-1} = \pi_{n+2}(\sigma) \cdot (j^{n+1}(\pi))$$

whence – well!

Let's pin our hopes on being able to take μ to be the identity. Then we get

$$j(\pi_n) \cdot \tau \cdot (\pi_n)^{-1} = \pi_{n+2}(\sigma) \cdot j^{n+1}(\pi)$$

but now we can identify τ:

$$\tau = (j\pi_n)^{-1} \cdot \pi_{n+2}(\sigma) \cdot j^{n+1}(\pi) \cdot \pi_n$$

which i think is

$$\tau = (j\pi_n)^{-1} \cdot \pi_{n+2}(\sigma) \cdot \pi_{n+1}$$

Symmetry

$$(\forall\sigma\exists\tau)(\forall xy)((V^\sigma \models x \in \tau(y)) \longleftrightarrow x \in y)$$

$$(\forall\sigma\exists\tau)(\forall xy)((\sigma_n(x) \in (\sigma_{n+2}(\tau)) \cdot \sigma_{n+1}(y)) \longleftrightarrow \sigma_n(x) \in (\sigma_{n+1}) \cdot \sigma^{-1}(y))$$

We can reletter '$\sigma_n(x)$' as 'x' to get

$$(\forall\sigma\exists\tau)(\forall xy)((x \in (\sigma_{n+2}(\tau)) \cdot \sigma_{n+1}(y)) \longleftrightarrow x \in (\sigma_{n+1}) \cdot \sigma^{-1}(y))$$

and we then invoke extensionality to get

$$(\forall\sigma\exists\tau)(\sigma_{n+2}(\tau) \cdot \sigma_{n+1} = (\sigma_{n+1}) \cdot \sigma^{-1})$$

so we want $\sigma_{n+2}(\tau)$ to be

$$\sigma_{n+1} \cdot \sigma^{-1} \cdot (\sigma_{n+1})^{-1}$$

which is to say we want τ to be

$$(\sigma_{n+2})^{-1}(\sigma_{n+1} \cdot \sigma^{-1} \cdot (\sigma_{n+1})^{-1})$$

which can surely be simplified further. Expand σ_{n+2}^{-1} to

$$\sigma^{-1} \cdot j(\sigma^{-1}) \cdot j^2(\sigma^{-1}) \cdots j^{n+2}(\sigma^{-1})$$

and reflect that $j^{n+2}(\sigma^{-1})(t) = t^\sigma$ for suitably large n.

More to do here

■

Thanks to work of Nathan's we now know that

THEOREM 7 *(Bowler)*
(i)′ is transitive and reflexive but not symmetrical.

Proof:
Bowler showed that, for any $\mathfrak{M} \models$ NF, there is $t \in \mathfrak{Internal}$ that conjugates jc and j^2c. (c is the complementation function). Hitherto the only known proof of this fact used AC_2 – which prevents the permutation obtained from being definable[2]. Bowler's permutation is definable in $\mathfrak{M}$. $\mathfrak{M}^t$ believes there is a nontrivial (internal) $\in$-automorphism (which used to be jc and is therefore an involution). Bowler shows that t is not definable in $\mathfrak{M}^t$. The text which follows is edited from an email of his.

"We showed in lemma 3 that if $\mathfrak{N}$ is a permutation model derived from $\mathfrak{M}$ then $\mathfrak{Setlike}^{\mathfrak{N}} = \mathfrak{Setlike}^{\mathfrak{M}}$.

Now the setup is that, according to $\mathfrak{M}$, there are permutations π and σ such that $\sigma \neq j(\sigma)$ but $\pi \cdot \sigma = j(\sigma) \cdot \pi$. [aside: σ is jc and π is the clever permutation found by Nathan that conjugates jc and j^2c.] Let s, t and p be the elements of $\mathfrak{Setlike}^{\mathfrak{M}}$ with $s^{\mathfrak{M}} = \sigma$, $t^{\mathfrak{M}} = j(\sigma)$ and $p^{\mathfrak{M}} = \pi$. Thus $p \cdot s = t \cdot p$ and $s \neq t$. Let $\mathfrak{N}$ be the model $\mathfrak{M}^{\pi}$. Then it is not hard to check that s is an automorphism of $\mathfrak{N}$ since, for any x and y, we have

(1)	$\mathfrak{N} \models s(x) \in s(y)$	iff (beco's $\mathfrak{N} = \mathfrak{M}^{\pi}$)
(2)	$\mathfrak{M} \models s(x) \in \pi(s(y))$	iff (beco's s is called σ when it is living in $\mathfrak{M}$);
(3)	$\mathfrak{M} \models \sigma(x) \in \pi(\sigma(y))$	iff (beco's $\pi \cdot \sigma = j\sigma \cdot \pi$)
(4)	$\mathfrak{M} \models \sigma(x) \in j(\sigma)(\pi(y))$	iff (beco's σ is a permutation)
(5)	$\mathfrak{M} \models x \in \pi(y)$	iff (beco's $\mathfrak{N} = \mathfrak{M}^{\pi}$)
(6)	$\mathfrak{N} \models x \in y$.	

It follows that $\mathfrak{N}$ believes that $s^{\mathfrak{N}}$ is an automorphism. Looking at things this way, it isn't hard to find the 'return' permutation; it is simply $(p^{-1})^{\mathfrak{N}}$. Our aim now is to show that this return permutation is not a definable element of $\mathfrak{N}$. Since $\mathfrak{N}$ believes that $(p^{-1})^{\mathfrak{N}}$ is the inverse of $p^{\mathfrak{N}}$, it suffices to show that $p^{\mathfrak{N}}$ is not definable in $\mathfrak{N}$. We will do this by exhibiting an automorphism of $\mathfrak{N}$ for which it is not a fixed point. So we need to understand how permutations act on elements coding permutations. The key identity, which is not hard to check, is that if ρ and τ are permutations then $j^3(\rho)(\tau) = \rho \cdot \tau \cdot (\rho^{-1})$; here I am assuming that permutations are encoded as sets of Wiener-Kuratowski pairs, in the usual way. This means that, according to $\mathfrak{N}$, $j^3((s^{\mathfrak{N}})^{-1})(p^{\mathfrak{N}}) = (s^{\mathfrak{N}})^{-1}$.

$$p^{\mathfrak{N}} \cdot s^{\mathfrak{N}} = (s^{-1} \cdot p \cdot s)^{\mathfrak{N}} = (s^{-1} \cdot t \cdot p)^{\mathfrak{N}} \neq p^{\mathfrak{N}},$$

since $s \neq t$. Thus $p^{\mathfrak{N}}$ is moved by the permutation given in $\mathfrak{N}$ as $j^3((s^{\mathfrak{N}})^{-1})$, and $\mathfrak{N}$ believes that this permutation is an automorphism since it believes that $s^{\mathfrak{N}}$ is an automorphism. Thus $p^{\mathfrak{N}}$ cannot be definable in $\mathfrak{N}$."

■

[2]Use of AC_2 is in any case bad practice in NF since AC – tho' admittedly not yet AC_2 – is known to be inconsistent with NF.

Not sure about (ii)′, but i think i am now ready to claim (ii).

THEOREM 8
Recall that the relation ((ii) above) is the relation that holds between two di Giorgi structures $\mathfrak{M}$ and $\mathfrak{N}$ when $\mathfrak{N}$ is $\mathfrak{M}^s$ for some permutation of A that is setlike for $\mathfrak{M}$. We claim that (ii) is symmetrical.

Proof:
We first have to show that if s is setlike for $\mathfrak{M}$ then s^{-1} is setlike for $\mathfrak{M}^s$. Secondly we would have to show that s^{-1} (which gives us a permutation model once we have established that s^{-1} is setlike for $\mathfrak{M}^s$ – if indeed we have) not only gives us a permutation model but takes us back to $\mathfrak{M}$. Let's grind out the first. Suppose s is 1-setlike for $\mathfrak{M}$. Is s^{-1} 1-setlike for $\mathfrak{M}^s$? Let x be a set of $\mathfrak{M}^s$. We want the collection of things that are s^{-1} of things in x to be a set of $\mathfrak{M}^s$. So we need there to be y such that, for all z, $z \in s(y)$ iff z is $s^{-1}(w)$ for some $w \in s(x)$. So we want $s(y)$ to be $\{z : s(z) \in s(x)\}$, and that set abstract is certainly a set beco's s^{-1} is setlike. So it looks OK.

Better check 2-setlike too!

Does s^{-1} take us back to $\mathfrak{M}$? We want $\mathfrak{M}^s \models x \in s^{-1}(y)$ iff $\mathfrak{M} \models x \in y$. This appears to be completely straightforward. $\mathfrak{M}^s \models x \in s^{-1}(y)$ becomes either $\mathfrak{M} \models x \in s \cdot s^{-1}(y)$ or $\mathfrak{M} \models x \in s^{-1} \cdot s(y)$ (and i'm not sure which!) but either way it simplifies to $\mathfrak{M} \models x \in y$. Which is what we want.

If this is correct (and it seems to be!) then it means that all permutations can be undone – *even those that aren't setlike*. But, if it *is* correct, how can i have missed it all these years?

■

(ii)′ (definable setlike permutations) might turn out to be degenerate. Any definable setlike permutation is going to be internal isn't it? I don't think we can have a setlike permutation that is definable by an unstratifiable expression but not by any stratifiable expression. I think there is a discussion of that possibility in these notes somewhere . . . p. 143. Very well: lemma 3 tells us that all permutation models of a given model (even using permutations that are merely setlike rather than actually internal) have the same internal permutations. (The question of whether or not they all see the same group of internal *automorphisms* is an old one which we will discuss below). Do they have the same *setlike* permutations? It seems not: the groups of permutations that one finds are not all the same, but at least they are all conjugate copies of one another. We are going to need the back of an envelope.

First a potentially useful observation:

REMARK 20
For all $\mathfrak{i}$*,* s *and* t*, if* $\mathcal{D}_{\mathfrak{i}}(s)$ *is defined then so is* $\mathcal{D}_{\mathfrak{i}\cdot t}(s)$*, and it is equal to* $t^{-1} \cdot \mathcal{D}_{\mathfrak{i}}(s) \cdot t$.

Proof:

If $\mathcal{D}_{\mathfrak{i}}(s)$ is defined it is beco's js is in the setwise stabiliser of $\mathfrak{i}``A$; if $\mathcal{D}_{\mathfrak{i}\cdot t}(s)$ is defined it is beco's $js$ is in the setwise stabiliser of $(\mathfrak{i} \cdot t)``A$ ... which last is the same as the setwise stabiliser of $\mathfrak{i}``A$, since – for any permutation $t$ of $A$ whatever – $\mathfrak{i}``A = (\mathfrak{i} \cdot t)``A$

So If $\mathcal{D}_{\mathfrak{i}}(s)$ is defined so is $\mathcal{D}_{\mathfrak{i}\cdot t}(s)$. That is to say, if s is 1-setlike wrt $\mathfrak{i}$ then it is 1-setlike wrt $\mathfrak{i} \cdot t$ for any permutation t. So whether s is 1-setlike wrt $\mathfrak{i}$ or not depends only on the range of $\mathfrak{i}$..

Now for the conjugacy observation .... $\mathcal{D}_{\mathfrak{i}}(s)$ is $\mathfrak{i}^{-1} \cdot js \cdot \mathfrak{i}$ and $\mathcal{D}_{\mathfrak{i}\cdot t}(s)$ is $(\mathfrak{i} \cdot t)^{-1} \cdot js \cdot \mathfrak{i} \cdot t$,

so $\mathcal{D}_{\mathfrak{i}\cdot t}(s)$ is

$$(\mathfrak{i} \cdot t)^{-1} \cdot \mathfrak{i} \cdot \mathcal{D}_{\mathfrak{i}}(s) \cdot \mathfrak{i}^{-1} \cdot \mathfrak{i} \cdot t$$

which simplifies to

$$t^{-1} \cdot \mathcal{D}_{\mathfrak{i}}(s) \cdot t.$$

■

And we have not assumed that t is $\mathfrak{i}$-setlike!

Fix t (not assumed to be $\mathfrak{i}$-setlike for the moment...)
s is 1-setlike wrt $\mathfrak{i}$ iff
s is 1-setlike wrt $\mathfrak{i} \cdot t$

s is 2-setlike wrt $\mathfrak{i}$ iff
$(\mathfrak{i} \cdot t)^{-1} \cdot js \cdot \mathfrak{i} \cdot t$
(which is)
$t^{-1} \cdot \mathfrak{i}^{-1} \cdot js \cdot \mathfrak{i} \cdot t$
is 1-setlike wrt $\mathfrak{i}$.

s is 3-setlike wrt $\mathfrak{i}$ iff
$(\mathfrak{i} \cdot t)^{-1} \cdot j((\mathfrak{i} \cdot t)^{-1} \cdot js \cdot \mathfrak{i} \cdot t) \cdot \mathfrak{i} \cdot t$.
is 1-setlike wrt $\mathfrak{i}$.
This is
$t^{-1} \cdot \mathfrak{i}^{-1} \cdot j(t^{-1} \cdot \mathfrak{i}^{-1} \cdot \underline{js \cdot \mathfrak{i}} \cdot t) \cdot \mathfrak{i} \cdot t$
Now $js \cdot \mathfrak{i} = \mathfrak{i} \cdot \mathcal{D}_{\mathfrak{i}}(s)$ so we can rewrite the underlined part to get
$t^{-1} \cdot \mathfrak{i}^{-1} \cdot j(t^{-1} \cdot \mathfrak{i}^{-1} \cdot \mathfrak{i} \cdot \mathcal{D}_{\mathfrak{i}}(s) \cdot t) \cdot \mathfrak{i} \cdot t$
which becomes
$t^{-1} \cdot \mathfrak{i}^{-1} \cdot j(t^{-1} \cdot \mathcal{D}_{\mathfrak{i}}(s) \cdot t) \cdot \mathfrak{i} \cdot t$
and we want this to be 1-setlike wrt $\mathfrak{i}$.

gulp. I Have No Idea what was going on here.

Recall that if s is setlike so are all the s_n. More generally, if s is n-setlike then s_n is 1-setlike.

To prove that relation (ii) above is symmetrical one would reason as follows (with fingers crossed!). Let $\mathfrak{M}$ be a model of NF, and s a permutation setlike for $\mathfrak{M}$. Then, by remark 19, s is also setlike for $\mathfrak{M}^s$. Does that mean that s^{-1}, too, is setlike for $\mathfrak{M}^s$? Presumably. If so, we can jump into $(\mathfrak{M}^s)^{s^{-1}}$ (whatever that means!) which ought to be $\mathfrak{M}$.

But there's many a slip twixt cup and lip.

Fix an injection $\mathfrak{i}$ and let s be $\mathfrak{i}$-setlike. We ask whether any permutation that is setlike wrt $\mathfrak{i}$ is also setlike wrt $\mathfrak{i} \cdot s$. Since $\mathfrak{i} \cdot s$ and $\mathfrak{i}$ have the same range anything that is 1-setlike wrt one is 1-setlike wrt the other.

Suppose t is setlike wrt $\mathfrak{i}$; is it going to be setlike wrt $\mathfrak{i} \cdot s$? Now t is a 2-setlike permutation wrt $\mathfrak{i} \cdot s$ iff $j(\sigma^{-1} \cdot \mathfrak{i}^{-1} \cdot jt \cdot \mathfrak{i} \cdot s)$ is in the setwise stabiliser of $(\mathfrak{i} \cdot s)$"A. But the setwise stabiliser of $(\mathfrak{i} \cdot s)$"A = the setwise stabiliser of $\mathfrak{i}$"A. Observe that $j(s^{-1} \cdot \mathfrak{i}^{-1} \cdot jt \cdot \mathfrak{i} \cdot s)$ is the result of conjugating $j(\mathfrak{i}^{-1} \cdot jt \cdot \mathfrak{i})$ by js, and $j(\mathfrak{i}^{-1} \cdot jt \cdot \mathfrak{i})$ is in the setwise stabiliser of $\mathfrak{i}$"A. So we need the setwise stabiliser of $\mathfrak{i}$"A to be closed under conjugation by js whenever s is setlike wrt $\mathfrak{i}$. is that true?

4.3.1 Suppose s and t are setlike; can V^s and V^t see each other?

That means that, if we place ourselves inside V^s we can see a permutation π such that, for all x and y, $V^s \models x \in \pi(y)$ iff $x \in t(y)$.

hang on: do we mean setlike or internal???

This is equivalent to

$$(\forall xy)(s_n(x) \in s_{n+2}(\pi) \cdot (s_{n+1}(y)) \longleftrightarrow x \in t(y))$$

and then

$$(\forall x, y)(x \in (js_n)^{-1} s_{n+2}(\pi) \cdot (s_{n+1}(y)) \longleftrightarrow x \in t(y))$$

giving

$$(\forall y)(js_n)^{-1} s_{n+2}(\pi) \cdot (s_{n+1}(y)) = t(y))$$

by extensionality and then

$$(js_n)^{-1} \cdot s_{n+2}(\pi) \cdot (s_{n+1}) = t$$

Compose both sides on the L with js_n to get

$$s_{n+2}(\pi) \cdot (s_{n+1}) = js_n \cdot t$$

and then compose both sides with $(s_n)^{-1}$ on the R to get

$$s_{n+2}(\pi) = js_n \cdot t \cdot (s_{n+1})^{-1}$$

whatever that means.

Check this

$\pi = (s_{n+2})^{-1}(js_n)t(s_{n+1})$

I've got the ns jumbled up a bit but the idea is roughly right.

And i think the same idea works even in the other setting where we consider setlike permutations rather than internal permutations.

4.4 Setlike Permutations

There are two sections both called 'Setlike Permutations '

> . . . which we establish that there are permutations that are not 1-setlike, permutations that are 1-setlike but not 2-setlike, and muse about the possibility of permutations that are 2-setlike but not 3-setlike. We also explore connections between k-setlike permutations and the normaliser of the group of internal permutations.

Quite which subgroups of Symm(A) can turn up as the group $\mathfrak{Setlike}^{\mathfrak{i}}$ of permutations of A that are $\mathfrak{i}$-setlike for some $\mathfrak{i}$ is not entirely clear to me at this stage. Certainly Symm(A) itself can be such a group: take $\mathfrak{i}$ to be a bijection between A and $\mathcal{P}_{\aleph_0}(A)$; every permutation of A is $\mathfrak{i}$-setlike for this $\mathfrak{i}$. But of course the model that results is not a model of NF. It might help readers who are not familiar with the di Giorgi presentation to say a bit about what it does model. ZF $\setminus$ Inf + $\neg$Inf perhaps. Does it obey TC? Clearly you can't expect foundation.

The concept of *setlike* permutation has two motivations. We are seeing one of them here, but there is another – and it is more general, in that it motivates a definition of setlike *function* not just setlike *permutation*. In NF or Zermelo any function that is (locally) a set obeys replacement. However, there might be functions that are not sets (even locally) but nevertheless still obey replacement. Such functions (too) are said to be 1-setlike. Thus: a function that obeys replacement is 1-setlike, and a function that, when lifted (once) still obeys replacement is 2-setlike, and so on up. There is no reason to suppose that the inverse of a function that obeys replacement in this way (even supposing such an inverse to be defined) will analogously obey replacement. But what does one want to say about a *permutation* that is k-setlike in this sense? If you lift a 2-setlike permutation once you get a 1-setlike permutation . . . ? The key word here is *permutation*. If you want the lift of a permutation to be an actual *permutation* then you need its inverse to be setlike in the same sense; see earlier concerns about the difference between one-sided and two-sided setlike permutations. And do we need the lift of a k-setlike permutation to be a $(k-1)$-setlike permutation? Yes, because we are looking for the appropriate generalisation of *sethood* that enable one to prove the lemmas of Coret, Boffa and Henson about eliminating from twisted stratified formulæ all the variables-over-permutations that have been used to twist them.

I noticed this some years ago.

REMARK 21 *(NF)*

Let t be the permutation $\lambda x.(\texttt{if } x \in \mathbb{N} \texttt{ then } Tx \texttt{ else } x)$.

(i) t is 1-setlike;
(ii) The assertion that t is 2-setlike is equivalent to the axiom of counting.

Proof:

(i) Evidently t is a permutation of A. Also $t``x$ is always a set. This is because $x = (x \cap \mathbb{N}) \cup (x \setminus \mathbb{N})$. And clearly $t``(A \cup B) = t``A \cup \tau``B$. So $t``x = T``(x \cap \mathbb{N}) \cup (x \setminus \mathbb{N})$.

So t is 1-setlike.

(ii) The assertion that t is 2-setlike is that the following is always a set:

$$t``x = \{T``(y \cap \mathbb{N}) \cup (y \setminus \mathbb{N}) : y \in x\}.$$

Set $X = \{\{Tn, \{n\}\} : n \in \mathbb{N}\}$; X is a set. By assumption t is 2-setlike, so $\{t``x : x \in X\}$ is a set. What is $t``x$ when $x \in X$? If $x \in X$ then $x = \{Tn, \{n\}\}$ for some n. So $t``x$ is $\{t(Tn), t(\{n\})\}$. Now $t$ fixes $\{n\}$ but moves $Tn$ to $T^2n$ so $t``x$ is $\{T^2n, \{n\}\}$. So $\{t``x : x \in X\}$ is $\{\{T^2n, \{n\}\} : n \in \mathbb{N}\}$. The intersection of this set of pairs with the set $\{\{Tn, \{n\}\} : n \in \mathbb{N}\}$ (and it is a set) is $\{\{Tn, \{n\}\} : Tn = n \in \mathbb{N}\}$ whence we can obtain the sethood of $\{n \in \mathbb{N} : n = Tn\}$, and this implies the axiom of counting.

For the other direction of (ii) reflect that the axiom of counting is equivalent to the assertion that t is the identity function, which is obviously 2-setlike. ■

However that was just an appetiser and we can do better, much better.

4.4.1 Setlike permutations and the normaliser of the group of internal permutations

We will establish that there are permutations that are not 1-setlike, and that there are permutations that are 1-setlike but not 2-setlike. Every 2-setlike permutation is setlike, and the group of setlike permutations is precisely the normaliser of the group of internal permutations

The *normaliser* of a subgroup H of G is the set of those elements $g \in G$ s.t. conjugation by g fixes H setwise.

First let us record that there are permutations that are not even 1-setlike.

Remark 22
Let σ be the permutation that fixes every set that is wellfounded or the complement of a wellfounded set, and swaps everything else with its complement.

Then σ is not 1-setlike.

Proof: .

Let A be $\{x : |x| \leq T|V|\}$. We will show that $\sigma``A$ is not a set. $A$ contains every wellfounded set but no complement of any wellfounded set. (By Bowler-Forster [?] the complement of a wellfounded set is always of size $|V|$.) The same goes for $\sigma``A$ since σ fixes all such sets. So $A \cap \sigma``A$ contains all wellfounded sets. What else does it contain? If $a \in A$ and $a$ is not wellfounded, then $\sigma(a)$ is the complement of a set the same size as a set of singletons, and no such set can be in $A$ – and *a fortiori* not in $A \cap \sigma``A$. So $A \cap \sigma``A$ is precisely the class of wellfounded sets, and that is not a set. So $\sigma``A$ was not a set either.

■

This σ is merely one of a host of possible illustrations; instead of *wellfounded* we could have used *strongly cantorian* or any of a wealth of others. Any property will do as long as its extension consists entirely of small sets and cannot itself be a set.

PROPOSITION 1 *There is a permutation that is*
(i) 1-setlike; but
(ii) not 2-setlike; and
(iii) is not in the normaliser of the group of internal permutations.

Proof:
There is a definable bijection

$$f : V \setminus \iota^2 ``V \longleftrightarrow V \setminus \iota``V.$$

We require f to have $T|V|$-many fixed points, so that there is a bijection $h : \mathtt{fix}(f) \longleftrightarrow \iota``V$. This is easily arranged since both the domain and the range are of size $|V|$, which is much bigger than $|\iota``V|$

So we can define a permutation π:

$$\pi(x) = \mathtt{if}\ x = \{\{y\}\}\ \mathtt{then}\ \{y\}\ \mathtt{else}\ f(x)$$

(i)

This π is clearly a permutation and is clearly 1-setlike: we obtain $\pi``x$ as the union of $\pi``(x \cap \iota^2 ``V)$ and $\pi``(x \setminus \iota^2 ``V)$, both of which are sets. The first is $\iota^{-1} ``(x \cap \iota^2 ``V)$ and the second is $f``(x \setminus \iota^2 ``V)$.

(ii)

To show that π is not 2-setlike consider the set P of all pairs $\{\{\{x\}\}, V \setminus \{\{x\}\}\}$. We will show that $j^2\pi(P)$ cannot be a set.

$j\pi$ of any pair $\{\{\{x\}\}, V \setminus \{\{x\}\}\}$ in P is $\{\{x\}, f(V \setminus \{\{x\}\})\}$. Now $f(V \setminus \{\{x\}\})$ is not a singleton, so we can sensibly do f^{-1} to the sole member of $\{\{x\}, f(V \setminus \{\{x\}\})\}$ that is not a singleton, getting $\{\{x\}, V \setminus \{\{x\}\}\}$. So, if $j^2\pi(P)$ is a set, so too is $\{\{\{x\}, V \setminus \{\{x\}\}\} : x \in V\}$. Now precisely one member of any such pair is a singleton, so we take the complement of the element that isn't a singleton, getting the set of all pairs $\{\{x\}, \{\{x\}\}\}$. This is impossible since V is not strongly cantorian.

(iii)

Now take σ to be any (set!) permutation that extends h. We will show that $\pi^{-1}\sigma\pi$ is not a set.

Suppose it were. Consider what $\pi^{-1}\sigma\pi$ does to $\{\{x\}\}$. First π sends it to $\{x\}$; then σ sends $\{x\}$ to $h(\{x\})$ which is fixed by f, so when we do π^{-1} to $h(\{x\})$ we still get $h(\{x\})$. So, as long as $\pi^{-1}\sigma\pi$ is a set, we can compose it on the left with h^{-1} to get $h^{-1}\pi^{-1}\sigma\pi$ whose restriction to $\iota^2 ``V$ is a bijection $\iota^2 ``V \to \iota``V$. This cannot be.

■

In fact (iii) follows from (ii); we shall show below (theorem 9) that every permutation in the normaliser is 2-setlike.

There is an elementary fact about applying permutations to permutations which will loom large in what follows. If we think of permutations as sets of ordered pairs then, for some n depending on how we implement ordered pairs, we find that, for any two permutations σ and τ, $j^n\sigma(\tau) = \sigma^{-1}\tau\sigma$. This underpins much in the study of Rieger-Bernays permutation models of NF, particularly with iterated R-B constructions. For us here, its significance is that it means that for any sufficiently large n, the normaliser of the group of internal permutations contains every n-setlike permutation.

In fact $n = 2$ is large enough.

Remark 23
Every 2-setlike permutation belongs to the normaliser of the group of internal permutations.

Proof:
We first prove, as a kind of warm-up, that every 3-setlike permutation belongs to the normaliser of the group of internal permutations. Let us suppose we are using Wiener-Kuratowski ordered pairs. Then, if τ is a permutation, $j^3\sigma(\tau)$ is a permutation for any permutation $j^3\sigma$. In fact $j^3\sigma(\tau)$ is τ^σ, the result of conjugating τ with σ. Now, as long as σ is 3-setlike, $j^3\sigma(\tau)$ is defined and equal to τ^σ. But τ was an arbitrary internal permutation, so this is telling us that this 3-setlike permutation σ is in the normaliser of the group of internal permutations.

However we made this claim for *2-setlike* permutations not merely 3-setlike permutations. Permutations can be thought of as sets of ordered pairs, as above, but – in particular – *involutions* can be thought of as sets of *un*ordered pairs – and we can thereby get away with using fewer levels. Thus, if σ is 2-setlike, $j^2\sigma$ can be applied to an involution τ, and the result will be τ^σ as before. Now, by Bowler-Forster [**?**], every internal permutation τ is a product of finitely many involutions $\pi_1 \cdot \pi_2 \cdots \pi_n$. So the result of conjugating τ by σ is

$$(\pi_1 \cdot \pi_2 \cdots \pi_n)^\sigma$$

which of course is

$$\pi_1^\sigma \cdot \pi_2^\sigma \cdots \pi_n^\sigma$$

which is

$$j^2\sigma(\pi_1) \cdots j^2\sigma(\pi_2) \cdots j^2\sigma(\pi_n)$$

Now σ is 2-setlike, so all the $j^2\sigma(\pi_i)$ exist. Therefore their product – which is τ^σ – exists too. Note that the n hedere is concrete – something less than 17 (i think).

■

For future reference, when we wish to prove this theorem in the context of Zermelo set theory, we record that the fact that every permutation is a product of involutions can be proved in NF *without* choice, whereas it requires at least *some* choice to prove this fact in Zermelo. If this trick using involutions is not available then we would have to rephrase remark 23 to read "every 3-setlike permutation belongs to the normaliser of the group of internal permutations" – as in the warm-up.

Observe that if $j\sigma$ is in the normaliser of the group of internal permutations, so is σ. If $j\sigma$ is in the normaliser and so in particular, if $j\tau$ is an internal permutation, so is $(j\sigma)^{-1}j\tau j\sigma$. But then so is $\sigma^{-1}\tau\sigma$. But τ was an arbitrary internal permutation. So σ is in the normaliser.

If σ is in the normaliser of the group of internal permutations then the permutation $\tau^\sigma = \sigma^{-1}\tau\sigma$ is a set for all internal permutations τ. Notice that this makes no mention of choice of pairing function, so this set – $\sigma^{-1}\tau\sigma$ – exists whatever pairing function we employ. Now $\sigma^{-1}\tau\sigma$ is $j^k\sigma(\tau)$, with k depending on the kind of pairing function we are using. So, however we encode τ as a set of ordered pairs (whatever pairing function we use) there is k such that $j^k\sigma(\tau)$ exists.

Theorem 9 *Every permutation in the normaliser of the group of internal permutations is setlike.*

Proof:

First we show (i) that, for σ in the normaliser, $j^n\sigma(X)$ exists as long as X is a permutation. Then we show (ii) how to code arbitrary sets X as permutations and thereby obtain $j^n\sigma(X)$.

(i)

Suppose σ is in the normaliser so that, whenever π is an internal permutation, so is $\sigma^{-1}\pi\sigma$. But, for some n depending on our choice of pairing function, this object is $j^n\sigma$ applied to π, which is to say the image of π in $j^n\sigma$. By judicious choice of pairing function we can take n to be any sufficiently large concrete natural. So at least *some* images in $j^n\sigma$ exist. Images of inner *permutations* exist, so how about images of arbitrary sets?

OK so far . . . !

(ii)

We note that there are the same number of sets as there are permutations. The idea will be to code up an arbitrary set x as a permutation, take the image of the permutation in $j^n\sigma$ and then decode it. With luck the thing you get back will actually be the image in $j^n\sigma$ of the set you started with.

Let's first establish that there are indeed precisely $|V|$-many permutations. $B(\emptyset$ is a moiety, it's the same size as V, so its power set $\mathcal{P}(B(V))$ is of size $|V|$ too. Send a subset $X \subseteq B(V)$ to $c \restriction X$, the restriction of the complement permutation to X. This map is homogeneous and injective, and it's a map from a set of size $|V|$ into Symm(V), the symmetric group on the universe. By composing this map on the right with an injection $V \hookrightarrow \mathcal{P}(B(V))$ we obtain an injection $V \hookrightarrow$Symm(V). By Cantor-Bernstein we can now cook up a bijection $V \longleftrightarrow$Symm(V). It won't be pretty but it will be definable by a homogeneous expression. Let us write it with a 'χ' for *c*oding.

If we are using Quine pairs (which are surjective) we can think of a set as a set of ordered pairs, as a relation. Then, for suitable small k, $j^k\sigma(R)$ is $\{\langle\sigma(x), \sigma(y)\rangle : R(x, y)\}$, which we can write as R^σ.

We will overload 'χ' by writing '$\chi(R, \tau)$' to mean $\chi(R) = \tau$.

Since $\chi(-,-)$ is homogeneous it is standard that

$$\chi(R, \tau) \longleftrightarrow \chi(j^k\sigma(R), j^k\sigma(\tau))$$

which is to say

$$\chi(R, \tau) \longleftrightarrow \chi(R^\sigma, \tau^\sigma).$$

Now, given R, we want to obtain $j^k\sigma(R)$ – which is R^σ. We first do χ to R, getting $\chi(R)$. This is a permutation, so we can conjugate it with σ (since σ is in the normaliser) getting $(\chi(R))^\sigma$, and this last thing therefore exists. It is also a permutation, so we can do χ^{-1} to it, getting R^σ, and this last is $j^k\sigma(R)$ – which was what we wanted.

But we could have chosen n as large as we like. So σ is setlike,

■

I'm no longer sure that i believe this last bit

COROLLARY 3

(i) The normaliser of the group of internal permutations is precisely the collection of setlike permutations.

(ii) Every 2-setlike permutation is setlike.

Proof:

By remark 23 every 2-setlike permutation is in the normaliser, and by theorem 9 everything in the normaliser is k-setlike for arbitrarily large k which is to say: setlike.

■

Corollary 3 was not expected. NFistes had been expecting that for every concrete k we would be able to find a permutation that is k-setlike but not $(k + 1)$-setlike. Remark 22 and prop. 1 fitted in with this picture but it stops there: it this holds only for $k = 0$ or 1.

No, try again. Let's not rush things. If σ is in the normaliser of the group of inner/internal (which word do we prefer?) permutations then $\sigma^{-1}\tau\sigma$ is an internal permutation whenever τ is. That means that $\sigma^{-1}\tau\sigma$ is a *set* whenever τ is. But which set? We can manage to think of this fact about σ *as a set-existence principle* only if we have settled on an implementation of permutations *as sets*. That is to say, we seek a three-place formula $\Pi^I(\pi, x, y)$ that says that π is a permutation and that it sends x to y. (The superscipt 'I' reminds us that the expression depends on the implementation). It has to obey the obvious axioms... things like $(\forall x)(\exists! y)\Pi^I(\pi, x, y)$ and $(\forall y)(\exists! x)\Pi^I(\pi, x, y)$. It's clear that $\Pi^I(\pi, x, y)$ has to be stratified, and that the two variables 'x' and 'y' have to be given the same type in any stratification. Notice that in these circumstances we must have $\Pi^I(\pi, x, y) \longleftrightarrow \Pi^I(j^n\sigma(\pi), j^k\sigma(x), j^k\sigma(y))$ for suitable k and n. This means that if π obeys these, so that

$$(\forall x)(\exists! y)\Pi^I(\pi, x, y) \wedge (\forall y)(\exists! x)\Pi^I(\pi, x, y)$$

then so must $j^k\sigma(\pi))$, to wit

$$(\forall x)(\exists! y)\Pi^I(j^k\sigma(\pi)), x, y) \wedge (\forall y)(\exists! x)\Pi^I(j^k\sigma(\pi)), x, y)$$

but this is as much as to say that $j^k\sigma(\pi))$ is a set, and an internal permutation.

Bibliography

[?] Nathan Bowler and Thomas Forster "Normal Subgroups of Infinite Symmetric Groups, with an Application to Stratifiable Set Theory". *Journal of Symbolic Logic* **74** (2009) pp 17–26.

Now the recovery starts.

Well, we can at least show that everything in the normaliser is 1-setlike. Suppose σ is in the normaliser, and we want σ"x. Let τ be any permutation that fixes everything in x and moves everything else (and don't give me any crap like "what if x is the complement of a singleton?" Grrr). Consider τ^σ – which (let us not forget) is a *set*. The set of fixpoints for τ^σ is precisely σ^{-1}"x. So there!

How about 2-setlike? Perhaps we could consider $j(\tau^\sigma)$'? and its fixed points...? Its fixed points are the sets of the form σ^{-1}"y where y is a union of τ-orbits. Any subset of x is such a union but we have to somehow scrape off the others. One possibility would be to find two such τ – τ_1 and τ_2 – say, with the property that the only sets fixed by *both* τ_1 and τ_2 are subsets of x.

No, i think the idea is to organise τ to have only fixed point and infinite cycles. Then we can request the set of those fixed points of $j(\tau^\sigma)$ that are unions of finite fixed points. That should work

Soyons précis

To show that a permutation σ in the normaliser is 1-symmetric, we show that σ"x is a set for any x. So: fix x and consider any τ that fixes everything in x and moves everything not in x, as above. If x is $V \setminus \{y\}$ then this can't be done, but that isn't a worry beco's σ"x can be constructed by hand. But we are appealing to the proposition that every set with at least two members has a permutation without fixed points, or one with at most one fixed point. "Every set is even or odd"! I am not sure of the strength of this assertion ... for sure it's not great, but i would guess that it isn't a theorem of NF. (Surely every countably amorphous set has this property, that every permutation has a fixpoint ...? Must ask JKT) Now Nathan's quasiorder comes in here. If $\tau \leq \pi$ and π has no fixed point then neither does τ.

The plan is to show that everything in the normaliser is setlike. I start by showing that everything in the normaliser is 1-setlike. Let σ be in the normaliser and x be any set. We want σ"x to be a set. Let τ be a permutation that fixes everything in x and moves everything else. (Don't worry about the special case where x is the complement of a singleton - we can deal with that by hand) Since σ is in the normalisr, $sigma^{-1} tau\sigma$ exists, and σ^{-1}"$x is the set of its fixed points.$

Of course if tau moves everything in x and fixes everything in $V \setminus x$ then we take the support of τ instead.

So, as long as, **for every x, x or $V \setminus x$ has a permutation with no fixpoints**, σ is 1-setlike.

To get 2-setlike you consider fixpoints for j of $\sigma^{-1}]tau\sigma$ or rather you consider fixpoints all of whose subsets are fixpoints. But that needs a bit of tidying up!

Not sure if the bit in **boldface** is provable. My guess is not, but it's v weak.

To show that every permutation σ in the normaliser is 2-setlike we want to show that $\{\sigma``y : y \in x\}$ exists for arbitrary $x$. So let $x$ be arbitrary. We consider $\tau^\sigma$ as before, and we want $\tau$ to fix everything in $x$ and move everything in $V \setminus x$. We are going to consider $j(\tau^\sigma)$ and we want to obtain $\{\sigma``y : y \in x\}$ as the set of fixed points for $j(\tau^\sigma)$. That set of fixed point consists of all preimages of τ-cycles in σ and not all of those are subsets of x. However we can detect when a τ-cycle is a subset of x beco's it consists of unions of singletons that are fixed points.

So: if every set is even or odd then every permutation in the normaliser is 2-symmetric.

A bit of a jumble from here to the end of tis section

What about 3-symmetric.

Let us consider the assertion that $\sigma``X$ exists, and what this tells us about $\sigma$ when $\sigma$ is definable. $y \in \sigma``X$ is $(\exists z)(z \in X \wedge \phi(z, y))$ and we want to be able to stratify ϕ so that an instance of weakly stratified comprehension will give us $\sigma``X$. We notice the curious fact that in $(\exists z)(z \in X \wedge \phi(z, y))$ there is only one occurrence of 'X'. Now if we can stratify ϕ we have an instance of stratified comprehension and σ will be outright setlike. It seems that ϕ has to be disjunctive, so we have something like

$$(\exists z)(z \in X \wedge (\phi_1(z, y) \vee \phi_2(z, y)))$$

which is equivalent to

$$(\exists z)(z \in X \wedge \phi_1(z, y)) \vee (\exists z)(z \in X \wedge \phi(z, y))$$

and this can be weakly stratified if – for example – ϕ_1 makes 'y' and 'z' have the same type and ϕ_2 makes 'y' and 'z' have different types.

This illustrates a point about how the rules for the existential quantifier can bugger up stratification. Let us consider the permutation t above. Why is t 1-setlike? For any x, $t``x$ exists. Now $t``x$ is $\{z : (\exists y \in x)(y \in \mathbb{N} \wedge z = Ty) \vee (\exists y \in x)(y \notin \mathbb{N} \wedge y = z)\}$, so the eigenformula for the instance of comprehension that gives us this is

$(\exists y \in x)(y \in \mathbb{N} \wedge z = Ty) \vee (\exists y \in x)(y \notin \mathbb{N} \wedge y = z)$

which is weakly stratified. However $\exists y F(y, x) \vee \exists y G(y, x)$ is equivalent to $\exists y(F(y, x) \vee G(y, x))$ which might not be stratified, for example if the type difference between 'y' and 'x' differs between F and G. This is in fact exactly what happens in this case; when we coalesce the two existential quantifiers we get

$$(\exists y \in x)((y \in \mathbb{N} \wedge z = Ty) \vee (y \notin \mathbb{N} \wedge y = z)$$

which is unstratified.

Does this matter?

Randall says one should be able to bring the Pétry-Henson-Forster completeness theorem into play here: that's the onse that says that stratified = preserved by setlike permutations

The idea is to show that if a permutation f is 2-setlike and is definable, then it is definable by a stratified – indeed *homogeneous* – formula and therefore is k-setlike for all k.

We make the assumption that f is definable so that we can write $y = f(x)$ as $\phi(x, y)$. Suppose further that f is 2-setlike. Then $\{f``y : y \in x\}$ exists for all $x$. Since $f$ is definable we obtain this object by means of a comprehension axiom – as $\{z : (\exists y \in x)(z = f``y)\}$. So we want $(\exists y \in x)(z = f``y)$ to be weakly stratified. Now expand $z = f``y$ to $(\forall u)(u \in z \longleftrightarrow (\exists w \in y)(z = f(w)))$ or (perhaps better, writing '$\phi(z, w)$' for '$z = f(w)$') to $(\forall u)(u \in z \longleftrightarrow (\exists w \in y)(\phi(z, w))$ so, in full, we get

$$(\exists y \in x)((\forall u)(u \in z \longleftrightarrow (\exists w \in y)(\phi(z, w)))) \tag{Ax}$$

and we need (Ax) to be weakly stratified. But if (Ax) is weakly stratified then ϕ is stratified. And then of course f is k-setlike for all k.

This isn't a proof that every 2-setlike permutation is setlike, but it's a straw in the wind. To state and prove such a theorem properly we probably need ML. But the ML version might not be true! I see no prospect of proving for an arbitrary class permutation that if it is 2-setlike then it's k-setlike for all k.

But there should be proper class permutations which str(NFC) proves to be setlike and which give rise to permutation models of NFC. It would be nice to have some examples.

But are there any...?

Observe that ι is a setlike *function* that is not a set. Any stratifiable inhomogenous function will do the same. But of course there are no *stratifiable* (inhomogeneous) *permutations* duh! Any such entity would force V to be cantorian. Can there be a setlike permutation that is not a set? Of course, as Randall says, all automorphisms are setlike, and it's not hard to cook up models of NF with non-trivial automorphisms that are not sets. But then those setlike permutations are not definable. It doesn't seem to be out of the question that there could be a highly unstratified formula with two free variables that defines a permutation that is setlike. It would be good to either find such a formula, or prove that there is none. So: two questions:

(i) Can there be a permutation defined by an unstratified formula?

(ii) If so, can there be a setlike permutation defined by an unstratified formula?

Consider the greatest fixed point for $A \mapsto \iota``A$. It's the collection of all $x$ such that $(\exists A)(x \in A \wedge A \subseteq \iota``A)$. Perfectly satisfactory unstratifiable property that probably doesn't determine a set. Abbreviate to F. Now consider the permutation that swaps everything in F with its complement. Is that setlike? More generally, if F is an unstratifiable formula with a single free variable we can consider the product $\prod_F(x, V \setminus x)$ that swaps every F-thing with its complement and fixes everything else. That has a chance of being setlike but not a set.

Lemma 4 *Suppose σ is a 2-setlike permutation, and X is a set that can be equipped with a class-wellordering. Then $\sigma \restriction X$ is a set.*

Proof:
Proof:
We can code wellorderings as ordernestings, so if o is an ordernesting so is $j^2\sigma(o)$ – and it exists beco's σ is 2-setlike by assumption. Now let $\langle X, <\rangle$ be a wellordering thought of as an ordernesting o. o and $j^2\sigma(o)$ are two wellorderings and so one is isomorphic to an initial segment of the other. If we could prove that o and $j^2\sigma(o)$ were isomorphic we could keep our fingers crossed and hope that this isomorphism was a restriction of σ. There is at the very least an isomorphism (which may be partial) $f : X \to \sigma``X$. The obvious thing to do is attempt to prove by induction on $<$ that $f = \sigma \restriction X$. The induction looks fine, but the statement we are trying to prove contains a class parameter, so the collection of putative counterexamples is a class not a set and might not have a least element. So we need $\langle X, <\rangle$ to be a class-wellordering. If we make this assumption then we can prove that $f = \sigma \restriction X$, and f of course is a set, so $\sigma \restriction X$ will be too.

■

But that's presumably not enuff to imply setlike. However it does enable us to show the following:

Remark 24 *If σ is 2-setlike then, for all k, $j^k\sigma(x)$ is a set as long as one of x or $V \setminus x$ is a subset of $\mathcal{P}^k(X)$ for some X that admits a class-wellordering (every nonempty subclass has a least member).*

Proof:
If $\sigma \restriction A$ exists then $(j\sigma) \restriction \mathcal{P}(A)$ exists too. Repeat k times. And $j^k\sigma(x) = V \setminus j^k\sigma(V \setminus x)$

■

We should be careful what we wish for, however, since we cannot expect σ to be a set. After all, σ cld be an external automorphism — but it would be nice if one could infer that is is 3-setlike.

4.5 Automorphisms

Being an automorphism is weakly stratified.

I still do not know how to show that any model of NF has a permutation model with no internal automorphism;

I still do not know how to show that any model of NF has a permutation model with a proper class of internal automorphisms.

At some point we are going to have to start thinking about $\in$-automorphisms of di Giorgi models. Automorphisms both internal and external. Do V and V^σ have the same external automorphism group when σ is a set of V? Or when σ is setlike? Remark 16 is surely relevant here.

Think about the subgroup of Symm(A) consisting of those permutations of A that $\mathfrak{M}$ thinks are $\in$-automorphisms; in fact do this for all permutation models $\mathfrak{N}$. There is no reason to suppose that these groups are all the same group. But they might be conjugate copies.

$$\begin{tikzcd} A \arrow[r, "\mathfrak{i}_1"] \arrow[d, leftrightarrow, "\pi"'] & \mathcal{P}(A) \arrow[d, leftrightarrow, "j(\pi)"] \\ A \arrow[r, "\mathfrak{i}_2"'] & \mathcal{P}(A) \end{tikzcd}$$

If this diagram commutes we have

$$\begin{array}{ll} \mathfrak{i}_2(y) = \pi``(\mathfrak{i}_1(\pi(y))) & \text{which gives} \\ (\forall x)(x \in \mathfrak{i}_2(y) \longleftrightarrow x \in \pi``(\mathfrak{i}_1(\pi(y)))) & \text{which gives} \\ (\forall x)(x \in \mathfrak{i}_2(y) \longleftrightarrow \pi(x) \in \mathfrak{i}_1(\pi(y))) & \text{(with a few -1 exponents thrown in)} \end{array}$$

making π an isomorphism between the two models arising from $\mathfrak{i}_1$ and from $\mathfrak{i}_2$. That's fine, but we are interested in weaker conditions on the diagram.

There are further families of subgroups of $\mathfrak{Setlike}$ and $\mathfrak{Internal}$ that will excite our interest.

Any model $\mathfrak{M}^s$ in the extended family obtained from $\mathfrak{i}$ and a permutation $s \in \mathfrak{Setlike}$ may or may not admit ($\in$)-automorphisms. These automorphisms may or may not be sets of $\mathfrak{M}^s$. And they may or may not be definable. Observe however that any $\in$-automorphism of any structure $\mathfrak{M}^s$ whatever is perforce setlike.

Thus we are led to consider, for each $s \in \mathfrak{Setlike}$,

(i) The group of external $\in$-automorphisms of $\mathfrak{M}^s$;
(ii) The group of internal $\in$-automorphisms of $\mathfrak{M}^s$;
(iii) The group of definable internal $\in$-automorphisms of $\mathfrak{M}^s$;
(iv) The group of definable internal permutations of $\mathfrak{M}^s$.

(i)–(iii) may of course all be empty.

There is also, for each $s \in \mathfrak{Setlike}$, the collection of internal permutations of $\mathfrak{M}^s$ that are definable in $\mathfrak{M}^s$ ...in the sense of being fixed by all $\in$-automorphisms. This, too, is a perfectly respectable subgroup of $\mathfrak{Internal}$. This in turn has a subgroup consisting of those internal permutations that are fixed by all *internal* automorphisms.

It is natural to wonder whether or not we get analogues of lemma 3. Forget di Giorgi models for a moment. Symm(V) acts on itself simply in virtue of the target being a set, the same way it acts on V, as a permutation group. Of course it also acts on itself by conjugation. Interestingly its action on itself by conjugation is the same as the action of its subgroup j^n "Symm(V) on Symm(V), for some small n. This is another way of saying that, for some small n depending on how we implement permutations, $j^n t(\pi) = \pi^t$. It's an application of Coret's lemma. If we use Wiener-Kuratowski pairs then $n = 3$.

So: the thought is that when we *appear* to be applying permutations to permutations as in the discussion above (for which the substrates have to be objects of the model) me might in fact be merely conjugating them – in which case they don't have to be sets.

But somehow we have to think of the permutations not of the elements of the models but of the elements of A.

But of course once we do $(\sigma_{n+2})^{-1}$ to

$$\sigma_{n+1} \cdot \sigma^{-1} \cdot (\sigma_{n+1})^{-1}$$

to obtain

$$(\sigma_{n+2})^{-1}(((\sigma_{n+1}) \cdot \sigma^{-1}) \cdot (\sigma_{n+1})^{-1})$$

the result might not be a permutation! That doesn't matter, co's it's only meant to be a *sleeper* for a permutation: something that *becomes* a permutation in V^σ. (Aside: i think it's a corollary of a result in my monograph that every object is a sleeper for a permutation in *some* model or other. Something to do with Fine's principle.)

This proof seems to depend on the fact that the permutations we are using are sets of the models concerned. (It doesn't look as if they will morph into proofs that the relation that holds between a model $\mathfrak{M}$ of NF and another model $\mathfrak{N}$ of NF when $\mathfrak{N} = \mathfrak{M}^s$ for s a setlike permutation of $\mathfrak{M}$ is an equivalence relation.) This is because – on the face of it at least – some of the permutations appear as arguments to other permutations, and of course you can't be an argument to a permutation unless you are a set of the model. But is this really true? Are we really applying π to σ or are we in fact only commuting σ with $j^n\pi$ for some n?

By lemma 3, $\mathfrak{M}$ and $\mathfrak{M}^\sigma$ have the same internal permutations. By assumption σ is a set of $\mathfrak{M}$ so it is also a set of $\mathfrak{M}^\sigma$. The collection $\mathfrak{Internal}$ of internal permutations is of course a group and is closed under inverse, so $\mathfrak{M}^\sigma$ houses σ^{-1} as well. The claim is that this manifestation – in $\mathfrak{M}^\sigma$ – of σ^{-1} is the return permutation that we seek. The idea is that, if, according to our FFM, s (aka 'σ')

is a permutation of A and is encoded (in $\mathfrak{M}$) by some atom in $\mathfrak{M}$ then, in the permutation model $\mathfrak{M}^\sigma$, there will be an atom that encodes s^{-1}; $\mathfrak{M}^\sigma$ believes that atom to be a permutation, and another atom that encodes the converse of that permutation, and that second atom is believed by $\mathfrak{M}^\sigma$ to be the "return" permutation that we seek.

4.6 Setlike, internal, and now *definable*

Permutation models modulo setlike permutations have the same internal permutations but do they have the same *definable* permutations? We care greatly about definable permutations because we are looking for a theorem that says something along the lines of: if $\sigma \in \mathfrak{Internal}$ then $\mathrm{Th}(\mathfrak{M})$ and $\mathrm{Th}(\mathfrak{M}^\sigma)$ are synonymous. If we are to answer questions like this we need a robust concept of 'definable'. There are at least four candidate definitions.

(i) A set is definable iff it is the unique thing which is ϕ for some ϕ;
(ii) A set is definable iff it is fixed by all $\in$-automorphisms;
(iii) A set is definable iff it is fixed by all internal $\in$-automorphisms;
(iv) A set is definable iff it n-symmetric for some n.

I think (i) $\rightarrow$ (ii) $\rightarrow$ (iii) $\rightarrow$ (iv). The only complication is that we want ϕ to be stratifiable in (i).

All these definitions seem to allow for the possibility that everything should be definable. Certainly if there is no nonidentity internal $\in$-automorphism then everything is definable in sense (iii). Definition (i) invites to wonder about sets uniquely identified by *un*stratifiable formulæ.

It turns out that there are plenty of examples of internal permutations π that are definable in V but are not definable in V^π. This is slightly surprising but it is very important. Randall points out the simple case of adding a single Quine atom by means of the transposition $\tau = (\emptyset, \{\emptyset\})$. τ is of course definable – in all of our above senses. In V^τ the old empty set has become a Quine atom, which we can call 'q'. We then needed the transposition $(q, \emptyset)$ to get back to square one. This is certainly going to be definable (in sense (i) at any rate) as long as q is the sole Quine atom, but if there are lots of Quine atoms then there may be no way of identifying in V^τ that Quine atom that arose from the empty set of the model with which we started. Does it matter? Won't any old Quine atom do? Perhaps, but perhaps not: the Quine atoms might not be indiscernible. Of course this isn't a *proof* that τ is an example, but it is a straw in the wind.

Randall suggests another example: Henson's permutation $\chi = \prod_{\alpha \in NO} (T\alpha, \{\alpha\})$ that gives a proper class of Quine atoms might not be definable in the model it gives rise to – at least not in the sense of being the denotation of a closed set abstract. I think Holmes is correct. In fact χ is probably worth a brief digression ...

In Henson's model V^χ, $T``NO$ has become a set $\mathfrak{S}$ that is a set of of singletons, in fact of singletons^n for all concrete n. It even contains lots of Quine atoms – a proper class of them in fact – since every old cantorian ordinal has become a Quine atom in $\mathfrak{S}$. (Indeed that was the point Henson was making, that there can be a proper class of Quine atoms[3].) In fact it is a fixed point for $x \mapsto$ the set of subsets of x of-size-1-at-most. Since it contains a proper class of Quine atoms – and a lot of big ordinals – it's clearly not the least fixed point (tho' it might be the least fixed point present *in the model* in which case it would be definable in sense (i)). Nor can it be relied upon to be the greatest fixed point, since it won't contain any of the Quine atoms that may have been present in the original model. All such Quine atoms would remain Quine atoms in V^χ and would have to belong to the gfp. Even if it is the gfp or the lfp – and is therefore denoted by a closed term – that doesn't make it definable in the strong sense (iv) of being symmetric (indeed it is demonstrably *not* symmetric) tho' it will be fixed by every $\in$-automorphism, thereby revealing itself to be definable in senses (ii) and (iii). It is wellordered. (This is beco's V^χ believes $\mathfrak{S}$ is wordered iff V believes $\chi_n(T``NO)$ is wellordered.) Now h fixes NO (by construction of χ) and the *higher* lifts of χ preserve the property of being wellordered – beco's higher lifts always do.) The permutation χ exists in V^χ as well of course; what does it do? It remains an involution, and it bijects old ordinals (= [some] new singletons) with ... with what?

[thinking aloud] The "return" permutation has to kill off precisely those singletons that arose from the ordinals in $T``NO$. Is there a first-order way in V^χ of detecting those singletons? And, even if there is, how do we turn them back into ordinals? Looks a hopeless task.

That was suggestive, but it is not an unequivocal illustration. Nevertheless one is to be had. Recent work of Nathan Bowler's provides us with a pair of permutation models that do not have the same internal $\in$-automorphisms. This is a side-effect of a proof that every model of NF has a permutation model that contains in internal $\in$-automorphism. I wrote this up in stratificationmodn.pdf.

Must permutation models have the same internal $\in$-automorphisms?

Thanks to Nathan Bowler we know that every model of NF has a permutation model containing an (internal) $\in$-automorphism, but at this stage we do not know whether or not NF has any models *without* $\in$-automorphisms. What we do know is that if $\mathfrak{M} \models$ NF contains an (internal) $\in$-automorphism, then all its permutation models arising from *definable* internal permutations also contain an (internal) $\in$-automorphism. (I proved this decades ago and i have been waiting for it to come to life ever since.) Actually we can show slightly more. First some definitions. $C_{J_0}(J_n)$ is the centraliser of J_n (the group of all those

[3] A word is in order at this point. Every cantorian ordinal in V becomes a Quine atom in $\mathfrak{S}$. The collection of cantorian ordinals is a proper class, so the collection of Quine atoms in $\mathfrak{S}$ is a proper class. That doesn't by itself imply that the collection of Quine atoms is a proper class, but if it were a set, then its intersection with $\mathfrak{S}$ would be a set, and we have just shown that it is a proper class.

permutations that are j^n of something) in J_0, aka Symm(V), the full symmetric group on the universe; it is the group of those permutations of V that commute with everything in j^n"Symm(V); $C_{J_0}(J_n)$ contains *inter alia* all permutations definable by formulæ using only n levels. $Aut(V)$ is the group of all internal $\in$-automorphisms.

REMARK 25
For every $\mathfrak{M}$, for every concrete n, and for every setlike p that is in the centraliser of the group of permutations of the carrier set M of $\mathfrak{M}$ that $\mathfrak{M}$ believes to be j^n of something, $Aut(\mathfrak{M})$ is a subgroup of $Aut(\mathfrak{M}^p)$.

Proof:
Let s be an element of $\mathfrak{Internal}^{\mathfrak{M}}$ such that $\mathfrak{M}$ thinks that $s^{\mathfrak{M}}$ is an $\in$-automorphism. We write 'σ' for '$s^{\mathfrak{M}}$' to keep things readable. Then

$$\mathfrak{M} \models (j\sigma)^{-1} \cdot \sigma = \mathbb{1}.$$

Multiply both sides on the right by π

$$\mathfrak{M} \models (j\sigma)^{-1} \cdot \underline{\sigma \cdot \pi} = \pi.$$

If $\mathfrak{M}$ believes that π is definable-in-$\mathfrak{M}$ in the sense of commuting with all $\in$-automorphisms of $\mathfrak{M}$ we can swap the underlined bits to get

$$(j\sigma)^{-1} \cdot \pi \cdot \sigma = \pi.$$

which says that σ is an $\in$-automorphism of V^π.

The only conditions on π that we needed were that it should be internal and commute with any automorphism σ.

■

[in other words, for every internal σ satisfying a quite weak definability condition every internal automorphism of $\mathfrak{M}$ is also an internal automorphism of $\mathfrak{M}^\sigma$. Moral: you can't kill off internal automorphisms with definable permutations. So we shouldn't expect to find a definable internal permutation σ such that we can prove that $\mathfrak{M}^\sigma$ contains no internal automorphisms.]

4.7 Undoing Permutations October 2018

The definable permutation that becomes undefinable in the permutation model to which it gives rise is one that is provided by ideas of Nathan. Nathan's key idea is that of an **embedding of permutations**. We say that f is an embedding from a permutation π of a set X to a permutation σ of a set Y if f is an injection $X \hookrightarrow Y$ such that $f(\pi(x)) = \sigma(f(x))$. If there is such an f we say $\pi \leq \sigma$. In practice we will only be interested in the simple situation where $X = Y = V$ and indeed only in the case where all permutations concerned are involutions. A permutation π is a **universal** involution if $(\forall\sigma)(\sigma^2 = \mathbb{1} \to \sigma \leq \pi)$. We need a few key facts.

(i) There is a Cantor-Bernstein theorem for $\leq$, in the sense that $\sigma \leq \pi \leq \sigma$ implies that σ and π are conjugate;

(ii) $\pi \leq \sigma \to j(\pi) \leq j(\sigma)$

(iii) Nathan has a proof that $j(c)$ is a universal involution.

The instance of the Cantor-Bernstein–style theorem that we want is the one that says that, since $jc \leq j^2c$ and $j^2c \leq c$ then jc and j^2c are conjugate. Since we can exhibit definable injections in virtue of which $jc \leq j^2c$ and $j^2c \leq jc$ then we trade on the fact that the proof of the Cantor-Bernstein–style theorem is effective enuff for the permutation that conjugates jc and j^2c to be definable. It might be an idea to spell this out.

To keeps things short i am not planning to prove any of these, and i s'pose i should flag the possibility of a mistake even at this early stage. Anyway! putting these together we can argue that $j^2(c)$, too, is a universal involution, and that therefore $j(c)$ and $j^2(c)$ are conjugate. It's actually quite easy to show that $j(c)$ and $j^2(c)$ are conjugate if we are allowed to use AC for pairs; the key move is to do it without using AC. The Cantor-Bernstein–style theorem to which we appeal can be proved using Knaster-Tarski, which will tell us that, whenever $\sigma \leq \pi \leq \sigma$, then there is a permutation that conjugates them, and there is a complete lattice of such permutations, so there will be a bottom element of that lattice, and that bottom element will be definable – and definable by a stratifiable expression. We next combine this with the old fact that if π conjugates σ to $j(\sigma)$ then, in V^π, σ has become an $\in$-automorphism.

OK, so what we have so far is the fact (if i have got this right) that NF proves the existence of a definable permutation such that the induced permutation model contains an $\in$-automorphism.

We now need two more facts:

(iv) any (internal) permutation can be undone, and

(v) If V contains an $\in$-automorphism and π is a definable permutation then V^π contains an $\in$-automorphism.

There must surely be a proof of this somewhere...

H I A T U S

It might be worth thinking about what Nathan's automorphism actually does. What properties does it inherit from jc and j^2c? Does it – for example – commute with everything in J_3, as c, jc and j^2c all do? And another thing – let's call it 'ν' for <u>N</u>athan.

While we are about it we might as well give a proof in similar style of the fact that if $\mathfrak{M} \models$ NF, and $\mathfrak{M}$ has an internal permutation σ, and $\mathfrak{M}^\sigma$ has an internal permutation τ, then there is an internal permutation π in $\mathfrak{M}$ such that $\mathfrak{M}^\pi$ is isomorphic to the permutation model obtained from $\mathfrak{M}^\sigma$ by means of τ.

We also want the analogous result for setlike permutations: let $\mathfrak{M} \models$ NF, and let s be an $\mathfrak{M}$-setlike permutation, and t is an $\mathfrak{M}^s$-setlike permutation, then there is an $\mathfrak{M}$-setlike permutation p such that $\mathfrak{M}^p$ is isomorphic to the permutation model obtained from $\mathfrak{M}^s$ by means of t.

These should both be routine hand-calculations.

4.7.1 An email from Nathan wherein he explains 'return' permutations

with some interjection/comments from your humble correspondent.

"Hi Thomas,

To help to think about this a little, it is useful to have a point of view in which the collection of permutations we are considering is given independently of the permutation model we use$^{(1)}$. So, given a model $\mathfrak{M} = (V, \in)$ of NF, let's define $S_{\mathfrak{M}}$ to be the set of all (external) permutations p of V which are coded by some element $p^{\mathfrak{M}}$ of $\mathfrak{M}$. It is clear that $S_{\mathfrak{M}}$ is a group and that it is isomorphic to the group of things that $\mathfrak{M}$ believes to be permutations. It is also clear that if $\mathfrak{N}$ is a permutation model derived from $\mathfrak{M}$ then $S_{\mathfrak{N}} = S_{\mathfrak{M}}$.

Notes on the above

$V^\tau \models$ "s is a permutation" iff $V \models \tau_n(s)$ is a permutation for suitable small concrete n. So τ_n is a bijection between (those members of A that are) permutations in V^τ and (those members of A that are) permutations in V.

Can it really be that easy??

So it seems that all permutation models see the same group elements and they agree on group multiplication (and inverse?) No reason to suppose they agree on the second-order theory.

Is this true? "There is a first-order theory of a group, expanded to have a name for every element. In every permutation model the symmetric group on the universe is a model of this theory"

This much is clear: the stratified theory of Symm(V) is the same in all permutation models.

4.8 Permutations and Synonymy

Given that permutations can be undone, the moral of this seems to be that, whenever σ is an NF-definable permutation with the property that σ^{-1} is definable in $\mathfrak{M}^\sigma$, then Th($\mathfrak{M}$) and Th($\mathfrak{M}^\sigma$) are synonymous. Beco's of the Pétry-Henson-Forster lemma, which says that all unstratified formulæ can be tweaked by permutation models, this ought to mean something like: NF is synonymous with any unstratified extension of it. But of course that's not true beco's there are unstratified extensions (Axiom of counting) that prove Con(NF), so we haven't stated it properly. And there is also the point that, even among the formulæ whose truth-values can be changed by permutations, not all can be changed by definable permutations σ whose inverses remain definable in $\mathfrak{M}^\sigma$. But if we sort that out we will be in a position to prove that whenever ϕ is an unstratified formula of a special kind, then NF and NF+ ϕ are synonymous. And that, of course, will be music to my ears, since it is another riff on the theme that all of mathematics is stratified.

There is probably quite a lot to be said about how and when it can happen, for $\mathfrak{M} \models NF$, and π setlike, that $Th(\mathfrak{M})$ and $Th(\mathfrak{M}^\pi)$ come to be synonymous.

Here's a simple formulation (Mangere airport sunday 12/i/20) that, culpably, i have never found before. If σ is a definable permutation (in the strong sense of being captured by a homogeneous formula of $\mathcal{L}(\in, =)$) we have an interpretation from $\{\phi : \text{NF} \vdash \phi^\sigma\}$ into NF, but that gives us no guarantee that there will be an interpretation in the other direction, let alone an interpretation that is inverse to the first interpretation. One needs special conditions on σ. Of course if σ is a definable permutation of $\mathfrak{M}$ then there is an interpretation of $Th(\mathfrak{M})$ into $Th(\mathfrak{M}^\sigma)$. It is true that there is a permutation taking us back to $\mathfrak{M}$ but that permutation won't give rise to an interpretation unless it is definable. But if it *is* definable, are the two interpretations mutually inverse?

So, there is a question. Suppose σ is a definable permutation such that V^σ believes there is a definable permutation undoing σ, are $Th(V)$ and $Th(V^\sigma)$ synonymous?

And again. Let T be a sensible set theory all of whose axioms are stratifiable (any stratifiable extension of KF will do, i think). Express T in the language of set theory with an extra constant, c. Consider the two theories:

$T_1 = T + (\forall x)(x \notin c)$, and
$T_2 = T + (\forall x)(x \in c \longleftrightarrow x = c)$.

Remark 26
T_1 and T_2 are synonymous in the sense that any model of one can be turned into a model of the other, and the two transformations are mutually inverse.

Proof:
[tidy this up: Start with a model of T_1 and use the transposition $(c, \{c\})$ to obtain a model of T_2; start with a model of T_2 and use the transposition $(c, \emptyset)$ to obtain a model of T_1]

Fix a set M with a designated element c and a binary relation $\in_1$ such that $\langle M, c, \in_1\rangle \models T_1$. We interpret T_2 into T_1 by means of the transposition $t = (\emptyset, \{\emptyset\})$ as usual, so we have a binary relation $\in_2$ such that $\langle M, c, \in_2\rangle \models T_2$.

$$x \in_2 y \text{ iff } (y = c \wedge x = c). \vee .(y = \{c\} \wedge x \in_1 c). \vee .(y \neq c \wedge y \neq \{c\} \wedge x \in_1 y)$$

where the singletons are to be written out using $\in_1$. The definiens simplifies to

$$(y = c \wedge x = c). \vee .(y \neq c \wedge y \neq \{c\} \wedge x \in_1 y)$$

and then (using $y \neq \{c\}$ iff $(\exists z)(z \in_1 y \longleftrightarrow z \neq c)$) to

$$(y = c \wedge x = c) \vee (y \neq c \wedge (\exists z)(z \in_1 y \longleftrightarrow z \neq c) \wedge x \in_1 y)$$

and then to

$$x = y = c \lor (y \neq c \land (\exists z)(z \in_1 y \longleftrightarrow z \neq c) \land x \in_1 y)$$

Now we define $\in_1$ in terms of $\in_2$ by:

- If y is the Quine atom according to $\in_2$ then y was the empty set originally so $x \notin_1 y$;
- if y is empty according to $\in_2$ then it was the singleton of the empty set originally so $x \in y \longleftrightarrow x = c$;
- if y is neither the Quine atom nor empty then $x \in_1 y \longleftrightarrow x \in_2 y$.

Thus we get

$$\begin{aligned} x \in_1 y \text{ iff } & (\forall z)(z \in_2 y \longleftrightarrow z = y) \land \bot \\ & \lor (\forall z)(z \notin y) \land x = c \lor \\ & (\exists z)(z \notin_2 y \longleftrightarrow z = y) \land (\exists z)(z \in_2 y) \land x \in_2 y. \end{aligned}$$

which of course simplifies to

$$\begin{aligned} x \in_1 y \text{ iff } & (\forall z)(z \notin_2 y) \land x = c \lor \\ & (\exists z)(z \notin_2 y \longleftrightarrow z = y) \land (\exists z)(z \in_2 y) \land x \in_2 y \end{aligned}$$

or (on one line)

$$x \in_1 y \longleftrightarrow (\forall z)(z \notin_2 y) \land x = c) \lor ((\exists z)(z \notin_2 y \longleftrightarrow z = y) \land (\exists z)(z \in_2 y) \land x \in_2 y)$$

If we expand this definiens performing the substitutions

$x = y = c. \lor .(y \neq c \land (\exists w)(w \in_1 y \longleftrightarrow w \neq c) \land x \in_1 y \;/\; x \in_2 y$

and

$z = y = c. \lor .(y \neq c \land (\exists w)(w \in_1 y \longleftrightarrow w \neq c) \land z \in_1 y \;/\; z \in_2 y$

we obtain

$$((\forall z)(\neg(z = y = c.\lor.(y \neq c \land (\exists w)(w \in_1 y \longleftrightarrow w \neq c) \land z \in_1 y) \land x = c) \lor ((\exists z)(z = y = c.\lor.(y \neq c \land (\exists w)(w \in_1 y \longleftrightarrow w$$

Now simplify *that*! (to '$x \in_1 y$', one hopes)

Actually it might make for easier reading if we have TWO constant symbols in the language, 'c' and 'd'. In T_1 they are $\emptyset$ and $\{\emptyset\}$ respectively and in T_2 they are a Quine atom and the empty set respectively.

So instead we claim:

Let T be a sensible set theory all of whose axioms are stratifiable (any stratifiable extension of KF will do, i think). Express T in the language of set theory with two extra constants, c and d. Consider the two theories:

$T_1 = T + (\forall x)(x \notin c) \land d = \{c\}$, and
$T_2 = T + c = \{c\} \land (\forall x)(x \notin d)$.

Remark 27
T_1 and T_2 are synonymous in the sense that any model of one can be turned into a model of the other, and the two transformations are mutually inverse.

Then we define $\in_2$ in terms of $\in_1$ by

$x \in_2 y$ iff $(y = c \wedge x = c) \vee (d \neq y \neq c \wedge x \in_1 y)$

and we define $\in_1$ in terms of $\in_2$ by

$x \in_1 y$ iff $(y = d \wedge x = c) \vee (d \neq y \neq c \wedge x \in_2 y)$

Then we get (substituting the first into the second)

$x \in_1 y$ iff $(y = d \wedge x = c) \vee (d \neq y \neq c \wedge ((y = c \wedge x = c) \vee (d \neq y \neq c \wedge x \in_1 y)))$

Let's simplify this. Make the substitutions $p/(y = d)$; $q/(x = c)$; $r/(y = c)$ to make things readable.

$$(p \wedge q) \vee (\neg p \wedge \neg r \wedge ((r \wedge q) \vee (\neg p \wedge \neg r \wedge x \in_1 y)))$$

Let's put it into DNF.

We tackle the second disjunct and distribute $\neg p \wedge \neg r$ over $(r \wedge q) \vee (\neg p \wedge \neg r \wedge x \in_1 y)$

to obtain

$$(\neg p \wedge \neg r \wedge r \wedge q) \vee (\neg p \wedge \neg r \wedge \neg p \wedge \neg r \wedge x \in_1 y)$$

which is (restoring the disjunct $p \vee q$

$$(p \wedge q) \vee (\neg p \wedge \neg r \wedge x \in_1 y)$$

$$(p \wedge q) \vee (\neg p \wedge \neg r \wedge x \in_1 y)$$

which implies

$$q \vee (\neg r \wedge x \in_1 y)$$

some error in calculation there...

which does indeed simplify to $x \in_1 y$. (Given that $p \wedge r$ is not possible)

So i'd bet good money that it works for my original T_1 and T_2 as well.

Mind you, the difficulty of this calculation shows that it's not going to be easy demonstrating that theories arising from permutations are synonymous.

■

However i think the following is true:

Theorem 10
Suppose σ is a definable permutation, so that '$x \in \sigma(y)$' is NF-equivalent to a stratifiable formula with just the two free variables 'x' and 'y'.
Then $\{\phi : NF \vdash \phi^\sigma\}$ is interpretable into NF.

Suppose further that σ^{-1} is definable in V^σ (in the sense that '$x \in y$' is NF-equivalent to a stratifiable formula in $\mathcal{L}(=, \in_\sigma)$ with just the two free variables 'x' and 'y').
Then NF and $\{\phi : NF \vdash \phi^\sigma\}$ are synonymous.

The point is that if σ^{-1} is definable in V^σ then each of $\in$ and $\in_\sigma$ are definable in terms of the other, so that if you substitute the definition of $\in$ in terms of $\in_\sigma$ into the definition of $\in_\sigma$ in terms of $\in$ you get a tautology. (and the other way round).

4.9 Appendix 2

Such a permutation is, for each concrete n, j^n of something that is n-setlike. So it preseves the shape of the $\in$-diagram of the transitive closure of any of its arguments.

sse $f = jg$, and g is setlike. we want $f``x$. We can form $g``\bigcup x$ and thence $\mathcal{P}(g``\bigcup x)$, and we expect $f``x$ to be a subset of it.

But none of this helps.

4.10 A conversation with Nathan about an idea of Randall's

If π is a permutation we have a permutation model V^π. Can we get back from V^π to V? Yes, by means of the *return permutation*. Is the return permutation for π definable if π is? Not reliably so, it seems. This is a situation that i don't understand, and i would like to get a grip.

Randall's idea is that if we start with a model containing no Quine atoms and then consider the permutation $\rho = \prod_{n \in \mathbb{N}} (\{n\}, Tn)$ (assuming the axiom of counting to keep things simple – just call everyone *Bruce*) then we obtain a model containing countably many Quine atoms that cannot be told apart. (And of course in these circumstances one would expect the return permutation not to be definable). This sounds as if it – or something very like it – should be true. Nathan has an idea for showing that unless the model we start with has some special features then we *can* tell the atoms apart, and get a definable return permutation.

One's first thought is that in the original model there is a well-behaved definable (with a homogeneous formula, no less) binary relation $R(x, y)$ that can be used to distinguish the entities (natural numbers, as it happens) which are destined to be Quine atoms in the permutation model. Given R, we want there to be a formula S (not necc stratifiable) with two free variables such that $V^\rho \models S(x,y)$ iff $V \models R(x,y)$. Thus armed we will be able to distinguish the Quine atoms in V^ρ. Now $V^\rho \models S(x,y)$ iff $V \models S^\rho(x,y)$. So we want $S(x,y)$ to be a formula such that when we rewrite $\in$ within it to $\in_\rho$ and simplify as best we can, we get $R(x,y)$. This sounds like rather a lot to ask! This doesn't mean that the atoms are going to be indistinguishable of course, but it does mean that the fact that they can be distinguished in the original doesn't mean they can be distinguished in the new one.

Holmes' model is quite complicated. For one thing, his permutation ρ (ρ for ρandall) has infinite support. Let us start with some permutations of finite support.

Return permutations always definable with parameters.

Consider $(\emptyset, \{\emptyset\})$. The return permutation has a stratified definition with parameters, namely $(a, \emptyset)$ where a is the unique Quine atom. It is also definable without parameters beco's there is a unique Quine atom ("swap any Quine atom with the empty set") but this definition is unstratified.

(Notice that we have to be careful with definable permutations. We can't have $\phi(x,y)$ defining a permutation with ϕ stratified and the two vbls of different levels, beco's that would imply $|V| = T|V|$. No objection to $\phi(x,y)$ defining a permutation if ϕ is horrendously unstratified. "Swap every Quine atom with its complement and fix everything else" is such a permutation. But that's not much use beco's that permutation is not selike.)

There is discussion somewhere of the possibility of the existence of unstratified setlike permutations. Must find it!

I think it's a straightforward application of Coret's lemma to show that, in any model of NF, the Quine atoms are indiscernible for *stratified* formulæ. The proof is just like the same result for *urelemente* in NFU.

Consider $(\emptyset, \{\emptyset\})(V, \{V\})$. In the associated permutation model the return permutation is not definable without parameters by any *stratifiable* formula. The permutation model contains two Quine atoms but it has difficulty telling them apart. If we call them $\top$ and $\bot$ then we have two candidates for the return permutation: $(\top, \emptyset)(\bot, V)$ and $(\top, V)(\emptyset, \bot)$. One of them is the return permutation but we don't know which unless we can tell which of $\top$ and $\bot$ came from V and which from $\emptyset$. Both these permutations are definable-with-parameters using only stratifiable formulæ so certainly the return permutation is definable-with-parameters using only stratifiable formulæ but the model lacks the resources to prove that the return permutation is in fact the return permutation!

What happens if we get it wrong and use the wrong candidate? Don't we get back to the original model anyway? Possibly not; the first thought is that we get back to the model V^τ arising from the transposition $\tau = (V, \emptyset)$. Is V^τ isomorphic to V? It might be, but i can't see offhand how, and in any case the answer doesn't matter to us very much.

However there seems to be no way of defining the return permutation by stratified means in such a way that we can prove that it is the return permutation.

How might we be able to tell $\top$ and $\bot$ apart? Suppose there is $x = \{x, \bot\}$ but no $x = \{x, \top\}$. That would do it. Can we arrange for this to happen?

Yes! Use a permutation that swaps some x with $\{x, \emptyset\}$ but doesn't swap any x with $\{x, V\}$. Nathan's plan is to scale this up to a modified version of the V^ρ construction.

We start off in a model with no self-membered finite sets. We've known how to do this since the last century. Details will be supplied on demand.

Then, working in this model, we create, for each $n \in \mathbb{N}$, a triple $y = \{y, Tn, \{n\}\}$. We do this by fixing a counted set Y and, for each $n \in \mathbb{N}$, pairing off n with some $y \in Y$ and swapping y with $\{y, Tn, \{n\}\}$. Presumably we are going to need the axiom of counting but it doesn't matter beco's someone else is paying. The permutation model contains, for each n, a triple $y = \{y, Tn, \{n\}\}$ – a *plot point*. These plot points are the only self-membered triples and this will matter at the next stage.

Next we perform Randall's permutation $\rho = \prod_{i \in \mathbb{N}} (\{i\}, Ti)$. The plot points are not affected by ρ, so they are still there, and ready to do their dread work.

In this new model V^ρ we have countably many Quine atoms, each one arising from a natural number. We can now define the return permutation for ρ. Each Quine atom q belongs to a unique self-membered triple $y = \{y, q, x\}$ and the return permutation swaps q with this x. I don't think this definition of the return permutation is stratified but the point is made.

Using the same idea we can show how to distinguish some atoms from others by choosing to add "plot points" for some but not all natural numbers. In V^ρ we can tell the difference between Quine atoms that have plot points and those that don't. Thus they are not indistinguishable.

4.11 Blend this in

I have a permutation π in a model V. I want to satisfy myself that whenever σ is a permutation of V, then there is something in V^σ that in some sense *is* – or at least contains the same information as – π.

Clearly the starting point is Henson's lemma.

$$V^\sigma \models \pi(x) = y \qquad \text{iff} \qquad V \models (\sigma_{n+1}(\pi))(\sigma_n(x)) = \sigma_n(y)$$

This is on the assumption that we are using Quine pairs, so the type difference is 1.

First we conjugate by $(\sigma_n)^{-1}$ to tidy up the RHS:

$$V^\sigma \models \pi(x) = y \qquad \text{iff} \qquad V \models (\sigma_{n+1}(\pi))^{(\sigma_n)^{-1}}(x) = y$$

This is a start, but it's the wrong way round. It doesn't tell us what thing in V^σ contains the same information as π does in V but rather what thing in V contains the same information as $(\sigma_{n+1}(\pi))^{(\sigma_n)^{-1}}$ does in V^σ. What we have to do is express π in terms of $(\sigma_{n+1}(\pi))^{(\sigma_n)^{-1}}$.

For any π and σ, π contains the same information in V^σ as $(\sigma_{n+1}(\pi))^{(\sigma_n)^{-1}}$ does in V.

So substitute $(\sigma_{n+1})^{-1}(\pi)$ for π obtaining

For any π and σ, $(\sigma_{n+1})^{-1}(\pi)$ contains the same information in V^σ as $\pi^{(\sigma_n)^{-1}}$ does in V.
Next substitute π^{σ_n} for π obtaining

For any π and σ, $(\sigma_{n+1})^{-1}(\pi^{\sigma_n})$ contains the same information in V^σ as π does in V.

In particular, when $\sigma = \pi$ we get

For any σ, $(\sigma_{n+1})^{-1}(\sigma^{\sigma_n})$ contains the same information in V^σ as σ does in V.

So if we do the same thing starting from $\sigma^{-1} \in V$ we get the following expression for the return permutation

$$(\sigma_{n+1})^{-1}((\sigma^{-1})^{\sigma_n}).$$

I am happy to have got this far and will stop here for the moment. One should note however that this expression probably simplifies significantly. $(\sigma_{n+1})^{-1}(x)$ expands to $\sigma \cdot j\sigma \cdot j^2\sigma \cdots j^{n+1}\sigma(x)$. Now when x is a permutation $j^{n+1}\sigma(x)$ becomes something like x^σ. I'd have to check the exponent but it's all to do with conjugation. It might all simplify to something sensible. Like $\sigma(\sigma)$ even …(!) It can't be *that* simple beco's that will be definable if σ is.

Assume Ore's principle (i think that's what it's called) that says that in a (full) symmetric group any two permutations of the same cycle type are conjugate. It's not hard to cook up a permutation σ of infinite order such that σ and $j\sigma$ have the same cycle type. So they must be conjugate; so there is a permutation model containing an $\in$-automorphism of infinite order. The thought now is that if there is a permutation of infinite order then the axiom of counting holds. But the axiom of counting is invariant, so it was true in the home model. So the axiom of counting follows from Ore's principle. But Ore's principle is stratified and we know that the axiom of counting is not a theorem of any consistent stratified extension of NF. So NF refutes Ore's principle.

But i think the existence of an $\in$-automorphism of infinite order isn't enuff to imply the axiom of counting.

Later. If σ is a permutation of infinite order then it has cycles of all finite order. But surely every finite cycle of an $\in$-automorphism is cantorian? Perhaps not…. Let σ be an automorphism of infinite order, and let n be a natural number. Then there is a σ-cycle x of length (size) n. But then $\iota``x$ is a $j\sigma$-cycle of size Tn.

But it's true!

If τ is an automorphism then every τ-cycle is cantorian. Fix x; then $(\forall n \in \mathbb{N})((j\tau)^{Tn}(x) = j(\tau^n)(x))$. Now $\tau = j\tau$ whence $(\forall n \in \mathbb{N})(\tau^{Tn}(x) = \tau^n(x))$. So, when n is the order of the τ-cycle containing x, $\tau^n(x) = x$. So $\tau^{Tn}(x) = x$. So $Tn = n$.

> *So "τ is a preautomorphism iff τ is conjugate to something in J_1" holds only if the Axiom of Counting holds.*
>
> *It also means that if there is an automorphism of infinite order then the Axiom of Counting follows.*

Let's flesh out this last remark. If σ is of infinite order then $j\sigma$ has cycles of all finite sizes. If $\sigma = j\sigma$ then all σ-cycles are cantorian. So every natural number is cantorian.

If there is a permutation of infinite order and the collection of automorphisms is a set then AxCount holds. I think it's now clear that every model of NF has a permutation model containing an automorphism of infinite order. If there is an automorphism τ of infinite order and a set A containing precisely the $\in$-automorphisms then we can prove by induction on n that every $\tau^n \in A$, so A is infinite. But A is stcan whatever happens, so AxCount would follow. So if we start in a model wheere AxCount fails and add an $\in$-automorphism of infinite order by a Rieger-Bernays construction then in that model he collection of $\in$-automorphisms isn't a set. So NF cannot prove that the collection of $\in$-automorphisms is a set.

4.11.1 "Undoing" Permutations

Any permutation can be "undone" in the sense that

$$(\forall\sigma\exists\tau)(\forall xy)((V^\sigma \models x \in \tau(y)) \longleftrightarrow x \in y)$$

Actually this has been known for years in the form of $p \to \Box\Diamond p$ being a theorem of the modal logic where $\Diamond p$ means "there is a permutation model in which p". However the above formulation is sharper.

We will process this expression into a form where it becomes clear that it is a theorem of NF.

Expand the V^σ stuff and exploit the fact that
$x \in y$ is the same as $\sigma_n(x) \in \sigma_{n+1} \cdot (\sigma^{-1}(y))$: Duplicates stuff on page **??**

$$(\forall\sigma\exists\tau)(\forall xy)(\sigma_n(x) \ \in \ (\sigma_{n+2}(\tau) \cdot \sigma_{n+1})(y)) \longleftrightarrow \sigma_n(x) \ \in \ (\sigma_{n+1} \cdot \sigma^{-1})(y))$$

Reletter '$\sigma_n(x)$' as 'x' to get

$$(\forall\sigma\exists\tau)(\forall xy)((x \in \sigma_{n+2}(\tau) \cdot \sigma_{n+1}(y)) \longleftrightarrow x \in \sigma_{n+1} \cdot \sigma^{-1}(y))$$

Reletter '$\sigma_{n+2}(\tau)$' as 'τ' to get

$$(\forall\sigma\exists\tau)(\forall xy)((x \in \tau \cdot \sigma_{n+1}(y)) \longleftrightarrow x \in \sigma_{n+1} \cdot \sigma^{-1}(y))$$

and invoke extensionality

$$(\forall\sigma\exists\tau)(\tau \cdot \sigma_{n+1} = \sigma_{n+1} \cdot \sigma^{-1})$$

But this is easy!

$$\tau = \sigma_{n+1} \cdot (\sigma^{-1}) \cdot (\sigma_{n+1})^{-1} \tag{4.1}$$

Is this really as straightforward as it seems? The object τ that we get is certainly believed by V^σ to be a permutation, which is a start .... Notice also that τ (thinking this time in V not in V^σ) is conjugate to σ^{-1}. This means that if σ is a transposition (and we of course start off by considering transpositions) then τ is also a transposition. That keeps things very simple.

Let's take a very (*very*!) simple case: the involution $\tau = (\emptyset, \{\emptyset\})$ that adds a Quine atom. In V^τ the empty set has become a Quine atom and $\{\emptyset\}$ has become the empty set. While we are about it, let's observe that $\{\{\emptyset\}\}$ has become the singleton of the empty set, and $\iota^{n+1}(\emptyset)$ has become the $\iota^n(\emptyset)$ of V^τ. What has τ become? By which we mean: "what object in V^τ contains the same information as ("is"!) τ? τ is an unordered pair and unordered pairs are not moved, so we are looking for an unordered pair. The obvious suspect is the unordered pair of the two things that $\{\emptyset\}$ and $\emptyset$ have become, namely the empty set and the Quine atom. This looks Extremely Promising. Now let's imagine ourselves in V^τ and perform this permutation. Are we back in the model we started with? That would be nice. I sense some hand-calculation coming up. Let us write '$\in$', '$\in_\tau$' and '$\in'$' for the three membership relations, and reserve the constant symbol 'q' for the Quine atom of V. We want $\in$ and $\in'$ to be the same.

We have

$$x \in_\tau y \longleftrightarrow \bigvee \left\{ \begin{array}{l} y = \emptyset \wedge x = y \\ \emptyset \neq y \neq \{\emptyset\} \wedge x \in y \end{array} \right\} \tag{4.2}$$

You might think that there is a clause missing, concerning the case where $y = \{\emptyset\}$ but in that case $x \in_\tau y$ never holds.

[We might need subsequently to expand some of this further: '$y = \emptyset$' is of course '$(\forall z)(z \notin y)$', but sufficient unto the day is the weevil thereof.]

$$x \in' y \longleftrightarrow \bigvee \left\{ \begin{array}{l} (\forall z)(z \notin_\tau y) \wedge x = q \\ q \neq y \neq \emptyset^\tau \wedge x \in_\tau y \end{array} \right\} \tag{4.3}$$

Now we want to do some substitutions .... But what becomes of the occurrences of 'q'?

Let's substitute for the occurrences of '$\in_\tau$' in the definition of $x \in' y$. There are two disjuncts. We want each disjunct to either disappear or turn into $x \in y$.

(i) $(\forall z)(z \notin_\tau y) \wedge x = q$.

When is $(\forall z)(z \notin_\tau y)$ true? When y is $\{\emptyset\}$ in the sense of the original model. And if $x = q$ then x is the empty set of the original model, so $x \in y$ as desired.

(ii) $q \neq y \neq \emptyset^\tau \wedge x \in_\tau y$.

If y is distinct from both q and $\emptyset^\tau$ then it is not moved by τ and $\in y$ is the same as $\in_\tau y$.

Randall does not seem suitably appreciative!

4.11.2 Does every model of NF have a permutation model containing the Zermelo Naturals?

Let's see...

Consider the permutation π that swaps 0 with $\emptyset$ (0 is zero and $\emptyset$ is the empty set) and thereafter swaps $Tn+1$ with $\{n\}$. The idea is that in V^π the old $\mathbb{N}$ has become the Zermelo naturals.

Reasoning in V^π we establish that $\mathbb{N}$ contains the empty set (the empty set of V^π is the old 0, which is $\mathbb{N}$ as desired) and is closed under ι.

The hard part is to show that $\mathbb{N}$ is the *least* member of V^π which contains 0 and is closed under ι. So let A be a set that V^π believes contains the empty set and is closed under ι. The idea is to prove by induction on $\mathbb{N}$ (in the original model) that every (old) natural number is in$^\pi$ A. No problem with zero aka the empty set. So suppose A contains a natural number n. We want it to contain $n+1$. Does it? One thing we know that it contains is the thing it believes to be the singleton of n, which is $Tn+1$. This is not what we want. We can probably show that A is downward closed, so if $n+1 \leq Tn+1$ we'd be OK. But that is a strong assumption.

In fact one can say a bit more. If $n > Tn$ then, in V^π the natural numbers below n form a set that contains 0 and is closed under ι.

So the answer to the question seems to be 'No'! Every model of NF contains a permutation that gives a permutation model in which there is a set that contains $\emptyset$ and is closed under ι but there won't reliably be a least such, which is what the Zermelo naturals would be.

Chiz chiz

I had the thought that the correct object of study is not the group of automorphisms but the group of *preautomorphisms* – permutations s.t. π and $j\pi$ are conjugate. The trouble is, this collection doesn't seem to be a group. It's closed under inverse, and is a union of conjugacy classes, but i see no reason why it should be closed under composition. I think that the preautomorphisms generate the whole of Symm(V).

But actually we know all about preautomorphisms, at least if we have GC, co's we can characterise them by their cycle types. In fact i think (assuming GC) that you are a preautomorphism iff you are conjugate to a member of J_2. Now THAT is a stratified property, whereas being a preautomorphism (at least *prima facie*) isn't. Thus GC (which is stratified) implies an unstratified existence axiom. Nothing obviously impossible about that, but it is a bit odd. Oh! Careful. Being conjugate to a member of J_2 isn't sufficient. Consider a group with $T|V|$-many p cycles and $T|V|$-many fixed points where p is a noncantrian prime. It's conjugate to something in J_2 but it's not a pre-automorphism. You need the axiom of Counting.

Incidentally this points up the possibility of an automorphism of order p where p is a cantorian prime with a noncantorian natural below it. Messy!

Chapter 5

Quine Pairs and Sequences

Cardinals of large finite rank satisfy ever-strengthening identities like $\alpha = \alpha + 1$, $\alpha = \alpha + \alpha$, $\alpha = \alpha \cdot \alpha$, and so on. Each such equation is telling you that if A is a set such that $|A| = \alpha$ then A is the same size as some cardinal ideal in $\mathcal{P}(A)$. This should be made precise.

Adam points out that the stream corresponding to a finite set (thought of as a stream, as `head`::`tail`) is eventually constant, and indeed eventually constantly the empty set!

If you decode $\{\mathbb{N}\}$ as a k-tuple you get $\langle \emptyset\emptyset \cdots \{\mathbb{N}\}\rangle$. So if you decode it as an infinite stream you get the stream of empty sets! But you get that also by decoding $\emptyset$.

Must write out a proof!!

Some material from November 2016

Which i had entirely forgotten about until Adam reminded me!

> This topic has for years been anchored at the bottom of my list of things-to-look-into-one-day, and I'm grateful to Adam for making me think about it now. Not before time(!) And timeliness trumps[1] content, so i shall be brief.

Quine has two "theta" functions which he uses to define a type-level pair.

$$\theta_0(x) = (x \setminus \mathbb{N}) \cup \{n+1 : n \in \mathbb{N} \cap x\}$$
$$\theta_1(x) = \theta_0(x) \cup \{0\}.$$

The type-level Quine pair $\langle x, y\rangle$ is now $\theta_0 “x \cup \theta_1 “y$.

We generalise Quine's θ functions ...

$$\theta(\alpha, x) = (x \setminus NO) \cup \{\alpha + 1 + \beta : \beta \in x\} \cup \{\beta : \beta < \alpha\}$$

[1] A little *timely* ha! joke there...

Observe that, in this definition, the two arguments are of different types: 'x' is one type higher than 'α'

The intention behind this definition is that we should be able to use it to define in a type-level way not just ordered *pairs* but sequences of arbitrary ordinal length. If these new theta functions are to serve their purpose they had better be injective. This could mean one of two things:

(i) If i am given y and α, can i recover x such that $\theta(\alpha, x) = y$?

(ii) If i am given y, can i recover x and α such that $\theta(\alpha, x) = y$?

Pro tem. let us write 'θ_α' for $\lambda x.\theta(\alpha, x)$. (I don't like the subscript notation, for reasons that i may or may not get round to explaining).

Suppose y is a value of θ_α. It has members that are not ordinals: we leave them alone. What can we say about those members γ of y that are ordinals? We can say at least that $\gamma \neq \alpha$, and that α is the least ordinal not in y. This means that if i am given y i can detect whether it is a value of θ. Unless $NO \subseteq y$ [and we will return to this later] there will be some ordinals not in y. If α is the least such then we know $y = \theta(\alpha, x)$ for some x. Can we recover x? Yes. Given any ordinal $\gamma \in y$, we know that $\gamma - (\alpha + 1)$ must have been in x ... beco's if $\gamma - (\alpha + 1)$ was in x then (by definition of θ) we put $\alpha + 1 + \gamma - (\alpha + 1)$, and this is precisely γ – by uniqueness of ordinal subtraction. So if $y = \theta(\alpha, x)$ then we can recover x as $(y \setminus NO) \cup \{\gamma - (\alpha + 1) : \alpha < \gamma \in y\}$.

So does this mean we can, in a type-level way, and for an arbitrary set X, encode sequences from X of arbitrary length? Obviously this theta function was set up with this in mind. Suppose we have a set S such that, for each α, θ_α^{-1}"$S \in X$, then clearly S encodes an X-sequence of length ... Ω_1. This is because Ω_1 is the length of the ordinals as a wellordered set. Of course there are wellorderings longer than the ordinals, so we can't encode sequences of *arbitrary* length. Frankly i am quite surprised that we can do this for quite large – indeed *noncantorian* – α, and that the endeavour doesn't crash at ω_ω – or even earlier for that matter. It's worth noting that the formula $(\forall \alpha)(\theta_\alpha^{-1}$"$S \in X)$ – saying that S is such a sequence – is not merely stratified but 'S' has type one higher than 'X' ... as one would expect: sequence from X are the same type as X itself.

So if y is $\theta(\alpha, x)$ one can recover α and x. Annoyingly not every y is of the form $\theta(\alpha, x)$. I think i am correct in saying that the y that are not of this form are precisely the supersets of NO, and it is easy to check that there are $|V|$ of them. Does this matter? I'm not sure.

We defined the new suite of theta functions using ordinals. But of course any wellordering whatever will do. And (tho' we tend not to harp on the fact) there are wellorderings longer than $\langle NO, <_{NO} \rangle$. I think the conclusion is that, for any wellordering whatever, we can set up a system of Quine-style theta functions that will enable us to define sets of sequences indexed by that wellordering. It's natural to wonder about senses in which these tupling systems *cohere*. There can't be any of course, but the failure might be illuminating.

Chapter 6

Ultrafilters

How many are there? The usual argument that there are 2^{2^κ} ultrafilters on a set of size κ works in NF + BPI but it shows only that there are $T^2|V|$ ultrafilters and that is surely not best possible.

THEOREM 11 *If there is a uniform ultrafilter on V then $|V|$ is indecomposible: $|V|$ is not the sum of two smaller cardinals.*

Proof:

This is Nathan's remark, tidying up a vastly complicated line of talk from me! If V is split into A and B any uniform ultrafilter must contain precisely one of them, so that one is of size $|V|$. Duh.

■

Here's a thought. If $f : A \twoheadrightarrow B$ and $\mathcal{U}$ is an ultrafilter of A then $\{f``a : a \in \mathcal{U}\}$ is an ultrafilter on $B$. So our question can be rephrased as: define $f \sim g$ for $f : V \twoheadrightarrow V$ and $g : V \twoheadrightarrow V$ by $(\forall x)(f^{-1}``x \in \mathcal{U} \longleftrightarrow g^{-1}``x \in \mathcal{U}\}$. We know there are $|V|$-many such f and g, but how many equivalence classes are there?

$(\forall x)((f^{-1}``x \texttt{ XOR } g^{-1}``x) \notin \mathcal{U})$.

Write $\mathfrak{U}$ for the set of all ultrafilters.

If we have BPI then, for any $x \subseteq \mathfrak{U}$, the set $x \cup \{V \setminus y : y \in \mathfrak{U} \setminus x\}$ has the fip and can be extended to an ultrafilter. If we can extend it in a canonical way (a chance would be a fine thing!) then we would have a surjection $\mathfrak{U} \twoheadrightarrow \mathcal{P}(\mathfrak{U})$. But it's not a lot to show for such a strong assumption.

Key factoids:

Any two moieties are 1-equivalent. So if one belongs to a nonprincipal ultrafilter they all do. Two moieties with finite intersection cannot belong to the same nonprincipal ultrafilter. There are $|V|$-many moieties. So we want to find a MAD family M of moieties, with $|M| = |V|$.

Once we have done that, we reason as follows. Send each ultrafilter $\mathcal{U}$ to $\mathcal{U} \cap M$ – which is a singleton. This gives a surjection from βV (or at least its nonprincipal part) onto ι“M.

What this shows is that $|\beta V| \geq^* |\iota$“$M|$ for any AD family M of moieties. It's never going to show that $|\beta V| = |V|$.

We can also use Bowler-Forster, which sez that every orbit is either a singleton or is huge. The 2-orbit of a nonprincipal uf consists entirely of nonprincipal ufs, so there are lots of them. We need to check the precise statement of Bowler-Forster which will tell us how big the orbit is. I fear it might not be any better than the $|\iota^2$“$V|$ that we alredy knew ...

Michael Beeson directs my attention to a binary operation + that Rosser (Logic for Mathematicians, 1st edn p 275) defines on sets:

$$x + y = \{a \cup b : a \cap b = \emptyset \wedge a \in x \wedge b \in y\}.$$

Its restriction to equinumerosity classes coincides exactly with + on those things if we think of them as cardinals. Dead cute.

I noticed also that if $\mathcal{U}$ and $\mathcal{V}$ are (distinct!) ultrafilters on a fixed set then $\mathcal{U} + \mathcal{V}$ is another ultrafilter, as follows.

(i) $\mathcal{U} + \mathcal{V}$ is closed under superset. Anything in $\mathcal{U} + \mathcal{V}$ is a set of the form $a \cup b$ with $a \in \mathcal{U}$ and $b \in V$ and $a \cap b = \emptyset$. Any superset $A \cup B$ with $a \subseteq A$ and $b \subseteq B$ and $A \cap B = \emptyset$ is also in $\mathcal{U} + \mathcal{V}$ since both $\mathcal{U}$ and $\mathcal{V}$ are closed under superset.

(ii) Similarly $\mathcal{U} + \mathcal{V}$ is closed under binary $\cap$ since both U and $\mathcal{V}$ are closed under $\cap$.

I think that makes $\mathcal{U} + \mathcal{V}$ an ultrafilter ... as long as $\mathcal{U} \neq \mathcal{V}$. This condition is a bit odd.

6.1 Models in the Ultrafilters

I've tho'rt about this, on and off, over many years. Time to get it straight.

It's an old and very fertile observation of the late Maurice Boffa that principal ultrafilters preserve $\in$, in the sense that $x \in y \longleftrightarrow B(x) \in B(y)$ ... where we are writing '$B(x)$' (for obvious reasons) for $\{y : x \in y\}$.

This means that if we take the set B“V of all principal ultrafilters (and it *is* a set, according to NF) and equip it with $\in$ we obtain a model for NF that is an isomorphic copy of the model we are working in. Let us write this model with a fraktur 'B': $\mathfrak{B} = \langle B$“$V, \in \rangle$.

The obvious first thought is that one might add stuff to B“V, so that one considers B“$V \cup X$ for suitable X and hope to obtain thereby a new model. The thought may be obvious, but the line of enquiry that it suggests has never been pursued. It's high time to try it. One can start by minuting the following elementary – and rather discouraging – observation.

REMARK 28
If x is a new object adjoined to $\mathfrak{B}$, then it has the same (old) members as the (old) object $B(\{y : B(y) \in x\})$.

So if $\mathfrak{B}' \supset \mathfrak{B}$ is a model of extensionality then there are no $\in$-minimal new elements.

This means that (for example) we cannot add new sets of naturals, nor can we add new wellfounded sets. This is rather like the situation with ultrapowers[1]

Indeed $\mathfrak{B}$ is *complete* in the sense that if $X \subseteq B``V$ is a class of $\mathfrak{B}$ then it is already a set of $B$. It's coded in $\mathfrak{B}$ by $B(B^{-1}``X)$. If $B(z)$ is an element of $\mathfrak{B}$ with $B(z) \in X$ then $z \in B^{-1}``X$, whence $B(z) \in B(B^{-1}``X)$.

This doesn't mean that we can't extend $\mathfrak{B}$, but it does give us another way of saying that we cannot add any new subsets.

If we want the inclusion embedding into the new model to be nice then that places constraints on the objects we can add. The following fact tidies things up nicely.

REMARK 29 *An extension $\mathfrak{B}'$ of $\mathfrak{B}$ preserves the boolean operations iff everything in $\mathfrak{B}' \setminus \mathfrak{B}$ is a nonprincipal ultrafilter.*

Proof:

If the extension is to preserve the boolean operations, and $\emptyset$ and V, then $B(x)$ and $B(V \setminus x)$ (which are complements in $\mathfrak{B}$) will have to remain complements in $\mathfrak{B}'$. So, if y is a new element, we will have to insist on $y \in B(x) \longleftrightarrow y \notin B(V \setminus x)$, which is to say $x \in y \longleftrightarrow (V \setminus x) \notin y$. If $B(\emptyset)$ is to remain empty then we must have $y \notin B(\emptyset)$ so $\emptyset \notin y$. And these implications can clearly be reversed,

We want $\subseteq^{\mathfrak{B}'} = \subseteq^{\mathfrak{B}}$. Suppose $\mathfrak{B} \models B(a) \subseteq B(b)$. This is simply to say $a \subseteq b$. (It's **not** the same as $B(a) \subseteq B(b)$!) If $\mathfrak{B}' \models B(a) \subseteq B(b)$ is to be true then we have to have $y \in B(a) \to y \in B(b)$, which is $a \in y \to b \in y$. That is to say, $(\forall ab)(a \subseteq b \to a \in y \to b \in y)$. This is the final item in the criteria for y to be an ultrafilter.

■

In fact we can strengthen "preserves the Boolean operations" to "preserves all the NF0 operations". It is a simple matter to verify that if the only things we are adding are ultrafilters then singletons remain singletons and values of B remain values of B.

I don't know how restrictive that is, but it certainly concentrates the mind. And it directs our attention to the Prime Ideal Theorem. It invites us to think of any model of NF obtained in this way as a subset of the Stone-Čech compactification βV of V. If $\mathfrak{B}'$ is an extension of $\mathfrak{B}$ that preserves the Boolean algebra structure then the carrier set of $\mathfrak{B}'$ is a subset of βV. Is there a nice topological characterisation of those subsets of βV that are models of NF?

[1]Let $\mathfrak{M}$ be a model of set theory, and $\mathfrak{M}^\kappa/\mathcal{U}$ an ultrapower. For $f \in \mathfrak{M}^\kappa/\mathcal{U}$ consider $\{x \in M : \mathfrak{M}^\kappa/\mathcal{U} \models Kx \in f\}$. This is $\{x \in M : \{\alpha < \kappa : Kx\alpha \in f(\alpha)\} \in \mathcal{U}\}$ which is $\{x \in M : \{\alpha < \kappa : Kx\alpha \in f(\alpha)\} \in \mathcal{U}\} = \{x \in M : \{\alpha < \kappa : x \in f(\alpha)\} \in \mathcal{U}\} =??$, which is a member of $\mathfrak{M}$ he says hopefully. So if f is a set of old elements it is itself an old element.

Worth getting out of the way is the fact that $\langle \beta V, \in \rangle$ is not going to be a model of NF – in fact it's not even a model of extensionality ... at least not if we have BPI. Consider: βV has the finite intersection property and can be extended to an ultrafilter in lots of different ways. But any ultrafilter that extends βV will have to be the universal set of the model $\langle \beta V, \in \rangle$.

As part of the project to understand what the ultrafilters get up to it might be an idea to see if there is anything one can say about the theory of the model $\langle \beta V, \in \rangle$. Here the Prime ideal theorem comes in handy, because it enables one to find witnesses to comprehension axioms. For example, $\langle \beta V, \in \rangle \models (\forall x)(\exists y)(\forall z)(z \in y \longleftrightarrow x \in z)$. How so? Piece of cake. Let $\mathcal{U}$ be any ultrafilter at all, and consider $\{\mathcal{V} : \mathcal{U} \in \mathcal{V} \wedge (V \setminus \mathcal{U}) \notin \mathcal{V}\}$. This collection has the fip (check this!) and so can be extended to an ultrafilter, which is the witness to the '$\exists y$' that we need.

A more general question: which comprehension axioms does $\langle \beta V, \in \rangle$ satisfy? (It's never going to satisfy extensionality!) I'm guessing it's going to be a model of SF.

To prove this one would need the following:

> Let A be a set. Then $(\beta V \cap A) \cup \{V \setminus \mathcal{U} : \mathcal{U} \notin A\}$ has fip.
>
> Equivalently:
>
> Sse $A \subseteq \beta V$; then $(\exists \mathcal{U} \in \beta V)(\mathcal{U} \cap \beta V = A)$.

This sounds as if it ought to be true (and for a long time i thought it was true, and something like it may yet turn out to be true) and it would be a consequence of the assertion that the subalgebra of the boolean algebra $\mathcal{P}^2(X)$ generated by βX is free. Now this is simply *not* true (altho' the subalgebra of the boolean algebra $\mathcal{P}^2(X)$ generated by the *principal* ultrafilters in βX is free – that much is easy to prove). Here's why: let $\mathcal{U}_1$ and $\mathcal{U}_2$ be two ultrafilters. Extend $\mathcal{U}_1 \cap U_2$ to a third ultrafilter (using BPI) and take the complement, the ideal $\mathcal{I}$. Then the intersection of $\{\mathcal{U}_1, \mathcal{U}_2, \mathcal{I}\}$ is empty, contradicting freeness.

But i bet something *like* that is true, and will be enuff to show that $\langle \beta V, \in \rangle \models$ SF.

I asked Imre, and got this reply:

> Dear Thomas,
>
> Sorry, lost your email (and then found it again).
>
> Yes, the ultrafilters do generate a free BA.
>
> To show this, enough to show that any n of them generate a free BA. So let's take $\mathcal{U}_1, ..., \mathcal{U}_n$ as our ultrafilters, and form all 2^n 'atoms' from them, by which I mean things like $\mathcal{U}_2 1 \cap \mathcal{U}_2 \cap U_3^c$ etc.
>
> Claim: these are non-empty and disjoint.
>
> (Then we are done, as we thus get all 2^{2^n} unions being distinct.
>
> Disjoint is obvious, as any two differ in some place like: one uses U_3 and the other uses U_3^c.

> Non-empty: we just need to check that, given distinct ultrafilters $\mathcal{U}, \mathcal{V} \ldots$ there is a set that belongs to $\mathcal{U}$ and to $\mathcal{V}$ but not to $\mathcal{W}$ or to $\mathcal{X}$. (Can't be bothered to write it ot for general finite collections as bored of subscripts.)
>
> Choose a set A that belongs to $\mathcal{U}$ not $\mathcal{W}$ (possible as $\mathcal{U}$ and $\mathcal{W}$ are distinct) so they differ at some set A, (and if A is in $\mathcal{W} \setminus \mathcal{U}$ then we take A^c instead). And a set B that belongs to $\mathcal{U} \setminus \mathcal{X}$. Then $A \cap B$ belongs to $\mathcal{U}$ but to neither of $\mathcal{W}$ or $\mathcal{X}$. Do the same for $\mathcal{V}$, and take the union of the two resulting sets.
>
> Best wishes, Imre

Now that isn't true, or it resembles something false – i think i didn't ask Imre the right question.

Meanwhile let's try to prove something alone the lines that, for all $\mathcal{U} \in \beta V$, $\mathcal{U} \cap \beta V$ is open. Park for the moment the case where $\mathcal{U}$ has no members that are ultrafilters, and consider $\bigcap(\mathcal{U} \cap \beta V)$. This set is nonempty (it must contain V) and it must have fip since it is a subset of an ultrafilter. So it can be extended to an ultrafilter (using BPI) and in fact it can be extended to lots of them. Let $\mathcal{V}$ be such an ultrafilter. Since $\mathcal{U}$ is ultra, it must contain either $\mathcal{V}$ or $V \setminus \mathcal{V}$. It cannot contain $V \setminus \mathcal{V}$ – because that is disjoint from $\bigcap(\mathcal{U} \cap \beta V)$ – so it must contain $\mathcal{V}$.

That was nice, but what have we just proved..?

We want to show that, for every $\mathcal{V} \in \mathcal{U}$, there is a basis element containing $\mathcal{V}$ and included in $\mathcal{U}$.

(i) Recall the ways in which local versions of $B(x)$ turn up in the definition of supercompact cardinals.

(ii) Consider an extension of the model $\mathfrak{B}$. Some elements of $\mathfrak{B}$ acquire new elements ($V^{(\mathfrak{B})}$ for example) and some don't. (This reminds me of the genesis of a normal ultrafilter when you have an elementary embedding). Do the elements of $\mathfrak{B}$ that do not acquire any new members form an ideal? Presumably. Suppose $\mathcal{U} \in B(x \cup y)$. Then $x \cup y \in \mathcal{U}$, whence $x \in \mathcal{U} \vee y \in \mathcal{U}$ (since $\mathcal{U}$ is ultra) whence $\mathcal{U} \in B(x) \vee \mathcal{U} \in B(y)$, but of course we don't get the corresponding infinitary result. No old singleton can acquire new members: Suppose $\mathcal{U} \in B(\{x\})$. Then $\{x\} \in \mathcal{U}$ which means that $\mathcal{U}$ was not nonprincipal.

So: let $\mathfrak{B}'$ be an extension of $\mathfrak{B}$ obtained by adding nonprincipal ultrafilters. Each element of the extension defines an ultrafilter on $\mathfrak{B}$. Yes, but it's not informative. Chiz.

6.2 How Many Nonprincipal Ultrafilters?

Clearly there are precisely $T^2|V|$ principal ultrafilters, and precisely $T|V|$ principal filters, so at least $T|V|$ filters. How can there not be more ultrafilters?? A perfect tree has more branches than nodes, doesn't it? Doesn't it?! Particularly if we have BPI?!

But of course it's nonprincipal filters we care about. If there are any nonprincipal ultrafilters at all, then how many? Well, if $\mathcal{U}$ is a nonprincipal ultrafilter and π any permutation, then $(j^2\pi)(\mathcal{U})$ is a nonprincipal ultrafilter, but that only gives us $T^2|V|$ of them. One would have hoped for more than that. Of course, on the face of it there might be none, so let's assume BPI and see how far that gets us. Here is the outline of a project to prove that – assuming BPI – there cannot be $\leq^* T^2|V|$ nonprincipal ultrafilters.

First we define a MAD family. This is a **Maximal Almost Disjoint** family, a family of infinite co-infinite sets any two of which have finite intersection[2].

The idea is to start by showing that there is no surjection $\iota``V \twoheadrightarrow M$ for any MAD family $M$. This should be a nice diagonal argument, somehow. In fact it should be pretty general. $\iota``X$ "is the same type as" any MAD family M of subsets of X, and one would expect the cardinality of any MAD family to be of the order of $|\mathcal{P}(X)|$, so there should be a Cantor-like diagonal argument. I haven't found any such argument yet but i'm hopeful. The other ingredient that's missing is an argument to the effect that there should so much as be any MAD families in the first place.

> If we don't want to make the Zorn-like assumption that there is a MAD family we would have to show that $\iota``X$ doesn't map on to all almost-disjoint families of subsets of X . . . and that sounds very optimistic.

Next we prove that, for any almost disjoint family F, the surjection $\mathcal{U} \mapsto \mathcal{U} \cap F$ maps $\mathfrak{U}$, the set of nonprincipal ultrafilters, onto $\iota``F$. Well actually we want the function that sends $\mathcal{U}$ to $\mathcal{U} \cap M$ if $\mathcal{U} \cap M$ is nonempty (in which case it will be a singleton) and send it to some fixed arbitrary member of M otherwise. Better dot the i's and cross the t's.

First: $\mathcal{U} \cap F$ has at most one member. Since $\mathcal{U}$ is nonprincipal it cannot contain any finite set, so it cannot have two members with finite intersection, and so it cannot house two members of M.

Second: every $\{X\} \in \iota``F$ is $\mathcal{U} \cap F$ for some $\mathcal{U}$. This is easy: assuming BPI every moiety belongs to a $\mathcal{U}$. Notice that – on the face of it – $\mathcal{U} \cap F$ might be empty. This doesn't matter; what matters is that $\mathcal{U} \mapsto \mathcal{U} \cap F$ should be surjective to $\iota``M$. We picked some arbitrary $x \in F$ to be the destination of $\mathcal{U}$ whenever $\mathcal{U} \cap F$ is empty.

Then we will be able to argue that there can be no surjection $\iota^2``V \twoheadrightarrow \mathfrak{U}$, the set of nonprincipal ultrafilters, since – composing such a surjection with the surjection $\mathcal{U} \mapsto \mathcal{U} \cap M$ that maps $\mathfrak{U} \twoheadrightarrow \iota``M$ we would have a surjection $\iota^2``V \twoheadrightarrow \iota``M$ and thence a surjection $\iota``V \twoheadrightarrow M$.

It occurs to me that one might be able to refute BPI in NF. The strategy is roughly as follows. Assume BPI in the form that every family of sets with the finite intersection property (fip) can be extended to an ultrafilter (on V; we are considering the boolean algebra of the universe). Consider the family $A \cup \{A\}$ where A is the set of all prime ideals. If $A \cup \{A\}$ has fip then, by BPI, we can extend it to an ultrafilter $\mathcal{U}$. $\mathcal{U}$ contains all prime ideals and therefore no

[2]Thank you Nathan Bowler for finding a bug in my definition

ultrafilters and in particular it doesn't contain itself. But we also have $A \in \mathcal{U}$ by construction, and $\mathcal{U} \supset A$ and $\mathcal{U}$ is closed under $\supset$ so $\mathcal{U} \in \mathcal{U}$ after all.

So we have to show that $A \cup \{A\}$ has fip; we want:

"*every finite intersection of prime ideals contains a prime ideal*".

We are allowed to use BPI of course. If A were just the set of *principal* prime ideals it'd be a doddle. A finite intersection of *principal* prime ideals is the power set of some cofinite set and every cofinite set extends a [principal!] prime ideal (obvious, but prove it[3]); but a finite intersection of *nonprincipal* prime ideals is. . . ?

But that's not going to work. We can even build a *single* prime ideal that contains no prime ideals, never mind a finite family that share no prime ideal as member: just build an ultrafilter that contains all prime ideals. The collection of all prime ideals has the fip beco's every ideal contains $\emptyset$.

A pity: it seemed such a promising idea.

Beware! a maximal antichain in V/finite difference is NOT the same as a MAD family.

I'm still not giving up

Probably need to delete this, up to **

Start with the Stone space $\mathcal{S}_1$ of all ultrafilters on V. It's compact and Hausdorff. How big is it? Clearly at least $T^2|V|$, simply beco's of the principal ultrafilters. Is it going to be any bigger if we have BPI? I sense some calculations coming up. . .

Consider the partition $\mathbb{P} = \{\{x, V \setminus x\} : x \in V\}$ of all complementary pairs. An ultrafilter is a special kind of transversal for Π. Unfortunately there are $|V|$ transversals, as follows. We want to send each ultrafilter $\mathcal{U}$ to $\{p \in \Pi : \emptyset \in p \cap \mathcal{U}\}$, which is a subset of Π; $|\Pi| = T|V|$ and this map is type-raising so the argument has to be $\{\mathcal{U}\}$. But this tells us only that the set of ultrafilters injects into V, and that is hardly news. It might be that we can identify an ultrafilter with subsets of Π of some very special kind but i don't fancy our chances.

[Let X be any set: Does $B``X \cup \overline{B}``(V \setminus X) \cup FIN$ have the fip? If so, BPI will tell us that it can be extended to an ultrafilter. We inserted FIN to ensure that that ultrafilter would be nonprincipal.]

I'm starting to think that the number of ultrafilters on V is $T^2|V|$ come what may: BPI or no BPI.

Anyway, consider next the space $\mathcal{S}_2$ of all ultrafilters on $\mathcal{S}_1$. $\mathcal{S}_1$ is compact and Hausdorff so every uf in $\mathcal{S}_2$ has a unique point of convergence. This gives us a map $\mathcal{S}_2 \to \mathcal{S}_1$. Is it onto? I bet it is. Let $\mathcal{T}$ be a compact Hausdorff space, and $x \in \mathcal{T}$. Then x is a point of convergence of any ultrafilter generated by the

[3] $\mathcal{P}(x) \cap \mathcal{P}(y) = \mathcal{P}(x \cap y)$; a principal prime ideal is $\mathcal{P}(V \setminus \{x\})$ for some x, so a finite intersection of principal prime ideals is $\mathcal{P}(V \setminus x)$ for some finite x, and that is a set of cofinite sets. Given a cofinite set $V \setminus x$ we seek a such that $x \cap \{y : a \notin y\} = \emptyset$, and we can find such an a as long as $\bigcap x \neq V$.

set of open neighborhoods of x. But $\mathcal{S}_2$ is – literally – a subset of $\mathcal{S}_1$. Is the surjection cts? Yes, obviously.

Sse $f : \mathcal{P}(X) \to \beta X$. It would be nice to show that f is not onto. Consider $\{A \subseteq X : A \notin f(A)\}$. Does it have the fip? If it does, extend it to an ultrafilter $\mathcal{U}$. Suppose further that $\mathcal{U} = f(A)$ for some A. If $A \notin \mathcal{U} = f(A)$ then $A \in \mathcal{U}$ by construction of $\mathcal{U}$; so $A \in \mathcal{U}$... but there doesn't seem to be anything going in the other direction.

Perhaps we could do some tidying. Sse $f : \mathcal{P}(X) \to \beta X$ as before. Sse, for some $A \subseteq X$ we have both $A \notin f(A)$ and $(X \setminus A) \notin f(X \setminus A)$. Then modify f to f' that sends A to $f(X \setminus A)$ and sends $X \setminus A$ to $f(A)$. We now have $A \in f'(A)$ and $X \setminus A \ \in f'(X \setminus A)$. (Values of f are ultrafilters, remember). Do this simultaneously for all such A. Reletter 'f'' to 'f'. We now have that $A \notin f(A) \to (X \setminus A) \in f(X \setminus A)$.

Now fix $a \in X$. Consider those A such that $A \notin f(A)$ and $(X \setminus A) \in f(X \setminus A)$. If $a \in A$ leave f alone; if $a \in (X \setminus A)$ then swap the two values of f to get f'. We now have $A \in f'(A)$ iff $A \in f(X \setminus A)$. But $f(X \setminus A)$ is an ultrafilter that (by hypothesis) contains $X \setminus A$ ... and therefore does not contain A! We also want $(X \setminus A) \in f'(X \setminus A)$. This is the same as $(X \setminus A) \in f(A)$. But $A \notin f(A)$ and $f(A)$ is an ultrafilter so $(X \setminus A) \in f(A)$ as desired.

This ruse has ensured that we now have f' with the same range as f, and so f' is surjective iff f was. But now the subsets of X that are in $\{A \subseteq X : A \notin f'(A)\}$ all contain a, so $\{A \subseteq X : A \notin f'(A)\}$ has the fip! But it's still the case, as i observed three paras ago, that there doesn't seem to be anything one can do with $A \in \mathcal{U}$. So near and yet so far!

[Not sure that this helps, but...] It is certainly true that, for any finite set of nonprincipal ultrafilters, one can find a selection set whose values are pairwise almost disjoint. So by compactness (so BPI should do it) there is a choice function on βV whose range is an almost-disjoint family. But we don't really know how big this a-d family is.

We want to show that BPI implies that there are more than $T^2|V|$ ultrafilters. This is not a specifically NF style problem, so let's cast it in a form that people like Asaf might like.

> "Can we show, using only BPI, that $|\beta X| > |X|$?"

Since V is idemmultiple, let us allow ourselves the extra assumption that X is idemmultiple.

An almost-disjoint family of subsets of X ("AD family") is an antichain wrt the relation $A \subseteq_{<\aleph_0} B$ which says that $|A \setminus B| \in \mathbb{N}$.

We record the useful fact that – assuming BPI – we can map βX onto any almost-disjoint family D of subsets of X. Every infinite subset of X belongs to an nonprincipal ultrafilter on X. (BPI gives us this, beco's the cofinite filter $\cup$ any singleton of an infinite set has fip.) If A and B are almost disjoint ($A \setminus B$ and $B \setminus A$ both finite) then no nonprincipal uf can contain both of them. The map $\beta X \twoheadrightarrow D$ is obtained as follows. Distinguish one member d of D. Send all principal ultrafilters to d. For $\mathcal{U}$ a nonprincipal ultrafilter that meets D, send it to the unique member of $D \cap \mathcal{U}$. If it doesn't meet D, send it to d.

We want to prove $|X| \not\geq^* |\beta X|$. Since βX maps onto any AD family of subsets of X it will be sufficient to find even one AD family D of subsets of X such that X does not map onto D.

Exploit the fact that X is idemmultiple, using the fact that $\mathcal{P}(X \times X)$ has much more structure than $\mathcal{P}(X)$ and is the same size! Does the set of wellorderings of subsets of X constitute an AD family of subsets of X? There is a version of Hartogs' lemma that says $\aleph(|X|) \leq^* |\mathcal{P}(X \times X)|$, so can we find an AD family of size $\aleph(|X|)\ldots$? No; two wellorderings of subsets of X might have infinite intersection, but think of PERs! (Not equivalence relations beco's [the graphs of] any two equivalence relations have infinite intersection, namely $\mathbb{1} \restriction X$). There seems no reason to suppose that we can't have large ADs of subsets of $X \times X$ consisting entirely of PERs of X. But of course PERs correspond canonically to partitions. But this reminds us that if X is idemmultiple we can embed $\Pi(X)$ (the set of partitions of X) into $\mathcal{P}(X)$ and it should be easy to find large families F of partitions of X with the property that, for $p, p' \in F$, $p \wedge p'$ has only finite pieces.

But before we get onto partitions, a few comments about wellorderings. The reference to Hartogs' lemma is not as crazy as it sounds. Wellorderings are WQOs, and the intersection of (the graphs of) two WQOs on the one carrier set is another WQO on that carrier set, so the intersection of two wellorderings of an infinite set X is a WQO on an infinite set and must be infinite. So it's not going to work. But what if we think of wellorderings as ordernestings? Or perhaps we should be thinking of ordernestings of prewellorderings? Trouble is, the ordernestings are of higher type.

Let us say that a partition is *fine* iff all its pieces are finite.

$\mathbb{P}_1 \wedge \mathbb{P}_2$ is of course the partition $\{p_1 \cap p_2 : p_1 \in \mathbb{P}_1 \wedge p_2 \in \mathbb{P}_2\} \setminus \{\emptyset\}$.

Let us say that two partitions $\mathbb{P}_1$ and $\mathbb{P}_2$ are *(mutually) orthogonal* iff $\mathbb{P}_1 \wedge \mathbb{P}_2$ is fine.

We are looking for large families of pairwise mutually orthogonal partitions of X. Specifically we hope to find one that X cannot be mapped onto.

Let us write $\mathbb{P}_1 \otimes \mathbb{P}_2$ for $\{p_1 \times p_2 : p_1 \in \mathbb{P}_1 \wedge p_2 \in \mathbb{P}_2\}$. Thus, if $\mathbb{P}_1$ and $\mathbb{P}_2$ are partitions of X, $\mathbb{P}_1 \otimes \mathbb{P}_2$ is a partition of $X \times X$.

The plan is to exploit the fact that $|X|$ is idemmultiple.

Of course if $\mathbb{P}_1$, $\mathbb{P}_2$ are fine partitions of X then $\mathbb{P}_1 \otimes \mathbb{P}_2$ is a fine partition of $X \times X$.

Here's a thought that might lead somewhere. If $\mathbb{P}_1$ and $\mathbb{P}_2$ are fine partitions of X, then the two partitions $\mathbb{P}_1 \otimes \{X\}$ and $\{X\} \otimes \mathbb{P}_2$ are orthogonal partitions of $X \times X$.

Might we be able to show that a mutually orthogonal family can be closed under $\otimes$ and still be mutually orthogonal?

But maybe it's simplest to continue thinking in terms of $\mathcal{P}(X \times X)$ and AD families of PERs therein.

Eric Wofsey writes

Recall the usual proof of $|\beta X| = 2^{2^{2^{|X|}}}$ using AC. To sketch the argument, you replace X with the set Y of pairs $\langle A, S\rangle$ where A is a finite subset of X and S is a finite set of finite subsets of X. Then, you explicitly construct a family of $2^{2^{|X|}}$ pairwise incompatible filters on Y, and extend them each to ultrafilters. This gives $2^{2^{|X|}}$ different ultrafilters on Y, and hence on X since $|X| = |Y|$.

So, how much of this can we rescue assuming only BPI and $|X|^2 = |X|$? First, we still have $|X| = |Y|$. To prove this, note that we can totally order X by BPI, and so from $|X|^2 = |X|$ we can obtain a family of injections $[X]^n \to X$ for each finite n. Also, $|X|^2 = |X|$ implies $\aleph_0 \leq |X|$ so $|X| \cdot \aleph_0 \leq |X|^2 = |X|$. Thus $|X| \leq [X]^{<\omega} \leq |X| \cdot \aleph_0 = |X|$, and so $|Y| = |[X]^{<\omega}[[X]^{<\omega}]^{<\omega}| = |X|^2 = |X|$.

Now, using BPI, we can extend each of our filters on Y to an ultrafilter, but we can't necessarily do this for all of the filters simultaneously to get a family of $2^{2^{|X|}}$ ultrafilters on Y. However, we still do get a surjection $\beta Y \twoheadrightarrow \mathcal{P}(\mathcal{P}(X))$, by sending each ultrafilter to the unique filter in our family it contains (or to some constant value if it does not contain any of our filters). By Cantor's theorem this proves that $\mathcal{P}(X)$ does not surject onto βY, and so neither does X. Since we know that $|X| \leq |\beta Y| = |\beta X|$ via the principal ultrafilters, this shows that $|\beta X| > |X|$.

`https://math.stackexchange.com/questions/2999390/how-many-ultrafilters-there-are-in-an-infinite-space`

This might be a way in. A set X is *almost disjoint* iff $(\forall u, v \in X)(u \neq v \to |(u \cap v)| \in \mathbb{N})$. How big can such a set be? No nonprincipal uf can contain more than one member of such an X. So as long as every infinite set belongs to an ultrafilter the set of ultrafilters can be mapped onto ι“X.

This might be the thing that breaks the logjam! $\{V \times \{x\} : x \in V\}$. This is a family of pairwise disjoint sets of size $T|V|$. An ultrafilter can contain at most one of them. So send each ultrafilter to the one it contains. At best this maps the set of ultrafilters onto ι^2“V ... which isn't much of an inprovement.

And we'd need “every infinite set belongs to an ultrafilter” tho' that presumably follows from BPI.

6.3 Choice Functions on Ultrafilters on V

Noam made me think about ultrafilters, by making the point that you can use a nonprincipal ultrafilter on $\mathbb{N}$ to prove Ramsey's theorem. I immediately thought: consider the NF context, and ultrafilters on V.

Can there be a choice function on an ultrafilter? Or would that wellorder the universe? I think a choice function on a principal uf would wellorder V.

Remark 30 *An ultrafilter on X supports a choice function iff it contains a wellorderable set.*

Proof: L $\to$ R

Suppose f is a choice function on $\mathcal{U}$, we keep on picking members, taking intersections at limits, building a set W, until the intersection $X \setminus W$ is no longer in $\mathcal{U}$. (This W is a legitimate inductively defined set: we do not need ordinals). We might wellorder X by this process, in which case $\mathcal{U}$ contains a wellorderable set as desired. If not, then $X \setminus W$ has fallen out of $\mathcal{U}$. Then $(X \setminus W) \cup W \in \mathcal{U}$, so – by ultraness of $\mathcal{U}$ – we have $(X \setminus W) \in \mathcal{U}$ or $W \in \mathcal{U}$. By assumption we do not have the first, so we must have $W \in \mathcal{U}$. But W is wellorderable.

R $\rightarrow$ L

Suppose $W \in \mathcal{U}$ is wellorderable. Equip it with a wellordering. Everything in $\mathcal{U}$ meets W. So, for each $A \in \mathcal{U}$, pick the first element of $A \cap \mathcal{U}$.

■

We do need the ultra-ness condition: the Fréchet filter of cofinite sets clearly contains no wellorderable sets, but it has a choice function anyway. This is beco's of the more general observation that if $\langle X, <_X\rangle$ is a wellordering then there is a choice function on $Ƅ(X)$; from $A \in Ƅ(X)$ pick the $<_X$-first element of it. And every cofinite set meets $\mathbb{N}$.

6.3.1 The Rudin-Keisler Ordering

$\mathcal{U}_1 \leq_{RK} \mathcal{U}_2$ iff $(\exists f : X \rightarrow X)(\mathcal{U}_1 = \{f^{-1}\text{“}Y : Y \in \mathcal{U}_2\})$

The literature doesn't seem to require f to be either injective or surjective. However, we do assume that it is total, so that $f^{-1}\text{“}X = X$. It is alleged that, for all $f : X \rightarrow X$ and $\mathcal{U}$, $\{f^{-1}\text{“}Y : Y \in \mathcal{U})$ is an ultrafilter on X iff $\mathcal{U}$ is. And, yes, Ramsey ultrafilters are R-K minimal.

Is this anything to do with many-one reduction?

Corollary 4 *If $\mathcal{U}_1 \leq_{RK} \mathcal{U}_2$ and $\mathcal{U}_1$ supports a choice function then so does $\mathcal{U}_2$.*

Proof:

Suppose $\mathcal{U}_1 = \{f^{-1}\text{“}Y : Y \in \mathcal{U}_2\})$ and that there is a choice function on $\mathcal{U}_1$. Consider an arbitrary $Y \in \mathcal{U}_2$. What do we pick from it? Well, we can pick x from $f^{-1}\text{“}Y$ by assumption on $\mathcal{U}_1$, so $f(x)$ will be our pick from Y. So: if $\mathcal{U}_1 \leq_{RK} \mathcal{U}_2$ and $\mathcal{U}_1$ admits a choice function so does $\mathcal{U}_2$. Of course: a surjective image of a wellorderable set is wellorderable.

■

6.4 Ultrapowers of $\langle V, \in\rangle$ in NF(U)

Annoying observation of Randall's [25/vi/18]

Working in NF(U), consider ultrapowers of the universe. There is no reason to suppose that an ultrapower is a model for the existence of singletons. After all, what is the singleton of $[f]$ to be? It would have to be $[\iota \cdot f]$!.

But this can't be right. There is always Łoś's theorem . Isn't there? For $f : \mathbb{N} \rightarrow V$ we can always consider $\{\langle Tn, \{x\}\rangle : \langle n, x\rangle \in x\}$. This is a perfectly respectable function $\mathbb{N} \to V$. I suppose it all comes down to whether or not the ultrafilter is closed under jT.

This certainly needs to be tidied up.

6.5 Digression on nonprincipal ultrafilters

Might there be a symmetric nonprincipal ultrafilter on V? One's first tho'rt is: obviously not. However, the more i think about it the less chance i see of refuting it. An n-symmetric nonprincipal ultrafilter on V corresponds naturally to a non-principal ultrafilter on the set of all n-equivalence classes. Why should there not be such a thing?

There is a family of generalisations of the last section that i haven't proved or even formulated yet, but which we might need when tackling the question of whether or not there might be a symmetric nonprincipal ultrafilter on V. Let us say that $x \leq_{J_n} y$ iff x can be partitioned into finitely many pieces which, once translated by elements of J_n, give rise to some pieces of a partition of y. $x \sim_{J_0} y$ iff etc ect. What we have just proved is that $x \sim_{J_1} y$ iff $|x| = |y|$.

Now suppose $\mathcal{U}$ is an n-symmetric ultrafilter, that $x \in \mathcal{U}$ and $x \leq_{J_n} y$. If we split x into finitely many pieces one of them must be in $\mathcal{U}$, since $\mathcal{U}$ is ultra. So $x' \subseteq x \in \mathcal{U}$. Then its translation under anything in J_n is also in $\mathcal{U}$, so any superset of that is too, so $y \in \mathcal{U}$.

If we can find x such that x and $V \setminus x$ are $\sim_{J_n}$ equivalent then any n-symm ultrafilter containing one must contain the other and we get a contradiction. Now we can easily enuff find x such that x and $V \setminus x$ are the same size. Can we find x such that x and $V \setminus x$ are 1-equivalent? No, beco's one of the two pieces must contain $\emptyset$ and there is no way of moving $\emptyset$. However that argument doesn't scupper the endeavour to chop $V \setminus \{V, \emptyset\}$ into two pieces that are 1-equivalent. How does this generalise? Presumably if we delete from V all cardinal numbers, plus V and $\emptyset$ plus $\{V\}$ plus $\{\emptyset\}$ then we can chop that into two pieces that are 2-equivalent. And so on. So what are we doing? We first cut off the set of those things that are n-symmetric, and then chop the rest into two $\sim_{J_n}$ equivalent halves. Any symmetric ultrafilter must contain precisely one of these three. It can't contain either of the last two beco's it would have to contain the other, so it contains the first.

Conclusion: one element of an n-symmetric ultrafilter on V is the set of $n-1$-symmetric sets. And in fact the converse is true. If $\mathcal{U}$ is an ultrafilter on (say) the set of 2-symmetric sets, then it is 4-symmetric (say) and the set of supersets of its members is likewise 4-symmetric and is an ultrafilter on V. Therefore no contradiction – so far at least!

One obvious thing to try is: what is the least n such that there is an n-symmetric nonprincipal ultrafilter on V? Notice that if $\mathcal{U}$ is an ultrafilter on $\mathcal{P}(X)$, then $\bigcap$ "$\mathcal{U}$ is a filter on X. Two things to check

1. Upward closed. Sse $\bigcup A \in \bigcup$ "$\mathcal{U}$ and $\bigcup A \subseteq B \subseteq X$. $A \cup \iota$"$(B \setminus \bigcup A)$ is now a member of $\mathcal{U}$.

2. Closed under finite intersection. Sse $A, B \in \mathcal{U}$. We want $\bigcup A \cap \bigcup B \in \bigcup$ "$\mathcal{U}$. We know $\bigcup(A \cap B) \subseteq \bigcup A$ and $\bigcup(A \cap B) \subseteq \bigcup B$ so we have $\bigcup(A \cap B) \subseteq (\bigcup A \cap \bigcup B)$. So $\bigcup A \cap \bigcup B$ is at least a superset of something in $\bigcup$ "$\mathcal{U}$. But $\bigcup$ "$\mathcal{U}$ is closed under superset as above, so we are done.

Notice that if $\mathcal{U}$ is n-symmetric, then $\bigcup$ "$\mathcal{U}$ is $(n-1)$-symmetric. However there is no reason to suppose that $\bigcup$ "$\mathcal{U}$ is ultra. It will be if ι"$V \in \mathcal{U}$ but that's not much help, beco's although it will ensure that $\bigcup$ "$\mathcal{U}$ is ultra, it won't ensure that ι"$V \in \bigcup$ "$\mathcal{U}$.

Notice that $F = \{x : |(V \setminus x)| \not\geq^* |V|\}$ is a filter, and it's 2-symmetric. How can $\bigcup$ "F possibly be a filter too?? It would have to be 1-symmetric! But there *is* a 1-symmetric filter on V, namely $\{V\}$. So we seem to have proved, if $V \setminus x$ cannot be mapped onto V, then $\bigcup x = V$. But we know that anyway: if $V \setminus x$ cannot be mapped onto V, then $V \setminus x$ cannot extend any $B(y)$, so x must meet every $B(y)$.

Can we show that $\bigcup$ "$\mathcal{U}$ is *never* ultra?

Sse F is a filter on V, and $X \in F$. Why not say $\{y : (B(y) \cap X) \in F\}$ is a typical element of the new filter? It's too crude. Either $B(y) \in F$ – in which case y belongs to all new elements, or it doesn't – in which case y belongs to none. We could try: "put y into the new set obtained from X if $B(y) \cap$ is F-stationary" but that probably won't fare much better.

To get a feel for this, try the filter of cofinite sets. Then say, for X a cofinite set, $X' := \{y : |B(y) \cap X| \notin \mathbb{N}\}$. But then $X' = V$ so it's trivial.

6.6 Let's hope i'm not tempting fate with this one...

It all started with a question about ultrafilters on V in NF. We know how many *principal* ultrafilters there are; what we don't know how is many *non*principal ultrafilters there are. On the face of it there doesn't seem any reason why there shouldn't be $T|V|$ or even $|V|$-many ultrafilters on V. Can we assume the prime ideal theorem? I don't know. If we do, are there going to be more nonprincipal ultrafilters than principal ones? That doesn't seem clear either. What is clear is that the set of nonprincipal ultrafilters on V must map onto any almost-disjoint family, An AD family is a set any two of whose (distinct) members have finite intersection. I think i have the makings of an argument that any $\subseteq$-maximal AD family (what Adrian Mathias calls a MAD family) must be quite big.

So, buckle your sets, i mean *seats*.

Suppose there is a MAD family M. Since it is $\subseteq$-maximal it contains all finite sets – and that includes the empty set. Suppose further that $f : \iota$"$V \twoheadrightarrow M$ is a surjection. If there is such a surjection there will be one where all the fibres are the same size, namely $T|V|$, so let's stipulate that f satisfies that extra condition. We can also stipulate that $(\forall m \in M)(m \subseteq \bigcup(M \setminus \{m\}))$ but that doesn't seem to help.

The hope is to derive a contradiction, so that we can infer that any MAD family must be very big indeed.

Consider $\{x : x \notin f(\{x\})\}$. Let us call this the *first diagonal set* (now *there's* a plot point for you!) Let's call it D_0. Do we have $D_0 \in M$? Clearly not, we get a standard contradiction as in the proof of Cantor's theorem. For suppose $D_0 = f(\{a\})$. We ask: ¿$a \in f(\{a\})$? $a \in f(\{a\}) \longleftrightarrow a \in D_0$ but $a \in D_0 \longleftrightarrow a \notin f(\{a\})$.

So $D_0 \notin M$. D_0 is infinite beco's $D_0 \notin M$ while every finite set is[4] in M. If D_0 had finite intersection with every member of M then $M \cup \{D_0\}$ would be a family of pairwise almost-disjoint sets properly extending M, contradicting the assumption that M is $\subseteq$-maximal, so it must be that D_0 has infinite intersection with at least some members of M. Let A_0 be the set of those members, so that $A_0 = \{x \in M : |x \cap D_0| \notin \mathbb{N}\}$. Consider the preimage $f^{-1}``A_0$ and the set – the *second diagonal set*, $\{\iota^{-1}(x) \in f^{-1}``A_0 : \iota^{-1}(x) \notin f(x)\}$ – which we are of course going to call D_1.

We want this process to continue transfinitely but i haven't been able to get it to work. There are two things we would need to keep it going. We want A_0 to be big enough, and we want $D_1 \notin M$. On the first point i can't see any reason why A_0 cannot be a singleton. On the second point, the diagonal construction gives us that $D_1 \notin A_0$ rather than the $D_1 \notin M$ which is what we wanted.

Here is my attempt to show that $D_1 \notin M$.

Evidently $D_1 \subseteq D_0$. Is $D_1 \in M$? If it is, D_1 is $f(\{x\})$ for some x. Naturally we ask "¿$x \in f(\{x\})$?". Well, $f(\{x\}) = D_1$, and D_1 is a set of things y such that $y \notin f(\{y\})$ so no. But, if $x \notin D_1$ then either $x \in f(\{x\})$, or $f(\{x\}) \notin A_0$. We know $x \notin f(\{x\})$, so we must have $f(\{x\}) \notin A_0$. But $f(\{x\}) = D_1$ by assumption, and is in M so $D_1 \cap D_0$ is finite. But $D_1 \subseteq D_0$ and so must be finite. **However i don't see any reason why it should not be finite.** *So we haven't established that $D_1 \notin M$.*

Am i missing something easy and obvious?

Nathan sez:

"I took a quick look at this, and I think you will need to put some extra conditions on the MAD family you are working with. Consider for example the set M containing V and all finite subsets of V. This is certainly a MAD family, but it has size only $T|V|$. If we apply your construction then we will find that $A_0 = \{V\}$, since A_0 must be a nonempty subset of M containing only infinite sets. Thus D_1 will be empty, and so will be contained in M. Does that make sense?"

He's right. I was using the wrong definition of MAD family. Wrong in the sense that the correct definition of MAD family causes there to be a surjection from the collection of non-principal ultrafilters onto any MAD family. An almost disjoint family must be a set of moieties any two fo which have finite intersection.

[4] There is a separate proof that D_0 is nonempty, which might come in useful later: if D_0 is empty, then $(\forall x)(x \in f(\{x\})$. But lots of things (in fact $T|V|$-many things) get sent to the same value by f, so every $f(\{x\})$ must be of size $T|V|$, contradicting the fact that some members of M are finite.

The idea is then to show that $\iota``V$ will not map onto any MAD family.

Chapter 7

Numerals and λ-calculus in the Quine Systems

The Thoughts of Chairman Holmes

In my Ph.D. thesis and in my paper “Systems of Combinatory Logic Related to Quine’s ’New Foundations’” (Annals of Pure and Applies Logic, vol. 53 (1991) pp. 103-33) I describe systems of combinatory logic, equivalent to untyped lambda-calculi with “stratification” restrictions on abstraction, which are of precisely the same consistency strength and expressive power as NFU + Infinity and extendible in parallel with NFU extensions (they are weakenings of a system equivalent to NF); this suggests computer science applications, as this system is similar to typed systems now in use. I have an unpublished essay in which I develop an intuitive motivation for this system in terms of security of the abstract data type “program” in a (very) abstract model of programming, along the same lines as the argument for set theory above; I also observe that the notion of ”strongly Cantorian set” seems to translate to the general notion of ”data type” internally to the model of programming. This is interesting, because “strongly Cantorian set” is a notion which has no analogue in ZFC; it is specific to NF and its relatives, and it is interesting to see it corresponding to anything outside that context.

7.1 Stuff to fit in

. . . in the form of some worked exercises that i set for myself.

Beeson’s pdf on the interpretation of Heyting Arithmetic in iNF.

Let me come in at a tangent. . .

Specker showed us that the axiom of infinity (“there is an infinite set”) is a theorem of NF. Or one could put it another way: NF interprets Peano Arithmetic. The proof is famously rebarbative and i am fond of saying that nobody really understands it. It is true that there are some hardened *NFistes* who can present this proof on a blackboard at the drop of a hat, but it's still the case that they (i mean *we*) don't really feel that we know why it's true. Is there perhaps a more illuminating proof?

Enter Michael Beeson, with the very interesting suggestion that one could profitably look at Church numbers in the Quine systems. Now, this suggestion of Beeson's was made not in a context of finding a new proof of Specker's result, but in a context of people promoting the question: is iNF consistent? (mainly me, admittedly... Beeson says I have been asking him about Con(iNF) every year for 20 years, and i fear he is right; i hope i haven't been *too* much of a bore about it). And (and this is the question here) does *i*NF interpret Heyting Arithmetic? Beeson says 'yes'. Now, if – as Beeson suggests – we can use Church numbers to interpret Heyting Arithmetic in iNF, then we can use the same constructions to interpret PA in NF. That proof should be much easier to assemble than Beeson's constructive edifice, because it doesn't require us to make all the steps constructive. Interestingly Beeson's current proof contains no reasoning about the cardinality of the universe or its tree. (As indicated above, such reasoning is essential to Specker's proof.) If his proof for iNF works, then it will also work for NF, and we would have a novel proof of the axiom of infinity in NF. That would be very good news indeed.

It's important to bear in mind that Church numbers and the Frege numbers (equipollence classes of finite sets) are good at very different things. Easy to show that Church successor is total, and easy to show that Frege successor is injective, but the remaining two combinations the other way round are non-trivial. Again, Frege exponentiation *raises* types; Church exponentiation *lowers* them. That makes the two settings look very different and kindles hope that an approach using Church numbers might illuminate by providing a different take.

However, one discouraging feature is the fact that Specker's proof involves reasoning about infinite cardinals – specifically $|V|$ – and there is no good notion of an infinite Church number. So, *prima facie*, we would expect a proof using Church numbers to look very different from the proof we know. Doesn't mean there isn't one, of course ... and it does make such a proof a very enticing prospect.

Beeson's proof kills two birds with one stone (or do i mean: rides two horses?) The two horses/birds are

> (i) a proof of the axiom of infinity inside NF that uses Church numbers and is not just a rephrasing of Specker's proof, and
>
> (ii) a constructive treatment of (i) that resolves the question of whether iNF is weak – tf's view – or strong (enough to interpret Heyting Arithmetic) – which is Holmes' view.

There is an important fact that we need to keep in mind as a continual reality-check. Since – as we know – NFU does not prove the axiom of infinity, it follows that any proof of the axiom of infinity in NF or *i*NF must appeal to the axiom that all empty sets are identical (this being what we have to add to NFU to get NF). It is clear where this assumption is used in the original proof of Specker's that uses cardinal trees (the presence of lots of distinct empty sets would sabotage the equation $|V| = 2^{T|V|}$ on which the construction relies) and accordingly it is clear how it comes that this proof doesn't work in NFU. If Beeson's new proof is correct then there will be somewhere in it an essential use of this assumption, and i cannot find one. And i don't think that is because i haven't looked hard enough; i think it's because there isn't one. And the reason why there isn't one is that there isn't (as far as i can see) anywhere that Beeson's proof strategy would require it.

With a view to killing bird/horse number (i) i think it would be helpful to the reader if Beeson could extract from the 47 pages of his ms a classical version of the proof. It would be much easier to follow (Specker's original article is only three pages long!), it would be much easier to scan for the error that i believe it harbours, it would be accessible to readers who are not familiar with constructive scruples, and – finally, if it works – it would be of independent interest. Such an extraction would also separate the two questions

(a) whether or not Beeson's proof strategy is inherently sound

from

(b) whether his treatment of it is constructively correct.

In principle there is the possibility of (a) being true while (b) is false. Given Beeson's known expertise in constructive logic this sounds unlikely, but – as emphasised – a is of considerable interest even if b is wrong. And – in any case – breaking up the project into parts will make it more digestible. Such a document would certainly be read with closer and more optimistic interest than was the original.

The best outcome would be that:

(i) Beeson's proof is correct, and the classical version is a proof in NF that there is an infinite set,

(ii) and that this proof is essentially different from Specker's; and

(iii) the constructive version shows that iNF interprets Heyting Arithmetic and is strong.

However i fear that the actual situation is that there is a mistake in Beeson's proof (it doesn't use the assumption that there is only one empty set); that iNF is weak (and does not interpret Heyting Arithmetic) and that Specker's proof of AxInf in NF is in some sense the only one.

It has to be essentially different from Specker's proof beco's Specker's proof apparently doesn't enable us to interpret Heyting Arithmetic in *i*NF. OTOH it has to use E: "all empty sets are identical"; and altho' it is easy to see how Specker's proof makes essential use of E, it is hard to see how a proof that reasons solely about natural numbers can exploit it.

If Beeson's proof is correct we have some surprising and rather gratifying developments.

> (i) We have a new proof of infinity inside NF that does not reason about the cardinality of V;
>
> (ii) We have an interpretation of Heyting Arithmetic inside *i*NF.

The above is a picture of what things looked like in 2020.

I think the situation is that Beeson has indeed provided an interpretation of HA into ... *i*NF + an extra axiom which is probably equivalent to the axiom of counting. He's even proved it in LEAN. It's reassuring, but it is hardly a giant leap for Mankind.

I was greatly struck by this thought and – altho' i no longer think it offers the quick fix to proving infinity in *i*NF that i at first hoped – it pointed to a *lacuna* in my understanding of these matters that the discipline of writing these notes might help to fill.

These notes have benefitted greatly from conversations with Randall Holmes, Michael Beeson and Albert Visser. They have also benefitted from lockdown at 375 Mutiny Road.

This text is not being offered as a piece of original work, rather as (as i say above) a folio of worked exercises that are good for the writer (Maurice always used to say “Mais, Thomas – il faut l'écrire”) and may be a useful resource, a source of summaries, for people who want to work on this stuff. I'm sure much of this has been/is being duplicated by others even as we speak, but it is all my own work [except where o/w indicated] so please give generously.

7.2 Stuff to be put in the right place

Can we exploit somehow the set-theoretic structure of Cnumbers in NF? Let us write ‘$t(x)$’ for the transitive closure of x.

If n is a Cnumber then $t(n)$ is the union of all powers of n; it would be nice to recover from this an expression for the set of all powers of n.

If n is a Cnumber then $t \circ n$ (We can't really write ‘$t \cdot n$’ beco's that would look too much like Cnumber multiplication) is the function $\lambda f.t(nf)$, which sends f to the union of all the mf for m a multiple of n: $\bigcup\{mf : n|m\}$. It would be nice to recover from this an expression for the union of all the multiples of n, and even an expression for the set of all multiples of n.

Good question.

Maybe not, but it does give us a homogeneous way of saying “n is a power of m”, namely $t(n) \subseteq t(m)$. This is noteworthy beco's the obvious (implementation-insensitive) way: ‘$(\exists k)(n = km)$’ is not homogeneous. Note that we cannot prove the equivalence of these two by induction beco's

$$(\forall nm)(t(m) \subseteq t(m) \longleftrightarrow (\exists k)(n = km))$$

isn't stratified. We'll have to sprinkle a few ‘T’s around.

We can do something similar with “n is a multiple of m” which is
but
this time there are no complications, because both formulæ

$t \circ n$ is the function $\lambda f.t(nf)$, which is the union of all multiples of n. (We can't really write '$t \cdot n$' beco's that would look too much like Cnumber multiplication).

are stratified:

$$(\forall nm)(\{t \circ x : x \in n\} \subseteq \{t \circ x : x \in m\} \longleftrightarrow (\exists k)(n = k \cdot m))$$

It would probably be a useful exercise to prove this by induction.

The inductively defined bijection between $\mathbb{N}_c$ and $\mathbb{N}$ is very useful for (among other things) showing that $\mathbb{N}_c$ is a discrete set, which is not o/w obvious(!)

7.2.1 Using Church numbers as cardinals

We write $CN|X|$ for the Church number of a finite set X/

$$CN|\emptyset| = 0_{CN}$$
$$CN|X \cup \{x\}| = \mathtt{succ} CN|X|$$

...as long as $x \notin X$

next we have to show that this definition is legitimate, since a finite set X can be obtained and $X' \sqcup \{x\}$ in lots of different ways. We prove that all decompositions give the same number by induction he says gaily. Gulp. True if X is empty. Sse true for X. Is it true for $X \cup \{x\}$? We seem to need the induction hypothesis not just for X but for $(X \cup \{x\}) \setminus \{y\}$ for all y. Perhaps we can exploit trichotomy somehow ...or find a clever induction ...

With Cnumbers [Church numerals] one knows that every number has a successor, but the classifier function $x \mapsto$ cardinal-of-x does not have a cute definition and it is not clear that it is total. With Fnumerals [usual Frege numeral, equipollence classes] it's the other way round: "cardinal-of" is cute and well-behaved but proving that every natural has a successor is problematic – and indeed still open in the constructive case.

With Cnumbers (but not with Fnumerals) one has a problem showing that the successor function is injective. Perhaps one should display this information in the form of a table.

And another thing...the cardinal-of function with Cnumbers needs to be thought about very hard. Presumably we want it to be homogeneous. I think we want to prove by induction in *i*NF that every Nfinite set admits permutations with precisely one cycle. OK. And we also want it to be the case that T^k of the cardinal number of that set (for some suitable k, depending on our pairing function) applied to any such permutation gives $\mathbb{1}$. What happens now if there is a dense Nfinite set $\mathcal{V}$. This set has a Cnumber. v, say. What becomes of the successor of that Cnumber?

Albert points out that every Cnumber ≥ 2 is `succ` of something other than a Cnumber! [probably worth spelling out why this is so] Beware!

For a long time the task of implementing arithmetic in NF by using equipollence classes of finite sets was held up by the necessity of proving that every natural number has a succcessor. This was solved by Specker – at great cost, and the scars are still visible. The situation with Church numerals is quite different. It is obvious that every *Church* natural number has a successor. So, the world being the imperfect place it is, one expects things to go wrong somewhere else. Let us see.

I don't know why it has taken me so long to see this. The equivalence relation on functions $V \to V$ of being-conjugated-by-a-permutation-of-V (which is roughly the same as the relation of having the same cycle type, but not quite) is, for each n, a congruence relation for the operation, well the *Cnumber* n.

To be more precise we say

$f \sim g$ iff $(\exists \pi)(\pi$ a permutation of $V \wedge f = \{\langle \pi(x), \pi(y)\rangle : \langle x, y\rangle \in g\})$

Thus one could think of the Cnumbers as acting not on functions $V \to V$ but on conjugacy classes of such functions.

That will make things m-u-c-h easier.

Well, no it won't, actually. Co's it's not a congruence relation for composition! [must illustrate that it is not a congruence relation for composition. Easy: remember that with a minimum of AC every permutation is the product of two involutions without fixed points.] However, the definition of T is slightly easier.

This is reminding me of the fact that to find a fundamental sequence for a ctbl ordinal you need to reason about an actual worder of that order type.

Albert describes the Cnumbers of NFU or *i*NF as being like the letter 'ρ': a **stick** followed by a **loop**. We need to get a handle (joke!) on the size of these things. Here is a potentially useful thought.

A Cnumber is – whatever else it is – a function, so you can restrict it to a subset of its domain. There are two ways of restricting Cnumbers that will be useful to us here.

(i) Restrict each Cnumber to the set of surjections $V \twoheadrightarrow V$;

(ii) Restrict each Cnumber to the set of all permutations of V.

These may be the same of course. The interesting possibility is that in *i*NF we fail to prove infinity, so we can't prove that they are different. But we might be unable to prove they are the same. Gives us a dangerous interesting space to explore.

Notice that both these restrictions give us structures that do not support a T function.

Anyway these two ways of restricting Cnumbers give us two `succ`-homomorphisms onto the two (sets of) restricted Cnumbers. I think they respect `plus` too. The image under the first kind of restricting (the more drastic one) looks either like $\mathbb{Z}$ or a loop. Notice that the fact that we have a homomorphism from $\mathbb{N}_c$ onto this structure tells us immediately (or is trying to tell us, through the constructive fog) that the circumference of Albert's loop is a multiple of the circumference of the homomorphic image.

In both cases we want to know the n such that the homomorphism is n-to-one.

The loop is a well-defined object of iNF. We define `succ` on $\mathbb{N}_c$ and observe that the definition is homogeneous so the graph is a set. Then we define X as the intersection of all sets containing $\mathbb{N}_c$ and closed under $A \mapsto \texttt{succ}``A$. Then X is a set, and so, too, is $\bigcap X$ – which is the loop. Beeson calls it $\mathcal{L}$.

Now what sort of structure does $\mathcal{L}$ have under `succ`? We want it to be a simple loop. In principle $\mathbb{N}_c$ equipped with `succ` is a tree and $\mathcal{L}$ contains those elements that have rank $\geq \omega$. (Unranked is rank ∞, which is greater than ω). Can we be sure that $\mathcal{L}$ contains no elements of rank ω? Yes, beco' then the set of elements of lower rank contains 0 and is closed under `succ`.

We would like $\mathcal{L}$ to be empty. So does $\mathbb{N}_c \setminus \mathcal{L}$ contain 0 and is it closed under `succ`? Perhaps not. But beware. If it isn't closed under `succ` (and the best guess is that it isn't) that doesn't mean it has a last element.

We want to show that everything is $\mathcal{L}$ is `succ` of something in $\mathcal{L}$.

7.3 Notation

Let's have a symbol for the set of Church numerals ("Cnumbers" to their friends): $\mathbb{N}_c$. I use the word '`succ`' to denote Church successor. Of course there is also successor on the usual (Frege) numerals ("Fnumerals") as well and i will use fraktur for the F-objects . . . 'f' for 'fraktur' and for 'F'rege, geddit?? So $\mathfrak{succ}$ will be successor for Frege naturals . . . . In circumstances where one wants to make clear that it is the usual – Frege – numerals (the equinumerosity classes) that one means one can write '$\mathbb{N}_{\mathfrak{f}}$'

For the moment we will assume that our pairs are Wiener-Kuratowski, since these (unlike Quine pairs) work not only in NF but also in NFU, NFI and *i*NF. W-K pairing and unpairing is constructive. I am endebted to Randall and PTJ for pointing this out to me: the first component of p is $\bigcap\bigcap p$; the second component is the unique thing that belongs to only one member of p

7.3.1 The Exercises

(i) Define the set $\mathbb{N}_c$ of Cnumbers and the arithmetic operations on it, and establish which have graphs that are sets.

(ii) Inductively define the maximal partial bijection between $\mathbb{N}_{\mathfrak{f}}$ and $\mathbb{N}_c$ and establish what its domain and range are.

(iii) Define `succ` and `prec` on $\mathbb{N}_c$, and ascertain whether they are total, injective, surjective, mutually inverse *etc* ...

(iv) Define the obvious partial order $\leq_{\mathbb{N}_c}$. Loops?

(v) Sort out the cardinality classifier whose values are the Cnumbers.

(vi) Prove commutativity of `mult` and `plus` and distributivity. Tho' that is probably routine.

(vii) Define T on $\mathbb{N}_c$.

7.4 Implementing the arithmétic Operations, Church-style

We start with successor. We can define `succ` as $\lambda n.\lambda f.\lambda x.f((nf)x)$. It is evident that '$m =$ `succ` n' is stratified with 'x' of lowest type, 'f' three types higher than 'x' (our pairs are W-K, remember) and 'n' and 'm' are three types higher still. Observe that '$m =$ `succ` n' is homogeneous; it has only two free variables: `succ` is not a variable but a defined term. The precise numerical value of the difference in levels between `succ` and 'n' and'm' is in some sense not part of mathematics, tho' the fact that it is greater than 0 emphatically *is* part of mathematics.

Albert points out that every Cnumber from 2 onward is `succ` of something other than a Cnumber. Of course these other things of which it is `succ`essor tend not to be Cnumbers, but it's a thing worth keeping in mind, particularly when we start trying to show that `succ` is injective ... we will mean *injective on Cnumbers*.

Next we define $\mathbb{N}_c$ as the intersection of all sets containing KI (which is what the Cnumber 0 turns out to be) and closed under Church successor:

$$\{x : (\forall y)((0 \in y \wedge \texttt{succ}``Y \subseteq Y) \to x \in Y)\}$$

This is a stratified set abstract, and so it is an axiom of iNF that it denotes a set. I think this is actually an axiom also of Crabb/'e's NFI [**?**], and this is true also of the axiom giving the set $\mathbb{N}_{\mathfrak{f}}$ of all Frege naturals[1].

[1] What is NFI?

Randall says (in his article on how *the set theoretic programme of Quine succeeded but nobody noticed*)

> "The other extensional fragment of interest is NFI, the version of NF with [...] extensionality and with a version of stratified comprehension which is restricted to those instances in which no type is assigned to a variable which is higher than the type which would be assigned to the set being constructed. This corresponds to a restriction on the impredicative formation of sets in TST. If the additional restriction is imposed that variables of the same type as the set being constructed must be parameters (must not be bound), we obtain the theory NFP (predicative NF)."

So it's worth keeping in mind the possibility/desirability of our definitions working also in *i*NF and NFI.

7.4.1 Addition, Multiplication and Exponentiation

One has to be careful here: `plus` and `mult` are homogeneous in the sense that there are homogenous formulæ $\texttt{Plus}(n, m, k)$ and $\texttt{Mult}(n, m, k)$ that say that $k = n + m$ and that $k = n \cdot m$ respectively:

$$\text{‘}\texttt{Plus}(n, m, \lambda f.\lambda x.(nf)(mfx))\text{’}$$

is stratified with ‘x’ of lowest type, ‘f’ three types higher than ‘x’ and ‘n’ and ‘m’ are three types higher still. `Mult` is similar: $\texttt{mult}\ n\ m = \lambda f.n(mf)$ giving

$$\text{‘}\texttt{Mult}(n, m, \lambda f.n(mf))\text{’}$$

However, if we want the graphs of the two functions $\mathbb{N}_C \times \mathbb{N}_C \to \mathbb{N}_C$ to actually be *sets* then we have to have Quine ordered pairs. (Of course there is no way of getting the graphs of the *curried* versions to be sets). This won't make much difference, but it's probably worth bearing in mind, even if only to curb one's enthusiasm. I shall use the words ‘`plus`’ and ‘`mult`’ to denote the *curried* functions ... and use the curried versions to conform with standard λ practice.

Given the inductive definition of $\mathbb{N}_c$ it is routine to prove that it is closed under `plus` and `mult`.

Church exponentiation is a stratified operation but it is not homogeneous. (It is function application and therefore gives its two arguments different types, the difference depending on our choice of pairing function. That difference is never zero). How significant is this fact? [and why has it taken me until April Fools' Day 2019 to see it?!] Perhaps one wants to connect this with the fact that there seems to be no synthetic definition of later Doner-Tarski operations.

There are other operations we need to think about: predecessor and – since we are doing NF – the T function.

Naturally you want n `succ` 0 to be n, and classically it is, of course, but it's six types lower (if i've counted right) so it must be $T^{-6}n$. More on that later. For the moment we'd better prove that

LEMMA 5 *Every Cnumber is of the form n `succ` 0 and every object of that form is a Cnumber.*

Proof:

First we check that n `succ` 0 really is a Cnumber. (Really this ought to be trivial, in that $\mathbb{N}_c$ is defined as the `succ`-closure of $\{0\}$.)

True when $n = 0$.

So suppose true for n, which is to say $n\,\texttt{succ}\,0 \in \mathbb{N}_c$. Then $(\texttt{succ}\ n)\ \texttt{succ}\ 0 = \texttt{succ}\ (n\ \texttt{succ}\ 0)$. Now (the RHS) n `succ` 0 is in $\mathbb{N}_c$ by induction hypothesis, and $\mathbb{N}_c$ is closed under `succ` by construction, so the LHS is also in $\mathbb{N}_c$, and we are done.

The other direction states that for all m in $\mathbb{N}_c$, there is n such that $m = n$ $\mathtt{succ}$ 0

We procede by induction on 'm'.

No problem with $m = 0$.

So suppose $m = n$ $\mathtt{succ}$ 0. We want $(\mathtt{succ}\ n)\ \mathtt{succ}\ 0$ to β-reduce (or somehow rearrange) to $\mathtt{succ}\ m$.

Now $\mathtt{succ}\ n$ is $\lambda f.f \circ (nf)$, so, putting this in for '$\mathtt{succ}\ n$' in

$$\text{`}(\mathtt{succ}\ n)\ \mathtt{succ}\ 0\text{'}$$

we get

$$(\lambda f.f \circ (nf)n)\ \mathtt{succ}\ 0$$

and then, substituting $\mathtt{succ}$ for f,

$$(\mathtt{succ}\ ) \circ (n\ \mathtt{succ})\ 0)$$

which is

$$\mathtt{succ}\ (n\ \mathtt{succ}\ 0),$$

and $(n\ \mathtt{succ}\ 0)$ is m so we get

$$\mathtt{succ}\ \ m$$

as desired.

■

So this function is total and surjective all right; the problem is that it mightn't be injective.

I am now very struck by the thought that the following seems to be a perfectly respectable set:

$$\{\langle n, \iota^6(n\ \mathtt{succ}\ 0)\rangle : n \in \mathbb{N}_C\}$$

and this is a bijection between $\mathbb{N}_c$ and ι^6"$\mathbb{N}_c$. (I hope that 6 is the correct number. *Mutatis mutandis*). This is pretty cool, but don't get carried away: $T^2\alpha = \alpha$ doesn't obviously imply $\alpha = T\alpha$ unless α is (for example) a natural number.

However it does give a simple proof that $|\mathbb{N}_\mathfrak{f}| \neq |\mathbb{N}_c|$ unless things look very very nice. This is beco's unless things look very nice we don't have $T^6|\mathbb{N}_\mathfrak{f}| = |\mathbb{N}_\mathfrak{f}|$. Doesn't $T^6|\mathbb{N}_\mathfrak{f}| = |\mathbb{N}_\mathfrak{f}|$ imply the axiom of infinity?

- We can inductively define the maximal bijection between initial segs of $\mathbb{N}_\mathfrak{f}$ and $\mathbb{N}_c$.
- Also we can send each Cnumber n to the Fnumber of the set of Cnumbers m such that n is in the $\mathtt{succ}$-closure of $\{m\}$
- You can relate an Fnumeral n to any Cnumber m such that $m\ \mathfrak{succ}\ 0 = n$.

All these things are trying to be bijections between $\mathbb{N}_\mathfrak{f}$ and $\mathbb{N}_c$.

Another thing you can do is to send a Cnumber n to $\iota^6(n\ \mathfrak{succ}\ 0)$.

We need to look very closely at predecessor functions for Cnumbers.

Predecessor

Say `pipeline` $\langle n, m\rangle = \langle m,$ `succ` $m\rangle$.

Then `pred` $n =$ `fst` (n `pipeline` $\langle 0, 0\rangle$).

We need to define pairing and unpairing:

`pair`$:= \lambda xyf.fxy$
`fst`$:= \lambda p.p$ `true`
`snd`$:= \lambda p.p$ `false`
`nil`$:= \lambda x.$`true`

But `pred` is not homogeneous (tho' it is stratified).

Wikipædia supplies another definition of `pred`:

$$\lambda n.\lambda f.\lambda x.n\ (\lambda g.\lambda h.h\ (g\ f))\ (\lambda u.x)\ (\lambda u.u)$$

but i haven't got my head round it yet.

What the above discussion shows is that $\mathtt{succ}^{-1}$ is single-valued when restricted to numerals that are T^6 of something. It doesn't show that every Cnumber has a predecessor.

Should get that down to: T of something.

There doesn't seem to be a straightforward implementation of `pred` for Church numerals in NF that is homogeneous, tho' I can see how do one using T. So let's get a definition of T.

7.4.2 The T-function for Church Numbers

Lemma 5 tells us that the function $n \mapsto n$ `succ` 0 is total and surjective[2]. Clearly n `succ` 0 is $T^{-2}n$. However one wants to be sure that this definition of T has the right behaviour. We expect that, for all n and all f,

$$(Tn)(jf) = j(nf).$$

This is because "f composed with itself n times is g" is a stratified expression, so we are in with a chance of proving

> $(\forall n)(\forall f)(\forall g)$("$f$ composed with itself n times is g" iff "jf composed with itself Tn times is jg").

So, if the pair $\langle f, g\rangle$ belongs to the Cnumber n, we want Tn to be the Cnumber that houses $\langle jf, jg\rangle$. The Cnumber we want is of course $\{\langle jf, jg\rangle : \langle f, g\rangle \in n\}$ (a thing one might sensibly notate 'n^j'). This thing isn't a Cnumber of course – beco's it is defined only on things in the range of j – but is it at least included in a unique Cnumber? And can it happen that $n \neq m$ but nevertheless n^j and m^j correspond to the same Cnumber?

It would be nice if it didn't, but don't hold your breath; as Randall puts it "functions of the form $j(f)$ do not exhibit all possible cycle lengths; so the

[2] Tho' of course it doesn't tell us that its graph is a set.

Church numerals limited to functions of this form "cycle" sooner than expected, as it were." What Randall is alluding to is the fact that (for example) a permutation $j\sigma$ must be of order Tn for some n.

I think we can at least show that n^j is not included in more than one Cnumber. The argument goes as follows. Suppose the pair $\langle f, g\rangle$ belongs to both of two distinct Cnumbers n and p. That is telling us that $f^n = f^p$. However, if $f^p = f^n$ then we can compute from n and p a k such that f^k is idempotent. But this tells us that there is a k such that *for every* f, f^k is idempotent. But actually that might be true. Oops.

work to do here ☠

So: $n \mapsto T^{-2}n$ is total and defined everywhere. Does that really suffice to show that T^{-1} is defined everywhere? Gulp. And anyway, just what exactly is this function that we have shown to be defined everywhere? Is this operation that i have so airily called 'T^{-2}' *really* the inverse of the operation defined earlier that i called (with some justification) 'T'? This needs to be proved!!

work to do here ☠

Actually i am guessing that these two operations cannot straightforwardly be proved to be mutually inverse, since T being a bijection $\mathbb{N}_c \longleftrightarrow \mathbb{N}_c$ (even if its graph is a class, which of course it is, *prima facie*) is, i think, enough to prove AxInf.

For consider: we will want the equality $T^2 n$ `succ` $0 = n$ and we will have trouble proving it beco's at this stage we know how to apply $T^2 n$ only to things that are j^2 of something.

7.4.3 Verifying that the Definitions work

Show that the two definitions of the order relation are equivalent.

(i) $n \leq_1 m$ iff $(\exists k)$(`plus` $n\ k = m$)

(ii) $n \leq_2 m$ if every `succ`-closed set containing n also contains m.

(ii) $\to$ (i). Fix n. We then prove by induction on 'm' that $n \leq_1$ `plus` $n\ m$.

For (i) $\to$ (ii), suppose `plus` $n\ k = m$. We wish to show $n \leq_2 m$. I think we do this by induction on 'k'.

work to do here ☠

`plus` and `mult`

I mentioned above that we ought to prove that $\mathbb{N}_c$ is closed under `plus` and `mult`.

`plus`:

We first show that `plus` obeys the obvious recursion:

$$\texttt{plus}\ n\ (\texttt{succ}\ m) = \texttt{succ}(\texttt{plus}\ n\ m).$$

We now fix n and prove by induction on 'm' that $(\forall m \in \mathbb{N}_{\mathfrak{f}})((\texttt{plus}\ n\ m) \in \mathbb{N}_{\mathfrak{f}})$, as follows. True for $m = 0$. Suppose true for m. Then, as we have just proved, `plus` n (`succ` m) = `succ`(`plus` $n\ m$) and we know the RHS to be in $\mathbb{N}_c$ by induction hypothesis and the fact that $\mathbb{N}_c$ is closed under `succ`.

to do here ☠

I am assuming `mult` will be analogous.

Perhaps i am making a fuss about nothing. Perhaps all this could be proved by β-manipulations of the appropriate λ-terms, but in situations where i am not confident that i understand everything i want stuff written out.

Lemma 6 *`succ` : $\mathbb{N}_c \to \mathbb{N}_c$ has no fixed point.*

Proof:

The obvious function to think of is complementation: `comp`: $x \mapsto V \setminus x$. (Thank you Randall). The thought is that n and `succ` n must disagree on `comp`, and therefore be distinct. Suppose *per impossibile* that $n =$ `succ` n. Then n `comp` $=$ `succ` n `comp`. Now, for any x whatever,

$$\texttt{succ}\ n\ \texttt{comp}\ x = V \setminus (n\ \texttt{comp}\ x)$$

so $n =$ `succ` n is impossible, beco's nothing is equal to its own complement.

■

Notice that this proof is constructive.

I think it shows that `succ` cannot have any odd cycles, since it should be possible to show $2n$ `succ comp` $=$ $\texttt{comp}^2$ by induction. We know constructively that $\texttt{comp}^2$ is idempotent, so `comp` cannot have any odd cycles. This should show that the "loop" cannot be of odd length.

This doesn't by itself prove that `succ` cannot have even cycles, but it does give us hope.

Suppose $n = m$ `succ` n, where m is odd. Then
$n\ \texttt{comp}^2\ x = m\ \texttt{succ}\ n\ \texttt{comp}^2\ x$. errr ...
is impossible, beco's nothing is

The key fact wot we exploited is that `comp` has no fixed points. Suppose f^2 has no fixed points, then we can argue that 2 `succ` has no fixed points. Suppose $n = 2$ `succ` n; then

$$n\ f = 2\ \texttt{succ}\ n\ f$$
$$n\ f\ x = 2\ \texttt{succ}\ n\ f\ x$$
$$n\ f\ x = f^2(n\ f\ x)$$

contradicting the assumption that f^2 has no fixed point.

So: to exclude the possibility of a loop, we need, for each n, a function f such that nf has no fixed points. How likely does that sound?

then

Lemma 7 *0 is not `succ` of anything.*

Proof:

This is Beeson's proof.

Suppose *per impossibile* that `succ` $n = 0$. By lemma 6 we have $n \neq 0$. Let a and b be two distinct sets, and consider applying the two things `succ` n and 0 to separately to Ka, and then applying the result to b. We get

$$\texttt{succ}\ n\ (Ka)\ b\ =\ a$$

but

$$0\ (Ka)\ b = b$$

■

Notice that this relies on being able to apply the fraudulent candidate for predecessor of 0 to things that are emphatically not permutations. If we consider the restrictions of Cnumbers to **permutations** (of V) we get a very different picture.

The bit i'm dreading is showing that Church `succ` is injective. This seems to be the only thing still left to do. How do you do it?

There sure as hell is going to be a problem proving that `succ` is injective beco's we *know* this doesn't work in NFU. And i have *absolutely no idea* how this can fail to work in NFU while nevertheless (presumably?) working in NF. You are going to have to use extensionality ... *on empty sets!!*.

work to do here ☠

Must investigate the obvious isomorphism between the Church numerals and the equipollence naturals. It's the $\subseteq$-least set of ordered pairs containing the ordered pair of the two zeroes and closed under the operation "If you find $\langle x, y\rangle$ put in the pair $\langle \texttt{succ}\ x,\ S(y)\rangle$ as long as $S(y)$ is defined". This is a well-defined kosher set of ordered pairs.

Does it use up all of one or the other? Or are there unpaired members on both sides? This should remind the student of the proof that the natural numbers are second-order categorical.

☠

In *i*NF the h-u-g-e difference between Cnumbers and Fnumerals is that successor is total on Cnumbers even if it isn't on Fnumerals. OTOH it is clearly injective on Fnumerals even if it isn't on Cnumbers. Notice that we can easily define a stratified formula that says that n belongs to a loop, namely n belongs to the `succ` closure of the singleton of `succ` n. The relation "m belongs to the succ-closure of $\{n\}$" wants to be a genuine partial order (i.e., antisymmetric) but it can't be relied on to be. One thing at least does work, and that is that the collection of Cnumbers that do *not* belong to a loop form a set. My guess is that that set is iso to $\mathbb{N}_{\mathfrak{f}}$, the set of Fnumerals.

If we are to prove that the Cnumbers of *i*NF give us an implementation of Heyting Arithmetic then at some point we are going to have exploit the fact that we are doing set theory, and get our hands grubby working on the *sets* that implement these gadgets. I suspect it might be an idea to think about boolean combinations of Cnumbers, or at least intersections and differences. The intersection of all Cnumbers is the singleton of the restriction of the identity relation to the set of all function. I'm guessing that successor distributes over binary intersection.

7.4.4 Digression on The Axiom of Counting

1 from philmatbok.tex

The reader may be bothered by the circumstance (**not** remarked on in any detail above!) that $|\mathbb{N}_C| = T^6|\mathbb{N}_C|$ or something similar, where the index might not be 6 ... it could be 2 if our pairs are Quine pairs. There is something to think about here, and it cannot be avoided altogether. I insert here some notes on this subject from elsewhere – a *digression*, indeed. This material may be reprocessed into something more obviously directly relevant.

Consider the two assertions:

1. $(\forall n \in \mathbb{N}_{\mathfrak{f}})(n = |\{m : m < n\}|)$;
2. $(\forall x)(x \text{ finite} \to |x| = |\iota``x|)$.

These two are usually assumed to be equivalent, and both are known in the NF literature as the *Axiom of Counting*, the name given to (1) by Rosser in [**?**].

However these two are actually completely distinct assertions: the first comes from the typing that comes with implementations, and the second is purely set-theoretic. It's probably worth minuting the following:

Theorem 12
For any (stratified) implementation of natural numbers let the two vertical bars denote the `natural-number-of` *function; let k be the type difference* (`type-of` '$|x|$') – (`type-of` 'x') *in that implementation and let $\mathbb{N}^{(k)}$ be the corresponding collection of implemented natural numbers, so that*

$$(\forall m \in \mathbb{N}^{(k)})(|\{n : n < m\}| = m)$$

is then the axiom of counting.

(Observe that any such implementation of cardinal-of will be setlike even if it is not locally a set.) Then

1. *If $k = -1$ then the axiom of counting is a theorem of NF;*
2. *In all other cases the axiom of counting is equivalent to "Every (inductively) finite set is strongly cantorian".*

(In this section we take an implementation of arithmetic to be a structure for the language of arithmetic PLUS a `natural-number-of` function which is assumed to be setlike but not assumed to be locally a set.

There is a further subtlety in that the T function on natural numbers – thought of as a permutation of V – is not setlike, but thought of as a permutation of $\mathbb{N}$ it is. There is a detailed discussion of this in another file but i cannot for the life of me remember which.)

Proof:

Case $k = -1$.

In this case the type of '$|\{n : n < m\}|$' is one less than the type of '$\{n : n < m\}$' which in turn is one greater than the type of 'm'. One greater? Yes; as long as '$x = |y|$' is stratified the relation $<$ on cardinals will be homogeneous. So '$|\{n : n < m\}|$' and 'm' have the same type. So the assertion $(\forall m \in \mathbb{N}^{(-1)})(|\{n : n < m\}| = m)$ is stratified and can be proved by mathematical induction.

Case $k \neq -1$.

For any implementation $\mathbb{N}^{(k)}$ the assertion

$$|\iota^{k+1}``x| = |\{m \in \mathbb{N} : m < |x|\}|$$

is stratified and can therefore be proved by induction on $|x|$. That we get anyway; the axiom of counting now tells us that

$$|x| = |\{m \in \mathbb{N} : m < |x|\}|$$

so we conclude that $|x| = |\iota^{k+1}``x|$. $**$

(Notice that in the case $k = -1$ the axiom of counting gives us no exploitable information.) Now if x were properly bigger (or properly smaller) than $\iota``x$ then, for each concrete $j$, $\iota^j``x$ would be properly bigger (or properly smaller – whichever it is) than $\iota^{j+1}``x$ so – by transitivity of – $<$ we would establish that $x$ was properly bigger (or smaller, *mutatis mutandis*) than $\iota^{k+1}``x$. But we have just shown – above, at $**$ – that this cannot happen. So x and $\iota``x$ are the same size. That is to say that x is cantorian.

However the claim was that x was *strongly* cantorian, so there is still work to be done. If every finite set is cantorian then Specker's T function restricted to $\mathbb{N}$ is the identity, so the relation $\{\langle\{n\}, Tn\rangle : n \in \mathbb{N}\}$ – which is a set, being the denotation of a closed stratified set abstract – is precisely $\iota\restriction\mathbb{N}$, which is to say that $\mathbb{N}$ is strongly cantorian. But any subset of a strongly cantorian set is strongly cantorian, and every inductively finite set can be embedded into $\mathbb{N}$[3] so every finite set is strongly cantorian. ■

There are many ways of implementing `natural-number-of` with a stratifiable formula – at least in NF(U).[4] To each such implementation we can associate a concrete integer k which is the difference (`type-of` 'y') – (`type-of` 'x') in '$y = |x|$'. In fact:

THEOREM 13
For every concrete integer k there is an implementation of `natural-number-of` *making '$y = |x|$' stratified with*
(`type-of` 'y') – (`type-of` 'x') $= k$.

Proof:

For $k = 1$ there is the natural and obvious implementation that declares $|x|$ to be $[x]_\sim$, the equipollence class of x – the set of all things that are the same size as x. For $k \geq 1$ we take $|x|$ to be $\iota^{k-1}([x]_\sim)$. (This works for all cardinals, not just for natural numbers).

---

[3]This needs AxInf

[4]I seem to remember that there is no way of implementing `natural-number-of` with a stratifiable parameter-free formula in ZF(C).

For $k < 1$ we have to do a bit of work, and although the measures we use will not work for arbitrary cardinals they do work for naturals. We need the fact that there is a closed stratified set abstract without parameters that points to a wellordering of length precisely ω. The obvious example is the usual Frege-Russell implementation of $\mathbb{N}$ as equipollence classes, which we have just used above with $k \geq 1$. However it is probably worth emphasising that we don't *have* to use the Frege-Russell $\mathbb{N}$ here; whenever we have a definable injective total function f where $V \setminus f``V$ is nonempty, with a definable $a \notin f``V$, then

$$\bigcap\{A : a \in A \wedge f``A \subseteq A\}$$

will do just as well. The usual definition of $\mathbb{N}$ as a set abstract is merely a case in point. (We have already noted that there is no such set abstract in Zermelo or ZF!) Let's use the usual $\mathbb{N}$-as-the-set-of-equipollence-classes.

Consider $\{\iota^k(n) : n \in \mathbb{N}\}$. It is denoted by a closed set abstract so it is clearly a set in NF, and it has an obvious canonical wellorder to length ω. For every inductively finite set x there is a unique initial segment i of this wellordering equipollent to it, and the function that assigns x to that initial segment is a set. We conclude that the function $x \mapsto \bigcup^k i$ is an implementation of `natural-number-of` that lowers types by k. ■

Here is another proof. We can take $|x|$ to be $[y]_\sim$ for any y such that $\iota^k``y \sim x$. (Here $\sim$ is equipollence as before.) This gives us a natural-number-of $x$ that is $k-1$ types lower than $x$. For us a natural-number-of $x$ that is $k+1$ types *higher* than $x$ take $|x|$ to be $[\iota^k``x]_\sim$.

■

Notice that the same does not go for `ordinal-of`, because if it did we would get the Burali-Forti paradox. It seems to be open whether or not one can have a `cardinal-of` function that lowers types. We can have an implementation of `ordinal-of` that lowers types if IO holds... specifically iff every wellordered set is the same size as a set of singletons. (This is related to the fact that there is no type-lowering implementation of pairing. Is it also related to the fact that WE - like P - is not entirely finitary..?)

7.5 Typed lambda calculus

It could be argued that this material belongs in a separate chapter as – until recently – it did.

7.5.1 A Question of Adam Lewicki's

Adam Lewicki reminds me that there seems nowhere to be a proof that $|V \to V| = |V|$. Clearly all we need to do is inject V into $V \to V$. Send X to

$$\lambda x.\ \texttt{if}\ x \in X\ \texttt{then}\ x\ \texttt{else}\ V \setminus x$$

7/v/2017

I don't know why i hadn't thought of this earlier, but Adam Lewicki has, and has made me think about it. Using Quine pairs every set is a set of ordered pairs, so the function that takes x and y and returns $x``y$ is well-defined and total – and homogeneous! What kind of algebra do we get? The operation clearly has a left-unit, which is just the identity relation, $\{\langle x, x\rangle : x = x\}$. What about K and S? We don't get K – we don't even get Kx for any x that isn't a singleton ... and presumably not S either. What *do* we get? Have you thought about this?

It's quite disgraceful that i have known about Quine pairs and about lambda calculus for years and have never thought about this algebra. How can i show my face in public?

What is $V``x$? Presumably it is $V$ unless $x$ is empty. $x``V$?

There is the set $\{\langle\langle x, y\rangle, (x \times y)\rangle : x, y \in V\}$. That does something nice.

One obvious question is: "what kind of combinatorial completeness does this algebra have?'

That is, for what functions $f : V \to V$ can we find x s.t $(\forall y)(f(y) = x``y)$. Hardly any!

What is Kx? Presumably $V \times x$ ... (or $x \times V$ depending on which way you write down your functions). It doesn't return x on being given the empty function, which is a bummer. Do we really have to leave out the empty function?? How annoying. I can start to see why Adam L wants to use $\emptyset$ as a `failure` flag.

The algebra supports all sorts of operations: anything you can define on functions, really. Inverse, composition, transitive closures .... The algebra is closed under all these operations, but of course that doesn't mean that they are internalised in the combinatorial completeness sense. The algebra's `Inverse` is the function $\{\langle\langle x, y\rangle, \langle y, x\rangle\rangle : x, y \in V\}$. Then `Inverse`$``R$ is just R^{-1}.

Notice there are $|V|$-many elements of this algebra but only $T|V|$ functions to which they can correspond, so extensionality fails badly.

Older material on the same topic

First we prove that V and $V \to V$ are the same size.

For a first try, let's send each set x to that function which sends everything in x to itself, and everything else to Λ, the empty set.

F: Input x; output $\lambda y.$`if` $y \in x$ `then` y `else` Λ.

We can recover x from $\mathbf{F}(x)$ as long as $\emptyset \notin x$ and $V \setminus x$ has at least two elements. In particular, if there are $|V|$ things that are of size $|V|$ (not containing $\emptyset$) and whose complements are of size $|V|$, then we're OK.

If $\emptyset \notin x$ then $x \times V$ satisfies this. There are $|V|$ things not containing Λ, so there are, in fact, $|V|$ things that are of size $|V|$ (not containing Λ) and whose complements are of size $|V|$ as desired.

$\mathbf{F}$ restricted to these things is 1–1. ($\mathbf{F}^{-1}(x) = \mathbf{F}``x \setminus \{\emptyset\}$) so there are $|V|$ functions from V into V.

The trouble now is that the K combinator cannot be a set. If it were, then $\lambda x.(V \times \{x\})$ would be a set and so would ι. Presumably S can't be a set either, tho' i can't see such a cute proof offhand. The only combinators that ought to be sets in *NF* are those of (polymorphic) type $\alpha \to \alpha$.

Some of this belongs in CHNF.tex

A certain amount of reinvention of the wheel going on here

7.5.2 How easy is it to interpret typed set theory in the typed λ-calculus?

This is related to the question of ascertaining the relative strengths of things like `HOL` and simply typed set theory.

If we can decide on elements 0 and 1 at each type, then we can regard a model of this λ-calculus as an extension of a model of T$\mathbb{Z}$T: we restrict attention to the hereditarily two-valued functions. (A hereditarily 2-valued function is an f such that range$(f) = \{0, 1\}$ and $\forall y. f`y = 1 \to y$ hereditarily 2-valued.) This is o.k. when we have atomic types, for we can take 0 and 1 at the atomic types to be whatever they are, and then procede to define $1_{\beta\to\alpha}$ and $0_{\beta\to\alpha}$ by recursion. This is slightly more delicate than one might think, since we want 1_α and 0_α to be hereditarily two-valued. The definition of $0_{\beta\to\alpha}$ as $\lambda x_\beta.0_\alpha$ is perfectly satisfactory but $1_{\beta\to\alpha}$ has to be λx_β. hereditarily-two-valued$(x) \to 1_\alpha \upharpoonright 0_\alpha$.

It is not clear how to do this when there are no atomic types to start the recursion, but a compactness argument will probably save the day.

To complete the interpretation we will need to have, for each type $\alpha \to \beta$, and each type γ, a λ-term $F_{\alpha\beta\gamma}$: $(\alpha \to \beta) \to \gamma$ such that, for any $t : \alpha \to \beta$, $Ft = 1_\gamma$ iff t is hereditarily two-valued and $= 0_\gamma$ otherwise. Presumably this can be done but not uniformly.

7.5.3 A Conversation with Adam Lewicki on 15/ix/19

We are using Quine pairs, so every set is a set of ordered pairs.

Thus to every set x there corresponds the function $y \mapsto x``y$. This is a rather nice function: $\subseteq$-continuous and determined entirely by what it does to singletons. That is, if i know $x``\{y\}$ for all y then i know the function and i know x. If we write $\mathcal{X}$ for this function we find that, for all y, $\mathcal{X}(y) = \bigcup_{z\in y} \mathcal{X}(\{z\})$.

Now think about this function $x \mapsto \lambda y.x``y$. This is $x \mapsto \{\langle u, y\rangle : u = x``y\}$. As long as we are using Quine pairs '$u = x``y$' is homogeneous so the the function $x \mapsto \lambda y.x``y$ lifts types by 1. So there are $T|V|$ functions that are "image functions" of this kind.

We'd better perform the sanity check of verifying that $x \mapsto \lambda y.x``y$ injective

Next we need to know that every function that satisfies this continuity property is j of something. [But of course that's not true!] So $\mathcal{X}$ is $j(\mathfrak{X})$ for some $\mathfrak{X}$. Observe that, in the displayed formula below, all the things between the arrows are of the same level:

$$\{x\} \mapsto^{(1)} \mathcal{X} \mapsto^{(2)} \{\mathfrak{X}\}$$

Let's consider arrow (1). $\mathcal{X}$ contains ordered pairs $\langle y, x``y\rangle$ and so is one type higher than y – which is the same type as x – so it's on the same level as $\{x\}$. We need to check that distinct xs give rise to distinct $\mathcal{X}$. That is easily done by considering an ordered pair $\langle u, v\rangle$ in the symmetric difference x_1 `XOR` y and considering what x_1 and x_2 do to v (or u if you are writing your ordered pairs the other way round).

Arrow (2) is a bit more work.

Sadly it's not true that any $\subseteq$-cts function that is determined by its values on singleton inputs is j of something. Let f be such a function then it is j of that function that sends u to $\bigcup f(\{u\})$. But $f(\{u\}$ might not be a singleton. Bugger.

So the attempt to prove that there are $|V|$ total functions fails. It might be true anyway of course...

Is it the case that any $\subseteq$-smooth function is $y \mapsto x``y$ for some x? That looks a lot more plausible. Suppose $f(y) = \bigcup_{z \in y} f(\{z\})$. Then consider $x = \{\langle w, u\rangle : w \in y \wedge u \in f(\{z\})\}$.

7.6 Arithmetic in NFU

A message from Ali Enayat

1. $I\Delta_0$ + Exp + $B\Sigma_1$ holds provably in the strongly cantorian natural numbers, provably in Jensen's NFU (I learnt this from Solovay, and the proof is fairly straightforward). The same goes for NF.

2. Jensen's NFU + ($\neg$Inf) is equiconsistent with $I\Delta_0$ + Exp (provably in PA). This is essentially due to Jensen. Solovay in 2002 proved that this equiconsistency is provable in $I\Delta_0$ + Supexp, but not in $I|Delta_0$ + exp.

3. $I\Delta_0$ + Exp + $B\Sigma_1$ does not interpret Jensen's NFU (Solovay, 2002).

4. Provably in PA, Holmes' NFU is equiconsistent with Mac Lane Set Theory, Mac Lane set theory is obtained from Zermelo set theory by weakening the scheme of separation to Δ_0 formulæ. (Jensen for one direction and Hinnion for the other).

5. Also, As shown by Hinnion (and fine-tuned by Holmes), there is an *interpretation* of ZFC   Powerset in Holmes' NFU (the interpretation is well-named: the Zermelian tower). Now, since ZFC   Powerset interprets PA (indeed it even interprets second order arithmetic), *this show in answer to your question that NFU does indeed interpret PA*.

6. Finally, regarding Randall's guess that $I\Delta_0$ + exp is precisely what NFU knows about s.c. natural numbers: perhaps Randall meant to include $B\Sigma_1$, but besides that, I SUSPECT (but details have to be checked)

that the strongly cantorian natural numbers are isomorphic to a cut of natural numbers of the aforementioned Zermelian tower interpretation of ZFC Powerset in NFU. If this is right, then Con(PA) would hold in s.c. natural numbers.

7. Moreover, one of the fascinating facts unearthed by Solovay in his emails was that there is an arithmetical sentence that holds in the s.c. natural numbers of all models of Jensen's NFU (even the ones satisfying $\neg$Inf) that is not provable in $I\Delta_0$ + exp + $B\Sigma_1$ (the proof is complicated, and involves his method of shorterning cuts).

Another message

1. The reason behind $B\Sigma_1$ holding in the strongly cantorian natural numbers is that the strongly Cantorian natural numbers form an initial segment of natural numbers without a last element (i.e., a "cut") that is closed under addition and multiplication in a model of NFU/NF. If this initial segment is a *proper* initial segment of the natural numbers of the ambient model, then it satisfies $B\Sigma_1$ by fact that any cut of a model of $I - \Delta_0$ (Induction for Δ_0 formulae) that is closed under plus and times satisfies $B\Sigma_1$ (so here we need the fact that NFU can prove that the set of natural numbers satisfies $I\Delta_0$ induction).

   On the other hand, if every natural number is strongly cantorian, then they satisfy PA (I think this is due to Rosser). I will add that it is a joint result of myself and Solovay (from around 2002) that the theory (Jensen's NFU) + ($\neg$Inf) + "every cantorian set is strongly cantorian" is equiconsistent with PA (and indeed this extension of Jensen's NFU interprets ACA_0).

2. Albert asked if Jensen's NFU is not finitely axiomatizable. The answer is that Jensen's NFU is finitely axiomaizable for the same reason that Quine's NF is (Thomas and Randall: please correct me if I am wrong since it has been a while since I last thought about this topic).

   Also: I will try to dig up Solovay's example of an arithmetical sentence not provable in $I\Delta_0$ + exp + $B\Sigma_1$ that holds in the strongly Cantorian natural numbers of every model of NFU.

Yet another message

Dear Friends,

I contacted Solovay to receive his permission to share some of his emails relating to "NFU and arithmetic". Below you will find three such.

Please note that what Solovay refers to as S is "Jensen's NFU: + negation of infinity, Exp is $I\Delta_0$ + the exponential function is total, and Supexp is $I\Delta_0$ + the superexponential function is total. Also he uses SC for the model of arithmetic consisting of the strongly cantorian numbers in a model of S.

Another key point to keep in mind (which I will try to elaborate in future emails) is that within a meta-theory that can "do basic model theory", the following two statements are equivalent for a countable model M of $I\Delta_0$ + Exp + $B\Sigma_1$.

(1) There is a model of S whose strongly cantorian numbers are isomorphic to $\mathfrak{M}$.

(2) There is an end extension $\mathfrak{N}$ of $\mathfrak{M}$ such that $\mathfrak{N}$ satisfies $I\Delta_0$, and $\mathfrak{N} \setminus \mathfrak{M}$ contains an element c in which Supexp(c) exists.

By the way, (1) $\rightarrow$ (2) does not need the countabliity of $\mathfrak{M}$ and is due to Solovay (based on an analysis of Jensen's construction). (2) $\rightarrow$ (1) arose from the joint work of Solovay and myself.

All the best,

Ali

Ali,

This series of letters will just be a first pass over the proof omitting various technicalities.

My first goal will be to describe the formula "$J^3(x)$ exists". But before that I have to introduce some of my "private notation".

1. We define the function of two variables $e(n, x)$ thus:

   $e(0, x) = x$;

   $e(n + 1, x) = 2^{e(n,x)}$.

   And we define the stack-of-twos function J thus:

   $J(n) = e(n, 0)$.

   It is a basic [but non-trivial] fact about weak subsystems of arithmetic that there is a Δ_0 formula that [provably in $I\Delta_0$] "adequately" expresses "$y = 2^x$". I believe that this result is presented in detail in the treatise of Hajek and Pudlak on the metamathematics of PA.

   Once one has this under one's belt, it is relatively easy to find Δ_0 predicates expressing "$y = J(x)$" or "$y = J(J(J(x)))$". [Of course, if one can handle one J one can handle 3.]

2. One other minor technical point. In elementary texts [such as Kleene's "Intro. to Metamathematics"] one takes the "numeral" for n [$n \in \omega$] to consist of n succesor symbols followed by the symbol for 0. But I prefer to use a more efficient notation where the numeral for n has length roughly proprotional to log n. Thus since $6 = 2 * 2 + 2$, I would take the numeral for 6 to be:

   $$+xSS0SS0SS0$$

   [Various things are slurred over here: Polish notation; Smullyan notation for digits. The details are not important for this outline and are carefully spelled out in my paper "Injecting Inconsistencies ...".]

3. We start our construction with a non-standard model M of PA. Let n be a non-standard element of M fixed for this discussion.

   We are going to construct a sentence [of non-standard length] that expresses "$J^3(n)$ exists" and we need to be a little pedantic in its construction.

   Let num(n) denote the closed term of length $O(\log\ n)$ which we have previously alluded to as the numeral for n.

   Let $\theta(x, y)$ be the [standard] Δ_0 forumula that expresses "$y = J^3(x)$" as discussed previously.

   Then the sentence we want is:

   $$(\exists x, y)(\theta(x, y) \wedge x = num(n)).$$

   Now consider the theory T = Exp + "$J^3(n)$ exists".

   $\mathfrak{M}$ is a model of T + Con(T).

   We now apply the techniques of my paper on "Injecting inconsistencies ... " to $\mathfrak{M}, T$. The result is a model $\mathfrak{N}$ of T which agrees with $\mathfrak{M}$ on the integers $\leq n$, and which thinks there is a proof of $0 = 1$ in T of length at most $e(1, n)$. $\mathfrak{N}$ will think that there is a slightly larger proof of "$J^3(n)$ does not exist" in Exp. Certainly this second proof will have length $< e(2, n)$.

   The model $\mathfrak{N}_1$ that will instantiate our theorem will be an initial segment of $\mathfrak{N}$. Precisely, this model will consist of those elements of $\mathfrak{N}$ which are less than $e(k, n)$ for some standard k.

   It is evident that $\mathfrak{N}_1$ is a model of Exp. It is perhaps not quite evident that $\mathfrak{N}_1$ thinks Con(Exp). This will follow from the facts that $\mathfrak{N}_1$ is an initial segment of $\mathfrak{N}$ and that $\mathfrak{N}$ thinks that $J^3(n)$ exists. But I will take up that point in the next installment of this letter.

[snip]

Ali,

If I haven't got $\mathfrak{N}$ and $\mathfrak{N}_1$ confused, the situation [in part] is as follows. $\mathfrak{N}$ is a model of Exp; n is a non-standard element of $\mathfrak{N}$; $\mathfrak{N}$ thinks that $J^3(n)$ exists.

$\mathfrak{N}_1$ is the initial segment of N consisting of all elements of $\mathfrak{N}$ that are less than $e(k, n)$ for stome standard k.

It is evident that $\mathfrak{N}_1$ is a model of Exp. Our goal is to show that $\mathfrak{N}_1$ thinks that Con(Exp).

[Although it plays no role in my proof, I could show, if I wanted that $\mathfrak{N}$ itself thinks that Exp is inconsistent.]

We first reduce the proof to the following lemma. We shall then discuss the proof of the lemma but I shall not, in this pass, prove it in all detail.

The lemma that follows can be proved in Exp:

Lemma 1: Let π be a proof of 0=1 in Exp of length m. Then $J^2(4m + 1)$ does not exist.

Some remarks.

1. I could improve this to $J^2(m)$. The $4m+1$ is an artefact of my relying on the presentation of Herbrand's thm. in the paper of Paris and Dimitracopoulos.

2. The lemma is a slight sharpening of the fact that Superexp proves Con(Exp).

   From the lemma the fact that $\mathfrak{N}_1$ is a model of Con(Exp) follows easily. Indeed, if π is a proof of length $< e(k,n)$ where k is standard, we have but to observe that $J^2(4e(k,n)+1) < J^3(n)$. This follows from the fact that

3. $e(k,n)+1 < e(k+1,n) < J(n)$, which is evident, since n is non-standard.

I am going to save my discussion of the proof of the lemma to the next installment. I'm not sure if I will get that installment written before I leave Edinburgh [on Saturday morning]. It is currently Thursday night.

I remark that so far, the proof has had nothing to do with S. Of course, S will appear presently. The crucial lemma that I need about S is that the following fact is provable in Exp.

For a certain standard integer m_0, we have: if $m \geq m_0$, then there is a proof in S that $J^3(m)$ exists and is Cantorian whose length is less than 2^m. [It's actually $O(m^2)$ in length where the constant implicit in the O(.) notation is again some specific standard integer.]

A counterexample

This example is a little involved. it also relies on a theorem of Pudlak which I hope I'm recalling correctly. [The theorem in question should be in the paper of Pudlak which you emailed to me.]

Theorem: There is a sentence Φ such that:

(1) Φ holds in the model SC of any model of S.

(2) There is a model of Exp + $B\Sigma_1$ + not-Φ.

Here we go. I need the formulas I_n that I constructed in my letter "Reasoning in S: III".

Φ will assert: "There is no proof of $0=1$ in Exp whose Godel number lies in I_{1000}".

I need the following result. With "some standard k" rather than 1000, I should prove it in a letter to you in a couple of days.

Lemma 1: Exp proves : If n is the Godel number of a proof of $0=1$ in Exp then $J^2(e(1000,n))$ does not exist.

[It's certainly true with $k = 1000$; alternatively you can replace references to 1000, 2000 in what follows by references to k and k' where k and k' are standard and $k' >> k >> 0$.]

Let's first prove "Φ holds in SC" in S. Well, if not let n be as given by the negation of Φ. Then $e(1000,n)$ lies in I_0^{SC}. So $J(e(1000,n))$ exists in SC.

By the main result of my first two letters on $S, J^2(e(1000, n))$ exists in AC. But this contradicts Lemma 1 since AC is a model of Exp.

The construction of a model of Exp + $B\Sigma_1 + \neg\Phi$ will be more difficult.

Working in Exp we define a cut I^* as follows:

If I_0 is closed under Exp, then I^* is the set of y such that $J(y)$ is in I_0; if I_0 is not closed under Exp, I^* is just I_0.

We define a series of cuts I^*_n from I^* much as we defined the I_n from I_0.

We now invoke a theorem of Pudlak: There is a model $\mathfrak{M}_0$ of Exp and an integer n such that:

(1) n lies in the cut I^*_{2000}.

(2) n is the Godel number of a proof of $0 = 1$ in Exp.

Claim 1: In $\mathfrak{M}_0, I_0$ is not all of $\mathfrak{M}_0$.

Proof: If it were, $\mathfrak{M}_0$ would be a model of SuperExp; but this contradicts property (2) of n.

Claim 2: In $\mathfrak{M}_0, I_0$ is not closed under Exp.

Suppose it is. Then $e(1000, n)$ is in I^* and since I_0 is closed under Exp, $J(e(1000, n)) \in I_0$. Hence, by the definition of $I_0, J^2(e(1000, n))$ exists. But this gives a contradiction via Lemma 1 and property (2) of n.

Claim 3: I_1 is a proper subcut of I_0.

This follows immediately from Claim 2.

Define an initial segment of $\mathfrak{M}_0$, call it $\mathfrak{M}_1$, as follows:

x is in $\mathfrak{M}_1$ if there is a $y \in I_1$ such that $x < J(y)$.

It is immediate that $\mathfrak{M}_1$ is a model of Exp. By claim 3, $\mathfrak{M}_1$ is a proper initial segment of $\mathfrak{M}_0$. So $\mathfrak{M}_1 \models B\Sigma_1$.

It is clear that I_0 as computed in $\mathfrak{M}_1$ is just I_1 as computed in $\mathfrak{M}_0$.

It follows [using the explicit definitions of the I_j's as given in a recent letter] that I_j as computed in $\mathfrak{M}_1$ is just I_{j+1} as computed in $\mathfrak{M}_0$.

In particular, $\mathfrak{M}_1$ thinks that there is a proof of $0 = 1$ from Exp in its I_{1000}.

So what S knows about SC is not just Exp + $B\Sigma_1$. Perhaps the correct answer is close at hand; perhaps not.

–Bob

On Oct 9 2019, Visser, A. (Albert) wrote:

Dear Ali,

Your question takes me back to the early years of this millennium, when I was intensely corresponding with Solovay about NFU. In order to order to answer your question let first point out that NFU is used in the literature for two related theories, one much weaker than the other:

A. Jensen's NFU (as in his 1968 Synthese paper on the subject) is the result of weakening Quine's NF by weakening the axiom of extensionality so as to allow urelements. In contrast to NF in which the axiom of infinity (Inf from now on) is provable, Jensen's NFU is demonstrably indecisive about Inf (as noted by

Jensen). So we get two natural consistent extensions of NFU, namely NFU + Inf, and NFU + Inf.

B. Holmes's NFU, on the other hand, is a natural extension of Jensen's NFU that includes Inf, as well as a type-level-pairing function.

Ah. I did not realise this.

This is a summary of what I know about "arithmetic" and NFU (with the proviso that it has been about a decade since I last worked on the subject).

1. $I\Delta_0$ + Exp + $B\Sigma_1$ holds provably in the strongly cantorian natural numbers, provably in Jensen's NFU (I learnt this from Solovay, and the proof is fairly straightforward). The same goes for NF.

The union of T+A already understood that it should contain $I\Delta_0$+Exp.

Q1: The $B\Sigma_1$ is since there is a cantorian number above the strong cantorian ones?

2. Jensen's NFU + ($\neg$Inf) is equiconsistent with $I\Delta_0$ + Exp (provably in PA). This is essentially due to Jensen. Solovay in 2002 proved that this equiconsistency is provable in $I\Delta_0$ + Supexp, but not in $I\Delta_0$ + exp.

Ah.

Q2: Is the cutfree equiconsistency provable in EA? [One of Ali's answers below shows that it does not.]

Q3: If I am not mistaken $I\Delta_0$ + Supexp proves the consistency of $I\Delta_0$ + Exp, so it must also prove the consistency of NFU$_j$ + $\neg inf$. Right?

Q4: Is it not true that NFU$_j$ + $\neg inf$ should interpret PA: on the cantorian numbers, plus and times work, so if Cantorian successor is total, then we have PA on the Cantorian numbers (since the interpretation is stratified). If that is so then NFU$_j$+ inf proves con(NFU$_j$) on the Cantorian numbers and hence on the strongly Cantorian numbers. Then, by G2, NFU$_j$+inf $\vdash$ con$^{\mathsf{sc}}$(NFU$_j$+incon$^{\mathsf{sc}}$(NFU$_j$)). So NFU$_j$+inf $\vdash$ con$^{\mathsf{sc}}$(NFU$_j$+$\neg inf$). Hence, by the interpretation existence lemma, NFU$_j$+inf interprets NFU$_j$+ $\neg$inf. Since, also NFU$_j$+$\neg$inf interprets NFU$_j$+$\neg$inf, we find, using a disjunctive interpretation, that NFU$_j$ interprets NFU$_j$ + $\neg$inf. So NFU$_j$ is mutually interpretable with NFU$_j$+$\neg$inf. This argument is undoubtedly verifiable in $I\Delta_0$+Exp.

But does inf indeed imply that Cantorian successor is total? If not, a version of the argument should work with inf replaced by inf plus the existence of Holmes' pairing function.

Q5: It should be true that NFU$_j$ is not finitely axiomatizable, right?

3. $I\Delta_0$ + Exp + $B\Sigma_1$ does not interpret Jensen's NFU (Solovay, 2002).

That answers Q2.

Q6: Does it locally or even model interpret it?

If it locally interprets it, then $I\Delta_0$ + Exp+ cutfreecon($I\Delta_0$ + Exp) interprets NFU$_j$. Of course that is a stronger theory.

4. Provably in PA, Holmes' NFU is equiconsistent with Mac Lane Set Theory, Mac Lane set theory is obtained from Zermelo set theory by weakening the scheme of separation to Δ_0 formulae. (Jensen for one direction and Hinnion for the other).

5. Also, As shown by Hinnion (and fine-tuned by Holmes), there is an interpretation of ZFC $\setminus$ Powerset in Holmes' NFU (the interpretation is well-

named: the Zermelian tower). Now, since ZFC \ Powerset interprets PA (indeed it even interprets second order arithmetic), this show in answer to your question that NFU does indeed interpret PA.

6 Finally, regarding Randall's guess that $I\Delta_0$ + exp is precisely what NFU knows about s.c. natural numbers: perhaps Randall meant to include $B\Sigma_1$, but besides that, I SUSPECT (but details have to be checked) that the strongly cantorian natural numbers are isomorphic to a cut of natural numbers of the aforementioned Zermelian tower interpretation of ZFC \ Powerset in NFU. If this is right, then Con(PA) would hold in s.c. natural numbers.

Nice.

7. Moreover, one of the fascinating facts unearthed by Solovay in his emails was that there is an arithmetical sentence that holds in the s.c. natural numbers of all models of Jensen's NFU (even the ones satisfying Inf) that is not provable in $I\Delta_0$ + exp + $B\Sigma_1$ (the proof is complicated, and involves his method of shorterning cuts).

I do not know whether I already collected enough knowledge to guess what that sentence is.

Best wishes,

Albert

Chapter 8

Models in the Ordinals

Lemma 8 *If $\langle M, \in\rangle$ is an initial segment of a nonstandard model of $Z + V = L$, with an endomorphism T, and for some fixed initial ordinal $\Omega > T\Omega$ then the T-fixed points below Ω give rise to a model of Z.*

Proof:

We shall assume that sets can be identified with ordinals so that we can concern ourselves only with ordinals. The least member of a fixed set must also be fixed, so the fixed sets satisfy extensionality (even though a fixed set may have some members that are not fixed). Indeed we can show that the fixed ordinals are an elementary substructure of the ordinals for which $T^n x$ is defined for all $n \in \mathbb{Z}$. If α is an initial ordinal that is fixed, then clearly the next initial ordinal after α is likewise fixed, so the fixed sets will be a model for power set. The fixed sets certainly satisfy the axiom of infinity since ω is fixed. Sumset is straightforward. What is not at all obvious is that the fixed sets are a model of aussonderung. We want to show that if x is a fixed set (a set coded by a fixed ordinal), then $x \cap \{y : \Psi(x, y)\}$ is fixed (coded by a fixed ordinal) *even if Ψ contains quantifiers restricted to fixed sets (ordinals)*. Why might this be true? The obvious problem is that T is not part of the language in which we have aussonderung, so we have to show that occurrences of T can be removed from Ψ.

Let us say Ψ is *good* if the statement

$$y = Ty \in x \ \wedge \ x = Tx \ \wedge \ \vec{z} = T\vec{z} \ \wedge \ \Psi(y, \vec{z})$$

is equivalent to

$$y = Ty \in x \ \wedge \ x = Tx \ \wedge \ \vec{z} = T\vec{z} \ \wedge \ \Psi'(y, \vec{z})$$

for some Δ_0^{Levy} formula Ψ' in which all parameters (including those possibly not appearing in Ψ) are fixed. The significance of good formulæ is as follows. Suppose $\Psi(y, \vec{z})$ is good and that $x = Tx$ and $\vec{z} = T\vec{z}$. Then $\{y \in x : \Psi'(y, \vec{z})\}$, which is a set of the model (is coded by an ordinal), has the same *fixed* members as $\{y \in x : \Psi(y, \vec{z})\}$ (though we know nothing about its unfixed members). This is sufficient to verify this instance of aussonderung in the model of fixed sets, for $\{y \in x : \Psi'(y, \vec{z})\}$ is fixed (since all its parameters are fixed) and therefore will be in the model we are interested in: the model of fixed sets. Its (possibly aberrant) unfixed members do not concern us.

We want to show by induction that all formulæ are good. Evidently any Δ_0^{Levy} formula Ψ with all parameters fixed is good, and to show that all formulæ are good it will suffice to deal with negation and $\exists$.

Negation

$\neg\Psi$ is good if Ψ is. This is because the statement $p \wedge q \wedge r \wedge \neg s$ is $p \wedge q \wedge r \wedge \neg(p \wedge q \wedge r \wedge s)$. Accordingly

$$y = Ty \in x \wedge x = Tx \wedge \vec{z} = T\vec{z} \wedge \neg\Psi(y, \vec{z})$$

is equivalent to

$$\neg(y = Ty \in x \wedge x = Tx \wedge \vec{z} = T\vec{z} \wedge \Psi(y, \vec{z})) \wedge y = Ty \in x \wedge x = Tx \wedge \vec{z} = T\vec{z}$$

which is

$$\neg(y = Ty \in x \wedge x = Tx \wedge \vec{z} = T\vec{z} \wedge \Psi'(y, \vec{z})) \wedge y = Ty \in x \wedge x = Tx \wedge \vec{z} = T\vec{z}$$

for some suitable Ψ' by induction hypothesis, which is equivalent to

$$y = Ty \in x \wedge x = Tx \wedge \vec{z} = T\vec{z} \wedge \neg\Psi'(y, \vec{z}).$$

Existential Quantification

Consider

$$y = Ty \in x \wedge x = Tx \wedge \vec{z} = T\vec{z} \wedge (\exists w = Tw)\Psi(y, \vec{z}, w).$$

where Ψ is good. We can take the existential quantifier outside to get

$$(\exists w = Tw)(y = Ty \in x \wedge x = Tx \wedge \vec{z} = T\vec{z} \wedge \Psi(y, \vec{z}, w))$$

and since Ψ is good this is

$$(\exists w = Tw)(y = Ty \in x \wedge x = Tx \wedge \vec{z} = T\vec{z} \wedge \Psi'(y, \vec{z}, w))$$

for some Δ_0^{Levy} formula Ψ' in which all parameters (including those possibly not appearing in Ψ) are fixed. Take the existential quantifier inside again:

$$y = Ty \in x \wedge x = Tx \wedge \vec{z} = T\vec{z} \wedge (\exists w = Tw)\Psi'(y, \vec{z}, w)$$

Now, for any x, y, $\vec{z}$ that are all fixed, the set X of witnesses to the existential quantifier is closed under T and T^{-1} in the sense that if it contains α, it contains $T\alpha$, and conversely[1]. Consider the minimal element ('κ' for the nonce) of X.

[1]... though not necessarily in the sense that if it contains α it must contain $T^{-1}\alpha$, for that might not exist.

$\kappa \leq T\kappa$ by minimality but, since $\kappa \leq T\kappa$, $T^{-1}\kappa$ is defined and so $\kappa \leq T^{-1}\kappa$; whence $\kappa = T\kappa$, so at least some of these w are fixed. There is also an upper bound on how far we have to look to find these witnesses. Consider the sup of the ordinals w' that for some $y' \in x$ are minimal such that $\Psi'(y', \vec{z}, w')$. We want to be sure that this sup is fixed, or at least has a fixed ordinal above it. Since Ψ' is Δ_0^{Levy} we can be sure that the first w' such that $\Psi'(y', \vec{z}, w')$ will be in L_{α^+}, where x and $\vec{z}$ are all in L_α.[2] That means that each minimal w' is dominated by a fixed ordinal. Now either this ordinal is the *same* for all of them (which is what we want) or it is not. If it is not, then the set of such w' (and it is a *set*) is cofinal in the fixed ordinals, and therefore the set of ordinals dominated by a fixed ordinal is a set. This is impossible, for otherwise we would be able to prove by induction that all ordinals are dominated by fixed ordinals. Therefore we are in the first case, and the sup of the ordinals w' that for some $y' \in x$ are minimal such that $\Psi'(y', \vec{z}, w')$ is either fixed or is dominated by a fixed ordinal; so we can introduce the notation '$\zeta(x, \vec{z})$' for a fixed ordinal that bounds the ordinals w' that, for some $y' \in x$, are minimal such that $\Psi'(y', \vec{z}, w')$. Since the least witness must be fixed, we can drop the condition $w = Tw$ and since witnesses must appear below $\zeta(x, \vec{z})$ if they appear at all, we can add the bound $< \zeta(x, \vec{z})$ to make the formula Δ_0^{Levy} again, getting

$$y = Ty \in x \wedge x = Tx \wedge \vec{z} = T\vec{z} \wedge (\exists w < \zeta(x, \vec{z}))\Psi'(y, \vec{z}, w)$$

which is Δ_0^{Levy}, and all parameters are fixed. ■

8.0.1 Making the ordinals look like L

Lemma 9 *There is a relation E definable on NO so that $\langle NO, E\rangle \models V = L$.*

Proof. We will mimic Gödel's construction inside NO. Since the Gödel construction of L also builds a bijection between V and On, one can copy over the $\in$ relation on L onto On, or better still, construct a "membership" relation on On itself and never bother to construct L at all. This is what we will do here. Order the set of all triples $\langle \alpha, \beta, i\rangle$, with $\alpha, \beta \in NO$, $0 \leq i \leq 8$ in the order-type of $\langle NO, \leq\rangle$, so that no triple appears earlier than any of its components. There is a standard construction that will eventually do this for us. Consider the canonical map used to show that if $\langle X, R\rangle$ is a wellordering of order-type $\alpha \geq \omega$, then $X \times X$ can be wellordered naturally to order-type α. We use it to wellorder $NO \times NO$ in the order-type of the ordinals. Evidently each pair of ordinals comes later than its components. We repeat the trick twice to get a wellordering of $NO^3 \times \{0, 1, 2, 3, 4, 5, 6, 7, 8\}$ as desired.

There is now a function g defined on NO so that $(\forall \alpha \in NO)(g(\alpha)$ is a triple whose components are all less than $\alpha)$. (Tho' i think this should be "less than or equal to α") This much is standard. Note that g and the corresponding projection functions are all definable and will therefore commute with any auto- or endo- morphisms of NO. This will be important later.

We can now define a relation $\mathcal{E}$ between ordinals by recursion as follows:

[2] I am endebted to James Cummings for untangling my thoughts on this matter.

1. $(\forall \alpha \in NO)\neg(\alpha\,\mathcal{E}\,0)$.
2. If the third component of $g(\alpha)$ is 0, then set $\delta\,\mathcal{E}\,\alpha$ iff $\delta < \alpha$.
3. If $g(\alpha)$ is $\langle \beta, \gamma, 1\rangle$, then set $\delta\,\mathcal{E}\,\alpha$ iff $\delta\,\mathcal{E}\,\beta \vee \delta\,\mathcal{E}\,\gamma$;
4. If $g(\alpha)$ is $\langle \beta, \gamma, 2\rangle$, then set $\delta\,\mathcal{E}\,\alpha$ iff there are ζ and η such that $\delta = \langle \zeta, \eta\rangle$ in the sense of $\langle NO, \mathcal{E}\rangle$ and $\delta\,\mathcal{E}\,\beta$ and $\eta\,\mathcal{E}\,\zeta$;
5. If $g(\alpha)$ is $\langle \beta, \gamma, 3\rangle$, then set $\delta\,\mathcal{E}\,\alpha$ iff $\delta\,\mathcal{E}\,\beta \ \wedge \neg(\delta\,\mathcal{E}\,\gamma)$;
6. If $g(\alpha)$ is $\langle \beta, \gamma, 4\rangle$, then set $\delta\,\mathcal{E}\,\alpha$ iff there are ζ and η such that $\delta = \langle \zeta, \eta\rangle$ in the sense of $\langle NO, \mathcal{E}\rangle$ and $\delta\,\mathcal{E}\,\beta$ and $\eta\,\mathcal{E}\,\gamma$;
7. If $g(\alpha)$ is $\langle \beta, \gamma, 5\rangle$, then set $\delta\,\mathcal{E}\,\alpha$ iff $\delta\,\mathcal{E}\,\beta$ and, for some η, the ordered pair $\langle \eta, \delta\rangle$ (in the sense of $\langle NO, \mathcal{E}\rangle$) $\mathcal{E}\,\gamma$;
8. If $g(\alpha)$ is $\langle \beta, \gamma, 6\rangle$, then set $\delta\,\mathcal{E}\,\alpha$ iff $\exists\eta\zeta\ \delta = \langle \eta, \zeta\rangle$ (in the sense of $\langle NO, \mathcal{E}\rangle$) $\wedge\, \delta\,\mathcal{E}\,\beta \wedge \langle \zeta, \eta\rangle$ (in the sense of $\langle NO, \mathcal{E}\rangle$) $\mathcal{E}\,\gamma$;
9. If $g(\alpha)$ is $\langle \beta, \gamma, 7\rangle$, then set $\delta\,\mathcal{E}\,\alpha$ iff $\exists\eta\zeta\chi\ \delta = \langle \eta, \zeta, \chi\rangle$ (in the sense of $\langle NO, \mathcal{E}\rangle$) and $\delta\,\mathcal{E}\,\beta$ and $\langle \eta, \chi, \zeta\rangle$ (in the sense of $\langle NO, \mathcal{E}\rangle$) $\mathcal{E}\,\gamma$;
10. If $g(\alpha)$ is $\langle \beta, \gamma, 8\rangle$, then set $\delta\,\mathcal{E}\,\alpha$ iff $\exists\eta\zeta\chi\ \delta = \langle \eta, \zeta, \chi\rangle$ (in the sense of $\langle NO, \mathcal{E}\rangle$) and $\delta\,\mathcal{E}\,\beta$ and $\langle \chi, \eta, \zeta\rangle$ (in the sense of $\langle NO, \mathcal{E}\rangle$) $\mathcal{E}\gamma$.

[*HOLE Pittsburgh march 2003: I'm not entirely happy about this. What do we do if one of the components of $g(\alpha)$ turns out to be α itself? And are we not supposed, every now and then, to set $\delta\,\mathcal{E}\,\alpha$ if $\delta < \alpha$?*]

In the standard construction (from which these clauses are of course copied wholesale) the fact that one might construct the same set several times is of no matter. Here it means that we have to remove duplicate ordinals by a recursive collapsing construction. If you have a pile of ordinals that have the same members-in-the-sense-of $\mathcal{E}$, discard all but the first, and cause it to $\mathcal{E}$ all the ordinals that the discarded ordinals were $\mathcal{E}$-members of. Since, for any ordinal, there comes some stage beyond which do not alter what its "members" are, this can be defined by a legitimate recursion. When this has finished, we have a definable set of ordinals, and a definable relation on it. We can now recursively collapse this set of ordinals so that $\mathcal{E}$ is defined on *all* ordinals (with the same caveat) so that we have, in effect, merely pulled back (*via* the standard enumeration) onto the ordinals the membership relation of L.

8.0.2 Nonstandard Models of $Z + V = L$

The hot topic now is, when can we show that this $\mathcal{E}$ relation on the bottom level gives rise to a model of Z? Well, if we know that there is no last initial ordinal i would guess that the answer is "yes" (tho' i can imagine it would be hard work proving it). I would guess also that if X is any initial segment of NO (even a class) containing no last initial ordinal then $\langle X, \mathcal{E}\rangle \models Z$. Assuming that this is all we have to do in order to obtain models of Z, we next consider how to obtain such classes.

Let us now think about T again. It embeds the ordinals of one level onto an initial segment of the ordinals of the next. But all these ordinals at higher types are supposed to be ordinals too. Therefore we might expect there to be models of this structure where the ordinals at higher types reappear in NO. Let us call these models *special*. To be precise, a special model is one where the ordinals of any higher level are simply (externally isomorphic to) an initial segment of NO. But we always have T which is an isomorphism between the ordinals of one level and an initial segment of the ordinals of the next level. In a special model therefore, T will manifest itself as an *endomorphism* of NO. That is to say, in a special model there will be an initial segment of NO that is an isomorphic copy of it. That is all we know about the endomorphism.

Presumably it is not too hard to find special models of nOA. In NF however there is a particularly simple special model, and it is simply the natural one built up from the set of all (Russell-Whitehead) ordinals as NO.

In fact all we are really interested in is finding a nonstandard model of nOA where NO is (externally) isomorphic to an initial segment of itself and no initial ordinal fixed by the isomorphism is the last initial ordinal. Such a model has[3] all the features we need to prove $Con(NF) \to Con(Z)$. Suppose we have such a model of nOA with an endomorphism π as above. π obviously fixes all definable things (like ω, ω_1 and so on.) and commutes with definable relations like $\mathcal{E}$. Now consider the collection of those ordinals which have an ordinal above them that is fixed by π[4]. Once we equip this class (for it is never a set of the model!) with the appropriate restriction of $\mathcal{E}$ *is the result a model of* Z*?*

Claim: yes!!!

Secret Agenda

I have been assuming that what we start with as *urelemente* is the set of all ordinals, and that this is much the same as starting with V and ignoring the bits we don't want. This second way you get exactly the same ordinals at each type and T is an injection. It is not entirely clear what happens if you do *literally* what i say. We need to compare Ω and $\aleph`(2^{T\Omega})$. Henson has a theorem on this.

It looks as if – if this strategy is likely to work at all – all we will need is the ambiguity scheme for ordinal arithmetic. Now isn't all arithmetic $\Delta_1^{\mathcal{P}}$ or something nice? And isn't $Amb(\Sigma_1^{\mathcal{P}})$ relatively consistent wrt $TSTI$? So if this worked we could do it relative to TST, so it doesn't work . . .

8.0.3 A message from Adrian

Consider the following model, using ideas of McAloon: start in ZF+V=L; add for each n in a set A to be determined a Cohen subset of $\aleph_n$; (where that will

[3] A brave prediction!
[4] Or possibly the collection of fixed ordinals – who cares which?

mean a non-constructible subset every initial segment of which is constructible); so that A will be definable in the resulting model; rig it so that 0 is not in A (so no new reals are added in a straightforward way) but so that A is not constructible; look at the things constructible before $\aleph_\omega$ from the sequence of Cohen subsets; then this looks like a plausible model in which comprehension fails: plausible means every initial segment of the model is jolly nice, but at $\aleph_\omega$ a new subset of ω becomes definable, and it won't have time to exist before the model closes off, ha ha.

I don't know what this shows, but it suggests that there is a real difficulty about proving full Z holds without being able to take a step after $\aleph_\omega$.

I'll also have a go at the following: let ζ be the least ordinal such that $L_\zeta \models$ MacLane. Now fatten it like mad in a non-AC way to get all of Zermelo to hold.

Kemeny has written to say his thesis was never published. I wonder if by chance there is a copy in Cambridge. It was called type theory versus set theory.

Adrian

Chapter 9

Antimorphisms, a Jumble

Sse σ is a polarity. What is $\sigma(\sigma)$?

$\sigma(\sigma) = V \setminus \sigma``\sigma$

$V \setminus \sigma(\sigma) = \sigma``\sigma$

$\sigma``\sigma = c``\sigma^{\sigma} = c``\sigma$ so

$\sigma(\sigma) = V \setminus c``\sigma$

which is OK We assume familiarity with Rieger-Bernays permutation models and Ehrenfeucht games.

DEFINITION 5

Let us call an antimorphism of order two a **polarity**.

A Boffa atom is an $x = B(x)$. ($B(x)$ is $\{y : x \in y\}$.) Hereafter Boffa atoms are batoms. *$\overline{Bx}$ is of course $V \setminus B(x)$.*

c is the complementation permutation: $c(x) = V \setminus x$.

$\mathbb{1}$ is the identity element of the symmetric group on the universe.

A moiety is a set the same size as its complement.

We start with some banal observations, whose proofs we leave to the reader.

LEMMA 10

1. *σ is an automorphism if $\sigma = j\sigma$; it is an antimorphism if $\sigma = j\sigma \cdot c$ (or equivalently $\sigma = c \cdot j\sigma$, beco's c commutes with $j\tau$ for all permutations τ.)*
2. *AC_2 implies that two involutions that fix the same number of things and move the same number of things are conjugate.*
3. *$V^{\pi} \models \sigma$ is an antimorphism iff $\pi\sigma\pi^{-1} = c \cdot j\sigma$*
4. *If σ is an antimorphism then σ^2 is an automorphism.*
5. *If σ is an automorphism and $n = Tn$ then σ^n is an automorphism.*
6. *If $\sigma = j\sigma$ and σ is of order n then $n = Tn$.*

7. *No antimorphism can have a fixed point.*
8. *The composition of an automorphism and an antimorphism is an antimorphism;*
9. *The composition of an antimorphism and an antimorphism is an automorphism;*
10. *The inverse of an antimorphism is an antimorphism; the inverse of an automorphism is an automorphism.*
11. *If* $\sigma^n = 1\!\!1$ *then* $(j\sigma)^{Tn} = 1\!\!1$ *and* vice versa*, so every automorphism has cantorian order.*
12. *If* σ *is an antimorphism,* σ^2 *is an automorphism and has cantorian order,* n*, say. So the order of* σ *must be either* n *or* $2n$*. Either way it is cantorian.*
13. *If* σ *is an automorphism and* $n = Tn$ *then* σ^n *is also an automorphism.*
14. *Let* σ *be an antimorphism – of order* n *– and* 2^k *be the largest power of 2 dividing the order of* σ*. Then* 2^k *is cantorian (being the largest power of 2 dividing a cantorian number). So* σ^{2^k} *is an automorphism and* $\sigma^{n/2^k}$ *is an antimorphism, in fact a polarity.*

These are left as exercises for the reader.

Actually some of these *exercises for the reader* are sufficiently hard to merit model answers. Anyone who writes a textbook should have somewhere at the back of their mind the thought that if it's fair to ask your readers to write out an answer for the good of their souls you should be capable of writing out such an answer on your own account. *Hic Rhodus, hic salta*[1]. In this spirit we look at item 11: "Every automorphism is of cantorian order."

We infer this from

$$(j\tau)^{Tn} = j(\tau^n)$$

thus. Substituting $\tau/j\tau$ (since τ is an automorphism) we get

$$\tau^{Tn} = j(\tau^n)$$

and next substituting $1\!\!1/\tau^n$ we get

$$\tau^{Tn} = j(1\!\!1).$$

Of course $j(1\!\!1) = 1\!\!1$, so we get

$$\tau^{Tn} = 1\!\!1$$

as desired

But how did we get $(j\tau)^{Tn} = j(\tau^n)$? We prove it by UG on 'τ' and induction on 'n' of course. Now if we are to prove it by induction it had better be stratified,

[1] Latin for: *Put up or shut up.*

so let's check that. I think there is a real expository problem here, and this is the point at which i feel i haven't found the right thing to say.

Watch my back...

We can think informally of the two sides of the equation as arising from the two results of doing something (namely composition) to a list of copies of τ (on the RHS) and a list of copies of $j\tau$ (on the LHS). j is an isomorphism between one list and the other. Now j is a type-raising operation, so it might change the size of a list – just as ι does. So the length of the list (the LHS) of js of τ's will be T of the length of the list (the RHS) of copies of τ.

Is the right thing to say to a beginner?

Ad 13 i'm wondering if we need the exponent to be cantorian. Is not every power of an automorphism an automorphism? It would help if we could show that the order of any automorphism is strongly cantorian.

We reason as follows. First we show that every cycle of a power of an automorphism is cantorian. Every cycle of a power of σ is a subset of a σ-cycle. Such a cycle may be the same as a σ-cycle (in a different order) or it might be a subset of it – a result of splitting a σ-orbit into pieces all the same size. Suppose σ is an automorphism of order $p \cdot q$. Consider σ^p, and let X be a σ-cycle. Starting at a random $x \in X$, σ^p picks out q evenly spaced vertices from the polygon that is the cycle X. Call this subset X_x. Think about what $j\sigma$ does to X_x. X_x belongs to a $j\sigma$ orbit, and this orbit is cantorian. This tells us that q was cantorian. But $p \cdot q$ was cantorian so p is cantorian. So every factor of the order of σ is cantorian and every cycle of a power of σ is also cantorian.

Not sure what to do next. It would be handy if n is strongly cantorian as long as all its factors are cantorian but that's not going to be true. Consider an automorphism of order p, p a nonstandard cantorian prime.

9.1 Introductory Patter

9.1.1 First Impressions of an Antimorphism

We noted above that σ is an antimorphism iff $\sigma = c \cdot j\sigma$.

This by itself is sufficient information to compute what σ does to all wellfounded sets: just set $\sigma(x) =: V \setminus \sigma``x$. This immediately gives $\sigma(\emptyset) = V$ and $\sigma(V) = \emptyset$, and lots more by recursion. Observe that this recursion defines the restriction to wellfounded sets uniquely, so that any two antimorphisms agree on wellfounded sets, and their restriction is a polarity. Some more data points:

$\sigma(\{\emptyset\}) = V \setminus \{V\}$;
$\sigma(\{V\}) = V \setminus \{\emptyset\}$;
$\sigma(\{\emptyset, V\}) = V \setminus \{V, \emptyset\}$;
$\sigma(\{\{\emptyset\}\}) = V \setminus \{V \setminus \{V\}\}$.

Remark 31 *Any two antimorphisms agree on all wellfounded sets.*

Proof: Let σ and τ be two antimorphisms. '$(\forall x)(\sigma(x) = \tau(x))$' is stratified so $\{x : \sigma(x) = \tau(x)\}$ is a set. All we have to do is show that it extends its own power set (is *fat* as we say). Then

$$\sigma(x) \;=\; V \setminus \{\sigma(y) : y \in x\} \;=\; V \setminus \{\tau(y) : y \in x\} \;=\; \tau(x).$$

■

9.1.2 The Duality Scheme

Let $\phi^{\circlearrowleft}$ for ϕ a formula in the language of set theory be the result of replacing '$\in$' by '$\not\in$' throughout in ϕ. The question is whether or not the scheme of biconditionals $\phi \longleftrightarrow \phi^{\circlearrowleft}$ is consistent relative to NF.

We've known for a long time that the stratified instances of this scheme are actually *provable*. This is because $\phi^{\circlearrowleft}$ is just ϕ^c (which is stratified iff ϕ is stratified) and of course stratified sentences are invariant.

By a remark of Specker's (with a correction by Chad Brown) a finite conjunction of biconditionals $\phi \longleftrightarrow \phi^{\circlearrowleft}$ is logically equivalent to another such biconditional.

By general model-theoretic nonsense if we have a model of NF satisfying the duality scheme then we can find a model with an external antimorphism.

But what about an *internal* antimorphism? One that is a set of the model?

One would expect to be able to prove by induction on n that if σ is an antimorphism then σ^n is an antimorphism if n is odd and an automorphism if n is even. However the induction is unstratified and cannot be performed. Nevertheless we can do the following:

PROPOSITION 2

1. *If σ is an automorphism then the order of σ is infinite or a cantorian natural;*

2. *If σ is an antimorphism then the order of σ is infinite or is a cantorian natural;*

3. *If σ is an antimorphism then the order of σ is infinite or is even.*

Proof:

1. If σ is of order n then $j\sigma$ is of order Tn.

2. If σ is an antimorphism then σ^2 is an automorphism and has cantorian order, n say. Then the order of σ must be n or $n/2$. Either way it's cantorian (or infinite).

3. Let σ be an antimorphism, and suppose $\sigma^{2n+1} = \mathbb{1}$. Then

$$c =^1 c^{T2n+1} =^2 (j\sigma)^{T2n+1} \cdot c^{T2n+1} =^3 (j\sigma \cdot c)^{T2n+1} =^4 \sigma^{T2n+1}$$

1 holds because c is an involution;
2 holds because $j\sigma^{T2n+1} = \mathbb{1}$;
3 holds because c commutes with j of anything;
4 holds because $\sigma = j\sigma \cdot c$.

So $\sigma^{T2n+1} = c$ whereas $\sigma^{2n+1} = \mathbb{1}$. Clearly $T2n + 1 \neq 2n + 1$.

But we know by (2) that the order of an antimorphism must be cantorian.

■

9.1.3 Permutation Methods: getting embroiled with AC_2

Can we get antimorphisms by permutation methods? It's simple enough to get a permutation model containing a (non-trivial) $\in$-automorphism, at least if we have AC_2: all we have to do is find a permutation π such that π and $j\pi$ are conjugate. We need a bit of choice to show that any two permutations with the same cycle type are conjugate. (Choice for arbitrary sets of (finite-or)-countable sets.) It's easy to find an involution π such that π and $j\pi$ have the same cycle type (= fix the same number of things and move the same number of things, in this case all four of these sets are moieties) and we need AC_2 to make π and $j\pi$ conjugate.

But *anti*morphisms? To obtain an antimorphism in a permutation model we need to find a permutation σ which is conjugate to $j(\sigma)\cdot c$. To keep things simple let us for the moment assume that we are trying to obtain an antimorphism of order 2, a polarity. That way we should need only AC_2, not the more general form. But we run up against the fact that we cannot use AC_2 because it implies that there are no antimorphisms! We'd better have a proof of this fact.

Proposition 3 *AC_2 implies that there are no antimorphisms.*

Proof: We noted above that no antimorphism can have a fixed point. (Is the fixed point a member of itself or not?) Now suppose that σ is an antimorphism of order 2. It has no fixed points, so the set of its cycles is a partition of V into pairs. Use AC_2 to pick a transversal for this partition. This transversal is obviously going to be fixed by $j\sigma \cdot c\ldots$ which is σ!

What happens if we drop the condition that σ be an involution? If we are to work the same trick we would need to know that every σ-cycle is even.

Let σ be an antimorphism of order $2n$. The σ-cycles partition V as before, and they are all even. Each σ-cycle splits naturally into two σ^2 cycles. Use AC_2 to pick, for each σ-cycle, one of the two σ^2-cycles. Take the union of all the chosen σ^2-cycles. This will be a fixed point for σ as before.

I claim in the preceding paragraph that every σ-cycle is even. We'd better prove it. (It's surprisingly tricky). We need a lemma:

Lemma 11 *If σ is an automorphism then the least odd number that is the length of a σ-cycle is cantorian.*

Proof:

Suppose x belongs to an odd σ-cycle of minimal length, $T2n+1$, say. What about the members of x? The lengths of the cycles to which they belong must divide $2n+1$: the largest they can be is $2n+1$ itself. So $T2n+1 \leq 2n+1$. For the other direction consider as before an x belonging to an odd σ-cycle of minimal length, $2n+1$. What is the length of the cycle to which $\{x\}$ belongs? It must be $T2n+1$, so $2n+1 \leq T2n+1$. So $T2n+1 = 2n+1$.

Now suppose *per impossibile* that σ is an antimorphism with some cycles of odd length. Then σ^2 is an automorphism with cycles of odd length. Indeed these two families of odd cycles are in 1-1 correspondence. This establishes that the least length of an odd σ-cycle is cantorian.

■

Finally we have to show that σ cannot have any odd cycles of cantorian length. For all x and y, $(\forall n)(x \in y \longleftrightarrow \sigma^{2n+1}(x) \notin (j\sigma)^{T2n+1}(y))$ by induction on n. Suppose x belongs to a σ-cycle of odd length. So, in particular, $x \in x \longleftrightarrow \sigma^{2n+1}(x) \notin (j\sigma \cdot c)^{T2n+1}(x)$. But $\sigma = j\sigma \cdot c$ and if $n = Tn$ and $x = \sigma^{2n+1}(x)$ we can simplify further to $x \in x \longleftrightarrow x \notin x$.

So no antimorphism has any odd cycles. So (recapitulating from above) if σ is an antimorphism each σ-cycle splits naturally into two σ^2 cycles. Use AC_2 to pick, for each σ-cycle, one of the two σ^2-cycles. Take the union of all the chosen σ^2-cycles. This will be a fixed point for σ as before. ■

At all events we have got to get straight the status of AC_2.

Proposition 4 *The following are equivalent:*

1. *Every set of disjoint pairs has a choice function;*
2. *Every set of pairs has a choice function;*
3. *Every partition of V into pairs has a choice function;*
4. *Whenever we partition V into pairs the two partitions are conjugate.*

(2) → (1), (2) → (3), (2) → (4) and are immediate.

We will prove

(1) → (2); (3) ⟷ (1); (4) ⟷ (2).

(1) → (2)

Let P be a set of pairs. We desire a choice function for it, but we know only (1) – not (2). Nathan Bowler has found an injection i from the set of pairs into the set of singletons: $i(\{x, y\}) = \{(x \times y) \ \ \texttt{XOR} \ \ (y \times x)\}$. The set

$$\{p \times i(p) : p \in P\}$$

is a family of disjoint pairs and therefore, by (1), has a choice function, f. We can recover a choice function f^* for P by $f^*(p) =: \mathtt{fst}(f(p \times i(p)))$. ■

(3) $\longleftrightarrow$ (1).

If we are given a set of pairs we can make disjoint copies of it by the trick we used above. In fact – by using an i whose range is a moiety of singletons – we can ensure that the sumset $\bigcup P$ of the disjoint family P of pairs we construct by this method has a complement that is the same size as V. The complement $V \setminus \bigcup P$ therefore has a partition P' into pairs. Then $P \cup P'$ is a partition of V into pairs. Any selection set for this partition will give us a choice function for the partition we started with. ■

(2) $\rightarrow$ (4)

Suppose $\mathbb{P}_1$ and $\mathbb{P}_2$ are two partitions of V into pairs. By AC_2 we have a selection set S for $\mathbb{P}_1$ and $\mathbb{P}_1$ is obviously a bijection between S and $V \setminus S$. So $|S| = |V|$ and $|\mathbb{P}_1| = T|V|$. We argue for $\mathbb{P}_2$ similarly of course. So there is a bijection π between $\mathbb{P}_1$ and $\mathbb{P}_2$. For each $p \in \mathbb{P}_1$ there are precisely two bijections between p and $\pi(p)$ and we use AC_2 to pick one. The union of all such chosen bijections is a permutation conjugating $\mathbb{P}_1$ and $\mathbb{P}_2$. ■

(4) $\rightarrow$ (2)

Assume (4). If $\mathbb{P}$ is a partition of V into pairs then by (4) it will be conjugate to the partition $\{\{x, V \setminus x\} : x \in V\}$. That is to say, there is a permutation π of V such that, for all $p \in \mathbb{P}$, $\pi``p$ is a pair $\{x, V \setminus x\}$. But clearly the partition $\{\{x, V \setminus x\} : x \in V\}$ has a choice function $f$ ("pick the element that contains $\emptyset$") so the choice function for $\mathbb{P}$ that we want is $p \mapsto \pi^{-1}(f(\pi``p))$. ■

I'm not yet convinced that we cannot add to this list the following weakening of (4):

(5) Whenever we partition V into pairs we get the same number of pairs.

This needs thinking about.

It may even be the case that AC_2 is equivalent to the assertion that there are very few conjugacy classes of partitions of V into pairs. I think this can probably be obtained as a consequence of Bowler-Forster.

Definition 6

An involution with no fixed points and no transversal set is **bad**.

Observe that, by proposition 4, the existence of bad involutions is precisely equivalent to $\neg\mathrm{AC}_2$

Bad involutions turn up in connection with antimorphisms.

Proposition 3 tells us that if τ is a polarity then τ is a bad involution. Evidently τ is an antimorphism iff $\tau = c{\cdot}j(\tau)$. If an antimorphism τ is an involution then any transversal set for τ will be a fixed point for it. Antimorphisms cannot have fixed points so any polarity must be – at the very least – a bad involution.

What are the prospects for a permutation model containing a polarity? Evidently it is necessary and sufficient to find a bad involution τ and a permutation σ so that $\tau^\sigma = c \cdot j(\tau)$; then, in V^σ, τ has become a polarity.

So we

(i) need a bad involution τ such that
(ii) $\tau \cdot j\sigma$ is also a bad involution, and – what's more –
(iii) $\tau \cdot j\sigma$ is conjugate to τ.

Aren't there a few 'c's missing?

(i) happens precisely if AC_2 fails. I can't see how to arrange for (ii), and achieving (iii) would seem to rely on some principle like: all bad involutions are conjugate, which sounds rather choice-like and sits ill with $\neg AC_2$.

We might find the following observation useful:

Remark 32 *If π is an involution then $c \cdot j(\pi)$ lacks fixpoints iff π is bad.*

9.2 Existence of antimorphisms is independent

Years ago Adrian Mathias made the following observation.

Remark 33 *(Mathias)*
If NF is consistent the existence of polarities is independent of NF.

We note that if a is a Boffa atom and π is an antimorphism then π is not a polarity: we have
$\pi(a) \in a = B(a)$ iff
$\pi(a) \in B(a)$ iff
$a \in \pi(a)$ (by definition of $B(a)$) iff
$\pi(a) \notin \pi^2(a)$ (since π is an antimorphism) so
$\pi(a) \in a$ iff $\pi(a) \notin \pi^2(a)$, whence $a \neq \pi^2(a)$ so π is not a polarity.

Now every model of NF has a permutation model containing a Boffa atom (use the transposition $(\emptyset, B(\emptyset))$ so if NF proved the existence of polarities the permutation model corresponding to this permutation would witness a contradiction. ■

However i think we can do better than this: if we could establish that the transposition $(\emptyset, B(\emptyset))$ preserved polarities then we would be able to show that there are no polarities. In fact there is nothing special about $\emptyset$ here; for any x the transposition $(x, B(x))$ will give a Boffa atom in the permutation model and enable us to run Mathias' *aperçu* .

This might work! We can think of a polarity as a partition $\mathbb{P}$ of V into pairs additionally satisfying

$$(\forall x, y)(\{x, y\} \in \mathbb{P} \longleftrightarrow (\forall w, z)(\{w, z\} \in \mathbb{P} \to (w \in x \longleftrightarrow z \notin y))) \qquad \textbf{(anti)}$$

. . . which is the only unstratified clause. Next we want to show that if $V \models$ "$\mathbb{P}$ is a polarity" then, for at least some closed term a, $V^{(a,B(a))} \models$ "$\mathbb{P}$ is a polarity". The hope is that it will suffice to ensure that $(a, B(a))$ doesn't move $\mathbb{P}$ or any of its members, co's that much is easy to arrange,

It'll bring bad luck to get ahead of ourselves like this but i'm going to do it anyway. Suppose τ is an antimorphism of order 6; then τ^3 is a polarity. So τ wasn't an antimorphism.

So Suppose τ is an antimorphism of order $4n+2$. Then τ^{2n+1} is a polarity. So τ wasn't an antimorphism. So every antimorphim is of infinite order or order a multiple of 4.

OK, let's check whether or not **anti** remains true in V^τ if it was true in V. τ is a transposition $(a, B(a))$ where a is not the antimorphism nor a pair in it. No polarity can swap $\emptyset$ with $B(\emptyset)$ so we can safely take a to be $\emptyset$.

First we write out **anti** in primitive notation by replacing $\{x, y\} \in \mathbb{P}$ by $(\exists u \in \mathbb{P})(x, y \in u)$

$$(\forall x, y)((\exists u \in \mathbb{P})(x, y \in u)) \longleftrightarrow (\forall w, z)((\exists v \in \mathbb{P})(w, z \in v) \to (w \in x \longleftrightarrow z \notin y)))$$

Writing 'τ' for the transposition $(\emptyset, B(\emptyset))$ this becomes

$$(\forall x, y)((\exists u \in \mathbb{P})(x, y \in u)) \longleftrightarrow (\forall w, z)((\exists v \in \mathbb{P})(w, z \in v) \to (w \in \tau(x) \longleftrightarrow z \notin \tau(y))))$$

Then it gets messy. $w \in \tau(x)$ becomes

$$x = \emptyset \wedge \emptyset \in w \ \vee x = B(\emptyset) \wedge w \in \emptyset \ \vee \emptyset \neq x \neq B(\emptyset) \wedge w \in x$$

The second disjunct is of course impossible so we can simplify

$x = \emptyset \wedge \emptyset \in w \ \vee \ \emptyset \neq x \neq B(\emptyset) \wedge w \in x$ which i think we can simplify further to

$$x = \emptyset \wedge \emptyset \in w \ \vee \ x \neq B(\emptyset) \wedge w \in x$$

Then $z \notin y$ becomes

$$\neg(y = \emptyset \wedge \emptyset \in y \ \vee \ y \neq B(\emptyset) \wedge z \in y)$$

$$\neg(y = \emptyset \wedge \emptyset \in y) \wedge \neg(y \neq B(\emptyset) \wedge z \in y)$$

$$\neg(y = \emptyset \wedge \emptyset \in y) \wedge (z \in y \to y = B(\emptyset))$$

$(y \neq \emptyset \vee \emptyset \notin y) \wedge (z \in y \to y = B(\emptyset))$ and we can simplify the left-hand conjunct to get

$$\emptyset \notin y \wedge (z \in y \to y = B(\emptyset))$$

All this needs to be checked!

So we would like to have $z \notin y$ iff $\emptyset \notin y \wedge (z \in y \to y = B(\emptyset))$. But i see no reason why that should hold.

However, if there really is a permutation that adds a Boffa atom but does not destroy antimorphisms then this should give rise to a contradiction directly in NF without the *détour* through permutations: there will be a term t where the antimorphism will crash and burn.

But can we spice up Mathias' argument with more batoms to refute antimorphisms of order 4?

But, let's face it, we are getting a bit ahead of ourselves. It's probably *not* sufficient to ensure that $(a, B(a))$ doesn't move $\mathbb{P}$ or any of its members.

Even if this does work, how much of it works in CUS?

H I A T U S

This connection between antimorphisms and Boffa atoms is a foretaste of things to come.

We needed a Boffa atom, but that comes free. It is easy to show that every model of NF has a permutation model containing a Boffa atom. In fact the original construction of Hinnion-Pétry can be refined to give

LEMMA 12 *For any concrete n and any symmetric relation R on n things and any model $\mathfrak{M}$ of NF, $\mathfrak{M}$ has a permutation model containing n Boffa atoms such that the membership relation among them is isomorphic to R.*

Proof:

The proof is very like the proof of what Barwise [] calls the *Solution lemma*.

To illustrate, let's arrange for two self-membered batoms a_1 and a_2 and a single non-self-membered Boffa atom b which is related to a_1 but not to a_2. We start by finding three sets a_1, a_2 and b such that $a_1 \in a_1$, $a_2 \in a_2$, $b \notin b$, $a_1 \notin b$, $b \notin a_1$, $a_2 \in b$ and $b \in a_2$. (In general, we find finitely many things, self-membered or not, *ad libitum*, such that $\in$ among them is symmetrical.) This we can do by the technique used in the proof that every countable binary structure embeds in the term model for NF0. The permutation π we want is $(a_1, B(a_1))(a_2, B(a_2))(b, B(b))$. (In general we swap each chosen object x with $B(x)$.)

It remains to check that a_1, a_2 and b are batoms.

$$(\forall x)(x \in a_1 \longleftrightarrow a_1 \in x)^\pi$$

is

$$(\forall x)(x \in \pi(a_1) \longleftrightarrow a_1 \in \pi(x))$$
$$(\forall x)(x \in B(a_1) \longleftrightarrow a_1 \in \pi(x))$$
$$(\forall x)(a_1 \in x \longleftrightarrow a_1 \in \pi(x))$$

This is OK if x is fixed. If x is a_1 or a_2 or b then $\pi(x) = B(x)$ and the RHS becomes $x \in a_1$. But we arranged for $\in \restriction \{a_1, a_2, b\}$ to be symmetrical. If x is $B(a_1)$ or $B(a_2)$ or $B(b)$ then $\pi(x) = B^{-1}(x)$ and the biconditional becomes

$$a_1 \in B(c) \longleftrightarrow a_1 \in c$$

What is this 'c'

which is all right because we arranged for $\in \restriction \{a_1, a_2, b\}$ to be symmetrical.

The proof for a_2 and b is exactly the same. ■

By compactness we can arrange for infinitely many.

Let's just recall that we can kill off Boffa atoms ...

Remark 34
No stratified extension of NF proves the existence of Boffa atoms.

Proof: Suppose not, so there is at least one Boffa atom. Let τ be the product $\prod(B(x), V \setminus B(x))$ of all transpositions swapping $B(x)$ with $V \setminus B(x)$. (This is well-defined since nothing can be both a value of B and the complement of a value of B.)

(There is no Boffa atom)$^\tau$ is $\forall x \exists y(x \in \tau(y) \longleftrightarrow y \notin \tau(x))$.

We have two cases to consider:

(i) If $x = \tau(x)$, then take y to be a Boffa atom. $\tau(y) = V \setminus y$ so $x \in \tau(y) \longleftrightarrow y \notin \tau(x)$ becomes $x \notin y \longleftrightarrow y \notin x$ which is $x \in y \longleftrightarrow y \in x$ which is true, because y is a Boffa atom.

(ii) If $x \neq \tau(x)$, then either (i) x is a value of B, in which case take y to be $\emptyset$ (making both halves of the biconditional false) or (ii) it is $V \setminus B(z)$ for some z. In this case take y to be $\{z\}$. We then have $\tau(y) = y$. The left hand side of the biconditional is $x \in \{z\}$, that is $V \setminus B(z) = z$, which is impossible (ask $z \in z$?). The right hand side is $\{z\} \notin B(z)$, which is false. ■

It would be nice to give a permutation model that didn't rely on the presence of a Boffa atom in the base model. .... We needed it for the case $x = \tau(x)$, when we have to find y such that $x \in \tau(y) \longleftrightarrow y \notin x$. Now if x is fixed, then it sure as hell isn't a Boffa atom, so there will certainly be things y such that $x \in y \longleftrightarrow y \notin x$, witnesses to the fact that x is not a Boffa atom. All we need is for one of these witnesses to be fixed. ("One drop would save my soul" says Faustus). But why should there be even one fixed witness?

I can't help suspecting that the difficulty we have in showing that every model of NF has a permutation model lacking Boffa atoms is of a piece with the difficulties we have in proving the consistency of the various Barwise approximants below. It is of course to be expected that it would be easier to find a permutation model containing a Boffa atom than to find a model lacking them altogether, just as it's easier to add a Quine atom than it is to get rid of them. (The $\exists\forall$ sentence is easier to prove consistent than the $\forall\exists$ one.)

9.3 T$\mathbb{Z}$T

(We can even use Ehrenfeucht games to give a proof that Rieger-Bernays permutation models preserve stratified formulæ – by reasoning about stratimorphisms. It might be worth while spelling this out)

So the biconditional scheme is a theorem scheme of T$\mathbb{Z}$T. So it's a theorem scheme of T$\mathbb{Z}$T+ Ambiguity. Now we appeal to general model-theoretic nonsense to claim that there must be a suitably saturated model of T$\mathbb{Z}$T+ Amb + duality and this will be both iso to its dual and iso to its shift (both these by back-and-forth constructions); will this give us a model of NF with an external antimorphism? The general-model-theoretic-nonsense argument says that T$\mathbb{Z}$T+ Amb will have a model $\mathfrak{M}$ that has a tsau τ and an antimorphism α. Because α is an antimorphism we must have

$$(\forall xy)(x \in \tau(y) \longleftrightarrow \alpha(x) \not\in \alpha(\tau(y))) \tag{9.1}$$

whatever τ is. However if this antimorphism σ is to give rise to an antimorphism of the model $\mathfrak{M}/\tau$ of NF that results by quotienting $\mathfrak{M}$ out by τ we must have:

$$(\forall xy)(x \in \tau(y) \longleftrightarrow \alpha(x) \not\in \tau(\alpha(y))) \tag{9.2}$$

because we want α to be an antimorphism for the relation $x \in \tau(y)$ and (9.2) is the formula that asserts this.

(9.1) and (9.2) are not equivalent unless τ and α commute – which they mightn't.

Might we not be able, on being given a suitably-saturated model of T$\mathbb{Z}$T+ Ambiguity, to construct the tsau τ and the antimorphism α by two interleaved back-and-forth constructions so that they commute? Let's try ...

Let $\mathfrak{M}$ be a suitably-saturated model of T$\mathbb{Z}$T. It is elementarily equivalent to its dual, so – by a standard back-and-forth construction – it has an antimorphism, which we shall write 'α'. Without loss of generality we can assume that α is in fact an involution. Although this asumption is not strictly neccessary for what follows, it does make life a bit easier. We now embark on a second back-and-forth construction – of a tsau, which we will write 'τ'. At each step – be it a 'back' step or a 'forth' step – where we are considering an argument x, once we have determined what $\tau(x)$ is to be we also thereby determine what $\tau(\alpha(x))$ is to be, since it is $\alpha(\tau(x))$; we have just determined $\tau(x)$ and we knew what α did to this object before we embarked on this second back-and-forth construction. (Had we not insisted that α be an involution we would have had a larger cycle to consider at this stage).

Even if this doesn't work the effort will not be entirely wasted. For the suitably-saturated model will surely have a type-shifting *anti*morphism. Let me write this type-shifting antimorphism 'τ' as before. Then τ^2 will be a tsau that lifts levels by two rather than by one. Tsaus that lift by two levels not by one give rise to quotients in the same way that tsaus-that-raise-levels-by-one give rise to models of NF. Each such tsau gives a two-sorted structure: a pair of set-theoretic structures U_1 and U_2 where elements of U_1 find their members among the elements of U_2 and where elements of U_2 find their members among the elements of U_1. The details: suppose σ is a tsau that lifts levels by two. The quotient structure has two *lobes*: levels `yin` and `yang`. The membership relation between level `yin` and level `yang` is the old membership relation between levels 0 and 1; the membership relation between level `yang` and level `yin` is $x_1 \in y_0$ iff $x_1 \in \sigma(y_0)$.

If the tsau-that-lifts-levels-by-two is τ^2 where τ is an antimorphism that lifts levels by one then τ survives as an antimorphism of the two-lobed structure (since τ and τ^2 will commute!), and τ is an antimorphism that swaps elements between the lobes.

This bilobate structure is merely the simplest example of a family of (conjectured) structures. The scheme $\phi \longleftrightarrow \phi^k$ (The exponent 'k' means that there are k '+' signs) has a corresponding notion of *glissant* model and a corresponding quotient, which is a typed structure where the type indices are integers mod k. We don't know that there are any such structures, because the consistency question for the scheme $\phi \longleftrightarrow \phi^k$ seems to be as hard as the ordinary ambiguity scheme[2]. However, let's go back to theorem **??** and consider the proof method in the context where we have a model $\mathfrak{M}$ of the version of type theory-with-levels-indexed-by-integers-mod-k and we are playing an Ehrenfeucht game of length n between $\mathfrak{M}$ and its dual, with $n << k$. This is just like the situation in theorem **??**. The situation is rather more complex when n is comparable in size to k, and this needs more discussion.

9.4 Barwise Approximants

Barwise has a cute theorem about Henkin quantifiers, and i am interested in applying it to the assertion

$$\begin{matrix}(\forall y_1)(\exists x_1)\\(\forall y_2)(\exists x_2)\end{matrix} \bigwedge_{i,j\leq 2} \begin{pmatrix} y_i \in x_j \longleftrightarrow x_i \notin y_j \\ x_i \in x_j \longleftrightarrow y_i \notin y_j \\ y_i = x_j \longleftrightarrow x_i = y_j \end{pmatrix} \tag{ϕ_2}$$

which says that there is an (external) polarity.[3] It generates an infinite family of approximants, and the deal is that if you can arrange for all the approximants to be true, then all the first-order consequences of the existence of an antimorphism of order two are true too.

The nth approximant is

$$(\forall y_1 \exists x_1) \ldots (\forall y_n \exists x_n) \bigwedge_{i,j\leq n} \begin{pmatrix} y_i \in x_j \longleftrightarrow x_i \notin y_j \\ x_i \in x_j \longleftrightarrow y_i \notin y_j \\ y_i = x_j \longleftrightarrow x_i = y_j \end{pmatrix} \tag{9.3}$$

Definition 7 *A_n is the nth approximant*

We need additionally the *list approximants*. These are like the approximants above except that each $\forall$ variable is replaced by a list of variables and its corresponding $\exists$ variable is replaced by a list of the same length. Thus, for example, the first list approximant is

$$(\forall y_1 \ldots y_n)(\exists x_1 \ldots x_n) \bigwedge_{i,j\leq n} \begin{pmatrix} y_i \in x_j \longleftrightarrow x_i \notin y_j \\ x_i \in x_j \longleftrightarrow y_i \notin y_j \\ y_i = x_j \longleftrightarrow x_i = y_j \end{pmatrix} \tag{9.4}$$

and the second is

[2]Annoyingly the question is open, and likely to remain so until we solve the consistency problem for NF. The only obvious way of getting a crowbar between them would be to prove in NF the consistency of the theory of the bilobate structure, and that doesn't sound plausible.

[3]I know of no proof that if there is an antimorphism there is a polarity.

$$(\forall y_1 \ldots y_n)(\exists x_1 \ldots x_n)(\forall y_{n+1} \ldots y_{n+m})(\exists x_{n+1} \ldots x_{n+m}) \bigwedge_{i,j \leq n+m} \left(\begin{array}{c} y_i \in x_j \longleftrightarrow x_i \notin y_j \\ x_i \in x_j \longleftrightarrow y_i \notin y_j \\ y_i = x_j \longleftrightarrow x_i = y_j \end{array} \right) \tag{9.5}$$

or perhaps

$$(\forall y_1 \ldots y_{n_1})(\exists x_1 \ldots x_{n_1})(\forall y_{n_1+1} \ldots y_{n_2})(\exists x_{n_1+1} \ldots x_{n_2}) \bigwedge_{i,j \leq n_2} \left(\begin{array}{c} y_i \in x_j \longleftrightarrow x_i \notin y_j \\ x_i \in x_j \longleftrightarrow y_i \notin y_j \\ y_i = x_j \longleftrightarrow x_i = y_j \end{array} \right) \tag{9.6}$$

What is the relation between the scheme of approximants and the duality scheme? Usual model-theoretic nonsense shows that every model of the duality scheme is elementarily equivalent to one with an antimorphism (possibly external) and similarly every model of the scheme of approximants is elementarily equivalent to one with an antimorphism (again, possibly external). This means that the schemes are equivalent. So every member of either scheme can be deduced from finitely many of the other axioms. I see no obvious way of finding these proofs ...

I think it works something like this. We want to deduce ϕ^* from ϕ. ϕ has, say, 10 alternations, so we assume that version of the 10th list approximant that has the appropriate number of variables in each block. Player $\forall$ (say) starts with a tuple. We can pretend that he started by playing the image of the tuple in the function whose existence the 10th approximant alleges. $\exists$ replies with a tuple. Very well, let $\exists$ reply with the image in this function of the tuple that $\exists$ replied to originally.

There is a theorem about NF0 with this sort of flavour, but, as we shall see, it goes only a very small part of the way.

THEOREM 14

Let R be an arbitrary definable binary relation on a set Y.
Then, for each m,

$$NF0 \vdash (\forall y_1 \exists x_1) \ldots (\forall y_m \exists x_m)(\langle Y, R \rangle \simeq \langle X, \in \rangle).$$

Proof: (X and Y are of course the set of things pointed to by the x variables and the y variables respectively.)

The proof is lifted from my book. I include it here only beco's the proof is an example of technique we will need to refine later. We must distinguish this from the much easier $(\forall y_1 \ldots y_m) \ldots (\exists x_1 \ldots x_m)(\langle Y, R \rangle \simeq \langle X, \in \rangle)$. To prove the formula we want we need to be able to construct an embedding i from $\langle Y, R \rangle$ into the term model for NF0 *in such a way that our choice of $i(y_k)$ depends only on $y_1 \ldots y_{k-1}$ and our choices of $i(y_1) \ldots i(y_{k-1})$.*

We will need an infinite supply of distinct $x \in x$ and distinct $y \notin y$, and such a supply can easily be found with the help of the B function. Let the nth *left* object be $B^n(V)$ and the nth *right* object be $B^n(\Lambda)$. All left objects are self-membered and no right objects are.

We start by setting $i(y_1)$ to be the first left or right object according to whether or not $y_1\ R\ y_1$. At later stages n we have to construct $i(y_n)$ as an NF0 term. Let O_n be the $2n$th left object, if $y_n\ R\ y_n$, or right object if not. Then $i(y_n)$ will be obtained from O_n by adding and removing only finitely many things.

We have four sets to consider:

A: $\{i(y_k) : k < n \ \wedge \ y_k\ R\ y_n\}$
B: $\{i(y_k) : k < n \ \wedge \ \neg(y_k\ R\ y_n)\}$
C: $\{i(y_k) : k < n \ \wedge \ y_n\ R\ y_k\}$
D: $\{i(y_k) : k < n \ \wedge \ \neg(y_n\ R\ y_k)\}$.

$i(y_n)$ must extend A, be disjoint from B, belong to everything in C, and to nothing in D. So our first approximation is $(O_n \setminus B) \cup A$. For each $i(y_k) \in C$ we want $i(y_n) \in i(y_k)$. Now $i(y_n) \in O_k \longleftrightarrow B^{-1}(O_k) \in i(y_n)$, so we can determine the truth value of '$i(y_n) \in O_k$' (at least) by putting $\{B^{-1}(O_k)\}$ into $i(y_n)$ or not. It will follow from

$$(\forall n \in \mathbb{N})(\forall k < n)(i(y_n) \in i(y_k) \longleftrightarrow i(y_n) \in O_k)$$

that this actually determines the truth value of '$i(y_n) \in i(y_k)$' as well. Consider a notion of rank of NF0 terms as the depth of nested occurrences of 'B'. To get $i(y_k)$ from O_k we remove and add only odd rank items $\neq i(y_i)$ or any $i(y_j)$ with $j < k$: neither can affect $i(y_n)$. ■

There are various consequences of this which are germane to this context of Barwise approximations, and which are worth noting even tho' they have no direct bearing on duality and antimorphisms.

REMARK 35 $NF0 \ \vdash \ (\forall y_1 \exists x_1) \ldots (\forall y_n \exists x_n) \ldots (\langle Y, \in \rangle \simeq \langle X, \notin \rangle)$

(where Y is of course the set of things denoted by the y variables and X is the set of things denoted by the x variables) and by Barwise's stuff on approximants this is enuff to give the relative consistency of

$$(\forall xy)(x \in y \longleftrightarrow f(x) \notin f(y))$$

In fact the same strategy will prove, for any definable permutation π,

$$(\forall y_1 \exists x_1) \ldots (\forall y_n \exists x_n) \ldots (\bigwedge_{i,j<\omega} (y_i \in_\pi y_j \longleftrightarrow x_i \in x_j) \wedge (\bigwedge_{i,j<\omega} (y_i = y_j \longleftrightarrow x_i = x_j))$$

which will be enuff to give a model of *NF* into which one can embed all its permutation models (mod definable permutations)

Indeed it will show that the first-order consequences of the following is a theorem scheme of NF0: every definable binary structure can be isomorphically embedded into $\langle V, \in \rangle$:

Corollary 5 *Every first-order consequence of*

$$(\forall \pi)\genfrac{(}{)}{0pt}{}{(\forall y\ )(\exists x\ )}{(\forall y')(\exists x')}(y \in \pi(y') \longleftrightarrow x \in \pi(x))$$

is already a theorem of NF.

There are three features that we want in order to get all the approximants true, and sadly the term-model-for-NF0-construction has only one of them.

- First we must be able to alternate quantifiers, so that the choice of x_i depends only on x_j and y_j for $j < i$. This feature is delivered by the term-model-for-NF0-construction. It is the **alternating condition**. The next two features that we want are features that the term-model-for-NF0-construction doesn't give.

- Altho' the term-model-for-NF0-construction deals with formulæ like $x_i \in x_j \longleftrightarrow y_i \notin y_j$ it doesn't deal with clauses like $x_i \in y_j \longleftrightarrow y_i \notin x_j$. We want clauses like $y_i \in x_j \longleftrightarrow x_i \notin y_j$. These clauses are the **mixing condition**.

  Suppose (and here we are considering the possibility that y_1 might be a vector) $\forall$ tosses a handful of y's into the ring. $\exists$ must reply with some x's, and she can do this by means of the term-model-for-NF0 construction. But this disregards some facts about the $\vec{y}$'s. $y_1 \cup y_2$ might be V, for example, in which case we will have to arrange that $y_i \notin (x_1 \cap x_2)$. Or maybe the union of all the $\vec{y}$'s is cofinite. That would be very nasty!

- The second problem is that we want clauses for the **involutive condition**: $x_i = y_j \longleftrightarrow y_i = x_j$. It is true that all we really need is an invertibility condition, but the weaker form is ridiculously complicated and in any case it is absurd to suppose that there are antimorphisms but that none of them are polarities. (I know i haven't proved that if there is an antimorphism there is an antimorphism that is a polarity but *really!* ...!)

9.4.1 Implications between the approximants

Definition 8 $\texttt{anti}(y, x)$ *is* $(y \in y \longleftrightarrow x \notin x) \wedge (x \in y \longleftrightarrow y \notin x)$

What do we need to deduce A_2 from A_1? The second says not only that for all y_1 there is an x_1 such that $\texttt{anti}(x_1, y_1)$ but also that for any y_2 there is an x_2 such that burble. Now the first part of this is precisely the content of A_1 , so all we need over and above in order to deduce A_2 is the $(\forall^* \exists^1 !)$ formula that says that for all y_1 and x_1 such that $\texttt{anti}(x_1, y_1)$ and for all y_2 there is x_2 such that burble.

So A_2 is a consequence of A_1 and a $\forall^*\exists^*$ sentence. Similarly in general A_{n+1} follows from A_n and a $\forall^*\exists^1$ sentence.

This sounds nice, but the $\forall^*\exists^*$ sentences are *not* those featuring in conjecture 1, which are $\forall^1\exists^*$! and in any case these $\forall^*\exists^*$ sentences are actually refutable!.

Let's start by looking at the formula which will enable us to deduce A_2 from A_1.[4] It says

$$(\forall y_1 x_1)(\mathtt{anti}(y_1, x_1) \to (\forall y_2)(\exists x_2)(\mathtt{anti}(x_2, y_2) \wedge \bigwedge_{i \neq j \leq 2} (x_i \in y_j \longleftrightarrow y_i \notin x_j))$$

It's surprisingly easy to find a counterexample to this. Set $y_1 := \Lambda$ and $x_1 := B(\Lambda) \cup \{\Lambda\}$. This gives $\mathtt{anti}(y_1, x_1)$. Then instantiate $y_2 := \{V\}$. Then $y_2 \notin x_1$ whence $x_2 \in y_1$ but y_1 is empty.

(Notice that the other thing one might think of in this meccano connection, using the list version of A_1 with three y vbls and three x vbls doesn't help either.)

This is a reflection of the fact that if y_1 is Λ then x_1 has to be V and *vice versa*. Actually this isn't anything to do with V and Λ being 1-symmetrical, beco's it works just as well if you take $\{\Lambda\}$ and $B(\{\Lambda\}) \cup \{\{\Lambda\}\}$.

The first approximant

The first approximant is $(\forall y)(\exists x)((x \in y \longleftrightarrow y \notin x) \wedge (y \in \ y \longleftrightarrow x \notin x)))$

(Notice that there is no bite in the involutive condition in this case!)

Observe that every model of NF has a permutation model in which the first approximant fails:

$$(\exists y)(\forall x)((x \in y \longleftrightarrow y \in x) \vee (y \in \ y \longleftrightarrow x \in x)))$$

is the negation of the first approximant and we can make it true by adding a Boffa atom, which will be a witness to the '$\exists y$'. Observe the connection with rem 33: you can't have both Boffa atoms *and* polarities.

We can use lemma 12 to find a permutation model of the first approximant. Let there be two batoms a and b with $a \in a$ and $b \notin b$ and set $\pi = (a, V \setminus a)(b, V \setminus b)$.

REMARK 36 *The first approximant is true in V^π.*

Proof:

We want $(\forall y)(\exists x)((x \in \pi(y) \longleftrightarrow y \notin \pi(x)) \wedge (y \in \pi(y) \longleftrightarrow x \notin \pi(x)))$

If y is fixed let x be a or b depending on whether we want $x \in \pi(x)$ or not. Of course there may well be lots of fixed witnesses to y not being a batom and any of them will do too.

[4] I suppose – if were going to get into this – that we should call this 'A_{1a}' by analogy with the notation for Meccano sets: the Meccano set $1a$ is the set that contains precisely the parts in set 2 that aren't in set 1.

If y is a we seek x such that

$$x \in V \setminus a \longleftrightarrow a \notin \pi(x) \quad \wedge \quad x \in \pi(x)$$

i.e., $x \notin a \longleftrightarrow a \notin \pi(x)$. Any fixed self-membered x will do. Analogously, if y is b then any fixed non-self-membered set will do.

If y is $V \setminus a$ we seek x such that

$$x \in a \longleftrightarrow V \setminus a \notin \pi(x) \quad \wedge \quad x \in \pi(x)$$

This becomes $V \setminus a \in \pi(x) \longleftrightarrow a \notin x$ and $x \in \pi(x)$. $x := V \setminus \{a\}$ will do.

If y is $V \setminus b$ we seek x such that

$$V \setminus b \in \pi(x) \longleftrightarrow x \notin b \quad \wedge \quad b \in \pi(V \setminus b) \longleftrightarrow x \in \pi(x)$$

which becomes $V \setminus b \in \pi(x) \longleftrightarrow b \notin x$ and $x \notin \pi(x)$. $x := \{V \setminus b\}$ will do.

■

That wasn't too awful. *One* of the reasons why it wasn't too awful was the emptiness of the involutive condition in this case. However there is a list version of the first approximant, which is obtained from the nth approximant by replacing the quantifier prefix '$(\forall y_1 \exists x_1) \ldots (\forall y_n \exists x_n)$' by '$(\forall y_1 \ldots y_n)(\exists x_1 \ldots x_n)$'. It of course *does* have an involutive condition! I don't see any way of meeting the involutive condition but the mixing condition should be doable.

[While we still have this model in mind it mihght be worth checking that the second approximant fails in it. I don't suppose anybody thought that the first appropximant implied the second but it can do no harm to prove it.]

So what we need is a finite collection A of sets $\{a_1 \ldots a_n\}$ such that any binary relation on a set of size two embeds into $\langle A, \in \rangle$, and the permutation we want will probably be something like $\prod_{a \in A}(a, V \setminus a)$. A brief meditation on the technique that proved the term-model-of-NF0 result will reassure us that we can certainly do this for binary relations on domains of size 2 and indeed on domains of size n for any n and even on all n by compactness. But even tho' this will take us slightly further than the term-model-for-NF0-construction that we started with (it meets the mixing conditions after all) it doesn't meet the involutive condition, and that is the killer.

(Actually we might be able to meet the involutive condition trivially by the simple device of ensuring that no y_i is ever chosen to be an x_j)

But even that still needs to be done. We have to worry about the cases when some of the y_i are moved.

One slightly annoying feature of this relative consistency proof is that we seem to need to start with a model containing Boffa atoms. Also we never need to use anything that isn't an involution, and in this case i have a hunch that one can make do with a subset of the complementation involution. Let's try this. We seek a property ϕ such that the involution that swaps things that are ϕ with their complements does the trick.

$$(\forall y)(\exists x)((y \in \pi(y) \longleftrightarrow x \notin \pi(x)) \wedge (x \in \pi(y) \longleftrightarrow y \notin \pi(x)))$$

Now $y \in \pi(y)$ is just $y \in y \longleftrightarrow \neg\phi(y)$ so we get four universal-existentials out of this

$$(\forall y)(\phi(y) \wedge y \in y. \to (\exists x)(\phi(x) \wedge x \notin x \wedge (x \notin y \longleftrightarrow y \in x)) \vee (\exists x)(\neg\phi(x) \wedge x \in x \wedge (x \in y \longleftrightarrow y \in x)$$

$$(\forall y)(\phi(y) \wedge y \notin y. \to (\exists x)(\phi(x) \wedge x \in x \wedge (x \notin y \longleftrightarrow y \in x)) \vee (\exists x)(\neg\phi(x) \wedge x \notin x \wedge (x \in y \longleftrightarrow y \in x)$$

$$(\forall y)(\neg\phi(y) \wedge y \in y. \to (\exists x)(\phi(x) \wedge x \in x \wedge (x \in y \longleftrightarrow y \in x)) \vee (\exists x)(\neg\phi(x) \wedge x \notin x \wedge (x \in y \longleftrightarrow y \notin x)$$

$$(\forall y)(\neg\phi(y) \wedge y \notin y. \to (\exists x)(\phi(x) \wedge x \notin x \wedge (x \in y \longleftrightarrow y \in x)) \vee (\exists x)(\neg\phi(x) \wedge x \in x \wedge (x \in y \longleftrightarrow y \notin x)$$

For example one could try $\phi(x) \longleftrightarrow (\forall z)(z \in x \longleftrightarrow V \setminus z \in x)$. Then the first two formulæ go thru' taking x to be $V \setminus y$.

9.4.2 The second approximant

The first nontrivial case seems to be the second approximant.

$(\forall y_1 \exists x_1)(\forall y_2 \exists x_2)$

$(x_1 \in x_1 \longleftrightarrow y_1 \notin y_1)$
$(x_1 \in x_2 \longleftrightarrow y_1 \notin y_2)$
$(x_2 \in x_1 \longleftrightarrow y_2 \notin y_1)$
$(x_2 \in x_2 \longleftrightarrow y_2 \notin y_2)$

with mixing conditions

$(y_1 \in x_1 \longleftrightarrow x_1 \notin y_1)$
$(y_1 \in x_2 \longleftrightarrow x_1 \notin y_2)$
$(y_2 \in x_1 \longleftrightarrow x_2 \notin y_1)$
$(y_2 \in x_2 \longleftrightarrow x_2 \notin y_2)$

and the involutive condition

$(y_1 = x_2 \longleftrightarrow x_1 = y_2)$

Let us say y is nice$_1$ if $(\exists x)(\mathtt{anti}(y, x))$. (That is to say, if player $\exists$ can stay alive for one move at least.) There is a corresponding notion of nice$_2$, which says that player $\exists$ can stay alive for two moves. And so on. But there is a slight niggle. Not even one set can be nice$_2$ unless every set is nice$_1$. So perhaps the correct definition of nice$_2(y)$ should be:

$(\exists x)(\mathtt{anti}(x, y) \wedge (\forall y_1)((\exists x_2)(\mathtt{anti}(y_2, x_2)) \to (\exists x_2)(\mathtt{anti}(y_2, x_2) \wedge$
the usual conditions

…and so on!

Suppose every set is nice: every set has a dual: $(\forall y)(\exists x)(\mathtt{anti}(y,x))$. Can we get a new skolem function sending sets to duals by lifting a skolem function in the obvious way? A second-degree dual for y is going to be an x that is the complement of a set of duals for members of y. That is to say, every $x' \in y$ has a dual that is not in x. But this is almost exactly what the second approximant says. Actually the second approximant is a bit worse, because it says that the skolem function for the first pair of quantifiers must agree with the skolem function for the second pair, which is a bit hard!

One step from the first to the second is this. Suppose there is a sub**set** R of the graph of `anti` which is symmetrical and extensional. (Should be easy to show that no such set can be the extension of a stratified formula) The idea is then that $\lambda x.(V \setminus R\text{``}x)$ is a skolem function for the first pair of quantifiers in the second approximant. One problem with this is that it won't respect complementation. Another is that there doesn't seem to be any reason why we should expect $\mathtt{anti}(V \setminus R\text{``}x, x)$.

The Obvious Permutation

But perhaps in general the obvious permutation to use is

$$\pi = \prod_{w \in V} (Bw, \overline{Bw})$$

We use this in a model where we have as many Boffa atoms as we want, using lemma 12.

Consider the first approximant:

$$(\forall y \exists x)(y \in y \longleftrightarrow x \notin x) \wedge (x \in y \longleftrightarrow y \notin x))$$

This gives

$$(\forall y \exists x)((y \in \pi(y) \longleftrightarrow x \notin \pi(x)) \wedge (x \in \pi(y) \longleftrightarrow y \notin \pi(x)))$$

1. y is moved

   Consider first the case where y is Bz.

   We seek x such that $(Bz \in \overline{Bz} \longleftrightarrow x \notin \pi(x)) \wedge (x \in \overline{Bz} \longleftrightarrow Bz \notin \pi(x)))$

   which becomes

   $(Bz \notin Bz \longleftrightarrow x \notin \pi(x)) \wedge (x \notin Bz \longleftrightarrow Bz \notin \pi(x)))$

   and then

   $(z \notin z \longleftrightarrow x \notin \pi(x)) \wedge (z \notin x \longleftrightarrow Bz \notin \pi(x)))$

   or

   $(z \in z \longleftrightarrow x \in \pi(x)) \wedge (z \in x \longleftrightarrow Bz \in \pi(x)))$.

   If y is $\overline{Bz}$ we analogously end up looking for x such that

$(z \in z \longleftrightarrow x \in \pi(x)) \wedge (z \in x \longleftrightarrow \overline{Bz} \notin \pi(x)))$.

In either case if $B(z)$ were fixed it would be an ideal candidate for 'x', but it isn't. However if we modify it trivially, say to $B(z) \cup \{\Lambda\}$ or to $B(z) \cup \{a, b\}$ we get something that works for both cases.[5]

2. y is fixed

   We seek x such that:

   $(y \in y \longleftrightarrow x \notin \pi(x)) \wedge (x \in y \longleftrightarrow y \notin \pi(x))$.

   This is where we use the fact that the base model has *countably* many batoms. We have two batoms a and b with $a \in a$ and $b \notin b$. If $y \in y$ we take x to be a; if $y \notin y$ we take x to be b.

■

[*HOLE Should insert here a proof that we can do this for the list version of the first approximant*].

This is what to do for the list version of the first approximant. Start with a model containing all the boffa atoms of all possible flavours (the "Tutti Frutti" model). The permutation will swap Boffa atoms with their complements and fix everything else. Then, on being given an n-tuple $\vec{y}$, we assign to each y_i a boffa atom of the correct flavour. This doesn't quite take care of the $x_i \in y_j$ conditions, so we might have to adjust by adding and taking away a few things from the Boffa atoms in the manner of the proof of my result about the term model of NF0. But this means that we have to swap with their complements not only all Boffa atoms but all things whose symm diff from a boffa atom is finite.

We still have to think about what happens if one of the $\vec{y}$ that we picked up is one of these things that are moved.

Can we do the same for the second approximant? The difficulty comes with witnesses to the 'x's with later subscripts. We can always find an x_i satisfying $x_i \in x_2$ or not: that's easy, because we can usually take xs to be batoms or things closely resembling them. The problem concerns membership conditions like $x_i \in y_1$. What happens if y_1 is something nasty like a countable set containing all self-membered batoms?

It may well be that tweaking this model will give us a model for some of the list versions of the first approximant, but that isn't enuff.

A technique like this will prove the consistency of the list version of the first approximant, beco's we can always go back and tweak our choice of x_is if need be. A sort of priority construction...

There is an added complication. We are given a hatful of instances of the y variables. The witnesses for the x variables that we want must satisfy the obvious atomic conditions, but there are some $\forall$ conditions as well. Suppose the union of the ys is V. Then none of the ys can belong to the intersection of all the xs. So any $\forall$ condition satisifed by the y vbls will give rise to a condition

[5] Do we have to worry about z being a batom?

on the x vbls. We've just seen one example. Another will arise from things like $y_i = B(y_j)$, or from y_j being a singleton, or a pair. In fact, we might need to take into account the whole NF0-visible strux of the y objects.

The point is that a kind of rippling adjustment in the spirit of my NF0 construction is not guaranteed to work, beco's of the $\forall$ conditions.

Conjoin all these conditions together, and put the result into DNF. We must try to make one disjunct true.

Let $R(y, x)$ abbreviate $(y \in y \longleftrightarrow x \notin x) \wedge (x \in y \longleftrightarrow y \notin x)$.

The first approximant is $(\forall y \exists x)(R(y, x))$.

Let's think about a weaker version of the second approximant:

$$(\forall y \exists x)(R(y,x) \wedge (\forall y' \exists x')(R(y',x') \wedge (y = y' \longleftrightarrow x = x') \wedge (y' = x \longleftrightarrow x' = y)))$$

To verify this it will be enuff (given the first approximant) for R to be extensional and symmetrical. It's clearly symmetrical, by elementary logic. But extensional?

9.5 Some more recent tho'rts on Ehrenfeucht games for duality

Consider an Ehrenfeucht game played on a model $\mathfrak{M}$ of NF and its dual. `Unequal` makes a move in one of these, and `Equal` must reply with a move in the other, satisfying the obvious duality condition

$$(\forall y)(\exists x)(x \in x \longleftrightarrow y \notin y)$$

No mixing conditions, beco's x and y belong in different structures. This assertion that `Equal` can survive one move is actually a theorem of NF. Indeed it is even a theorem of NF that `Equal` can survive one move even if `Unequal` plays a *tuple* of things for his first move. After all, all she has to do is find a tuple of things whose $\in$-structure is the complement of the $\in$-structure enjoyed by the tuple presented by `Unequal`, and we know that every finite structure can be embedded in the term model of NF0. However things are very different once `Unequal` moves again, even if he's only playing single sets not tuples of sets. It's not difficult to see that the best way for him to twist the knife is to make his second move in the structure that `Equal` has just moved in. After all, if $\mathfrak{M}$ contains a Quine atom but no Quine antiatom his obvious first move is to play a Quine atom, and poor `Equal` has to find a Quine antiatom. Of course she can't do that, and `Unequal` then goes in for the kill with a witness to the fact that her choice is not a Quine antiatom. I don't see how he can force a win in two moves by moving in the same structure as he played in first time. So, although he can (legally) move in either structure, he'd be crazy not to reply in the structure she has just played in:

$$(\forall y)(\exists x)((x \in x \longleftrightarrow y \notin y) \wedge (\forall y' \exists x') \bigwedge \begin{pmatrix} x' \in x' \longleftrightarrow y' \notin y' \\ x' \in y \longleftrightarrow y \notin x' \\ y' \in x \longleftrightarrow x \notin y' \end{pmatrix} \qquad (\phi_4)$$

I suspect that this formula is true in any model with no Quine atoms, no Quine antiatoms, no Boffa atoms and no Boffa antiatoms.

9.6 Internal Antimorphisms in Models of NF_3?

We can assert, using only three types, that there is a bad involution. By proposition 4 the existence of a bad involution is equivalent to $\neg AC_2$ so let's start with a model of $TT_3 + \neg AC_2$. It will contain a bad involution τ (which will be an element of the top level), and let's suppose that the model satisfies the saturation condition that every element that has infinitely many atoms below it is the join of two such elements that are disjoint.

So my question is, if we perform our back-and-forth construction (of the tsau) with sufficient care, can we ensure that τ is a polarity of the quotient model of NF_3?

Now how does this work? Any two countable atomic boolean algebras are iso as long as the quotient of each over the Fréchet ideal is atomless. The quotients are iso by a back-and-forth argument and we can extend the isomorphism to the original algebras.

Be that as it may, we still have to find a model of TST_3 which has a bad involution whose lift is also bad. I think the following FM construction will work. Let A_0 be a countable set of atoms, and let π be a partition of A_0 into pairs. Every subset of π can be thought of as an involution and the power set of π is in fact a group – G, to give it a name. Let A be our bottom level, and let level 1 be the set of those subsets of A that are fixed by G... which is to say those subsets of A that are sumsets of subsets of π. (there are uncountably many of them, so we will have to throw some away. Find a countable atomic subalgebra B of $\mathcal{P}(\mathbb{N})$ and retain only those subsets of A_0 that are union of a B-subset of π ...)

The difficulty i'm having is finding something that is both an FM model of NF_3 AND is countable....

9.6.1 Self-dual formulæ

Definition 9
A formula is self-dual if it is logically equivalent to its own dual: $\phi \longleftrightarrow \phi^{\circlearrowleft}$.

Perhaps should go back to using$\widehat{\ }$...

Remark 37 *A propositional formula ϕ is self-dual iff there is ψ such that ϕ is equivalent to $\psi \longleftrightarrow \psi^{\circlearrowleft}$.*

Proof: Start by expressing ϕ in disjunctive normal form. Since ϕ is self-dual the set of disjuncts that comprise it (each disjunct is a conjunction of literals) is closed under the dual operation and there will be an even number of them. (No consistent disjunct can be self-dual, after all!) There will also be an even number of conjunctions of literals that do *not* comprise ϕ, and that set too is closed under the dual operation. This splits the set of conjunctions of literals that comprise $\neg\phi$ into a set of pairs of conjunctions of literals, where each pair contains a conjunction of literals and its dual. Pick one conjunction from each pair, and form the disjunction of all the conjunctions you have chosen. Call this ψ and think about $\psi \longleftrightarrow \psi^{\circlearrowleft}$. ψ and $\psi^{\circlearrowleft}$ cannot be simultaneously true, but they can be simultaneously false, and when they are, ϕ holds. ■

For 10 more marks Say something about how many ways there are of doing this

This reminds me a bit of the proof that two permutations of the same cycle type are conjugated by an involution.

Corollary 6 *(Specker, ("Dualität"))*

For any involutive automorphism of a (propositional) language the conjunction of finitely many biconditionals between a formula and its dual is equivalent to another such biconditional.

Proof: This is beco's the conjunction of finitely many such biconditionals is self-dual.

There is a problem about incorporating '=' into this treatment but i think it can be got round. A much bigger problem is quantification. After all, anything of the form $\psi \longleftrightarrow \psi^{\circlearrowleft}$ is going to be Δ_2 if ψ is Σ_1 so it would tell us that no (strictly) Σ_1 thing can be self-dual. For example $(\exists x)(\forall y)(x \in y \longleftrightarrow y \in x)$ is self-dual but i defy anyone to find ψ such that it is equivalent to $\psi \longleftrightarrow \psi^{\circlearrowleft}$. Isn't it true that a formula is prenex normal form is self-dual as long as its matrix is self-dual? And conversely – every self-dual formula, once put into normal form, has a self-dual matrix?

There are other phenomena like this. If ϕ is necessary then $\phi \longleftrightarrow \Box\phi$ is logically true: is every necessary thing of the form $\phi \longleftrightarrow \Box\phi$? Similarly invariance....

Let's try a simple example

$$B : (\forall x)(\exists y)(x \in y \wedge y \notin x \ \vee \ x \notin y \wedge y \in x)$$

Assume B and let σ be a permutation in the centraliser of J_1 (the set of all permutations that are j of something). We will prove B^σ by UG. Let x be arbitrary. Two cases

1. $\sigma(x) = x$.
2. $\sigma(x) \neq x$

9.7 A conversation with Nathan

tf:

Suppose σ is a flexible permutation, and it lives on a moiety M. Then we can copy it over to a permutation living on $V \setminus M$, because there is an involution π mapping M onto $V \setminus M$. I now think of σ as a digraph. How do i move along an edge of σ? Well, i can move over into $V \setminus M$ by π (which is a good involution). Then i come back to M by means of the involution that swaps each x in ths support of the copied version of σ (that lives in $V \setminus M$) with $\sigma(\pi(x))$.

Nathan:

Call this second involution τ. Then for x in the support of σ, $\tau(\pi(x)) = \sigma(\pi(\pi(x))) = \sigma(x)$, which is a good sign. However, $\tau \cdot \pi$ also moves some other stuff. Let x be in the support of the copied version of σ. So $\pi(x)$ is in the support of σ. What does τ do to $\pi(x)$? Well, consider $y = \pi(\sigma^{-1}(\pi(x)))$. y is also in the support of the copied version of σ, and so τ swaps y with $\sigma(\pi(y)) = \pi(x)$. That is, $\tau(\pi(x)) = y$, so $\tau \cdot \pi$ does not equal σ, which fixes x.

Indeed, with sufficient lack of choice there cannot be a way to represent every permutation as a product of two involutions. Suppose that there is some permutation σ consisting of one cycle C_n of each finite odd size n, where there is no choice function on those cycles. Suppose further (for a contradiction) that $\sigma = \tau \cdot \pi$, where τ and π are involutions. Then $\tau \cdot \sigma \cdot \tau = \pi \cdot \tau = \sigma^{-1}$, so τ conjugates σ to σ^{-1}. In particular, τ takes fixed points of σ to fixed points of σ and elements of C_n to elements of C_n for each n. Identifying C_n with the integers modulo n, with the action of σ being addition of 1, we get $\pi(x+1) = \pi(x) - 1$, for any x, so that by induction $\pi(x) + x$ is constant on C_n. Say it takes the value k. Then $x = \pi(x)$ iff $x = k - x$ iff $2x = k$ iff $x = k/2$ modulo n. As n is odd, there is a unique solution of this equation modulo n. That is, π fixes precisely one element of C_n for each n. This gives a choice function on the C_n, which is the desired contradiction.

tf:

Ah, i think i see …The point is that $\tau \cdot \pi$ is not σ but the union of σ and its copy in $V \setminus M$.

Nathan:

Exactly so. But this is certainly progress. Suppose now that we have some permutation σ, supported on a moiety, that we want to represent as a product of involutions. By the argument you suggested, we can get the permutation σ' consisting of countably many copies of σ and countably many copies of σ^{-1}: σ' is a product of two involutions. Then composing σ' with the conjugate of σ' which cancels all the copies of σ^{-1} and all but one of the copies of σ, we get

σ as a product of four involutions (this was my original argument, but not the argument in the paper).

tf:

OK, am i right? I think i have reconstructed your thought-processes... Tell me...

Nathan:

Well, this isn't what I was thinking of, but it does work, and (with a little tweaking) shows that every flexible permutation is a product of at most 4 good involutions. Let's say we have some flexible permutation σ. Identify V with $V \times \mathbb{Z}$, where $\mathbb{Z}$ is the set of integers, and let π be the permutation which moves each copy of the universe up one place: $\langle x, m\rangle \mapsto \langle x, m+1\rangle$. Let τ be the permutation which moves almost everything down one place: $\langle x, m\rangle \mapsto \langle x, m-1\rangle$ unless $m = 1$, and $\langle x, 1\rangle \mapsto \langle \sigma(x), 0\rangle$. Then τ and π are both products of Z-cycles-with-distinguished-elements, so that each of τ and π is a product of two good involutions. σ is conjugate to $\tau \cdot \pi$, so is a product of 4 good involutions.

I think we can delete from here to ***
Some light can be shed on the orders of antimorphisms by reflecting on the fact that if σ is an antimorphism then σ^2 is an automorphism: σ is an automorphism iff $\sigma = j\sigma$.
For any n, if σ has a cycle of order n then $j\sigma$ has a cycle of order Tn (take singletons). Also if $j\sigma$ has a cycle of length Tn then for every factor m of Tn it has a cycle of length m.
We know that $j\sigma$ commutes with c and that c is an involution so $j\sigma \cdot c$ is also of order Tn. So if $\sigma = j\sigma \cdot c$ is of order n then $n = Tn$.

The idea is that no antimorphism can have odd order, since an odd power of an antimorphism is another antimorphism. But this cannot be proved by induction. However we can prove that no antimorphism can have cantorian odd order.
Suppose

$$\begin{aligned}
\sigma &= j\tau \cdot c\\
\sigma^{2n+1} &= (j\tau \cdot c)^{2n+1}\\
&= (j\tau)^{2n+1} \cdot c^{2n+1}\\
&= j(\tau^{T2n+1}) \cdot c^{2n+1}\\
&= j(\tau^{T2n+1}) \cdot c
\end{aligned}$$

So, in particular, if $\sigma = j\sigma \cdot c$, then $\sigma^{2n+1} = j(\sigma^{T2n+1}) \cdot c$.
Let's look at this very closely.
We first prove by induction on n that $j(\sigma^n) = (j\sigma)^{Tn}$ No doubt you will be asking: "Where does the 'T' come from?"
Consider the three-place expression $R(\sigma, \tau, n)$ that says "$\tau = \sigma^n$". This is stratified. 'τ' and 'σ' clearly have the same type. What is the type of 'n'?

Actually it doesn't matter; all that matters is that it can be determined from the types of 'τ' and 'σ' . This means that

$$(\forall \sigma, \tau)(\forall n)(R(\sigma, \tau, n) \longleftrightarrow R(j\sigma, j\tau, Tn))(A)$$

is stratified and we have a chance of proving it by induction on 'n'. This means that if $\sigma = j\sigma$ (so that σ is an automorphism, then the order of σ is cantorian. Now if σ is instead an antimorphism, then σ^2 is an automorphism, and its order is cantorian. So every antimorphism has cantorian order.

Now suppose that σ is an antimorphism of order $2n + 1$. First we show that σ^{2n} is an automorphism. Well, σ^2 is an automorphism, and if τ is an automorphism, and $n = Tn$ then τ^n is also an automorphism. So σ^{2n} is an automorphism. The product of an automorphism and an antimorphism is an antimorphism, so σ^{2n+1} is an antimorphism and is therefore not the identity.

However we are going to need something even stronger, namely that no antimorphism can have an odd cycle. Observe that for all x, y, τ and n,

$$x \in y \longleftrightarrow \sigma^n(x) \in (j\sigma^{Tn}(y))$$

$$x \in y \longleftrightarrow \sigma^{2n+1}(x) \in (j\sigma^{T2n+1}(y))$$

Let σ be an antimorphism, and x belong to a σ-cycle of

***

9.7.1 Can we construct an antimorphism by permutations?

This should probably be in `stratificationmodn.tex`

Fix two elements a and b – it doesn't matter what they are. They divide the universe into a pair of moieties: $B(a)$ `XOR` $B(b)$ (which we will call 'X') and its complement (which we will call 'Y'). Let σ be the permutation that fixes x if it either contains both a and b or contains neither. If x contains one but not the other swap it with $V \setminus x$. Then σ fixes a moiety and moves a moiety.

$$\sigma =: \prod_{x \in B(a) \ \mathtt{XOR}\ B(y)} (x, V \setminus x)$$

...and therefore so also does $j\sigma$. Let's check this: σ has a moiety of fixed points and a moiety of things that it moves. We want the same to hold for $j(\sigma)$. Every set of σ-fixed points is a $j(\sigma)$-fixed point, so everything in $\mathcal{P}(Y)$ is fixed by $j(\sigma)$. We also need $j(\sigma)$ to move $|V|$ things. For any nonempty $y \subseteq Y$ clearly $y \cup \{\{a\}\}$ is moved by $j(\sigma)$. So there are at least as many things moved by $j(\sigma)$ as there are subsets of Y, namely $|V|$.

To complete the house of cards we need $j\sigma \cdot c$ to fix a moiety and move a moiety, and this is where things come unstuck. We can say this much: $j\sigma$ fixes $|V|$ things, and no set is equal to its complement, so $j\sigma \cdot c$ moves $|V|$ things... but how many things does it *fix*? Well, this is the same as asking: how many sets x are there such that $\sigma“x = V \setminus x$? And the answer to that is: *none*, beco's some sets are fixed by σ, and each of those fixed sets must belong to x or to $V \setminus x$ and cannot be moved from one to the other by σ!

Evidently we weren't clever enough – or not lucky enough. It might be worth trying harder to find an involution π such that π and $j\pi \cdot c$ are conjugate. If we succeed then we refute AC_2, and the failure might be instructive.

Chapter 10

Leftovers from the Boffa festschrift paper

There are various loose ends to be tidied up.

- There is the game G^*_X played like G_X except that player `I` wins if it ever comes to an end (as opposed to being the last player!). There is a dual version in which `II` is trying to get it to end.

- Some miscellaneous facts about $\subset_\infty$.

  We know that $\subset_\infty$ is a strict partial order. Is it also a complete lattice? The (easy) answer is: no. Consider the two sequences of a_n and b_n as above.

$$a_0 := \emptyset; \quad a_{n+1} := \{b_n\}; \quad b_0 := V; \quad b_{n+1} := V \setminus \{a_n\}$$

  If we were to have $a_\infty := \bigvee_{i \in \mathbb{N}} a_i$ and $b_\infty := \bigwedge_{i \in \mathbb{N}} b_i$ we would have $a_\infty = \{b_\infty\}$ and $b_\infty = V \setminus \{a_\infty\}$. This is independent of (for example) *NF*. (See Forster [1995] proposition 3.1.5.)

Antimorphisms not monotonic on the $\subset_\infty$. For suppose they were. Then let σ be an antimorphism. Then

$$\sigma`x \ < \ \sigma`y$$

iff

$$-\sigma``x \ < \ V \setminus \sigma``y$$

iff

$$\sigma``y \ < \ \sigma``x$$

iff (several cases! such as)

$$(\exists z \in \sigma``(x \setminus y))(\forall w \in \sigma``(y \setminus x))(z < w)$$

Now reletter

$$(\exists z \in (x \setminus y))(\forall w \in (y \setminus x))((\sigma^{-1}\text{‘}z < \sigma^{-1}\text{‘}w)$$

and invoke monotonicity

$$(\exists z \in (x \setminus y))(\forall w \in (y \setminus x))(z < w)$$

which is

$$y < x$$

so σ would have to be antimonotonic.

Note that $(\forall\sigma)(j^n\text{‘}\sigma$ is an automorphism of $\langle V, \subseteq_n\rangle)$. So the class of automorphisms of the canonical p.o. is closed under j.

Now consider the CPO $V \times V$ ordered by pointwise set inclusion. Let S be the map $\lambda X.\langle\mathcal{P}(\texttt{snd}X), \text{Ƅ}(\texttt{fst}X)\rangle$ which is an increasing map $V \times V \to V \times V$. $V \times V$ is clearly chain complete (directed-complete, indeed), and so has a fixed point for S. The displayed formula tells us that the least such fixed point is the pair $\langle\texttt{II}, \texttt{I}\rangle$. We will need this slightly cumbersome formulation in the proof of the following theorem which ties together the $\in$-game and fixed points for P.

Theorem 15

$$(\forall x \in \texttt{II})(\forall y \in \texttt{I})(x \subset_\infty y)$$

Proof:

There is a simple proof by induction on pseudorank. If $y \in \texttt{I}$ and $x \in \texttt{II}$ then there is $z \in y \cap \texttt{II}$. This z cannot be in x, because $x \subseteq \texttt{I}$ and by induction hypothesis it precedes everything in x. So $x \subset_\infty y$. ■

However, some readers might prefer something a bit more general and robust.

Proof:

Suppose $P(R) \subseteq R$. Suppose $A \cap B = \emptyset$ and $\langle A, B\rangle$ satisfies $(\forall x \in A)(\forall y \in B)(xRy)$. Then so does $\langle\mathcal{P}(B), \text{Ƅ}(A)\rangle$. $\mathcal{P}(B) \cap \text{Ƅ}(A) = \emptyset$ is easy. Suppose $x \in \mathcal{P}(B)$, $y \in \text{Ƅ}(A)$. Notice that $y \setminus x$ is nonempty because y meets A and $x \subseteq B$. Everything in $x \setminus y$ is in B, and there must be something in $y \setminus x$ that is in A, so $\langle x, y\rangle \in P(R)$ whence $\langle x, y\rangle \in R$.

Now consider the CPO $\mathcal{P} = \langle P, \leq_P\rangle$ where P is the set of of pairs $\langle A, B\rangle$ where $(\forall x \in A)(\forall y \in B)(xRy)$, and $\leq_P$ is pointwise set inclusion. Let S be the map $\lambda X.\langle\mathcal{P}(\texttt{snd}X), \text{Ƅ}(\texttt{fst}X\rangle$ which is an increasing map $\mathcal{P} \to \mathcal{P}$. $\mathcal{P}$ is clearly chain complete (closed under directed unions), and so has a fixed point for S. But this fixed point for S must be above the least fixed point for S in the CPO $V \times V$, so by induction we infer that the least fixed point for S, namely $\langle\texttt{II}, \texttt{I}\rangle$ satisfies $(\forall x \in \texttt{II})(\forall y \in \texttt{I})(x\,R\,y)$. ■

Andy Pitts suggested to me that x and y are Forster/Malitz bisimilar iff there is a bisimulation between the transitive closures $TC(x)$ and $TC(y)$. This isn't quite true. The left-to-right implication is good: if $X \sim_{min} Y$ then $=$ has a strategy to stay alive in the game $G_{X=Y}$ for ever. The union of any number of nondeterministic strategies to do this is another nondeterministic strategy, so think about the union of all of them. It's a bisimulation. But the converse direction is not good. Consider V and $V \setminus \emptyset$. These have the same transitive closure but $\not=$ Wins the Malitz game by picking $\emptyset$. To state the version of this *aperçu* that *is* true we need the notion of a **layered bisimulation**.

A layered bisimulation between X and Y is a family of binary relations $\simeq_n \subseteq \bigcup^n X \times \bigcup^n Y$ such that $\simeq^+_{n+1} = \simeq_n$. Then

Remark 38 *$X \sim_{min} Y$ iff there is a layered bisimulation between X and Y.*

Proof: Obvious.

10.1 Lifts

I'n beginning to understand this better. Lifts defined using a leading existential quantifier will preserve irreflexivity and are to be used on strict partial orders; lifts defined using universal quantifiers preserve reflexivity and are to be used on quasiorders. Partial orders are a red herring.

10.1.1 Lifts for strict partial orders

Let's look at some lifts defined using existential quantifiers, and apply them to strict partial orders.

First there is the 'obvious' one:

$$AP(>)B \text{ iff } (\exists x \in A)(\forall y \in B)(x < y)$$

Clearly if $<$ is irreflexive then $P(<)$ is irreflexive, and if $<$ is transitive then $P(<)$ is transitive, so it carries strict partial orders to strict partial orders. It actually – quite separately – preserves asymmetry but (for the moment) we don't care.

Only trouble is, $P(<)$ is an incredibly strong relation. Let's redefine P so as to get a lift that might be more useful.

$$A\,P(>)\,B \text{ iff } (\exists x \in A \setminus B)(\forall y \in B \setminus A)(x < y)$$

Evidently $P(<)$ is always irreflexive. It preserves asymmetry.

Sadly it does not preserve transitivity, as the following example shows.

Define $<$ on the domain $\{a, b, c, d\}$ by $a < b$ and $c < d$. Then $\{a, c\}P(<)\{a, d\}$ and $\{a, d\}P(<)\{b, d\}$ but not $\{a, c\}$ below $\{b, d\}$.[1]

Despite this we have the following small factoid which may be useful one day:

Let $<$ be a strict total order, then $P(<)$ is transitive.

[1] Is this yet another example of the bad behaviour of the set some combinatorists call '$\mathbb{N}$'? – beco's its graph looks like the letter 'N'. See Rival, Contemp Maths **65** pp. 263-285. Actually this thing is not an N but we could add one arm and get an N

Proof:

Let A, B and C be three sets such that $A\ P(>)\ B$ and $B\ P(>)\ C$. That is to say, there is $a \in A \setminus B$ which $<$ everything in $B \setminus A$, and $b \in B \setminus C$ which $<$ everything in $C \setminus B$. We seek an $x \in A \setminus C$ which $<$ everything in $C \setminus A$. In fact it will turn out that this x can always be taken to be a or b. Since a may be in $A \setminus C$ or in $A \cap C$, and b may be in $B \setminus A$ or $B \cap A$ there are four cases to consider.

$a \in A \setminus C \ \wedge\ b \in B \setminus A$

> Then $a < b$, so $a <$ everything in $C \setminus B$ and we need only check that $a <$ everything in $(B \cap C) \setminus A$. But $a <$ everything in $B \setminus A$. So set x to be a.

$a \in A \cap C \ \wedge\ b \in B \setminus A$

> This case is impossible because $b \in (B \setminus A)$ implies $a < b$ and $a \in A \cap C$ implies $a \in (C \setminus B)$ whence $b < a$.

$a \in A \setminus C \ \wedge\ b \in B \cap A$

> Both a and b are in $A \setminus C$ in this case so both are candidates for x. $a <$ everything in $(B \setminus A)$ and $b <$ everything in $(C \setminus B)$. Since $<$ is a total order one of them is smaller, and that smaller one is $<$ everything in $(B \setminus A) \cup (C \setminus B)$ which is certainly a superset of $C \setminus A$.

$a \in A \cap C \ \wedge\ b \in B \cap A$

> $b <$ everything in $C \setminus B$ so in particular $b < a$. But $a <$ everything in $B \setminus A$ so $b <$ everything in $((C \setminus B) \cup (B \setminus A))$ which is certainly a superset of $C \setminus A$ as before, and $b \in A \setminus C$ so we can take x to be b.

■

Sadly this really needs the input to be a strict *total* order.
It might be worth ascertaining what properties P preserves
This suggests that we should use instead the following definition.

Definition 10 *$x\ P(>)\ y$ if there is a finite antichain $a \subseteq (x \setminus y)$ such that $(\forall y' \in y \setminus x)(\exists x' \in a)(y' > x')$.*

Why an antichain? Well, if it is just a subset then P of a strict partial order might not be irreflexive. And why finite? This is to ensure that P is monotone. That is to say, if $\leq'$ is stronger than $\leq$ then $P(\leq')$ is stronger than $P(\leq')$. If we do not require antichains to be finite we might find that $X\ P(\leq')\ Y$ in virtue of some antichain $\subseteq Y \setminus X$ and we can add ordered pairs to $\leq$ to get a relation according to which the antichain is a chain with no least element. If the antichain is required to be finite this cannot happen.

Lemma 13 *P is a monotone function from the CPO (chain-complete poset) of all strict partial orders of the universe (partially ordered by set inclusion) into itself.*

Proof: This new P evidently preserves irreflexivity as before. The only hard part is to show that it takes transitive relations to transitive relations.

Let $>$ be a transitive relation and let A, B and C be three subsets of Dom($>$) such that $A\ P(>)\ B$ and $B\ P(>)\ C$. That is to say, there are antichains $a \subseteq A \setminus B$ such that everything in $(B \setminus A) >$ something in a, and $b \subseteq B \setminus C$ such that everything in $(C \setminus B) >$ something in b.

We will show that the antichain included in $A \setminus C$ that we need as a witness to $A\ P(>)\ C$ can be taken to be $(a \setminus C) \cup (b \cap A)$. Or rather, it can be taken to be that antichain obtained from $(a \setminus C) \cup (b \cap A)$ by discarding nonminimal elements.

We'd better start by showing that $(a \setminus C) \cup (b \cap A)$ cannot be empty. Suppose it were and $x \in b$. Then x is in $B \setminus A$ and is bigger than something in a, y, say. Then $y \in C \setminus B$ and is bigger than something in b contradicting the fact that b is an antichain. This argument will be recycled twice in what follows.

Let w be an arbitrary element of $C \setminus A$. We will show that w is above something in $(a \setminus C) \cup (b \cap A)$. There are two cases to consider.

(i) $w \in C \cap B$. Then it is bigger than something in a. If it is bigger than something in $(a \setminus C)$ we can stop, so suppose it isn't. Then it is bigger than something, x say, that is in $a \cap C$. Things in $a \cap C$ are in $C \setminus B$ and so must be bigger than something in b. If x is bigger than something in $b \cap A$ we can stop (since this implies that w is bigger than something in $b \cap A$), so suppose x is bigger than something in $b \setminus A$. Things in $b \setminus A$ are in $B \setminus A$ and therefore are bigger than something in a, so x is bigger than something in a. But this is impossible because $x \in a$.

(ii) $w \in (C \setminus B)$. Then it is bigger than something in b. If it is bigger than something in $(b \cap A)$ we can stop, so suppose it isn't. Then it is bigger than something, x say, that is in $b \setminus A$. Things in $b \setminus A$ are in $B \setminus A$ and are bigger than something in a. If x is bigger than something in $a \setminus C$ we can stop (since this implies that w is bigger than something in $a \setminus C$) so suppose x is bigger than something in $a \cap C$. Things in $a \cap C$ are in $C \setminus B$ and so are bigger than something in b, so x is bigger than something in b. But this is impossible because $x \in b$.

■

This assures us that we can safely conclude that there is a least fixed point for P and that it is indeed a strict partial order. (Notice that the collection of strict partial orders of an arbitrary set is merely a CPO under $\subseteq$ *not* a complete lattice – unlike the collection of quasi-orders of an arbitrary set – so there is no presumption that there will be a unique greatest fixed point.

Let's just check that the same works for P defined the "right" way round.

Definition 11 *$x\ P(>)\ y$ if there is a finite antichain $a \subseteq (x \setminus y)$ such that $(\forall y' \in y \setminus x)(\exists x' \in a)(y' < x')$.*

Only the last occurrence of '$<$' has been changed.

equivalently

$yP(<)x$ if there is a finite antichain $a \subseteq (x \setminus y)$ such that $(\forall y' \in y \setminus x)(\exists x' \in a)(y' < x')$.

10.1.2 Lifts of quasiorders

The structure of this section should echo that of section ref, the the obvious $\forall\exists$ lift is well understood, so we procede immediately to

$$XP(\leq)Y \longleftrightarrow (\forall x \in X \setminus Y)(\exists y \in Y \setminus X)(x \leq y)$$

$P(\leq)$ is vacuously reflexive: no problem there. Trouble is, it isn't transitive.

Consider the carrier set $\{a, b, c\}$, with $c \leq a$, $b \leq a \leq b$. Set $Z := \{a\}$; $Y := \{b, c\}$; $X := \{a, c\}$. Then $XP(\leq)Y$ and $YP(\leq)Z$ but not $XP(\leq)Z$.

It is not yet clear to me whether or not this feature relies on this $\leq$ being a quasi order and not a partial order.

I think i now have a slightly clearer idea why this finite antichain is a good idea, to the extent that it is. I think the point is that if $\langle Q, \leq\rangle$ is a WQO, then $\langle \mathcal{P}(Q), P(\leq)\rangle$ is one too. When comparing two subsets of Q all we have to look at is the two (finite!) sets of minimal elements of them. To complete this explanation i need to establish that if $\langle Q, \leq\rangle$ is a WQO, then the set of antichains in Q is WQO by "everything in me $\leq$ something in you".

This ought to be easy!

Notice that this operation P is obviously monotone but not obviously increasing, in the sense that we do not expect (the graph of) $P(<)$ to be a superset of the graph of $<$. For example if $x = \{y\}$ and $y = \{x\}$ and we add the ordered pair $\langle x, y\rangle$ to a relation R over a domain containing x and y we find that $P(R)$ contains $\langle y, x\rangle$.

	antisymmetrical	not antisymmetrical
reflexive	partial order	quasi-order
irreflexive	strict partial order	?

The question mark is my way of reminding myself that there isn't a nice (read "horn") property that looks like transitivity with strictness (irreflexivity) and nontrivial failure of antisymmetry. This is because $R(x, y)$ and $R(y, x)$ give $R(x, x)$ by transitivity, contradicting irreflexivity. We would need to assert that $R(x, y) \wedge R(y, z)$ implies $R(x, z)$ only if $x \neq z$.

Perhaps this next bit belongs in TZTstuff.tex

No model of T$\mathbb{Z}$T can contain all copies of the set `II`. (That is to say, it cannot have `II` at all types). (This is proved very similarly to the way that we prove the non-obvious fact that WF cannot be a set at any level of any model of T$\mathbb{Z}$T.) Suppose it does. Think about `I` at level n. This set is a win for player `II` and has rank α, say. Its rank is the sup of the ranks of its members beco's `I` can choose how long he wants to live. Now think about `I` two levels up. `I` is going to lose this game of course, but he can play {`II`}, forcing `II` to pick the set `II` at level n so the rank of `II` at level $n+2$ must be greater. This gives us a descending sequence of ordinals.

Notice now that if `II` is present at any level it is present at all later levels, which is impossible, so there are no levels containing `II`.

In fact this doesn't depend on the model being $\in$-determinate.

Isn't the point that if `I` or `II` exist at any type then they exist at all types, and that is impossible, rather in the way that WF if it exists at one level exists at all levels. I think this is correct: if we have `I` and `II` at a given type we can recover `I` and `II` one type down beco's b and $\mathcal{P}$ are injective.

Can we obtain models of strong extensionality by omitting types?

10.1.3 Totally ordering term models

NF_2 is the set theory whose axioms are extensionality, existence of $\{x\}$, $V \setminus x$ and $x \cup y$. NF0 is the set theory whose axioms are extensionality and comprehension for stratified quantifier-free formulæ. This is actually the same as adding to NF_2 an axiom $(\forall x)(\exists y)(\forall z)(z \in y \longleftrightarrow x \in z)$. The operation involved here is notated "$B(x)$". $\overline{Bx}$ is $V \setminus B(x)$. We need a notion of **rank** of NF0 terms.

Rank of $\emptyset$ is 0; rank of $V \setminus t :=$ the rank of t;

rank of $t_1 \cup t_2 := max(\text{rank of } t_1, \text{rank of } t_2)$;

rank of $\{t\} :=$ (rank of t) + 1.

Those were the NF_2 operations. They increase rank only by a finite amount. Finally we have the characteristic NF0 operation.

rank of $B`t :=$ the first limit ordinal $>$ the rank of t.

Another fact we will need is that

Remark 39 $X \subset_\alpha Y \longleftrightarrow (V \setminus Y \subset_\alpha V \setminus X)$.

We now prove by induction on rank that

Theorem 16 $\subset_{\omega+\alpha}$ *(strictly) totally orders NF0 terms of rank at most* α.

Proof:

We will actually prove something a bit stronger, since the lift we will be working with here gives a weaker strict order than the P we considered earlier. We will use the lexicographic lift:

$$X\, P(\leq)\, Y \text{ iff } (\exists y \in Y \setminus X)(\forall x \in X \setminus Y)(y \leq x).^2$$

The reasons for our abandoning it originally – namely that it does not always output transitive relations – do not cause problems in this special context.

We start with a discussion of terms of finite rank. Consider the two sequences $a_0 := \emptyset;\;\; a_{n+1} := \{b_n\}$ and $b_0 := V;\;\; b_{n+1} := V \setminus \{a_n\}$. It is simple to prove by induction on n that the $\{a_i : i < n\}$ are the first n things and $\{b_i : i < n\}$ the last n things in the poset of NF_2 terms ordered by $\subset_\omega$. (The b_n don't matter,

[2] The quantifiers could be in either order and so could the inequality. Four possibilities!

but we will need to make use of the fact that the collection of a_n is wellordered by $\subset_\omega$.)

Now we can consider terms of finite rank. The case $\alpha = 0$ is just $\emptyset$ and V. The remaining cases where α is finite are those with NF_2 constructors only. Suppose we are trying to compare two sets X and Y denoted by terms of rank at most α. In NF_2 every term denotes either a finite object or a cofinite object. If X and Y are both finite we can compare the least member of $X \setminus Y$ with the least member of $Y \setminus X$ by induction hypothesis; if X and Y are cofinite then $V \setminus X$ and $V \setminus Y$ are finite and we can use remark 39 to reduce this case to the preceding one. The same trick reduces the final case (one of X and Y finite, the other cofinite) without loss of generality to comparing a cofinite object with a finite object.

Now we appeal to the fact that the a_n with $n \in \mathbb{N}$ form an initial segment of V under $\subset_\omega$. Any finite object can contain only finitely many of them and any cofinite object must contain all but finitely many of them. If the finite object contains none of the a_n then it is later than the cofinite object in the sense of $\subset_\omega$. Otherwise compare the bottom a_n in the cofinite object with the bottom a_n in the finite object.

Now for terms of transfinite rank. Assume true for $\beta < \alpha$. A directed union of strict total orders is a total order and P of a strict total order is a total order so irrespective of whether α is successor or limit $\subset_\alpha$ (restricted to terms of rank no more than α) is at least transitive. We already know that it is irreflexive so all that has to be proved is trichotomy.

Consider a couple of $NF0$ terms of rank at most α: $\bigvee_{i \in I} \bigwedge_{j \in J} t_{i,j}$ and $\bigvee_{k \in K} \bigwedge_{l \in L} s_{k,l}$ where each s and t is $B`r$ or $\overline{Br}$ for rs of lower rank.

If

$$\bigvee_{i \in I} \bigwedge_{j \in J} t_{i,j} \subset_\alpha \bigvee_{k \in K} \bigwedge_{l \in L} s_{k,l}$$

is to be true there is an antichain $\subseteq$ the set on the right (minus the set on the left) that is below everything in the set on the left (minus the set on the right) in the sense of $\subset_\beta$ (with $\beta < \alpha$)[3]. In fact we will even be able to show that the antichain has only one element, because we are simultaneously proving by induction that the order is total! Now both the set on the left and the set on the right have finitely many $\subset_\beta$ minimal elements. This is because they are a union of finitely many things each of which is an intersection of things of the form $B`x$ and $\overline{By}$, and any such intersection has a unique $\subseteq$-minimal member which will also be the unique $\subset_\beta$-minimal member.

So if there is a thing in the set on the right (minus the set on the left) that is is below everything in the set on the left (minus the set on the right) in the sense of $\subset_\beta$ then it must be one of those minimal elements, and it is enough

[3]Readers who feel that the subscript should be $\omega + \alpha$ should remember that if $\alpha \geq \omega^2$ these two ordinals are the same

to check that it is less than the minimal elements of the set on the left (minus the set on the right). Now these minimal elements are just finite sets of things of lower rank. By induction hypothesis all terms of lower rank are ordered by some $\subset_\beta$ (with $\beta < \alpha$) and so certainly finite sets of them are too. So really all we have to do is compare the minimal elements of the set on the left (minus the set on the right) with the minimal elements of the set on the right (minus the set on the left). There is only a finite set of them and it is totally ordered, so there is a least one (in the sense of $\subset_\beta$).

The alert reader will have noticed that this is not the most general form of an $NF0$ word. There should be addition and deletion of singletons. But this makes no difference to the fact that we only need consider a finite basis, which is the bit that does the work! ■

As it happens NF0 has a model in which every element is the denotation of a closed term, a **term model**. This model is unique.

Corollary 7 *The term model for NF0 is totally ordered by the least fixed point for P*

Of course term models can always be totally ordered in canonical ways, but one does not routinely expect to be able to describe such a total ordering within the language for which the structure is a model. For some light relief, I shall write out this formula in fairly primitive notation.

NF0 is too weak to manipulate ordered pairs so we will have to represent strict partial orders as the set of their initial segments. This motivates the following definitions.

Let $\text{Prec}(R, x, y)$ ("x precedes y according to R") abbreviate

$$(\forall z \in R)(y \in z \to x \in z) \wedge x \neq y.$$

Let $\text{Refines}(R, S)$ ("R refines S") abbreviate
$(\forall xy)(\text{Prec}(S, x, y) \to \text{Prec}(R, x, y))$.
Let $\text{Prec}(R^+, x, y)$ abbreviate

$$(\exists x' \in y \setminus x)(\forall y' \in x \setminus y)(\text{Prec}(R, x', y'))).$$

Then finally
$x \subset_\infty y$ is $(\forall R)(\text{Refines}(R, R^+) \to \text{Prec}(R, x, y))$
Then in the term model it is true that $\subset_\infty$ is a strict total order.

It would be nice to know whether or not this result extends to theories stronger than NF0.

What can one say about other fixed points for P? We can invoke a fixed-point theorem for CPO's to argue that P must have lots of fixed points – a CPO of them in fact. One can then invoke Zorn's lemma to conclude that there are maximal fixed points. By reasoning in the manner of the standard proof of the order extension principle from Zorn's lemma one can deduce that any maximal fixed point must be a total order. We now reach a point at which the na¨ive set theory in which we have been operating will no longer work. Let us assume DC for the moment, and let $\langle X, \leq \rangle$ be a total order that is not wellfounded. Take $X' \subseteq X$ with no $\leq$-least element. Use DC to pick two descending sequences $\langle a_n : n \in \mathbb{N} \rangle$ and $\langle b_n : n \in \mathbb{N} \rangle$ with $b_{n+1} < a_n$ and $a_{n+1} < b_n$. The domains of these two sequences are a pair of subsets of X which are incomparable under $P(\leq)$. In other words, P of a strict total order R is a strict total order only if R is a wellorder, and even then $P(R)$ will not be wellfounded. So if DC holds, no fixed point for P can be a total order. But any maximal fixed point must be a total order, and Zorn's lemma tells us that there are some. Therefore the axiom of choice is false.

The message seems to be that this is the point at which we should start treating these ideas axiomatically. That should be the scope of another article.

10.2 Lifting quasi-orders: fixed points and more games

The obvious order on partitions of a set is simply the lift of the identity relation on the set.

If X is a set that meets $\mathcal{P}(X)$, its power set, and $\sim$ is an equivalence relation on X, and if $\sim^+$ agrees with $\sim$ on $X \cap \mathcal{P}(X)$ we say that $\sim$ is a **bisimulation**. (Hinnion called them **contractions** but this usage doesn't seem to have caught on.) Typically we will be interested in this only when $X \subseteq \mathcal{P}(X)$, which is to say when X is **transitive**.

If $\leq$ is a transitive relation on a domain D define $\leq^+$ on $\mathcal{P}(D)$ by $X \ \leq^+ \ Y$ by $(\exists y \in Y)(\forall x \in X)(y \leq x)$.

This operation preserves transitivity but apparently not much else.

It is simple to check that the collection of quasi-orders on the universe is a complete lattice and that $+$ is a continuous increasing function from this complete lattice into itself. Thus by the Tarski-Knaster theorem there will be a complete lattice of fixed points. The following is the Aczel-Hintikka game for these fixed points.

HOLE

Now we are in a position to show that the least bisimulation is indeed the intersection of a quasi-order and its converse.

THEOREM 17 $(\forall x)(\forall y)(x \sim_{min} y \longleftrightarrow (x <_o y \wedge y <_o x))$

Proof: L $\to$ R

Clearly if $x \sim_{min} y$ then $=$ has a strategy to win $G_{x=y}$ in finitely many moves. Arthur can use $=$'s Winning strategy to play in both $G_{x \leq y}$ and $G_{y \leq x}$. Since $=$'s strategy wins in $G_{x=y}$ in finitely many moves, Arthur must win $G_{x \leq y}$ and $G_{y \leq x}$ in finitely many moves.

R $\to$ L

Now suppose $x <_o y$ and $y <_o x$. That is to say that Arthur has winning strategies σ and τ in the open games $G_{x \leq y}$ and $G_{y \leq x}$. Player $=$ can use these in $G_{x=y}$ as follows. Whatever $\neq$ plays in x (or y), $=$ can reply in y (or x) using τ (or σ). Since she is never at a loss for a reply, she Wins the closed game $G_{x=y}$. ■

We note without proof that an analogous result holds for the greatest fixed points. That is to say, if we define $x \sim_{max} y$ to hold iff $=$ Wins the *open* game $G_{x=y}$ and $x <_c y$ as above then $(\forall x)(\forall y)(x \sim_{max} y \longleftrightarrow (x <_c y \wedge y <_c x))$.

Might be an idea to check this

If R is a binary relation, let R^+ be $\{\langle X, Y\rangle : (\forall x \in X)(\exists y \in Y)(R(x, y))\}$.

I think this '+' notation is due to Hinnion. It takes quasiorders to quasiorders and the set of all quasiorders is a complete lattice under $\subseteq$ and has lots of fixed points. The least fixed point corresponds to the game where Arthur wins all infinite plays and the greatest fixed point corresponds to the game where Bertha wins all infinite plays.

Say $x <_o y$ if Bertha has a Winning strategy for the open game and $x <_c y$ if Bertha has a Winning strategy for the closed game.

I shall use the molecular letter '$\rho\beta$' ("ranked below") to range over fixed points and prefixed points and postfixed points.

The first point to notice is that if R is reflexive then R^+ is a superset of $\subseteq$. The operation is increasing in the sense that $R \subseteq S \to R^+ \subseteq S^+$. Suppose $R \subseteq S$ and xR^+y. Then for every $z \in x$ there is $w \in y$ $R(z, w)$ whence $S(z, w)$ whence $R^+ \subseteq S^+$.

Now for limits. Suppose $R_\infty = \bigcup_{i \in I} R_i$. Clearly, for all $i \in I$, ${R_i}^+ \subseteq {R_\infty}^+$ so $\bigcup_{i \in I} {R_i}^+ \subseteq {R_\infty}^+$. For the converse

$x{R_\infty}^+y$ iff $(\forall z \in x)(\exists w \in y)(zR_\infty w)$ iff $(\forall z \in x)(\exists w \in y)(\exists i)(zR_i w)$ so it is not cts at limits. (Presumably this is for the same reason that $\mathcal{P}$ is not continuous.)

Remark 40 $\in \subseteq$ *the GFP*

Proof: If $x \in y$ then $(\forall z \in x)(\exists w \in y)(z \in w)$ ... and the w is of course x itself. That is to say $\in \subseteq \in^+$: $\in$ is a postfixed point

Obvious questions: does $\rho\beta$ extend $\in$? Is it connected? Is it wellfounded? Is $\rho\beta$ restricted to wellfounded sets wellfounded? Is it a WQO or a BQO?

There are other way of deriving a rank relation. We could consider sets containing $\emptyset$ and closed under $\mathcal{P}$ and

(i) unions or

(ii) directed unions or

(iii) unions of chains.

Then if X is such a set we say $x\,\rho\beta\,y$ if $(\forall Y \in X)(y \in Y \to x \in Y)$. For each of these three we can prove by induction that the least fixed point consists (for any $X \supseteq \mathcal{P}(X)$), entirely of sets in X. We should also prove that if X is a prefixed point under the heading (i) (ii) or (iii) then every wellfounded set is in a member of X.

We need to check that the LFP and the GFP are nontrivial. The identity is a postfixed point and the universal relation is a prefixed point. (Incidentally this shows that the GFP is reflexive) But LFP $\subseteq$ GFP? It is if there is a fixed point.

Remark 41 *The GFP is transitive*

Proof:

First we show that $\rho\beta^+ \subseteq \rho\beta \wedge \rho\beta'^+ \subseteq \rho\beta' \to (\rho\beta \circ \rho\beta')^+ \subseteq \rho\beta \circ \rho\beta'$.

Suppose $\langle X, Z\rangle \in (\rho\beta \circ \rho\beta')^+$. That is to say, $(\forall x \in X)(\exists z \in Z)(\langle x, z\rangle \in \rho\beta \circ \rho\beta')$. This is $(\forall x \in X)(\exists z \in Z)(\exists y)(\langle x, y\rangle \in \rho\beta \wedge \langle y, z\rangle \in \rho\beta)$. or $(\forall x \in X)(\exists y)(\langle x, y\rangle \in \rho\beta \wedge (\exists z \in Z)(\langle y, z\rangle \in \rho\beta))$. Then for this y we have $\langle X, \{y\}\rangle \in \rho\beta^+$ and thence $\langle X, \{y\}\rangle \in \rho\beta$ and $\langle \{y\}, Z\rangle \in \rho\beta'^+$ and thence $\langle \{y\}, Z\rangle \in \rho\beta'$ which is to say $\langle X, Z\rangle \in \rho\beta \circ \rho\beta'$.

Similarly the set of post-fixed points is closed under composition, which means that the GFP is transitive.

We can prove by $\in$-induction that any fixed point is reflexive on wellfounded sets.

Remark 42 *Any two fixed points agree on wellfounded sets.*

Proof: Let $\rho\beta$ and $\rho\beta'$ be fixed points. We will show that for all wellfounded x and for all y, $\langle x, y\rangle \in \rho\beta$ iff $\langle x, y\rangle \in \rho\beta'$.

We need to show that $\mathcal{P}(\{x : (\forall y)(\langle x, y\rangle \in \rho\beta \longleftrightarrow \langle x, y\rangle \in \rho\beta')\}) \subseteq \{x : (\forall y)(\langle x, y\rangle \in \rho\beta \longleftrightarrow \langle x, y\rangle \in \rho\beta')\}$.

Let X be a subset of $\{x : (\forall y)(\langle x, y\rangle \in \rho\beta \longleftrightarrow \langle x, y\rangle \in \rho\beta')\}$. Then for all Y
$\langle X, Y\rangle \in \rho\beta$ iff
$(\forall x \in X)(\exists y \in Y)(\langle x, y\rangle \in \rho\beta)$ which by induction hypothesis is the same as
$(\forall x \in X)(\exists y \in Y)(\langle x, y\rangle \in \rho\beta')$ which is
$\langle X, Y\rangle \in \rho\beta'$

We will also need to show that for all wellfounded y and for all x, $\langle x, y\rangle \in \rho\beta$ iff $\langle x, y\rangle \in \rho\beta'$.

We need to show that $\mathcal{P}(\{y : (\forall x)(\langle x, y\rangle \in \rho\beta \longleftrightarrow \langle x, y\rangle \in \rho\beta'\}) \subseteq \{y : (\forall x)(\langle x, y\rangle \in \rho\beta \longleftrightarrow \langle x, y\rangle \in \rho\beta'\}$.

Let Y be a subset of $\{y : (\forall x)(\langle x, y\rangle \in \rho\beta \longleftrightarrow \langle x, y\rangle \in \rho\beta'\}$. Then for all X
$\langle X, Y\rangle \in \rho\beta$ iff
$(\forall x \in X)(\exists y \in Y)(\langle x, y\rangle \in \rho\beta)$ which by induction hypothesis is the same as
$(\forall x \in X)(\exists y \in Y)(\langle x, y\rangle \in \rho\beta')$ which is
$\langle X, Y\rangle \in \rho\beta'$

Remark 43 *If* $\rho\beta^+ \subseteq \rho\beta$ *then*
$(\forall y \in WF)(\forall x)(\langle x, y\rangle \in \rho\beta \vee \langle y, x\rangle \in \rho\beta)$

Proof:

We prove by $\in$-induction on 'y' that $(\forall x)(\langle x, y\rangle \in \rho\beta \vee \langle y, x\rangle \in \rho\beta)$. Suppose this is true for all members of Y, and let X be an arbitrary set. Then either everything in Y is $\rho\beta$-related to something in X (in which case $\langle Y, X\rangle \in \rho\beta^+$ and therefore also in $\rho\beta$) or there is something in Y not $\rho\beta$-related to anything in X, in which case, by induction hypothesis, everything in X is $\rho\beta$-related to it, and $\langle X, Y\rangle \in \rho\beta^+$ (and therefore in $\rho\beta$) follows.

■

Remark 44 *If* $\rho\beta \subseteq \rho\beta^+$ *and* $\mathcal{P}(X) \subseteq X$ *then* $(\forall y \in WF)(\forall x)(\langle x, y\rangle \in \rho\beta \to x \in X)$.

If $\rho\beta \subseteq \rho\beta^+$ and $\mathcal{P}(X) \subseteq X$ we prove by $\in$-induction on 'y' that $(\forall x)(\langle x, y\rangle \in \rho\beta \to x \in X)$. Suppose $(\forall y \in Y)(\forall x)(\langle x, y\rangle \in \rho\beta \to x \in X)$ and $\langle X', Y\rangle \in \rho\beta$. $\langle X', Y\rangle \in \rho\beta$ gives $\langle X', Y\rangle \in \rho\beta^+$ which is to say $(\forall x \in X')(\exists y \in Y)(\langle x, y\rangle \in \rho\beta)$. By induction hypothesis this implies that $(\forall x \in X')(x \in X)$ which is $X' \in \mathcal{P}(X)$ but $\mathcal{P}(X) \subseteq X$ whence $X' \in X$ as desired. ■

Corollary 8 *If* $\rho\beta \subseteq \rho\beta^+$, $y \in WF$ *and* $x\ \rho\beta\ y$ *then* $x \in WF$

One obvious conjecture is that if $\rho\beta$ is a fixed point then $x \in y \to \langle x, y\rangle \in \rho\beta$.

There is an obvious proof by $\in$-induction on 'x' that $(\forall y)(x \in y \to \langle x, y\rangle \in \rho\beta)$ but the assertion is unstratified and so the inductive proof is obstructed, at least in *NF*.

Suppose $\rho\beta^+ \subseteq \rho\beta$ and x is an illfounded set such that $y\ \rho\beta\ x \to y \in WF$. Since x is illfounded it has a member x' that is illfounded. $\neg(x' \rho\beta\ x)$ because everything related to x is wellfounded. Now suppose $y \rho\beta x'$. Then $\{y\}\rho\beta^+ x$ and $\{y\}\rho\beta x$ (since $\rho\beta^+ \subseteq \rho\beta$) and $\{y\}$ is wellfounded. So y is wellfounded as well, and x' is similarly minimal.

Now suppose x is such that $G \circ F(x) \subseteq x$. Then $F(x) \in x$. $G \circ F(x \setminus \{Fx\}) \subseteq G \circ F(x) \subseteq x$ As before, we want '$x \setminus \{Fx\}$' on the RHS. So we want

$z \in G \circ F(x \setminus \{Fx\}) \to z \neq Fx$ which is to say $Fx \notin G \circ F(x \setminus \{Fx\})$. But this follows by monotonicity and injectivity of F and the fact that $F(x \setminus \{Fx\})$ is the largest element of $G \circ F(x \setminus \{Fx\})$.

So $G \circ F(x \setminus \{Fx\}) \subseteq (x \setminus \{Fx\})$ and x was not minimal. ■

10.2.1 Fremlin: A transitive ordering on the class of relations

I extract an idea from a lecture given by T.Forster, 27.9.00.

Definition Let R and S be relations and X_0 and Y_0 sets. Consider the following game $G(X_0, R, Y_0, S)$.

Player A chooses $y_0 \in Y_0$.
Player B chooses $x_0 \in X_0$.
Player A chooses x_1 such that $(x_1, x_0) \in R$.
Player B chooses y_1 such that $(y_1, y_0) \in S$.
Player A chooses y_2 such that $(y_2, y_1) \in S$
Player B chooses x_2 such that $(x_2, x_1) \in R$
Player A chooses x_3 such that $(x_3, x_2) \in R$

and so on. Generally, at the nth move, for $n \geq 3$,

if $n = 4k$, Player B chooses y_{2k-1} such that $(y_{2k-1}, y_{2k-2}) \in S$,
if $n = 4k + 1$, Player A chooses y_{2k} such that $(y_{2k}, y_{2k-1}) \in S$,
if $n = 4k + 2$, Player B chooses x_{2k} such that $(x_{2k}, x_{2k-1}) \in R$,
if $n = 4k + 3$, Player A chooses x_{2k+1} such that $(x_{2k+1}, x_{2k}) \in R$.

If a player cannot move, the other wins; if the game continues for ever, A wins.

Now say that $(X_0, R) \preccurlyeq (Y_0, S)$ if A has a winning strategyi in the game $G(X_0, R, Y_0, S)$.

Note that because the payoff set for A is closed in $V^{\mathbb{N}}$, where V is such that $X_0 \cup Y_0 \subseteq V$ and $R \cup S \subseteq V \times V$, and is given the discrete topology, the game is determined.

Proposition $\preccurlyeq$ is transitive.

Proof: Suppose that $(X_0, R) \preccurlyeq (Y_0, S)$ and that $(Y_0, S) \preccurlyeq (Z_0, T)$. Let σ be a winning strategy for A in $G(X_0, R, Y_0, S)$ and τ a winning strategy for A in $G(Y_0, S, T, Z_0)$.

Construct a strategy υ for A in $G(X_0, R, T, Z_0)$ as follows.

A starts by playing $z_0 \in Z_0$, the first move prescribed by the strategy τ, and also by playing $y_0 \in Y_0$, the first move prescribed by σ.

B replies with $x_0 \in X_0$.

A plays x_1 prescribed by the rule σ in the game starting (y_0, x_0), and y_1 prescribed by the rule τ in the game starting (z_0, y_0).

B plays z_1.

A plays z_2 prescribed by the rule τ in the game starting (z_0, y_0, y_1, z_1), and y_2 prescribed by the rule σ in the game starting (y_0, x_0, x_1, y_1).

B plays x_2.

A plays x_3 prescribed by the rule σ in the game starting $(y_0, x_0, x_1, y_1, y_2, x_2)$, and y_3 prescribed by the rule τ in the game starting $(z_0, y_0, y_1, z_1, z_2, y_2)$.
Generally,

B plays x_{2k},

A plays x_{2k+1} following the rule σ in the game starting $(y_0, x_0, x_1, \ldots, y_{2k}, x_{2k})$, and y_{2k+1} following the rule τ in the game starting $(z_0, y_0, y_1, \ldots, z_{2k}, y_{2k})$,

B plays z_{2k+1},

A plays z_{2k+2} prescribed by the rule τ in the game starting $(z_0, y_0, \ldots, y_{2k+1}, z_{2k+1})$, and y_{2k+2} prescribed by the rule σ in the game starting $(y_0, x_0, \ldots, x_{2k+1}, y_{2k+1})$.
Since (if B has played legally) A always has a move, A wins. So $(X_0, R) \preccurlyeq (Z_0, T)$.

Problem:

Find invariants of relations from which it is easy to decide whether $(X_0, R) \preccurlyeq (Y_0, S)$.

If we define the game $G(X_0, R)$ as follows:

> A plays $x_0 \in X_0$,
>
> B plays x_{2k+1} such that $(x_{2k+1}, x_{2k}) \in R$,
>
> A plays x_{2k+2} such that $(x_{2k+2}, x_{2k+1}) \in R$,

with A winning if either B cannot move or the game goes on for ever, then if B wins $G(X_0, R)$ and A wins $G(Y_0, S)$, $(X_0, R) \preccurlyeq (Y_0, S)$. On the other hand, even if A wins $G(X_0, R)$, it is still possible to have $(X_0, R) \preccurlyeq (Y_0, S)$ if A can win $G(Y_0, S)$ sooner.

`From fremdh@essex.ac.uk Thu Sep 28 15:04:54 2000`

I extracted an idea from your talk and wrote it up in my own preferred language.

David Fremlin

`From t.forster@dpmms.cam.ac.uk Fri Sep 29 15:38:56 2000`

Dear David,

Thanks for your note. I think what is going on is that simultaneous displays of open (or closed) games give rise to quasiorders. With your usual merciless acuteness you spotted that the fact that this is a game played on $\in$ is completely irrelevant (but this was supposed to be a meeting on sets and games, after all) which i had been trying to conceal for that reason. I hadn't reflected on the fact you draw my attention to, namely that the binary relation in the two games need not be related in any way at all. What i find so intruiging about game theory is that one never ever seems - or at least i never feel that i manage - to reach the appropriate level of generality. With those games of Martin, for example, it seems to me that he is considering games where the two players pick elements from a set - as it might be X, and thereby build a play which is an element of $[X]^\omega$. The clever bit is using extra structure on X to put extra structure on the play, so it isn't just an ω-string. Where will it all end?

I found myself wondering to what extent this quasiorder is the same, au fond, as the quasiorder of Conway Games. I don't think i can pursue that for the present, as i have to turn this into something for the Boffa festschrift in a very small number of weeks....

Let's talk about this some more before too long. I seem to recall you have dining rights in Churchill - as do i, and very handy to the new building it is too. Do you come here often?

v best wishes

Thomas

10.3 The Equality Game

This is familiar: just maximal and minimal bisimulations.

10.3.1 Prologue on Aczel-Hintikka Games

Aczel-Hintikka games are a very pretty way of presenting fixed points. In general they add nothing of substance to the material they enable one to present, and this is presumably why Aczel never published the work he did on them in the early '70's. However they are worth using in this context because there are other games involved and this makes a game-theoretic treatment of fixed points more sensible.

Hintikka games

Hintikka games are games played with formulæ and models. The formulæ are all built up from atomics and negatomics by means of $\wedge$, $\vee$, $\forall$ and $\exists$ and the two restricted quantifiers.

I am assuming that the reader knows the usual rules for the Hintikka game G_ϕ. Here we have two extra rules for the restricted quantifiers, which are as follows. When the players are confronted with $(\forall x \in a)\Phi$ player **False** picks an element b of a (if he can, and loses at once if he can't) and they play $G_{\Phi[b/x]}$; when the players are confronted with $(\exists x \in a)\Phi$ player **True** picks an element b of a (if she can, and loses at once if she can't) and they play $G_{\Phi[b/x]}$.

What Aczel did to Hintikka games

If ϕ belongs to any normal sensible language (i.e., to a language that is a recursive datatype) the Hintikka game G_ϕ is of course a game of finite length. Interesting things happen, however, if ϕ is a nasty formula of the kind that Aczel calls a *syntactic fixed point*.

We start as we mean to go on, with an example that will concern us later. Suppose # is a formula with two free variables in it, such that when we put 'X' and 'Y' in for the two free variables in # we obtain

$$(\forall x \in X)(\exists y)(y \in Y \wedge ???)) \wedge (\forall y \in Y)(\exists x)(x \in X \wedge ???))$$

where the question marks identify a subformula which is the result of putting 'x' and 'y' in for the two free variables in # and adding a prime to the two outermost variables bound by restricted quantifiers. It is clear that any formula satisfying this condition must be infinite and – worse! – must have an illfounded subformula relation. Nevertheless formulæ that are *syntactic fixed points* can have a perfectly intelligible semantics provided by means of the corresponding Hintikka games.

Let us consider the Hintikka game for this formula. In a play of this game, **False** picks a member of X or a member of Y, and **True** has to reply with a member of the other. They continue doing this until one of them is unable to play, and thereby loses. This game was discovered independently by Malitz many years later, and i do not at present know if he knew if this game could be seen as arising in this way from Hintikka games. For obvious reasons i prefer to call the players "$\neq$" and "$=$" instead of "**False**" and "**True**". Let us notate this, the **Malitz game**, "$G_{X=Y}$".

This is an illustration of a more general phenomenon. If a relation of interest comes to us as a fixed point for an operation, so that $\psi(x, y) \longleftrightarrow \Gamma(x, y)$ where ψ occurs as a subformula of Γ, then $\psi(x, y)$ gives rise to a *syntactic fixed point*, a formula whose subformula relation is illfounded. The Hintikka game for this formula then gives us a game with the feature that if `I` (say) has a winning strategy for it then $\psi(x, y)$.

In Forster [1982] I published another set-theoretical game designed to capture contractions and not surprisingly it turned out to be equivalent. This game is played as follows. $=$ announces a binary relation which is a subset of $x \times y$ whose domain is x and whose range is y. $\neq$ then picks an ordered pair $\langle x', y' \rangle$ in this set and they play $G_{x'=y'}$. The first player to be unable to move loses.

This does not tell us who wins an infinite play. Any bisimulation corresponds to a valuation (a "referee") awarding each infinite ("disputable") play of $G_{x=y}$ to $=$ or to $\neq$. (There's no need for a referee to decide who wins completed plays of finite length!) The valuation that awards no disputable plays to $=$ corresponds to the least fixed point, and the valuation that awards all disputable plays to $\neq$ corresponds to the greatest fixed point. There will in fact be a greatest fixed point because the collection of equivalence relations on a set is always a complete lattice and $+$ is a strict monotone function.

REMARK 45 *The open (resp. closed) Forster game and the closed (resp. open) Malitz Game are equivalent.*

Proof:

The equivalence is the wrong way round because $=$ moves first in the Forster game but moves second in the Malitz game. This is a good reason for not retaining Malitz's notation.

We sketch a way of turning strategies for $=$ in one game into strategies for $=$ in the other.

Suppose $=$ Wins the Forster game $G_{x=y}$. Then she Wins the Malitz game as follows. Because she has a winning strategy in the Forster game $G_{x=y}$, she has a binary relation R which is a subset of $x \times y$ whose domain is x and whose range is y. When $\neq$ plays $x' \in x$ or $y' \in y$, she replies with an R-relative of x' (or y' *mutatis mutandis*). (What's a bit of AC between friends?)

Conversely suppose $=$ Wins the Malitz game $G_{x=y}$. Then she Wins the Forster Game as follows. She has a strategy, and the strategy, initially at least, is a map from x to y and a map from y to x. But this gives her a binary relation R which is a subset of $x \times y$ whose domain is x and whose range is y, which is what she needs to make her first move in the Forster Game.

■

Since the Forster games and the Malitz games are equivalent we can concentrate our treatment on only one of them. Henceforth the game $G_{x=y}$ will be the Malitz game, so that when we speak of the open game $G_{x=y}$ we mean the game in which the player who goes first (namely $\neq$) wins, if at all, after finitely many moves.

DEFINITION 12 *Let us say x and y are Forster/Malitz bisimilar iff $=$ Wins the closed game $G_{x=y}$. Let us write this $x \sim_{min} y$.*

Evidently $\sim_{min}$ is an equivalence relation. We note that

REMARK 46 *$\sim_{min}$ is the least fixed point for $+$.*

Proof:

Notice that the least fixed point is not the equality, as one might think. Strictly, it's not even an equivalence relation at all, but only a PER. If x is a set that is not wellfounded, so that $\langle x_n : n < \omega \rangle$ is a descending $\in$-chain ($x_0 = x$ and $(\forall n)(x_{n+1} \in$

x_n)), then player $\neq$ can stave off defeat in $G_{x=x}$ indefinitely by picking x_n for his nth move. Player $=$ certainly cannot do any better than to copy him. That means that if x is not wellfounded then it is not bisimilar even to itself (according to the least fixed point). In fact the least fixed point is the identity relation restricted to wellfounded sets.

Generally Malitz was interested only in the maximal fixed point for $+$, corresponding to the open game in which $\neq$ has to win in finitely many moves if at all. This is because in all the usual models of the set theory he was studying this maximal bisimulation is equality. He points out that $=$ will win the open game $G_{V=V\setminus\{V\}}$. For consider: what can $\neq$ do? He cannot pick something in $V \setminus \{V\}$ that isn't in V so his only hope is to pick something in V that isn't in $V \setminus \{V\}$, namely V. But even if he does pick V, $=$ need only pick $V \setminus \{V\}$ and they are back where they started. Anything else allows $=$ to copy his moves blindfold and, if not actually win in finitely many moves, at least never lose in finitely many moves, which is enough to ensure that she can Win the open game. This means that the ordered pair $\langle V, -\{V\}\rangle$ belongs to the *greatest* fixed point for $+$.

A moment's reflection will reveal that this depends only on very general properties of V and $V \setminus \{V\}$, and that what this reasoning proves is the following

REMARK 47 *If $x \in x$ and $(x \setminus \{x\}) \in x$ then $x \sim (x \setminus \{x\})$ where $\sim$ is the greatest fixed point for $+$.*

A rather bizarre corollary of this now appears in Malitz's set theory. Even tho' V is a set, $V \setminus \{V\}$ isn't! If it existed it would have to be distinct from V. However the maximal bisimulation is the identity, and V is maximally-bisimilar to $V \setminus \{V\}$.

Malitz noticed that in consequence of this Quine's *NF* cannot have a model in which player $=$ has a Winning strategy in $G_{x=y}$ iff $x = y$. This is an infelicity. The revised version of Malitz' identity game, with an eye on an axiom of strong extensionality that *is* compatible with Quine's *NF*, is the following.

On being presented with x and y, player $\neq$ has two further possibilitiss in addition to the two possibilities of picking a member of x or a member of y. He now can pick something that is not in x or something that is not in y. If he picks something in $V \setminus y$, $=$ must reply with something in $V \setminus x$. In general $=$ cannot distinguish (merely from observing $\neq$'s move) whether he has picked something in x, or something in y, so she doesn't even know what she is supposed to do next, let alone how to succeed in it. So the rules must specify that $\neq$ has to say "I have picked a member of x" (or whatever).

Actually the same holds in the original game. This time there is the additional problem that $=$ can't distinguish between $\neq$ picking something in y and something in $V \setminus x$.

It becomes clearer what is going on if we go back to Aczel formulæ: again. The equivalence relation we are interested in is this one: $A \sim B$ iff $(\forall x)(\exists y)(x \sim y \wedge (x \in A \longleftrightarrow y \in B))$. or, more symmetrically in 'A' and 'B':

$$(\forall x)(\exists y)(x \sim y \wedge (x \in A \longleftrightarrow y \in B) \wedge (x \in B \longleftrightarrow y \in A)).$$

It looks a bit like one of the Barwise approximants.

Sse $X = \{a, b, c, d\}$; $Y = \{c, d, f, g\}$; $Z = \{b, d, e, f\}$.

we desire $X \leq Y \leq Z$ but $X \not\leq Z$.
so we want
$a < g \vee a < f$
$b < g \vee b < f$
$g < e \vee g < b$
$c < e \vee c < b$
and $(c \not< e \wedge c \not< f \vee a \not< e \wedge a \not< f)$
So this is the DNF. Each row is a conjunction.
$a < gb < gg < ec < ec \not< ec \not< f$
$a < gb < gg < ec < bc \not< ec \not< f$
$a < gb < gg < bc < ec \not< ec \not< f$
$a < gb < gg < bc < bc \not< ec \not< f$
$a < gb < fg < ec < ec \not< ec \not< f$
$a < gb < fg < ec < bc \not< ec \not< f$
$a < gb < fg < bc < ec \not< ec \not< f$
$a < gb < fg < bc < bc \not< ec \not< f$
$a < fb < gg < ec < ec \not< ec \not< f$
$a < fb < gg < ec < bc \not< ec \not< f$
$a < fb < gg < bc < ec \not< ec \not< f$
$a < fb < gg < bc < bc \not< ec \not< f$
$a < fb < fg < ec < ec \not< ec \not< f$
$a < fb < fg < ec < bc \not< ec \not< f$
$a < fb < fg < bc < ec \not< ec \not< f$
$a < fb < fg < bc < bc \not< ec \not< f$
$a < gb < gg < ec < ea \not< ea \not< f$
$a < gb < gg < ec < ba \not< ea \not< f$
$a < gb < gg < bc < ea \not< ea \not< f$
$a < gb < gg < bc < ba \not< ea \not< f$
$a < gb < fg < ec < ea \not< ea \not< f$
$a < gb < fg < ec < ba \not< ea \not< f$
$a < gb < fg < bc < ea \not< ea \not< f$
$a < gb < fg < bc < ba \not< ea \not< f$
$a < fb < gg < ec < ea \not< ea \not< f$
$a < fb < gg < ec < ba \not< ea \not< f$
$a < fb < gg < bc < ea \not< ea \not< f$
$a < fb < gg < bc < ba \not< ea \not< f$
$a < fb < fg < ec < ea \not< ea \not< f$
$a < fb < fg < ec < ba \not< ea \not< f$
$a < fb < fg < bc < ea \not< ea \not< f$
$a < fb < fg < bc < ba \not< ea \not< f$
Now to process them
$a < gb < gg < ec < ec \not< ec \not< f$ imposs ce
$a < gb < gg < ec < bc \not< ec \not< f$ imposs cbge
$a < gb < gg < bc < ec \not< ec \not< f$ imposs ce
$a < gb < gg < bc < bc \not< ec \not< f$ imposs bggb
$a < gb < fg < ec < ec \not< ec \not< f$ imposs ce

$a < gb < fg < ec < bc \not< ec \not< f$ imposs cbf
$a < gb < fg < bc < ec \not< ec \not< f$ imposs ce
$a < gb < fg < bc < bc \not< ec \not< f$ imposs cbf
$a < fb < gg < ec < ec \not< ec \not< f$ imposs ce
$a < fb < gg < ec < bc \not< ec \not< f$ imposs cbge
$a < fb < gg < bc < ec \not< ec \not< f$ imposs ce
$a < fb < gg < bc < bc \not< ec \not< f$ imposs bggb
$a < fb < fg < ec < ec \not< ec \not< f$ imposs ce
$a < fb < fg < ec < bc \not< ec \not< f$ imposs cbf
$a < fb < fg < bc < ec \not< ec \not< f$ imposs ce
$a < fb < fg < bc < bc \not< ec \not< f$ imposs cbf
$a < gb < gg < ec < ea \not< ea \not< f$ imposs age
$a < gb < gg < ec < ba \not< ea \not< f$ imposs age
$a < gb < gg < bc < ea \not< ea \not< f$ imposs bggb
$a < gb < gg < bc < ba \not< ea \not< f$ imposs bggb
$a < gb < fg < ec < ea \not< ea \not< f$ imposs age
$a < gb < fg < ec < ba \not< ea \not< f$ imposs age
$a < gb < fg < bc < ea \not< ea \not< f$ imposs agbf
$a < gb < fg < bc < ba \not< ea \not< f$ imposs agbf
$a < fb < gg < ec < ea \not< ea \not< f$ imposs af
$a < fb < gg < ec < ba \not< ea \not< f$ imposs af
$a < fb < gg < bc < ea \not< ea \not< f$ imposs af
$a < fb < gg < bc < ba \not< ea \not< f$ imposs af
$a < fb < fg < ec < ea \not< ea \not< f$ imposs af
$a < fb < fg < ec < ba \not< ea \not< f$ imposs af
$a < fb < fg < bc < ea \not< ea \not< f$ imposs af
$a < fb < fg < bc < ba \not< ea \not< f$ imposs af

the bggb lines are impossible only becos of antisymmetry. If we drop antisymmetry, so that ¡ is merely a quasiorder then these become possible counterexamples. So perhaps transitivity of the lift holds if the imput is antisymmetrical. But does it preserve antisymmetry? No, consider two disjoint mutually cofinal sequences.

We haven't shown that it takes partial orders to quasiorders but even if we did it wouldn't be useful to us beco's this shows that we can't expect it to preserve antisymmetry.

Chapter 11

Arithmetic-with-an-automorphism and wellfounded sets in stratified set theories

Definition 13 *We will make frequent use of the following permutation:*

$$\alpha = \prod_{n \in \mathbb{N}} (Tn, \{m : m\,E\,n\})$$

where $m\,E\,n$ iff the mth bit of n is 1. We will call it 'α' for Ackermann.

It is a commonplace in stratified set theories that ι, the singleton function, is not necessarily a set, even locally, and we let $T|x| = |\iota``x|$. $x$ is finite iff $\iota``x$ is finite and in fact T is an automorphism of $\mathbb{N}$.

Thus x and $\iota``x$ do not automatically have the same cardinal, even if $x$ is finite. If there are finite $x$ such that $|x| \neq |\iota``x|$ we have a nontrivial automorphism of $\mathbb{N}$, usually written T. Among assertions about this automorphism the most obvious to adopt as an axiom is the assertion that it is the identity, and this is the axiom of counting, identified as important – and named – years ago by Rosser. It turns out that a weaker assertion, namely that $(\forall n \in \mathbb{N})(n \leq Tn)$ is equivalent to assertions about the consistency of the existence of particular countable inductively defined wellfounded sets.

In "Trois résultats concernant les ensembles fortement cantoriens dans les "New Foundations" de Quine, *Comptes Rendues hebdomadaires des séances de l'Académie des Sciences de Paris série A* **279** (1974) pp. 41–4, Roland Hinnion proved that if the Axiom of counting holds, then there are permutation models containing severally V_ω, the set of von Neumann naturals (hereafter "$\mathbb{N}_{\mathrm{vN}}$") and the Zermelo naturals (hereafter "$\mathbb{N}_{\mathrm{Zm}}$"). (Notice that the existence of these things is not an obvious consequence of the comprehension scheme of *NF*.) It is a reasonable and natural question to ask if Hinnion's result is best possible: can the hypothesis can be weakened to the extent that there are converses to any of these results? The idea is that the ("possible") existence of things like V_ω, $\mathbb{N}_{\mathrm{vN}}$, $\mathbb{N}_{\mathrm{Zm}}$ may turn out to be equivalent to assertions inside arithmetic-with-T. It is claimed in Forster [1992] that if there is a permutation model in which V_ω is a countable set then $\mathrm{AxCount}_{\leq}$ holds. Although this proof is erroneous and the proposition almost certainly false, converses like this can be proved, and it is the purpose of this note to prove one. All the necessary background is to be found in Forster [1995].

All the collections whose potential sethood in permutation models was proved by Hinnion to follow from the axiom of counting are sets inductively defined by unstratified inductions. For example, the collection of Zermelo integers is $\bigcap\{y : (\Lambda \in y) \wedge (\iota``y \subseteq y)\}$. There are at least some inductively defined collections of this kind that cannot be sets at all. To take an example from *NF*, if Ω is the length of $\langle NO, \leq_{NO}\rangle$ (the set of all ordinals wellordered in the obvious way) then the collection $\{\Omega, T\Omega, T^2\Omega \ldots\}$ cannot be a set. Suppose there were a set that was the intersection of all sets containing Ω and closed under T. It clearly contains only ordinals, so look at the least ordinal in it, κ, say. It's closed under T, so $\kappa \leq T\kappa$ by minimality. $\kappa = T\kappa$ is not possible (o/w we could safely delete κ) so $\kappa < T\kappa$. But then $T^{-1}\kappa$ exists and is less than κ, and is therefore not in our set. But if $T^{-1}\kappa$ is not in our set, we can safely delete κ from it too.

This sharpens the problem of finding the correct statement of a converse. This definition is not $\Delta_0^{\mathcal{P}}$, and it will turn out that this is a large part of the trouble. We will prove the following:

Theorem 18
The Axiom of Counting is equivalent to the assertion that there is a permutation π such that

$$V^\pi \models (\exists x)(\forall y)(y \in x \longleftrightarrow (\forall z)(\Lambda \in z \wedge f``z \subseteq z \to y \in z))$$

for all functions f such that '$y = f(\vec{x})$' is in $\Delta_0^{\mathcal{P}}$.

Proof:

Right-to-Left

It is actually an old result of Henson's that any set of Von Neumann ordinals is strongly cantorian, so if the Von Neumann ω is a set there is an infinite strongly cantorian set, and this is one version of the axiom of counting. However we want to deduce the axiom of counting from the existence of the Von Neumann ω defined as that inductively defined set constructed by closing the singleton of the empty set under the operation $\lambda x.(x \cup \iota`x)$. We cannot use Henson's result unless we know that everything in this set i have given the inductive definition of is indeed a Von Neumann ordinal, and that it is infinite.

So suppose

$$\bigcap\{X : \Lambda \in X \wedge (\forall y)(y \in X \to y \cup \{y\} \in X)\}$$

exists.

Let us write S_{vN} for von Neumann successor, and let $\mathbb{N}_{\mathrm{vN}}$ be the von Neumann ω. First we note that if $\mathcal{P}(x) \subseteq x$ then x contains Λ and is closed under von Neumann successor, so that our set is wellfounded. Wellfoundedness of $\mathbb{N}_{\mathrm{vN}}$ implies that S_{vN} is 1-1, as follows: $x \cup \iota`x = y \cup \{y\} \to x = y \vee (x \in y \wedge y \in x)$. The second disjunct contradicts foundation and can be discarded.

The strategy is to show that $\in$ and $\subseteq$ agree on $\mathbb{N}_{\mathrm{vN}}$.

- First we show $(\forall xy \in \mathbb{N}_{\mathrm{vN}})((x \subseteq y \wedge x \neq y) \to x \in y)$.

  Let x be an arbitrary member of $\mathbb{N}_{\mathrm{vN}}$. Consider $\{y \in \mathbb{N}_{\mathrm{vN}} : x \subseteq y \wedge x \neq y \wedge x \notin y\}$. This is a set because the matrix is weakly stratified. This set must have an $\in$-least member, $z \cup \iota`z$. So we know the following:

  (i) $x \subseteq z \cup \iota`z$

  (ii) $x \neq z \cup \iota`z$

  (iii) $x \notin z \cup \iota`z$

  (iv) $x \subseteq z \wedge x \neq z \to x \in z$.

  ...and we want to derive a contradiction from this.

  By (i) $x \subseteq z$ unless possibly if $x = z$, but by (iii) that cannot happen, so x is a proper subset of z. Therefore $x \in z$ by (iv) which contradicts (iii).

- Now for the converse. We want $(\forall xy \in \mathbb{N}_{\mathrm{vN}})(x \in y \to x \subseteq y)$. As before let x be an arbitrary von Neumann integer and y an $\in$-minimal object such that $x \in y \wedge x \not\subseteq y$. Without loss of generality $y = z \cup \iota`z$. As before this looks unstratified but isn't, so we have

  (i) $x \in (z \cup \iota`z)$

  (ii) $x \not\subseteq (z \cup \iota`z)$

  (iii) $x \in z \to x \subseteq z$.

  By (i) either $x \in z$ or $x = z$. If $x \in z$ then by (iii) we have $x \subseteq z$, so either way $x \subseteq z$. This contradicts (ii). Therefore, for $\mathbb{N}_{\mathrm{vN}}$, $\in$ and $\subseteq$ are the same.

Next we check that distinct things in $\mathbb{N}_{\mathrm{vN}}$ have distinct members in $\mathbb{N}_{\mathrm{vN}}$. For suppose two chaps in $\mathbb{N}_{\mathrm{vN}}$ are distinct. Without loss of generality they can be taken to be $x \cup \iota`x$ and $y \cup \{y\}$. If these two chaps have the same members we infer $y \in (x \cup \iota`x)$ and $x \in (y \cup \{y\})$. These two conditions are equivalent to $x \in y \vee x = y$ and $y \in x \vee y = x$ respectively. By hypothesis we have to discard the second disjunct, so we have $x \in y \in x$, contradicting wellfoundedness.

Now $\subseteq$ restricted to $\mathbb{N}_{\mathrm{vN}}$ is a set, so $\in$ restricted to $\mathbb{N}_{\mathrm{vN}}$ is a set too. But if $\in$ is a set restricted to x then $stcan(x)$ follows immediately because we can send

$\iota`x$ to $\{y \in \mathbb{N}_{\mathrm{vN}} : y \subseteq x\}$ which is just x, by substitutivity of the biconditional and extensionality.

$\iota`x \mapsto \{y \in \mathbb{N}_{\mathrm{vN}} : y \subseteq x\} =$
$\{y \in \mathbb{N}_{\mathrm{vN}} : y \in x\} =$
$x \cap \mathbb{N}_{\mathrm{vN}} \mapsto$
the unique $z \in \mathbb{N}_{\mathrm{vN}}$ $z \cap \mathbb{N}_{\mathrm{vN}}$
$= x \cap \mathbb{N}_{\mathrm{vN}} = x$.

This tells us that S_{vN} is actually a set of ordered pairs. We already know it is 1-1, so $\mathbb{N}_{\mathrm{vN}}$ is infinite. So we can conclude that $\exists \mathbb{N}_{\mathrm{vN}} \longleftrightarrow$ The Axiom of Counting. But since the antecedent is invariant, we have proved:

$$\Diamond \exists \mathbb{N}_{\mathrm{vN}} \longleftrightarrow \text{The Axiom of Counting}$$

Left-to-right

It is a simple matter to verify that if we start in a model of *NFC*, α gives us a permutation model containing V_ω, and this set is clearly strongly cantorian, so we have all the comprehension that is known to hold for strongly cantorian sets. This is certainly enough to prove the existence of the Von Neumann ω and indeed any other inductively defined subset of V_ω

■

Two brief points. (i) Of course if all one wants is a permutation model in which the Von Neumann ω is a set then it is easier to use Hinnion's permutation. (ii) The same ideas will be used to prove the corresponding direction of the next theorem, and there we have to be more alert.

We will need the following lemma

Lemma 14 *If $f : \mathbb{N} \to \mathbb{N}$ is an increasing function that commutes with T then*

$$(\forall n \in \mathbb{N})(n \leq f`Tn) \to AxCount_{\leq} .$$

Proof: If there is an $n > Tn$ then consider the Tnth member of the sequence $\{0, f`0, f^2`0, \ldots f^n`0 \ldots\}$. This will be a counterexample to the antecedent.

Theorem 19 *Let α be the Ackermann permutation. Then $AxCount_{\leq}$ holds iff V^α contains all sets inductively defined as the closure of $\{\Lambda\}$ under any finite number of finitary* **stratified** *(but not necessarily homogeneous) $\Delta_0^{\mathcal{P}}$ operations.*[1]

Proof: Examples of sets defined in this way are $\mathbb{N}_{\mathrm{Zm}}$, the Zermelo naturals and V_ω (the closure of $\{\Lambda\}$ under the operation $\lambda xy.x \cup \{y\}$).

Right-to-Left

[1]OUCH: do we need the result to be free?

This is in Forster [1995] but we recapitulate for the sake of completeness. We deduce AxCount$_\leq$ from the existence of the set of all finite V_ns. Suppose the collection

$$\bigcap\{y : (\Lambda \in y) \wedge (\mathcal{P}(y) \subseteq y)\}$$

is a set. We'd better have a name for it, $\mathtt{X}$, say. We are going to deduce AxCount$_\leq$.

First we show that $\mathtt{X}$ is wellfounded. This is less than blindingly obvious, because not every x such that $\mathcal{P}(x) \subseteq x$ is closed under $\mathcal{P}$. However the power set of any such is, so we can reason as follows. Suppose $z \in \mathtt{X}$. $\mathcal{P}(x) \subseteq x$. Then $\mathcal{P}((\mathcal{P}(x)) \subseteq \mathcal{P}(x)$ and $\Lambda \in \mathcal{P}(x)$ so $z \in \mathcal{P}(x)$. But $\mathcal{P}(x) \subseteq x$ so $z \in x$ as desired.

Next we show that $\mathtt{X}$ is totally ordered by $\subseteq$. Let x be $\in$-minimal such that $(\exists y)(x \not\subseteq y \not\subseteq x)$, and let y be $\in$-minimal such that $x \not\subseteq y \not\subseteq x$. In fact we can take these to be power sets $\mathcal{P}(x)$ and $\mathcal{P}(y)$ and so we have x and y such that $x \subseteq y \vee y \subseteq x$ (by $\in$-minimality) but $\mathcal{P}(x) \not\subseteq \mathcal{P}(y) \not\subseteq \mathcal{P}(x)$ which is clearly impossible.

Since $\mathtt{X}$ is totally ordered by $\subseteq$ we must have $(\forall x)(x \subseteq \mathcal{P}(x) \vee \mathcal{P}(x) \subseteq x)$. The second disjunct contradicts foundation so we must have $(\forall x \in \mathtt{X})(x \subseteq \mathcal{P}(x)$.

Next we prove by induction that each member of $\mathtt{X}$ is finite (has cardinal in $\mathbb{N}$). Suppose not, and let $\mathcal{P}(x)$ be a $\in$-minimal infinite member of $\mathtt{X}$. But if $|\mathcal{P}(x)| \notin \mathbb{N}$ then clearly $|x| \notin \mathbb{N}$ too.

Notice also that there can be no $\subseteq$-maximal member of $\mathtt{X}$, for if x were one we would have $\mathcal{P}(x) \subseteq x$ and $x \in x$ contradicting foundation.

Therefore the sizes of elements of $\mathtt{X}$ are unbounded in $\mathbb{N}$. Now let n be an arbitrary member of $\mathbb{N}$. By unboundedness we infer that for some $x \in \mathtt{X}$ we have $|x| \leq n \leq |\mathcal{P}(x)|)$ and therefore $|x| \leq n \leq |\mathcal{P}(x)| \leq 2^{Tn}$. But n was arbitrary, so $(\forall n \in \mathbb{N})(n \leq 2^{Tn})$.

But by lemma 14 this implies AxCount$_\leq$.

Left-to-Right

If f is an operation of the kind we are interested in, there will be a corresponding operation on natural numbers. For example $\lambda x.\{x\}$ corresponds to $\lambda n.2^n$. If f is the operation we start with, let us notate the correponding operation on natural numbers 'f^*'. For example, if f is the singleton operation, f^* is $\lambda n.2^n$. Suppose now we have a number of such operations (one is easiest for illustration!!) and consider the result of closing $\{0\}$ under f^*.

It will turn out that in V^π this is the smallest set containing Λ and closed under f. Showing that it contains Λ and is closed under f is easy. We need AxCount$_\leq$ to show that it is the least set containing Λ and closed under f.

For the moment, consider the following illustration, which just happens to be lying around. (Later i'll write out a more general proof)

Let us write $n\, E^T\, m$ for $Tn\, E\, m$. That is to say: $n\, E^T\, m$ iff the Tnth bit of m is 1.

We will need to know that AxCount$_\leq$ implies that E^T is wellfounded.

Suppose it isn't, and $X \subseteq \mathbb{N}$ has no E^T-minimal member. Let n be the least member of X. Since n is not E^T-minimal, it follows that there is $m \in X$, $m \geq n$ and $m\, E^T\, n$. But then $Tn \leq Tm < n$ contradicting AxCount$_\leq$.

The converse (that E^T wellfounded implies AxCount$_\leq$) is also true but we don't need it here. (This is in Forster [1995].) If we have AxCount$_\leq$ we know that E^T is wellfounded and we use this to prove by induction on it that if y is a set such that $V^\pi \models \mathcal{P}_{\aleph_0}(y) \subseteq y$ then all naturals belong to y. So `a is minimal with this property, and is indeed` V_ω `in` V^π`.`

What is a. . . ?

We will show also that AxCount$_\leq$ $\rightarrow (\forall y)(V^\pi \models (\mathcal{P}_{\aleph_0}(y) \subseteq y) \rightarrow (\forall n \in \mathtt{a})(n \in y))$

We prove this by UG on 'y' and by induction (on E^T) over the naturals. Since AxCount$_\leq$ implies that E^T is wellfounded, this task is precisely that of proving

$$(\forall y)(V^\pi \models (\mathcal{P}_{\aleph_0}(y) \subseteq y) \rightarrow (\forall n \in \mathtt{a})(n \in y))$$

by E^T-induction.

Now

$$V^\pi \models ((\mathcal{P}_{\aleph_0}(y) \subseteq y) \rightarrow (\forall n \in \mathtt{a})(n \in y))$$

is

$$((\mathcal{P}_{\aleph_0}(\pi`y) \subseteq \pi``\pi`y) \rightarrow (\forall n \in \mathtt{a})(n \in \pi`y))$$

(since $\mathtt{a}$ is fixed by π) and we can reletter $\pi`y$ to get

$$((\mathcal{P}_{\aleph_0}(y) \subseteq \pi``y) \rightarrow (\forall n \in \mathtt{a})(n \in y))$$

Now let y be an arbitrary object satisfying $(\mathcal{P}_{\aleph_0}(y) \subseteq \pi``y)$. Suppose $(\forall m)(m\, E^T\, n \rightarrow m \in y)$ Consider $\{m : m\, E^T\, n\}$. This is a finite set, so is in $\mathcal{P}_{\aleph_0}(y)$ and therefore in $\pi``y$. Therefore $\pi^{-1}`\{m : m\, E^T\, n\} \in y$. But $\pi^{-1}`\{m : m\, E^T\, n\}$ is n. This proves the induction. ■

Remark 48 $\mathtt{X}$ *exists iff* V_ω *exists and a rank function on* V_ω *exists.*

Proof: If $\mathtt{X}$ exists then its sumset is V_ω. The rank of a set in V_ω is the number of elements of $\mathtt{X}$ to which it doesn't belong.

Conversely, if V_ω exists and a rank function – f, say – on V_ω exists, then $\mathtt{X}$ is $\{f^{-1}``\{n\} : n \in \mathbb{N}\}$

■

Suppose the inductively defined set V_ω exists. Can we even show that it is countable? There is no total order of V_ω definable by a stratified formula.

If V_ω is countable, does AxCount$_\leq$ follow?

We can show it is countable if there is a countable set X equal to the set of its finite subsets because then $V_\omega \subseteq X$. There is a always a permutation model in which such a set exists (even if $\neg$AxCount$_\leq$) so the idea is: show not that $V_\omega \subseteq X$ (which would be true in the permutation model), but rather that there is an embedding from $V_\omega \hookrightarrow$ the set that becomes X in the permutation model. In other words, map V_ω recursively into $\mathbb{N}$. The obvious thing would be to define a map by recursion on $\in$ but this we cannot do!

Definition 14 $\nu = |V_\omega|$

Remark 49 .

1. $\aleph_0 \leq \nu \to \nu = \nu^2$
2. $T\nu \leq \nu \leq 2^{T\nu}$
3. $\aleph_0 \leq_* \nu$
4. $\nu = T\nu \to \aleph_0 \leq \nu$
5. *If α is a cardinal such that there is a set in V_ω of size α, then there is β such that $2^\alpha \cdot \beta = \nu$*
6. $T^2(\nu^2) \leq \nu$

Proof:

(1) By coding ordered pairs

(2) V_ω is transitive and contains all its singletons.

(3) By wellfoundedness V_ω cannot be finite (i.e. $\nu \notin \mathbb{N}$). Therefore it has subsets (and consequently members) of all finite (in $\mathbb{N}$) sizes, and a countable partition.

(4) follows from a lovely theorem of Tarski's that says (in *NF*-speak) that if there is a bijection between $\iota``X$ and $\mathcal{P}_\kappa(X)$ then $X$ has a wellordered subset of size $\aleph(\kappa)$. The proof is as follows: There is a bijection $f : V_\omega \longleftrightarrow \iota``V_\omega$. We define a sequence

$$g`0 = \Lambda$$

$$g`(n+1) = (g`n) \cup f`\{y \in g`n : y \notin f^{-1}`\{y\}\}$$

(5) Let X be a member of V_ω of size α. Consider the equivalence relation on members of V_ω defined by

$$x \sim y \longleftrightarrow (x \cap X) = (y \cap X)$$

For each equivalence class there is a subset $X' \subseteq X$ such that all member of that equivalence class are of the form $X \cup y$ where $y \in \mathcal{P}_{\aleph_0}((V_\omega \setminus X)$. Therefore all equivalence classes are the same size, namely $|\mathcal{P}_{\aleph_0}((V_\omega \setminus X)|$. Since there is also a canonical representative for each equivalence class (each equivalence class contains precisely one subset of X) we infer that 2^α divides ν.

(6) Follows from the availability of Wiener-Kuratowski ordered pairs in V_ω. Similar results hold for higher exponents.

■

A consequence of (5) would appear to be that for each $n \in \mathbb{N}$ there is β such that $\beta^{2^n} = \nu$. This does not seem to be be about to turn into a proof that $\nu = \aleph_0$.

There doesn't seem to be any proof that $\aleph_1 \not\leq \nu$, and i can't see any reason why we should expect *NF* to be able to prove things like that.

Discussion

let f, g be bijections $\iota``\mathbb{N} \longleftrightarrow \mathcal{P}_{\aleph_0}(\mathbb{N})$ and think about the structures $\langle \mathbb{N}, \{\langle x, y\rangle : x \in f`\{y\}\}\rangle$ and $\langle \mathbb{N}, \{\langle x, y\rangle : x \in g`\{y\}\}\rangle$. Call these $\langle \mathbb{N}, \in_f\rangle$ and $\langle \mathbb{N}, \in_g\rangle$. Notice that all these structures are – or ought to be – end extensions of $\langle H_{\aleph_0}, \in\rangle$

A morphism from f to g is an injection $\pi : \mathbb{N} \hookrightarrow \mathbb{N}$ such that

1. $(\forall x, y \in \mathbb{N})(x \in f`\{y\} \longleftrightarrow \pi`x \in g`\{\pi`y\})$

2. $(\forall x, y \in \mathbb{N})((x \in_g `\{y\} \wedge (y \in \pi``\mathbb{N})) \to x \in \pi``\mathbb{N})$

(That is to say $\langle \mathbb{N}, \in_g\rangle$ is an end-extension of $\langle \mathbb{N}, \in_f\rangle$ iff there is an arrow from f to g.)

Now the assertion that this category has an initial object is stratified. It therefore cannot imply $\mathrm{AxCount}_{\leq}$. It is a consequence of $\mathrm{AxCount}_{\leq}$, though. We'd better prove this. The idea is that if $\mathrm{AxCount}_{\leq}$, then we take $f`\{n\} = \{m :$ the mth bit of n is $1\}$ and construct an embedding by recursion of $\in_f$ which we know is wellfounded.

probably snip from here . . .

What happens if $\neg\mathrm{AxCount}_{\leq}$? Work in a model $\mathfrak{M}$ of $\neg\mathrm{AxCount}_{\leq}$ and consider $\mathfrak{M}^\pi$. On the face of it there are three possibilities:

1. V_ω does not exist;

2. $\mathtt{a}$ (the old $\mathbb{N}$) is the new V_ω;

3. Some other set is the new V_ω.

First we show that case 3 is impossible. V_ω would be (in $\mathfrak{M}$) a subset of the old $\mathbb{N}$. In fact it would have to be an initial segment. Think of its size. This would have to be a number $n = 2^{Tn}$ (since a finite set equal to the set of all its finite subsets is in fact a set equal to its power set) and we know this is not possible. A slightly more elementary proof reasons that a finite V_ω would have to be self-membered, contradicting wellfoundedness.

To deal with case 2 we note that if n is a power of 2 and $2^{Tn} < n$ then the integers below n form a set which – in $\mathfrak{M}^\pi$ – thinks it contains all its finite subsets. (write this out) But, as long as $\neg\mathrm{AxCount}_{\leq}$, there will be such n and so the old $\mathbb{N}$ cannot be the new V_ω.

This leaves only 1. So we have proved

$$\neg\mathrm{AxCount}_{\leq} \to \Diamond\neg\exists V_\omega$$

but *not*

$$\neg\mathrm{AxCount}_{\leq} \to \neg\Diamond\exists V_\omega$$

. . . to here

11.1 Sideshow: Hereditarily Dedekind-finite sets

It might be an idea to think about the set of hereditarily Dedekind-finite sets. It isn't directly involved, but it lives next door, and might illuminate the events at home. Later still we can think about hereditarily countable sets, and other collections that cannot be coded as subsets of V_ω. Perhaps the correct way to deal with them is to think about BF instead of $\mathbb{N}$.

> We can prove that a set with a finite partition into finite pieces is finite. (By induction on n, any union of n finite sets is finite). We can also prove that a set with a dedekind-finite partition into dedekind-finite pieces is dedekind-finite. (If it weren't then we would have a dedekind-finite partition of a countable set into dedekind-finite pieces, which we can't have.) Curious that these two proofs should be so different!

$H_{\text{Dedfin}} = \bigcap\{y : \mathcal{P}_{\text{dedekind-finite}}(y) \subseteq y\}$. In *ZF* we can prove that this collection is V_ω without any use of choice: V_ω exists and, because it is countable, it is a y such that $\mathcal{P}_{\text{dedekind-finite}}(y) \subseteq y$ so $H_{\text{Dedfin}} \subseteq V_\omega$. In *KF* or *NF* we know a lot less.

REMARK 50 *(NZF)*
If V_ω exists and is countable then H_{Dedfin} exists and is equal to V_ω.

Proof: If V_ω exists and is countable then it contains all its dedekind-finite subsets. Therefore $H_{\text{Dedfin}} \subseteq V_\omega$. The inclusion in the other direction is easy.
But we do seem to need the assumption that V_ω is not Dedekind-finite.

■

DEFINITION 15 $\delta = |H_{\text{Dedfin}}|$

REMARK 51 .

1. $T\delta \leq \delta \leq 2^{T\delta}$
2. $\aleph_0 \leq \delta$
3. $\delta = \delta^2$
4. *If α is a cardinal such that there is a set in H_{Dedfin} of size α, then there is β such that $2^\alpha \cdot \beta = \delta$*

Proof:
(1) just as with ν
(2) First, since H_{Dedfin} is the intersection of all sets extending their set of finite subsets it must be wellfounded. In particular it is not self membered so it cannot be dedekind-finite. So it has a countable subset. (If we knew

$can(H_{\text{Dedfin}})$ we could derive this from Tarski's theorem but we can do it anyway!)

(3) H_{Dedfin} has a countable subset so we can fake Quine ordered pairs.

(4) Let X be a member of H_{Dedfin}. Consider the equivalence relation on members of H_{Dedfin} defined by

$$x \sim y \longleftrightarrow (x \cap X) = (y \cap X)$$

For each equivalence class there is a subset $X' \subseteq X$ such that all members of that equivalence class are of the form $X \cup y$ where $y \in \mathcal{P}_{\text{dedekind-finite}}(V_\omega \setminus X)$. Therefore all equivalence classes are the same size, namely $|\mathcal{P}_{\text{dedekind-finite}}(V_\omega \setminus X)|$. Since there is also a canonical representative for each equivalence class (each equivalence class contains precisely one subset of X) we infer that 2^α divides δ. (This is just like the corresponding proof for V_ω)

11.2 Discussion

Can we show $\aleph_1 \not\leq \delta$?

All of this talk of small permutations involving $\mathbb{N}$ can be done in *KF* too of course. In this context it seems important to note that *KF*+AxCount$_\leq$ is no stronger than *KF*, even tho' *NF*+AxCount$_\leq$ probably is stronger than *NF*.

Eight propositions about wellfounded sets: second version

(A bottomless set is one with no $\in$-minimal element; $PFIN$ is the set of finite power sets)

1. $\{V_n : n \in \mathbb{N}\}$ exists.
2. V_ω exists.
3. There is an infinite transitive wellfounded set.
4. There is an infinite wellfounded set.
5. Every natural number contains a wellfounded set.
6. WF has no finite superset.
7. (i) Every fat set is infinite; (ii) $\langle PFIN, \in\rangle$ is wellfounded; (iii) every bottomless set of power sets consists entirely of infinite sets.
8. $\Diamond(\in\restriction FIN$ is wellfounded).

Everything in this list implies everything below it. All the propositions in item 7 are equivalent. I do not know how to reverse any of the arrows. If you strip the '$\Diamond$' off item 8 you get something that implies item 7

$6 \to 7$. If WF has no finite superset then it certainly has no finite fat superset. But every fat set is a superset of WF, so there are no finite fat sets.

Various forms of 7: if x is fat then $\{\mathcal{P}(x)\}$ is a bottomless set of power sets.

"There are infinitely many (wellfounded) hereditarily finite sets" (aka: "no finite set contains every hereditarily finite wellfounded set") doesn't seem to fit into this linear sequence …Let's think about this last one. It follows from (3), as follows. Suppose x is an infinite transitive wellfounded set, and y is a finite set containing all hereditarily finite sets. Consider $x \setminus y$. This must have an $\in$-minimal member. (It's worth spelling out why this is the case, beco's "I have an $\in$-minimal member" is not stratified and cannot be proved by $\in$-induction. However we can prove that every member and every subset of a wellfounded set is wellfounded, and certainly every wellfounded set is regular: If u is wellfounded every v with $u \in v$ is disjoint from one of its members). Clearly $x \setminus y$ is nonempty and all its members are wellordered, so it has an $\in$-minimal member – w, say. Now $w \in x$ so $w \subseteq x$ by transitivity of x. By $\in$-minimality we have $w \subseteq x \cap y$, so w is finite (co's y is finite) so w is a finite set of hereditarily finite sets and so is hereditarily finite, contradicting assumption. …and it implies 6.

So what we should do now is either:

(i) show that if there is an infinite wellfounded set then no finite set can contain all hereditarily finite (wellfounded) sets; or

(ii) show that if no finite set can contain all hereditarily finite (wellfounded) sets then there is an infinite wellfounded set.

Another thing to look at is this.
$F(X) := \{x \in FIN : x \cap FIN \subseteq X\}$
$\mathcal{F}$:= least set containing $\emptyset$ and closed under F. Or the even stronger:
Where does the existence of $\mathcal{F}$ fit in all this?

Eight propositions about wellfounded sets

Consider the following assertions.
1 $\{V_n : n \in \mathbb{N}\}$ exists
2 V_ω exists
$2'$ There is an infinite transitive wellfounded set
3 There is an infinite wellfounded set
4 There is no finite bound on the size of wellfounded sets
$5'$ Every set of power sets with no $\in$-minimal member has only infinite members
5 $\mathcal{P}(x) \subseteq x \to x$ is infinite
6: There are infinitely many wellfounded sets
7: There are infinitely many (wellfounded) hereditarily finite sets

Randall has recently shown that $2'$ is not a theorem of any consistent invariant extension of NF.

I think i prove somewhere that the least fixed point for Hinnion's + is wellfounded on the wellfounded sets. To be clear about it, there is a binary relation $x \leq y$ iff$_{df}$ $(\forall R)(R^+ \subseteq R \to \langle x, y\rangle \in R)$, and this is wellfounded in the sense that any set of wellfounded sets has an R-minimal element. And anything $\leq$ a wellfounded set is wellfounded. (I think all this is true)

I then go on to say

> "So if there is an infinite wellfounded set there is one of minimal rank".

But i think this is wrong. The collection of infinite wellfounded sets is not a set. so i think i cannot therefore draw the conclusion i claimed:

"So if there is an infinite wellfounded set there must be infinitely many hereditarily finite sets"

Obviously 3 implies 6. Does 6 imply 7? It ought to, but we can't reason about ranks here! Certainly in V^α 6 implies 7. Suppose 7 is false. Then $\text{AxCount}_\leq$ fails. But if $\text{AxCount}_\leq$ fails, there is $n > 2^{Tn}$ and in V^α this becomes a finite thing extending its own power set, so all wellfounded sets are finite.

Similarly in V^α 5 implies 7. Suppose 7 is false. Then $\text{AxCount}_\leq$ fails. But if $\text{AxCount}_\leq$ fails, there is $n > 2^{Tn}$ and in V^α this becomes a finite thing extending its own power set. In general one would expect that 5 doesn't imply 7.

If one suspects these things are separate, then we will have to reason in things other than V^α to prove it.

It would be nice to be able to prove that 2 implies 3. Suppose 2 is true but 3 is false, and X is a finite set that contains all hereditarily finite sets. Then every infinite wellfounded set has an infinite wellfounded member. This is no use unless the class of infinite wellfounded sets is a set! If we had an axiom of transitive closures we'd be ok....

(Should try to fit in "Every finite wellfounded set has a transitive closure". Come to think of it, can we even prove that the transitive closure of a wellfounded set – if it exists – must be wellfounded? I don't see how!)

Obviously $1 \to 2 \to 3 \to 4 \to 5$. The point about 1 is that it is equivalent to the assertion that there is a rank function on V_ω. If 5 looks out of place, remember that a wellfounded set is simply something that is included in all x such that $\mathcal{P}(x) \subseteq x$ so there cannot be any infinite wellfounded sets at all unless 5 is true. At the moment there is the theoretical possibility that all the arrows might be reversed, however improbable such an outcome may seem. My guess is that *none* of them can be. We have seen that $\Diamond 1$ is an equivalent of $\text{AxCount}_\leq$, but none of the others seem to imply $\text{AxCount}_\leq$, so there remains the unexcluded possibility that $NF \vdash \Diamond 2$. I don't believe that either. In fact i don't believe even that $NF \vdash \Diamond 5$, even tho' 5 is so weak that we have to hang a '$\Box$' on the front of it to get anything strong enough to be obviously equivalent to $\text{AxCount}_\leq$.

REMARK 52 $\Box 5$ *and* $\textit{AxCount}_\leq$ *are equivalent.*

(We already know that $\text{AxCount}_\leq$ and $\Diamond 1$ are equivalent).

Proof:

L $\to$ R: (By contraposition) If $\mathrm{AxCount}_{\leq}$ fails, there is $n > 2^{Tn} \in \mathbb{N}$. Since whenever $x \notin x$, $B`x$ is a set of size $|V|$ disjoint from its power set, we can find, for any cardinal n, a set of size n disjoint from its power set. In particular if n is the finite cardinal promised above (so $2^{Tn} < n$) then we have a set x of size n disjoint from its power set and an injection p from $\mathcal{P}(x)$ into x. This can be extended to a permutation π of V. This proves $\Diamond\neg 5$.

R $\to$ L: If π is a permutation such that V^{π} thinks that some set x is finite and a superset of its power set, then V contains a map (namely a suitable restriction of π) from some finite power set $\mathcal{P}(x)$ into x and therefore a natural number $n = |x|$ such that $2^{Tn} < n$, which contradicts $\mathrm{AxCount}_{\leq}$. ■

sept 2003: a brilliant idea. Clearly if there is an infinite wellfounded set then there can be no finite x with $\mathcal{P}(x) \subseteq x$. However, we can even show, in those circumstances, that $\in$ restricted to finite power sets is wellfounded. Indeed we can prove even that if A be a family of power sets without a $\in$-minimal member then every member of A is infinite. Let A be a set of power sets with no $\in$-minimal member. We prove by $\in$-induction that every wellfounded set belongs to every member of A. (reality check: $\emptyset$ obviously does, so we are pointing in the right direction!!). Let $\mathcal{P}(x)$ be an arbitrary member of A, and a a family of sets each of which belongs to every $\mathcal{P}(y) \in A$. We want $a \in \mathcal{P}(x)$. Beco's A has no $\in$-minimal member, there is $\mathcal{P}(y)$ in A with $\mathcal{P}(y) \in \mathcal{P}(x)$. Then $a \subseteq \mathcal{P}(y)$ by induction hypothesis, so $a \subseteq \mathcal{P}(y) \in \mathcal{P}(x)$ but $\mathcal{P}(x)$, being a power set, is $\subseteq$-downward closed, so $a \in \mathcal{P}(x)$ as desired. This means that if A contains even one finite set then every wellfounded set is finite. So if there is an infinite wellfounded set then not only is $\in$ restricted to finite power sets wellfounded but any bottomless set of power sets consists entirely of infinite sets. Indeed we can even prove the following:

REMARK 53 *If A is a bottomless set of power sets, then $\bigcap A$ is a self-membered power set.*

Proof: Clearly any intersection of a lot of power sets is a power set, so $\bigcap A$ is a power set. We want $\bigcap A \in \bigcap A$. So we want $\bigcap A \in \mathcal{P}(x)$ for every power set $\mathcal{P}(x) \in A$. Let $\mathcal{P}(x)$ be an arbitrary member of A. Now A is bottomless, so there is another power set $\mathcal{P}(y)$ in A such that $\mathcal{P}(y) \in \mathcal{P}(x)$, which is to say $\mathcal{P}(y) \subseteq x$. Now $\mathcal{P}(y) \in A$ gives $\bigcap A \subseteq \mathcal{P}(y)$. But then $\bigcap A \subseteq \mathcal{P}(y) \subseteq x$ and $\bigcap A \subseteq x$ and $\bigcap A \in \mathcal{P}(x)$. But $\mathcal{P}(x)$ was an arbitrary member of A, so $\bigcap A \in \bigcap A$ as desired. ■

So, to recapitulate:

If every finite cardinal contains a wellfounded set, then there can be no finite self-membered power set. So every bottomless set of power sets consists entirely of infinite sets. So membership restricted to finite power sets is wellfounded. So using the clever Boffa-style permutatation of remark 54 (i think this reference is correct!) we get a model in which membership restricted to finite sets is wellfounded.

(Sse A is a set of finite power sets with no $\in$-minimal element. Let $\mathcal{P}(M)$ be an element of A of minimal size. Then $\mathcal{P}(N) \in \mathcal{P}(M)$ for some N. ($|M| = m$,

$|N| = n$ of course). Then $\mathcal{P}(N) \subseteq M$) so $Tn < 2^{Tn} \leq m \leq n$ (this last by minimality of $\mathcal{P}(M)$). This contradicts AxCount$_{\leq}$. Thus AxCount$_{\leq} \rightarrow \in\restriction$ finite power sets is wellfounded. A similar argument will show that if there is a bottomless set A of power sets, with $\kappa = inf(\aleph``\{|x| : x \in A\}$, then $\kappa > T\kappa$. But this isn't big news. We know stronger results than this already.)

So if there are arbitrarily large finite wellfounded sets, then every bottomless set of power sets consists entirely of infinite sets. How surprising is this? Are there any bottomless sets of power sets at all?? Yes: $\{V\}$ is one!

So the general argument now goes as follows. Let κ be strongly inaccessible, and suppose that there are wellfounded sets of arbitrarily large size below κ. So if B is a collection of self-membered power sets then nothing in B is κ-large. So $\in\restriction\{\mathcal{P}(x) : |x| < \kappa\}$ is wellfounded. Then use a Boffa permutation as above to obtain a model in which $\in\restriction\{x : |x| < \kappa\}$ is wellfounded.

We can also show

Remark 54 $\Diamond 5' \longleftrightarrow \Diamond(\in$ *restricted to finite sets is wellfounded*)

Proof:

This requires a Boffa-style permutation.

R $\rightarrow$ L.

> We can prove this even with the $\Diamond$ stripped off, which we will now do. The right-hand side implies that no finite set is selfmembered and in particular that no finite power set is selfmembered. Now let A be a set of power sets with no $\in$-minimal member. Then $\bigcap A$ is a self-membered power set by remark **??**. If any member of A had been finite, then $\bigcap A$ would be finite too. So A consists entirely of infinite sets. This is $5'$. ■
>
> [This takes us very close to a proof of a result of Tonny Hurkens for Zermelo set theory: that the relation $F(x, y)$ iff $\mathcal{P}(x \cap y) \subseteq y$ is wellfounded. Suppose not, and that there is a set X with no F-minimal element. Consider $\bigcap X$. Let y be an arbitrary member of X. Then there is $x \in X$ with $F(x, y)$). We have $\bigcap X \subseteq x$ and $\bigcap X \subseteq y$ whence $\mathcal{P}(\bigcap X) \subseteq \mathcal{P}(x)$ and $\mathcal{P}(\bigcap X) \subseteq \mathcal{P}(y)$. These last two imply $\mathcal{P}(\bigcap X) \subseteq \mathcal{P}(x) \cap \mathcal{P}(y = \mathcal{P}(x \cap y) \subseteq y)$. But y was an arbitrary member of X; so $\mathcal{P}(\bigcap X$ is included in every member of X, so $\mathcal{P}(\bigcap X) \subseteq \bigcap X$, contradicting Zermelo's axioms.]

L $\rightarrow$ R.

> Let π be the permutation
>
> $$\prod_{|x| \in \mathbb{N}} (\langle \mathcal{P}((\bigcup(\texttt{fst}``x \cap FIN)), V \setminus x\rangle, x)$$
>
> (FIN is the set of finite sets). Clearly if x is finite then $\pi(x)$ isn't.

Suppose $V^\pi \models x \in y$, both finite. Then $x \in \pi(y)$, and $\pi(x)$ and $\pi(y)$ are both finite, so

$$x = \langle \mathcal{P}(\bigcup \texttt{fst}``(\pi(x))) \cap FIN, V \setminus \pi(x) \rangle$$

and

$$y = \langle \mathcal{P}(\bigcup \texttt{fst}``(\pi(y))) \cap FIN, V \setminus \pi(y) \rangle.$$

$x \in \pi(y)$ so $\texttt{fst}(x) \in \texttt{fst}``\pi(y)$. That is to say

$$\langle \mathcal{P}(\bigcup \texttt{fst}``(\pi(x))) \cap FIN, V \setminus \pi(x) \rangle \in \texttt{fst}``(\pi(y)).$$

Now $\texttt{fst}``\pi(y)$ is a subset of $\mathcal{P}(\bigcup \texttt{fst}``(\pi(y)))$ and consists entirely of finite sets so

$$\texttt{fst}``\pi(y) \subseteq \mathcal{P}(\bigcup \texttt{fst}``(\pi(y))) \cap FIN$$

which is $\texttt{fst}(y)$.

This tells us that $\texttt{fst}$ is a homomorphism from $\langle FIN^\pi, \in_\pi \rangle$ to $\langle PFIN, \in \rangle$ where $PFIN$ is the set of finite power sets. And $5'$ certainly implies that this second structure is wellfounded. So the first must be wellfounded too. ■

It's natural to wonder if we can do this for properties other than finiteness, for other notions of smallness. Remark 54 exploits the fact that a union of finitely many finite sets is finite, which is a bit of a downer. In general we will have difficulties beco's $|\mathcal{P}_\kappa(X)| > \kappa$ so $\mathcal{P}_\kappa(X) \subseteq X$ is not the same as $\mathcal{P}_\kappa(X) \in \mathcal{P}_\kappa(X)$. We seem to need κ to be strong limit.

So, if there is an infinite wellfounded set, $\Diamond$($\in$ restricted to finite sets is wellfounded). The first of several interesting questions this suggests is: is there a converse?

> If $\Diamond$($\in$ restricted to finite sets is wellfounded) is there an infinite wellfounded set?

A second question arises from the observation that of course the interesting assertion is not " $\Diamond$($\in$ restricted to finite sets is wellfounded)" but $\Delta_{\aleph_0}$, which says "$\Diamond$($\in$ restricted to finite sets is wellfounded and the graph of the rank function is a set)". The second question is

> Might $\Delta_{\aleph_0}$ follow from "There is an infinite *transitive* wellfounded set"?

This is suggested to me by the way in which Holmes' permutation highlights the rôle of transitivity in this setting. Is it time to review the question of whether or not transitive closures of wellfounded sets (when they exist) are likewise wellfounded?

That sounds like something worth looking at:

> "If a wellfounded set x has a transitive closure $TC(x)$, is $TC(x)$ wellfounded?"

A funny translation task

As always happens when i encounter a new idea, i cannot leave it alone. Here are some tho'rts on how to take it further.

Suppose we are in a model $\mathfrak{M}$ where we have some stratified property ϕ such that $\mathfrak{M} \models \phi(x) \to x \notin x$. A good example would be Boffa's model, where ϕ is $|\mathtt{fst}``x| \not\geq_* \aleph_0$. ϕ is closed under surjections. Consider the permutation

$$\pi = \prod_{|\mathtt{fst}``x| \not\geq_* \aleph_0} (x, \langle V \setminus \mathtt{fst}``x, x\rangle)$$

We want to show

$$V^\pi \models \psi(x) \to x \notin x$$

for some suitable ψ. This is

$$V \models \psi(\pi_n`x) \to x \notin \pi`x$$

Now we have constructed π so that, for instance:

$$V \models |\mathtt{fst}``x| \not\geq_* \aleph_0 \to x \notin \pi`x$$

so what we want is to find ψ such that $(\forall x)(\psi(\pi_n`x) \to |\mathtt{fst}``x| \not\geq_* \aleph_0)$. This is equivalent to

$$(\forall x)(\psi(x) \to |\mathtt{fst}``(\pi_n`x)| \not\geq_* \aleph_0).$$

Therefore we want to see if the property

$$|\mathtt{fst}``(\pi_n`x)| \not\geq_* \aleph_0$$

turns out to be implied by anything sensible. (Remember π is definable, so 'x' is the only free variable!

11.3 Is it consistent relative to *NF* that there should be an infinite wellfounded set?

I tho'rt i'd proved it:

Let us work in Friederike's model which contains a natural number k such that $(\forall n \in \mathbb{N})(n > k \to n < Tn)$. Let π be the permutation that for $n \in \mathbb{N}$ swaps $\{n \cdot k\}$ with $(Tn + 1) \cdot k$ for $n > 0$, swaps Λ with 0 and fixes everything else. In V^π the set that was $\{n \cdot k : n \in \mathbb{N}\}$ (let us call this set $\mathtt{b}$) has become the Zermelo integers, which is to say the intersection of all sets containing the empty set and closed under singleton. Suppose $V^\pi \models 0 \in y \wedge (\forall x \in y)(\{x\} \in y)$, we want $V^\pi \models \mathtt{b} \subseteq y$. That is to say, $\pi(\mathtt{b}) \subseteq \pi(y)$. $\pi(\mathtt{b}) = \mathtt{b}$.

$V^\pi \models \Lambda \in y \wedge (\forall x \in y)(\{x\} \in y)$ is just $\pi`\Lambda \in \pi`y \wedge (\forall x \in \pi`y)(\pi`\{x\} \in \pi`y)$. We know $\pi`\Lambda = 0$. So we must show

$$(\forall y)(0 \in y \wedge (\forall x \in y)(\pi`\{x\} \in y) \to \mathtt{b} \subseteq y)$$

We cannot prove by induction on $\mathtt{b}$ that if $n \in \mathtt{b}$ then $(\forall y)((0 \in y \wedge (\forall x \in y)(\pi`\{x\} \in y)) \to n \in y)$ because this is not a stratified induction. We do it instead by UG on '$y$'. Let $y$ be a set such that $0 \in y \wedge (\forall x \in y)(\pi`\{x\} \in y)$. We want $\mathtt{b} \subseteq y$.

0 is in y, by hypothesis. Let $n \cdot k$ be minimal such that $n \cdot k \notin y$. Now $n = \pi`\{T^{-1}(n-1) \cdot k\}$. But – since $(\forall x \in y)(\pi`\{x\} \in y)$ – we must have $T^{-1}(n-1) \cdot k \notin y$ too. But $T^{-1}(n-1) \cdot k$ is bigger than k so $T^{-1}(n-1) \cdot k < (n-1) \cdot k$ and $(n-1) \cdot k < n \cdot k$ contradicting minimality of $n \cdot k$.

This tells us that, in V^π, $\mathtt{b}$ is the intersection of all sets containing the empty set and closed under singleton. This set is clearly wellfounded, because if $\mathcal{P}(X) \subseteq X$ then X contains the empty set and is closed under singleton. Now to show it is infinite. We have $|\mathtt{b}| = T|\mathtt{b}| + 1$, so clearly $|b|$ is not a natural number.

. . . but of course in the last paragraph but one the sentence beginning "But $T^{-1}(n-1) \cdot k$ is bigger than k . . . " should go on to say that $T^{-1}(n-1) \cdot k < (n-1) \cdot Tk$ which of course is buggerall use to man or beast.

Nevertheless, the idea of trying to prove $\Diamond\exists\mathbb{N}_{\mathrm{Zm}}$ from nothing seems a good one. All we need is a k such that $(\forall n \in \mathbb{N})(n \cdot k < Tn \cdot k)$ rather than $(\forall n > k)(n < Tn)$. But this is just $\mathrm{AxCount}_{\leq}$.

Inductively define a subset $\mathtt{X}$ of $\mathbb{N}$ as follows: $0 \in \mathtt{X}$; if $x \subseteq \mathtt{X}$ then $k \cdot \Sigma_{n \in x} 2^n \in \mathtt{X}$.

Define E' on $\mathtt{X}$ by $x \, E' \, y$ iff the xth bit of y/k is 1.

Now swap Tn with $\{m : m \, E' \, n\}$, for n and Tn in $\mathtt{X}$. The trouble is, for this to work we seem to need $\mathtt{X}$ to be closed under T.

There is this idea abroad that if $\in$ restricted to FIN is wellfounded, then we should be able to get an infinite wellfounded set. Let $f : FIN \to \mathbb{N}$. Define $f^* : FIN \to \mathbb{N}$ by $f^*`x = T \sup\{f`y + 1 : y \in x \cap FIN\}$. If we could show that * had a fixed point we would be able to infer that $<^T$ is wellfounded. But this is far too strong. So the obvious approach doesn't work!

Think again about trying to get an infinite wellfounded set at no cost. What does a natural have to do to be a wellfounded set in the Ackermann permutation model? Clearly the restriction of E^T to $E^T``\{n\}$ has to be wellfounded. One way of ensuring this is to require that $(\exists m)(\forall k)(T^k n \leq m)$. (Forgive abuse of notation!).

So we are led to the proposition that the collection of naturals n such that there is such an m is unbounded.

$$(\forall m')(\exists m \geq m')(\exists k)(\exists A \subseteq \mathbb{N})(T^{-1}``A \subseteq A \wedge m \in A \wedge (\forall a \in A)(a < k))$$

Of course one could be more kosher about it and concentrate on the property

$$V^\alpha \models (\forall x)(\mathcal{P}_{\aleph_0}(x) \subseteq x \to n \in x)$$

where α is the Ackermann permutation. This is $(\forall x)(\mathcal{P}_{\aleph_0}(x) \subseteq x \to n \in x)^\alpha$ $(\forall x)(\mathcal{P}_{\aleph_0}(x) \subseteq \alpha``x \to n \in x)$. I think without loss of generality we can restrict attention to subsets of $\mathbb{N}$.

$(\forall x \subseteq \mathbb{N})((\forall \text{ finite } x' \subseteq x)((\Sigma_{y \in x'} 2^{Ty}) \in x) \to n \in x)$

We want there to be an infinite set of such n. An obvious question is: is this collection downward-closed? I think it is clear that if x is a member and the Tkth bit of x is 1 then k is a member.

The obvious thing to do is a recursive definition:

> 0 is a wellfounded* natural; if X is a finite set of wellfounded* naturals, then $T(\Sigma_{n\in X}2^n)$ is wellfounded*

Is this class unbounded? Does it have an unbounded subset? This is something to do with finite sets extending their own power sets. I suspect it is unbounded iff the following class is unbounded:

> 0 is a widget; if n is a widget, so is 2^{Tn}.

(This takes us back to the Zermelo naturals!)

What we seem to have done is shown that in V^α there are infinitely many hereditarily finite sets iff the collection of wellfounded* naturals is unbounded.

Think about the family of inductively defined collections: $\{0, Tf(0), T(f(Tf(0)))\ldots\}$ indexed by the family of definable homogeneous maps $f : \mathbb{N} \to \mathbb{N}$ which commute with T. Are these equally unbounded, as it were? Start with the closure of $\{0\}$ under $\lambda n.T(n + 1)$. The assumption that this is unbounded implies $\mathrm{AxCount}_{\leq}$. For sse there is $x > Tx$. Then the initial segment bounded by x contains 0 and is closed under $\lambda n.T(n + 1)$. So this is a strong assumption. Where f is more rapidly increasing this could be a weaker assumption. So how about: $\exists f : \mathbb{N} \to \mathbb{N}$ which commutes with T such that $\{0, Tf`0, T(f`Tf`0)\ldots\}$ is unbounded? Is this the same as $\exists f : \mathbb{N} \to \mathbb{N}$ which commutes with $T$ such that $(\forall n)(f`Tn \geq n)$?

So suppose there is an x such that $x > f`Tx$. Then the initial segment bounded by $x$ contains 0 and is closed under $\lambda n.T(f`n)$, so the sequence $\{0, Tf`0, T(f`Tf`0)\ldots\}$ is bounded. Contraposing, if $\{0, Tf`0, T(f`Tf`0)\ldots\}$ is unbounded, then there is no x such that $x > f`Tx$, so $(\forall n \in \mathbb{N})(n \leq f`Tn)$. But, as we have seen earlier (lemma 14), if f commutes with T then $(\forall n \in \mathbb{N})(n \leq f`Tn) \to \mathrm{AxCount}_{\leq}$ . (If there is an $n > Tn$ then consider the $Tn$th member of the sequence $\{0, f`0, f^2`0, \ldots f^n`0\ldots\}$. This will be a counterexample to the antecedent.)

So we seem to have proved:

Theorem 20 *Let f be a definable homogeneous map $\mathbb{N} \to \mathbb{N}$ which commutes with T. The inductively defined collection: $\{0, Tf`0, T(f`Tf`0)\ldots\}$ is unbounded iff $AxCount_{\leq}$. [HOLE What happens if f isn't a unary thing like this?]*

No, we haven't shown that: there is a gap in the proof. We shouldn't be considering an n such that $n > f(Tn)$ but an n which bounds the collection. But i think what is true is that if $f : \mathbb{N} \to \mathbb{N}$ is monotone increasing and commutes with F then $\mathrm{AxCount}_{\leq}$ is equivalent to the assertion that the only initial segment of $\mathbb{N}$ closed under $f \circ T$ is $\mathbb{N}$ itself. We reason as follows. (This will need a bit of tidying up) Sse $f : \mathbb{N} \to \mathbb{N}$ is monotone increasing and commutes with F. If f isn't the identity then we will have $n+1 \leq f(n)$. Sse now that $[0, n+1]$ is closed under $f \circ T$. Then

$$f(Tn) < n + 1 \leq f(n)$$

whence

$$f(Tn) < f(n)$$

and $Tn < n$. So, unless AxCount$_{\leq}$ fails, no proper initial segment of $\mathbb{N}$ can be closed under $f \circ T$. So the intersection of all initial segments closed under $f \circ T$ is $\mathbb{N}$ itself. Is this *exactly* the same as saying that the closure of $\{0\}$ under $f \circ T$ is unbounded...? This relies on the downwards closure of the closure of $\{0\}$ under $f \circ T$ being the same as the intersection of all initial segments of $\mathbb{N}$ closed ubnder $f \circ T$. This should be easy to check one way or another.

Even that case is easy. Suppose there is an x such that $x > T(\Sigma_{y<x} 2^y)$. Then the collection of wellfounded* naturals is bounded. But there will be such an x unless AxCount$_{\leq}$.

Corollary 9
If V^α contains infinitely many hereditarily finite sets then AxCount$_{\leq}$.
(The converse is easy because, if AxCount$_{\leq}$ holds, then V^α contains V_ω).

So if we want infinitely many wellfounded sets cheaply we will have to try something else. For example: work in a model where $\in$ restricted to finite sets is wellfounded.

One might think that one should be able to collapse FIN to get H_{fin}. The collapsing function is a fixed point of a TRO so one might hope to add it by permutation while keeping FIN wellfounded. j is a map from $FIN \to FIN$ into itself, and the collapsing function is the lfp. We would need an f that was n-similar to jf and that might require weak forms of choice. Sounds hard. In any case the output of this construction (if there is one) would be H_{fin} and this is much stronger than the result we are looking for, namely the existence of an infinite welfounded set.

Instead ask: is there an infinite extensional set of finite sets? If X is such a set consider the permutation $\prod_{x \in X}(x, x \cap X)$. Extnsionality of X ensures that these transpositions are disjoint. In the resulting permutation model X has become an infinite wellfounded set.

So we have shown:

Remark 55 *If $\in\restriction FIN$ is wellfounded, and there is an infinite extensional subset of FIN, then $\Diamond\exists$ infinite wellfounded set.*

$\in\restriction FIN$ can be made wellfounded at no cost, so the only hard part is getting an infinite extensional subset of FIN. If we can't do that, then every infinite set X of finite sets contains x and y such that $x \cap X = y \cap X$. Can we do anything with this?

Here's an idea i had while invigilating one day.

Suppose no permutation model contains an infinite wellfounded set. Let's derive something nasty from it.

$$\Box\forall x((\forall y)(\mathcal{P}(y) \subseteq y \to x \subseteq y) \to Fin(x))$$

$$\forall\sigma\forall x((\forall y)(\mathcal{P}(\sigma``y)) \subseteq y \to x \subseteq y) \to Fin(x))$$

$$(\forall x)(\ (\forall\sigma)(\forall y)(\mathcal{P}(\sigma``y)) \subseteq y \to x \subseteq y) \to Fin(x))$$

$$(\forall x)(\ (\exists\sigma)(\forall y)(\mathcal{P}(\sigma``y)) \subseteq y \to x \subseteq y) \to Fin(x))$$

Snooze ... does this become

$$(\forall\alpha)(\ (\exists\sigma)(\forall y)(\mathcal{P}(\sigma``y)) \subseteq y \to \alpha \leq |y|) \to \alpha \in \mathbb{N})$$

Idea: show that every singleton is $\{\Lambda\}$ in some permutation model. This is easy. $\{x\}$ is $\{\Lambda\}$ in $V^{(x,\Lambda)}$. Then show that every pair is wellfounded in V^σ for some σ, and so on.

What we now have to do is find something slightly stronger than '$(\exists\sigma)(\forall y)(\mathcal{P}(\sigma``y) \subseteq y \to x \subseteq y)$' that might enable us to deduce "any set that embeds into all fat sets is finite"? Either there are arbitrarily large cardinals of such finite sets in which case $\aleph_0$ is such a cardinal (any fat set that is not inductive is dedekind-infinite) which contradicts hypothesis, or there aren't. If not, there finite sets too big to embed in a fat set. But then there are smaller fat sets. But this was any old base model. So we would have established that if no permutation model contains an infinite wellfounded set, every permutation model contains a fat finite set. The consequent sounds improbable!!!!

Mind you, i'd've tho'rt that $(\exists\sigma)(\forall y)(\mathcal{P}(\sigma``y) \subseteq y \to x \subseteq y)$ is strictly stronger than x embedding into all fat sets.

Randall sez:

Wed Sep 23 11:42:23 1998

Note on permutation idea

Aim is to make an infinite well-founded set. The idea is to ensure that all sets which include their power sets include $\mathbb{N}$.

The strategy is to permute in such a way that any set which excludes part of $\mathbb{N}$ has a subset mapped outside itself. We postulate further that this subset will be a subset of N (necessarily a proper subset). We postulate further still that this subset of $\mathbb{N}$ will be mapped to an element of $\mathbb{N}$.

So the situation we envisage in one in which each proper subset of $\mathbb{N}$ (the part of $\mathbb{N}$ included in a set missing part of $\mathbb{N}$) itself has a subset which is assigned by the permutation as the extension of a natural number not in the original proper subset.

For each proper subset A of $\mathbb{N}$ there is a subset B of A such that $\pi`n = B$ for some natural number n in $\mathbb{N} \setminus A$.

It is not clear that this is possible, but it is also not clear that it is impossible.

11.3.1

How about this for a clever idea. We need to think about permutation models with more wellfounded sets than V^α, yes? Now say

Try $\sigma \leq \tau$ iff
$(\forall Y)((\mathcal{P}(Y) \subseteq Y)^\tau \to (\exists X)((\mathcal{P}(X) \subseteq X)^\sigma \wedge \sigma`X \subseteq \tau Y))$
which simplifies to

$$(\forall Y)((\mathcal{P}(Y) \subseteq \tau``Y) \to (\exists X)((\mathcal{P}(X) \subseteq \sigma``X) \wedge X \subseteq Y))$$

Now some questions about $\leq$.

- Is $\leq_w$ wellfounded? It certainly should be!
- Is it connected? This is really a question about how ragged *WF* can be. If it can't be ragged then $\leq$ might be connected.
- Is it invariant (in the sense that closed formulæ containing only $\leq_w$ and $=$ are invariant? (need a word for this!)
- Can we show that $\leq$ is not the universal relation? Slightly more likely is the assertion that ϕ^{WF} is invariant for all ϕ. How about $\phi^{H_{fin}}$ being invariant? That ought to be easy!

Let's have a look at this last one. We might be able to do something if ϕ is stratified.

Expand ϕ^{WF}. The variables in it have types. The quantifiers become things like $(Qx)((\forall X)(\mathcal{P}(X)) \subseteq X \to x \in X) \to \ldots)^\sigma$
which is
$(Qx)((\forall X)(\mathcal{P}(\sigma_n`X)) \subseteq \sigma_{n+1}`X \to \sigma_{n-1}`x \in \sigma_n`X) \to \ldots)$
relettering we get:
$(Qx)((\forall X)(\mathcal{P}(X)) \subseteq (j^{n+1}\sigma)`X \to \sigma_{n-1}`x \in X) \to \ldots)$
and the trouble now is that saying that something at type n is wellfounded is different from saying that something at type $n+1$ is wellfounded, unless there is something really clever we can do.

The obvious way to show that WF^σ and WF^π are elementarily equivalent w.r.t. stratified expressions is to show that they are stratimorphic. For them to be stratimorphic one would expect there to be a permutation of V that maps one onto the other. Such a permutation one would expect to be definable in terms of σ and π and there isn't anything obvious.

The same difficulty occurs when trying to show $\phi^{H_{fin}}$ invariant. This suggests that even the stratified theory of hereditarily finite sets isn't clean! Clearly there is a gap here. I can imagine no way of showing that the stratified theory of hereditarily finite sets is invariant and no way of showing that it isn't.

One might think it would be worth trying
$\pi \leq \sigma$ iff $\exists$ partial injection $f : V \rightharpoondown V$ such that $f`x = \sigma^{-1}`\{f`z : z \in_\pi x\}$, which is to say
$\pi \leq \sigma \longleftrightarrow (\exists f : V \rightharpoondown V)(\sigma^{-1}(j`f)\pi \subseteq f)$.

That is to say
$\sigma \leq \tau$ iff $(\exists h : V \rightharpoondown V)(\tau^{-1} \circ (j`h) \circ \sigma \subseteq h$.
...which looks nice because these h's look a bit like germs. The trouble with this is that it is symmetrical! We should also presumably have the banally obvious: $(\exists h : V \rightharpoondown V)$ such that if V^σ thinks x is wellfounded then $h`x$ is defined and believed by V^τ to be wellfounded.

Let Γ be the set of sentences in the language of arithmetic-with-T which become truths of arithmetic when one erases 'T' from them.

1. Are the $f \in \mathbb{N}^{\mathbb{N}}$ that commute with T cofinal in the partial order under dominance?

2. $\text{AxCount}_{\leq} \to (\forall \alpha < \omega_1)(\alpha \leq T\alpha)$? Is this perhaps related to the question of whether or not there is an assignment of fundamental sequences to ctbl limit ordinals that commutes with T. But this formula is in Γ. I have the very strong feeling that no form of AC (eg fns assigning fundamental sequences) will help prove them.

3. If Φ is a sentence in arithmetic-with-T that is true of the identity but not provable in arithmetic-with-T is there an Ehrenfeucht-Mostowski model in which it fails?

4. André's question. $(\exists n \in \mathbb{N})(n \neq Tn \wedge (\forall m < n)(m \leq Tm))$

5. Does "$\in$ restricted to FIN is well founded" imply "$(\exists f \in \mathbb{N}^{\mathbb{N}})(\forall n \in \mathbb{N})(f(Tn) > n))$, namely the existence of a Körner function? Is the existence of Körner functions provable in *NF* already? It's certainly invariant.

6. Is it consistent with *NF* that there should be a $f : T``NO \to NO$ such that $(\forall \alpha)(f`\alpha \geq T^{-1}\alpha)$?

If there is $f : T``NO \to NO$ such that $(\forall \alpha)(f(T\alpha) \geq \alpha)$ – namely a Körner function on the ordinals – then surely something interesting must happen. For suppose α is an ordinal to which f can be applied as often as we like, and think about $F(\alpha) := \sup A$ where $A := \{f^n(\alpha) : n \in \mathbb{N}\}$. We have $f^n(\alpha) \geq T^{-1}f^{n-1}(\alpha)$ for each n, so everything in $T^{-1}``A$ is $\leq$ something in A so $F(\alpha) \leq T(F(\alpha))$. Without extra assumptions we're not going to be able to do much: commutes with T, monotone, nondecreasing.... Let's face it, might there not be such a function with no assumptions at all: $\lambda\alpha.\Omega + \alpha$? No – it doesn't work for $\alpha > \Omega$.

Suppose there is a Körner function on the ordinals: $f : T``NO \to NO$ such that $(\forall \alpha)(f(T\alpha) \geq \alpha)$. Then we can make it continuous by filling it in, and we can make it nondecreasing, by recursively setting $g(\alpha) := \max(f(\alpha), \sum_{\beta<\alpha} g(\beta))$. Or at least that's what i tho'rt. The trouble is, that sup might not always be defined, as $\{g(\beta) : \beta < \alpha\}$ might be cofinal in the ordinals. So we have to use my version of Erdös-Rado. Two-colour the complete graph on NO according to whether or not $(\alpha < \beta) \longleftrightarrow (f(\alpha) < f(\beta))$. There will be a monochromatic set of size $T^3|NO|$ and on it f must be monotone. Then things get a bit tricky, but the idea is to pull the map back via T. No, not even that will work, as there is no guarantee that enough of the image under f of the monochromatic set is in the range of T. We have to do various manœuvres like work not on f but on $f|\{\alpha : \alpha > \Omega\} \cup f^T$.

6 is obviously something to do with $\in$ restricted to something quite being wellfounded: set $\Omega_x = \bigvee_{On}\{\alpha + 1 : \alpha$ is a second component of an ordered pair in $x\}$. That sort of thing.

It implies that $cf(\Omega)$ is cantorian. Is the converse true?

Is there any connection between 4 and the cofinality of Ω? (Must check that if there is such a function there is one that is continuous) Suppose there is a map h from the ordinals below α cofinally into $T``NO$. Suppose also that $\alpha = T\alpha$. Then there is also a map g from the ordinals below α cofinally into NO. We prove by induction that $(\forall \beta < \alpha)(Tg`\beta = h`T\beta)$ We now define an f as in 4 as follows. First we define it on things in the range of h. For $\beta < \alpha$ set f of $h`\beta$ to be $g`\beta$.

Whatever happened to the idea that every assertion of cardinal arithmetic is $\Box$ some assertion about sets? For example, $\text{AxCount}_{\leq}$ is equivalent to

$$\Box(\text{if } x \text{ is a finite set then } x \text{ is not a proper subset of } \iota``x).$$

I think that in the *NF* case it is complicated by the fact that equinumerosity and 1-equivalence are not the same, but in *ZF* they are. Obviously their negations are $\Diamond$ of some piece of combinatorics.

André once cheered me greatly by saying that it was very significant that cardinal arithmetic is invariant. I'd always tho'rt i was the only solipsist. It now occurs to me that the fact that it has an implementation that is invariant must be something to do with the fact that it is a theory of virtual entities.

Does every model of NF have a permutation model containing the Zermelo Naturals?

Let's see...

Consider the permutation π that swaps 0 with $\emptyset$ (0 is zero and $\emptyset$ is the empty set) and thereafter swaps $Tn + 1$ with $\{n\}$. The idea is that in V^π the old $\mathbb{N}$ has become the Zermelo naturals.

Reasoning in V^π we establish that $\mathbb{N}$ contains the empty set (the empty set of V^π is the old 0, which is $\mathbb{N}$ as desired) and is closed under ι.

The hard part is to show that $\mathbb{N}$ is the *least* member of V^π which contains 0 and is closed under ι. So let A be a set that V^π believes contains the empty set and is closed under ι. The idea is to prove by induction on $\mathbb{N}$ (in the original model) that every (old) natural number is in$^\pi$ A. No problem with zero aka the empty set. So suppose A contains a natural number n. We want it to contain $n+1$. Does it? One thing we know that it contains is the thing it believes to be the singleton of n, which is $Tn+1$. This is not what we want. We can probably show that A is downward closed, so if $n + 1 \leq Tn + 1$ we'd be OK. But that is a strong assumption.

In fact one can say a bit more. If $n > Tn$ then, in V^π the natural numbers below n form a set that contains 0 and is closed under ι.

So the answer to the question sems to be 'No'! Every model of NF contains a permutation that gives a permutation model in which there is a set that contains $\emptyset$ and is closed under ι but there won't reliably be a least such, which is what the Zermelo naturals would be.

Chiz chiz

Chapter 12

Miscellaneous Cardinal Arithmetic

Julia Millhouse writes to me from Paris about how she likes the theorem that $(\forall\alpha \in NC)(T\alpha \leq \alpha)$ implies the axiom of counting. It occurs to me that it is equivalent to: "every set of singletons embeds into its sumset". Can we usefully generalise it? I think the correct generalisation is: replace "set of singletons" by "set of pairwise disjoint sets"; it says:

$$(\forall\alpha, \beta)(\beta \leq^* \alpha \to T\beta \leq \alpha).$$

Interesting. . . this is a choice principle as well as a cardinality principle. It's the partition principle – more-or-less.

12.0.1 Some factoids useful in connection with T$\mathbb{Z}$T and Bowler-Forster

For the moment let the variable 'κ' range over alephs. Then we can prove things like: a union of κ-many κ-sized sets cannot be of cardinality $\geq \kappa^{++}$; a union of κ-many $< \kappa$-sized sets cannot be of cardinality $\geq \kappa^{+}$. In the study of T$\mathbb{Z}$T we sometimes need arguments that rely on facts like these. Can a union of $T|V|$ many sets each of size $T|V|$ be of size $|V|$? Well, yes – obviously. But how about a union of $T^2|V|$ many sets each of size $T^2|V|$? We need to worry about things like that.

It turns out that the old methods work quite well. Drop the assumption that κ is an aleph, but assume $\kappa^2 = \kappa$. This is actually reasonable, because it holds of $|V|$.

Here's a taster. Suppose $\kappa^2 = \kappa$. Then a union of κ-many things of size κ cannot be of size 2^{2^κ}. Here's why. By Sierpinski-Hartogs any set $\mathcal{P}^2(K)$ of size 2^{2^κ} has a subset of size $(\aleph(\kappa))^{++}$. (Well, you might need an extra exponent but you get the idea) The subset is a union of $\leq \aleph(\kappa)$ sets each of size $\leq \aleph(\kappa)$ and therefore cannot be of size $(\aleph(\kappa))^{++}$ after all.

We need to push the boat out a bit. Suppose $\kappa^2 = \kappa$ as before. Then can a union of $\leq \kappa$-many things of size $\leq \kappa$ be of size 2^{2^κ}? Presumably not, and by the same proof. But then can a union of $\leq^* \kappa$-many things of size $\leq^* \kappa$ be of size 2^{2^κ}? We may well need results like that and they may well be much harder to obtain.

Perhaps we should spare some thought for the parenthetical remark a couple of paragraphs ago. I write there as if $(\aleph(\kappa))^{++} \leq 2^{2^\kappa}$ as long as $\kappa = \kappa^2$. But that's not secure. Let K be a set of size κ. Send every prewellordering of a subset of K to its length and send everything not a prewellordering to 0. This maps $\mathcal{P}(K \times K)$ onto a set of size $\aleph^*(K)$ and – since $\kappa^2 = \kappa$ – we get $\aleph^*(\kappa) \leq^* 2^\kappa$. Analogously we of course also get $\aleph^*(2^\kappa) \leq^* 2^{2^\kappa}$. So certainly $(\aleph^*(\kappa))^+ \leq \aleph^*(2^\kappa) \leq^* 2^{2^\kappa}$. It doesn't seem to want to come out at the moment. It probably doesn't matter (for T$\mathbb{Z}$T applications at any rate) having to have another layer of exponentiation.

Here's why. By Sierpinski-Hartogs any set $\mathcal{P}^2(K)$ of size 2^{2^κ} has a subset of size $(\aleph(\kappa))^{++}$. (Well, you might need an extra exponent but you get the idea) The subset is a union of $\leq \aleph(\kappa)$ sets each of size $\leq \aleph(\kappa)$ and therefore cannot be of size $(\aleph(\kappa))^{++}$ after all.

12.0.2 Other Stuff to fit in

Cardinality of Σ_V?

While thinking about the question of whether or not AxCount$_\leq$ implied that $(\forall \alpha \leq \omega_1)(\alpha \leq T\alpha)$ I found that this would follow from the assumption that every ordinal (in T^2“NO) contains a wellordering that commutes with T. This should make us think of the term model, because although there are clearly definable functions (definable as stratified set abstracts) $NO \to NO$ which do not commute with T (send α to 0 if $T^{-1}\alpha$ is not defined and to 1 o/w) something along the lines: every definable wellordering of ordinals commutes with T. But isn't $(\forall \alpha \in NO)(\alpha \geq T\alpha)$ strong? This *might* enable us to show that *NF* has no term models.

12.1 Wellfounded extensional relations

Randall has been thinking for some time about whether or not $\mathcal{P}(NO)$ can be wellordered. Since if we can wellorder $\mathcal{P}(NO)$ we can wellorder the power set of any wellordered set, this reminded me that there is an old theorem of Rubin's that if the power set of a wellordered set is always wellordered, then every wellfounded set is wellordered. Since one of my current preoccupations is the theory of wellfounded sets in NF (I conjecture that it is precisely KF) I was intrigued! However the induction Rubin used is highly unstratified and there seems no hope at all of reproducing it in NF. Something Rob Solovay said made it clear to me that the correct thing to do with Rubin's proof is to use it to prove something about domains of wellfounded extensional relations rather than about wellfounded sets. This i do below.

There is an old problem of Hinnion's: in his thesis he did a lot of work on relational types of wellfounded extensional relations and asked whether one could show that there is no wellfounded extensional relation on V. I know of no progress with this problem[1] However, what we can now say (if i've got this right - and i am not staking my life on the correctness of this broadcast!) then if $\mathcal{P}(NO)$ can be wellordered there is no wellfounded extensional relation on V. Since Holmes has recently pointed out that if $\mathcal{P}(NO)$ cannot be wellordered there is a cantorian wellordered set whose power set is not wellorderable something interesting is doomed to come out of this one way or another. To prove Holmes' result, simply consider the least aleph α such that 2^{α} is not an aleph. If α is such an aleph so is $T\alpha$, so $\alpha \leq T\alpha$. Therefore $T^{-1}\alpha$ is defined and is another such aleph, so $\alpha \leq T^{-1}\alpha$ whence $\alpha = T\alpha$. Finally Richard Kaye remarked to me some time ago that there seems a natural way in which models of NF with wellfounded extensional relations on V might arise, and i append his message on the end of this broadcast with his permission.

I shall prove the following

THEOREM 21 *the following are equivalent*

$\mathcal{P}(NO)$ is wellorderable;

The power set of a wellordered set can be wellordered;

The domain of a wellfounded extensional relation is wellorderable;

$|\mathcal{P}(NO)| < |NO|$

Proof:

$1 \rightarrow 2$ is fairly easy. Let X be an arbitrary wellordered set. Then ι"X is the same size as some subset of NO and therefore its power set is wellordered. 4 comes into the picture because it is a theorem of Henson's that $|NO| \not\leq |\mathcal{P}(NO)|$.

To prove $3 \rightarrow 1$ notice that we can define a wellfounded extensional relation on $\mathcal{P}(NO)$. For starters, we can define a relation E on subsets of NO that are not initial segments by setting $\{\alpha\}\, E\, X$ iff $\alpha \in X$ (so that the only things that E anything are singletons) and distinguishing between singletons by saying $\alpha\, E\, \beta$ iff $\alpha < \beta$. Now a simple application of Bernstein's lemma shows that NO has as many subsets that aren't initial segments as it has subsets, and we use the bijection to pull back the relation to the whole of $\mathcal{P}(NO)$.

To prove $2 \rightarrow 3$ we need the induction in Rubin. This is lifted wholesale from Rubin (or rather the version of it in Jech *The Axiom of Choice*) the only difference being that here it is cast in the more general setting of an arbitrary wellfounded extensional relation. It seems highly unlikely that one could prove it over $\in$ in *NF*, since the induction is unstratified and $\in$ is not a set.

Assume 2. Let R be a wellfounded extensional relation with domain X. We will show that X can be wellordered. Without serious loss of generality we can

[1] May 2023. We now know that it is consistent relative to NF that there should be a nontrivial $\in$-automorphims of V (every model of NF has a permutation modal admitting such an automorphism) so there can be no *definable* wellfounded extensional relation on V.

assume that the rank of R is reasonably small, by considering $\texttt{RUSC}^n(R)$ for n sufficiently large (3 will be large enough) because X can be wellordered iff ι^n“X can be.

To each member x of X we can associate the rank of $*R^{-1}$“$\{x\}$. Call this the **rank** of x. Let N_α be the set of things of rank $\leq \alpha$. We will need to know that there is an ordinal too big to be the rank of any element of X. (This is the reason for reasoning with $\texttt{RUSC}^3(R)$ instead of R, just to be on the safe side). Let K be some set of size $\aleph$‘$|x|$, and fix $\leq_K$ and $\leq_{PK}$ wellorderings of K and $\mathcal{P}(K)$ respectively. We are going to show that there is a canonical injection

$$i_\alpha : N_\alpha \hookrightarrow K$$

where the range of i_α is an initial segment of K in the sense of $\leq_K$.

For $\alpha = 0$ it is easy. For the induction step from α to $\alpha + 1$ notice that i_α lifts to

$$j‘(i_\alpha) : \mathcal{P}(N_\alpha) \hookrightarrow \mathcal{P}(K)$$

Since R is extensional there is a canonical map ι“$N_{\alpha+1} \hookrightarrow \mathcal{P}(N_{\alpha+1})$ so we compose the two to get a map ι“$N_{\alpha+1} \hookrightarrow \mathcal{P}(K)$. Since $\mathcal{P}(K)$ is wellordered by $\leq_{PK}$ this gives us a (canonical) wellordering of $N_{\alpha+1}$. Now compare this wellordering of $N_{\alpha+1}$ with $\langle K, \leq_K\rangle$. Remember that K has been chosen so that it has a wellordering $\leq_K$ too long to be isomorphic to any wellordering of any subset of X. Therefore there is a (canonical) injection $N_{\alpha+1} \hookrightarrow K$ obtained by the recursive construction of the canonical map between two wellorderings.

This is not the end of the story, because we want to ensure that the various i_α agree on their intersections, so that we can take sums at limits. Therefore we have to ensure that everything in $N_{\alpha+1}$ goes after everything in N_α. So, given our injection from $N_{\alpha+1}$ into K, use it to order things in $N_{\alpha+1} \setminus N_\alpha$ (by pulling back $\leq_K$) and map them to the terminal segment of $\langle K, \leq_K\rangle$ consisting of things not in the range of i_α.

The case where α is a limit is easy as long as each i_α is an end-extension of all earlier i_β, and we have arranged for this by construction.

This shows that N_α is wellordered for all α. Since there is some ordinal too big to be the rank of any member of X, (call it γ) we know that N_γ must be the whole of X. Therefore X is wellordered.

■

A metamathematical remark. Many people find it difficult, on being told Rubin's result, to reconstruct the proof. If you are told it relies on foundation, you try to prove by induction on $\in$ that every wellfounded set is wellordered. **But is isn't a proof by induction on $\in$, it's a proof by induction on rank.**

Don't forget that Henson proved that $|NO| \not\leq |\mathcal{P}(NO)|$.

Corollary 10
Either $\mathcal{P}(NO)$ is wellordered, in which case there is no bfext on V or it isn't,

in which case there is $\aleph = T\aleph$ *such that* $2^{\aleph}$ *isn't an aleph*
So if there is a wellfounded extensional relation on V *there is a bad aleph.*

Can we find a proof that is a bit more effective? This one uses cut (the cut formula is '$|\mathcal{P}(NO)| \in WC$').

Consider the minimal rank of wellfounded relations on x.

We need a notion of relative jaggedness of a wellfounded relation. We have a notion of *hole*, and of *rank* of holes. We can make a relation less jagged by chipping off some elements that do not bear R to anything, and putting them in holes. We say $R < S$ if some of the holes in R are filled in S, and any of the holes in S that are not holes in R are of higher rank than those in R but not S. It should not be too hard to show that every descending chain under $<$ has a lower bound. Any minimal element is something very like a V_α. We can consider a version of $<$ on isomorphism types.

Suppose every set has a wellfounded extensional relation on it. Does this follow from the assertion that V has a wellfounded extensional relation on it? In either case consider the least ordinal α such that $\exists$ a wellfounded extensional relation on V. Should be easy to show $\alpha \leq T\alpha + 1$

Suppose there is a wellfounded extensional relation on x. Then there is also one on $\iota``x$. How about $\mathcal{P}(x)$? Some of the holes we would want to fill up with elements of $\mathcal{P}(x)$ are already occupied, so we can only accomodate $2^{T|x|} - T|x|$. But this is likely to be at least $2^{T|x|}$, at least if $2.|x| = |x|$.

Existence of wellfounded extensional relations on V generalises upward in models of T$\mathbb{Z}$T, and is $\Sigma_1^{\mathcal{P}}$.

12.1.1 Inhomogeneous wellfounded extensional relations on V

Given a set X we say that a relation $R \subseteq \iota``X \times X$ such that if $x_1 \neq x_2$ both in X then there is a singleton R-related to one but not the other is **skew-extensional**.

Do not free-associate to *skew-injective*!

If X admits such a relation then there is a map $f : X \hookrightarrow \mathcal{P}(X)$ defined by $\lambda x_X. \bigcup\{z : z\, R\, x\}$. Since not all sets can be embedded into their power sets, this is nontrivial. The corresponding move with bfexts does nothing.

Say $R \subseteq \iota``X \times X$ is **skew-wellfounded** iff $(\forall X' \subseteq X)(\exists x \in X')(\forall y \in X')(\neg(\{y\}Rx))$.

We shall say that R is a skew-extensional skew-wellfounded relation **on** X if its range is X, and let us call these relations 'Kbfext's.

Naturally the existence of Kbfexts is related to the existence of transitive wellfounded sets. For example, V is the same size as a transitive welllfounded set iff $\Diamond$ there is a kbfext on V.

We'd better have a proof of this.

If $b : V \to X$ is a bijection between V and a transitive wellfounded set X, Without loss of generality X is a power set. Now $\{\langle\{x\}, y\rangle : \text{Ƀ}(x) \in \text{Ƀ}(y)\}$ is skew-extensional and skew-wellfounded.

Conversely, if $R \subseteq \iota``V \times V$ codes a Kbfext, and f is a bijection between V and a moiety, then $\{\langle\{f`x\}, f`y\rangle : \langle\{x\}, y\rangle \in R\}$ codes a Kbfext on a moiety.

If R is a Kbfext on a moiety X, let π be a permutation of V extending the map $\lambda x, \bigcup R^{-1}``\{x\}$. Then in V^π $\pi^{-1}`X$ has become a transitive wellfounded set the same size as the universe.

It seems so extraordinarily unlikely that V should even be the same size as a wellfounded set, let alone a *transitive* wellfounded set, that i've never taken much interest in Kbfexts on V.

It is a recent theorem of Bowler's that V is not the same size as a wellfounded set

Now i claim the following. $\Diamond\exists H_{\aleph_0}$ iff there is a skew-wellfounded skew-extensional structure satisfying the obvious. And generalisations of this are true.

For suppose

$$V^\pi \models \exists x \forall y (y \in x \longleftrightarrow (\forall z)(\mathcal{P}_{\aleph_0}(z) \subseteq z \to y \in x)$$

this is

$$\exists x \forall y (y \in x \longleftrightarrow (\forall z)(\mathcal{P}_{\aleph_0}(z) \subseteq \pi``z \to y \in x))$$

Fix $\mathbf{a}$, a witness to this. We then prove that $\mathbf{a}$ with the relation $x\,R\,y$ if $x \in \pi(y)$ is a skewthingie. The way to do this is to consider

$Z = \{y \in \mathbf{a}\colon (\forall w \subseteq \mathbf{a})(y \in w \to (\exists x \in w)(\forall u \in w)(\neg(u \in \pi`x)))\}$. It is easy to check that Z is a z such that $\mathcal{P}_{\aleph_0}(z) \subseteq \pi``z$ and therefore contains everything in $\mathbf{a}$. All we have to do is verify that if $v \subseteq Z$ is finite then it is π of something in Z.

The other direction is easy. Suppose $\langle X, R\rangle$ is a skewthingie. Without loss of generality we can assume $X \cap \mathcal{P}(X)$ is empty, so that the product of transpositions

$$\prod_{x \in X} (x, \{y : \{y\}\,R\,x\})$$

is well-defined. That does it.

Suppose we have a set X and a map i that accepts small subsets of X and returns members of X. Suppose further that the relation $x\,R\,y$ iff $(\exists X' \subseteq X)(x \in X' \wedge f(X') = y)$ is wellfounded. Without loss of generality we can assume that all members of X are the size of the universe.

Then consider the product π of transpositions $(x, \langle x, i(X \cap \mathtt{snd}``x)\rangle)$ over all sets x with the property that all partitions of x are small. Notice that if x is small $\pi(x)$ isn't.

Notice that if n is a Körner number we can take X to be $\mathbb{N}$ and $i(x) = Tsup(x) + 1$.

In V^π membership restricted to sets all of whose partitions are small is wellfounded.

(Write this out)

Now is it possible to have such an X where "small" means "cannot be mapped onto V?

12.1.2 A message from Richard Kaye

If x is a transitive set in a model $\mathfrak{M}$ of ZF (say), J is an automorphism of $\mathfrak{M}$ and $f \in \mathfrak{M}$ is a bijection from $y = J(x)$ to $\mathcal{P}(x)$. Then $\{u \in \mathfrak{M} : \mathfrak{M} \models u \in x\}$ is the domain of a model of *NF*, the epsilon being $u \in_{new} v$ iff $u \in fJ(v)$. This much is standard.

The point is, since $\bigcup x \subseteq x$,

$$R = \{\langle u, v\rangle : u \in v \in x\} \subset \mathcal{P}^n{}^{\text{‘}}x$$

is a relation on the universe, actually a set (or rather, you probably want $(fJ)^{-n}(R)$ for some suitable n) and is wellfounded (but certainly won't be the new $\in$ relation). There is some minor trouble in checking that this set really is a well-founded relation in the sense of the new model, but this shouldn't be too bad, as it is certainly wf in the original. It doesn't seem to contradict anything particular, so one might think that if models of *NF* exist at all, they might arise in this way. Incidently models of NFU like this do exist. That's why it occurred to me.

I think I need the original model to satisfy rather more than KF. Foundation is obviously necessary. Perhaps this is enough. I'm not sure exactly what you've written, (i.e. what base theory is implied) so maybe you should check this point. Otherwise it sounds OK.

Best wishes, Richard

I'm pretty sure it should be $R = \{\langle\{u\}, v\rangle : u \in v \in x\}$.

Consider also the situation (which admittedly seems rather unlikely) of a transitive wellfounded set X the same size as its power set, with some bijection π. This of course gives us a model of *NF*. Now consider the fate of the set $\{\langle \iota\text{‘}x, y\rangle : x \in y \in X\}$ which is going to be a set of the new model, Y, say. Clearly the relation $\langle\{x\}, y\rangle \in Y$ is going to be wellfounded. However it doesn't give rise to a wellfounded extensional relation on the new universe because it isn't homogeneous, and so (and here we return to the metamathematical remark) it doesn't enable us to carry out Rubin's proof beco's Rubin's proof is an induction on rank not on the wellfounded relation itself. A pity, really.

However there is an old observation (i think it is in the yellow book) that if there is a definable wellfounded extensional relation on V then there is no nontrivial automorphism of $\langle V, \in\rangle$. This works even if the definable wellfounded extensional relation is not homogeneous. Therefore, if there is a Kaye model, it has an element that is moved by all automorphisms.

Wellfounded sets all over the place!

Remarks on wellfounded sets are scattered all over the place! Here is another one to go somewhere one day.

Remark 56 *We cannot prove that if $\aleph_0$ contains a wellfounded set then so does every other aleph.*

Proof:

Suppose we could prove that if $\aleph_0$ contains a wellfounded set then so does every other aleph. Then we could prove $\Box$(if $\aleph_0$ contains a wellfounded set then so does every other aleph), and therefore if $\Box(\aleph_0$ contains a wellfounded set) $\to \Box$(every aleph contains a wellfounded set). Now it is easy to arrange for a permutation model with an $x \in T^2|V|$ extending its own power set, which makes the consequent false, so the antecedent would be refutable in *NF*, which seems rather unlikely. ■

There are other observations of this kind.

We can prove that every concrete natural contains a wellfounded set. We know (because Hinnion has done it) that we can at least prove in *NF* (as opposed to *NF*+AxCount$_\leq$) that $\Diamond$(every strongly cantorian natural contains a wellfounded set). Can we prove that every strongly cantorian natural contains a wellfounded set? If *NF*+ AxCount$_\leq$ + $\neg$ AxCount is consistent then there are models of *NF* with finite noncantorian wellfounded sets

12.2 Does the universe have a wellordered partition into finite sets?

If it does, the size of the partition is the last aleph. Remember, a union of $\aleph$ finite sets cannot be of size $\geq \aleph^+$.

Suppose it does: we hope to show that the universe is wellordered. It is obvious that if the universe has a wellordered partition into finite sets then any set has a wellordered partition into finite sets. So any ordered set can be wellordered: consider a wellordered partition into finite pieces, order all the pieces uniformly and the result is a wellordering. In particular, the power set of a wellordered set is wellorderable.

So far so good. We will now use the assumption that every set has a wellordered partition into finite sets to derive a version of the order-extension principle (I hope!)

Let X be an arbitrary set, and $\leq$ a partial order on it. Let $\mathcal{X}$ be the set of partial orderings of X that refine $\leq$. $\mathcal{X}$ has a wellordered partition $\mathbb{P}$ into finite sets, and $\mathbb{P}$ is in fact the set of atoms of an atomic subalgebra $\mathbf{B}$ of $\mathcal{P}(\mathcal{X})$. Now $\mathbf{B}$ is, up to isomorphism, the power set of $\mathbb{P}$, which is wellordered, so $\mathbf{B}$ is wellordered too. The idea is that we can use the fact that $\mathbf{B}$ is wellordered to show that every filter in $\mathbf{B}$ can be extended to an ultrafilter in $\mathbf{B}$ and then rely on the fact that $\mathbf{B}$ is nearly the same as $\mathcal{P}(\mathcal{X})$ to be able to extend any filter $\subseteq \mathcal{P}(\mathcal{X})$ to an ultrafilter $\subseteq \mathcal{P}(\mathcal{X})$. Unfortunately this doesn't work.

Again, Consider the simple case where a set Y has a countable partition into pairs, and $\mathbb{R}$ is wordered. Then there is an ultrafilter on the index set ($\mathbb{N}$) but not – or not obviously – on Y. No dice.

For each pair x, $y \in X$, set $N_{\langle x, y\rangle}$ be the set of partial orders refining $\leq$ that decide whether or not $x < y$ or $y < x$. $N_{\langle x, y\rangle}$ is not in general going to be an element of **B**, but $\bigcup\{z \in \mathbb{P} : z \cap N_{\langle x,\, y\rangle} \neq \Lambda\}$ is. Let us abbreviate it to $\mathcal{N}_{\langle x,\, y\rangle}$. It is obvious that the $N_{\langle x,\, y\rangle}$ form a filter base in $\mathcal{P}(\mathcal{X})$, so it follows that the $\mathcal{N}_{\langle x,\, y\rangle}$ form a filter base in **B**. Now **B** can be wellordered, so we can extend this filter base to an ultrafilter $\mathcal{U} \subseteq \mathbf{B}$.

So this bombs out.

However, if we put a finite bound (any bound) on the size of the pieces we get the result we need. They don't even have to be disjoint. This is beco's of a result in Jaune 5 to the effect that if $|x| = |x|^2$ and x is a union of a wellordered family of n-tuples then x can be wellordered. In fact a trivial reworking of the proof in Jaune 5 allows us to weaken the hypothesis to $|x| \geq_* |x|^2$. If there is no finite bound on the size of the tuples it doesn't seem to work. All we get is that V is the union of countable many very funny much smaller sets.

Some random tho'rts. If V is the union of a wellordered set of finite sets then the power set of a wellordered set is wellordered. Does this show there is no last aleph and that the cofinality of Ω is uncountable? If V is indeed a union of countably many finite sets one can ask about the cardinality of the number of n-tuples. This gives us an ω-sequence of alephs, and one should think about its sup. Notice that a union of $\aleph$ finite sets has no partition of size $\aleph^+$ so one should be able to do something there ...

Thinking aloud. If V is the union of a wellordered family of finite sets then the power set of every wellordered set is wellordered. Now let α_n be the sup of those alephs that are $\beth_n$ of something. These sets get smaller so the αs form a nonincreasing sequence and must be eventually constant. (We can do this anyway but perhaps if the power set of a wellordered set is wellordered something interesting will happen)

Let n_0 be the least n such that α_m is constant for $m > n$. Then every cardinal that is $\beth_{n_0}$ of something is $\leq$ a cardinal that is $\beth_{n_0+1}$ of something.

If V is a union of a wellordered family of finite sets then we can use the fact that $V = V \times V$ to refine the partition in various ways until we reach a partition whose corresponding equivalence relation is a sort of congruence relation for `pair`, `fst` and `snd`. We can do things like this. Let $<$ be the prewellorder and $\sim$ the equivalence relation. Let P be a piece of the partition and ordain that, for $x, y \in P$, $x <' y$ iff $\{x\} \times P' <^+ \{y\} \times P'$ where P' is the first piece of the partition that can tell then apart. Of course we can do multiplication on the L too. Similarly any piece P can be prewellordered lexicographically by $<$ since every set is a pair. When we reach a fixed point we must have that, for all pieces P, `fst`“P is a single piece and $|$`fst`“$P = |P|$ – and of course `snd`“P, too, is a single piece and $|$`snd`“$P = |P|$.

The trouble is, I don't seem to be able to show that a fixed point for all these refinements must be a wellorder!!!

12.3 A theorem of Tarski's

We know from this result of Tarski that every set has more wellordered subsets than singletons. So consider the operation that sends $T|x|$ to $|\{y \subseteq x : \text{wellorderable}(y)\}|$. This behaves in various ways like exponentiation. Can we work tricks on it like we do with ordinary exponentiation? First (silly) problem: how do we notate it? Try $wexp`\alpha$. Perhaps there is some mileage to be got out of considering operations f which – like $wexp$ and ordinary exponentiation – satisfy

$$f(x+y) = f`x \times f`y$$

and suchlike. Do categorists have anything to say about this?

12.4 The Attic

This is what Andrei Bovykin calls the big sets of NF.

Developments in set theory since the 1960s have shown that large cardinal axioms (which talk about sets of high rank) can tell us things about sets of low rank. (This story is usually told as *large sets giving us information about small sets* but my take is that it is the *rank* (rather than the size) that is doing the work. Given that large sets have to have large rank it might be complained that I am arguing about nothing, but I shall press on). This matters to people beyond set theory because these sets of low rank are the sets that we use to implement mathematical objects of the kind that most mathematicians care about, and the information they give us might solve old problems about the reals and other similar small objects.

Illfounded sets are sets whose internal $\in$-structure is so complicated that they have not so much *high* rank as rank that is – in Cantor's sense – absolutely infinite. Seeing them in this light one would expect the sets of the attic to have things to tell us about the sets of low rank that implement reals etc, just as the sets of high rank do. However, things are not entirely straightforward, since there can be sets that lack rank for silly reasons: Quine atoms for example. Clearly illfounded sets *per se* do not necessarily have anything to tell us about sets of low rank. ZF + antifoundation gives us no new stratified theorems (which is to say no new facts about reals). If we want novel information about sets of low rank, or about reals, then we will have to look to illfounded sets of a kind not compatible with ZF, to wit, the sets that NF keeps in the attic. So: does the attic tell us anything about arithmetic? Well, yes: the obvious example is the proof of the axiom of infinity! That's not much use, beco's we knew that already, but – by showing that the attic *does* have things to tell us – it may be a harbinger of results of the kind we seek.

Say something about models here CO

But when these results start coming in, should we believe them? In short, do we/are-we-going-to believe that NF is consistent? Most set theorists would exhibit scepticism and caution in response to this question. There is an instructive parallel here with the early days of large cardinal axioms. The initial reaction to them was caution and scepticism: for example it is clear, reading between the lines of Keisler-Tarski, that the authors expected measurable cardinals to be proved inconsistent. Back in those days rumours of inconsistency proofs received a much more attentive and respectful hearing than they do nowadays. What has brought about the change? Man is a sense-making animal, as Quine says, and the mere fact that no inconsistency has turned up in sixty years spurs us to find explanations for this absence, and stories about cumulative hierarchies are co-opted to provide them. It is clear how a belief that the cumulative hierarchy can and should be extended as far as possible can explain the Mahlo cardinals, but measurables are another matter. One cannot altogether escape the unworthy thought that the real reason why measurables, supercompacts etc are now accepted as part of the set-theoretic zoo is simply that nobody has yet refuted them – so it seems reasonable to adopt them. To quote another American: "so convenient a thing it is to be a reasonable creature, for one can always find or make a reason for that which one has a mind to do". The moral of this *null hypothesis* is that what goes for measurables and supercompacts and the rest of them goes also for NF. In sixty years time, when NF has still not been proved inconsistent, people will accept whatever consequences NF has for wellfounded sets, just as my generation accepted that there must be nonconstructible sets of reals, because measurable cardinals say so.

It's worth asking why this hasn't happened *already*.

My guess is that it's merely that taking a universal set on board is a more radical departure than taking a measurable cardinal on board, or at least is generally felt to be.

Summary:

(i) Most of the mathematical entities that people care about can be implemented in a theory of sets of low rank;

(ii) theories of sets of high rank tell us important things about the sets of low rank that perform the implementations;

(iii) illfounded sets are like sets of high rank only more so, so they might tell us yet more about sets of low rank; the illfounded sets we can find in models of ZF-minus-foundation don't tell us anything new, but

(iv) the sets we find in the attic of NF just might. Certainly worth a rummage.

There is a temptation to think that wellfounded sets and illfounded sets are such different kinds of chap that there is an interpolation-lemma argument to show that facts about the second cannot tell you anything about the first. However, a close inspection reveals no lemma corresponding to the intuition.

NF knows about certain structures (Specker trees like $\mathcal{T}|V|)$) which can be seen from outside to be illfounded, but which it can prove to be wellfounded. Thus any model of NF contains structures which it steadfastly believes to be wellfounded (and therefore to have a rank) but which the outside world knows to be illfounded. This means that the more the model knows about the world outside it, the bigger it believes those ranks to be. This is a source of large ordinals. (Might it be that all the information we get about sets of low rank from the attic is channeled through large ordinals in this way?)

Assumptions about natural numbers tell us things about the attic: AxCount$_{\leq}$ implies that $\rho(\mathcal{T}(|V|)) > \omega$, for example. But i don't think that's what people mean. Here are three ways in which we can use cardinal trees to extract information from the attic.

- Assume the axiom of counting. Then there are lots of cardinals (whose Specker trees are) of infinite rank. Observe that a tree (whose top element is) of rank λ (where λ is limit) has nodes of all ranks below λ, so there are lots of (cardinal) trees of rank ω. If you are a node of rank ω then the set of ranks of your children is an unbounded subset of $\mathbb{N}$, which is to say (in some sense) a real – definable with a single parameter. Similarly if you are node of rank $\omega+\omega$ you have children of rank $\omega+n$ for arbitrarily large n. Below each of these children is a node of rank ω and of course a real as before. So every cardinal of rank $\omega+\omega$ gives us a set of reals – again, definable with a single parameter. Since counting (or even AxCount$_{\leq}$) tells us that there are lots of such cardinals inside $\mathcal{T}|V|$ we have sets of reals definable with parameters *from the attic*.

- Let κ be any cardinal of infinite rank. Recall that $\mathcal{T}(\kappa)\restriction_{NO}\beta$ is the tree consisting of those elements of $\mathcal{T}(\kappa)$ that are of rank at least β. All these trees are wellfounded, and therefore support games. So to any $\beta < \rho(\mathcal{T}(\kappa))$ we can associate `I` or `II` depending on who has a winning strategy in the game over $\mathcal{T}(\kappa)\restriction_{NO}\beta$. Thus κ comes to define a subset of the ordinals below $\rho(\mathcal{T}(\kappa))$.

- Every cardinal not in SM corresponds to an ω-sequence of ordinals, as follows. $\alpha \mapsto (\lambda n \in \mathbb{N})(\rho(\beth_n(\alpha)))$. But there are other tricks we can do. $\mathcal{T}\alpha$ is a wellfounded tree and gives rise to a determinate game. ("pick a logarithm-to-base-2 and lose if you can't!"). For ordinals below $\rho(\alpha)$ we can do the following recursive construction. $[\mathcal{T}\alpha]_0 := \mathcal{T}\alpha$; thereafter remove endpoints at successor stages and take intersections at limits. Each tree $[\mathcal{T}\alpha]_\zeta$ is either a Win for `I` or for `II`, so α gives us a sequence of length $\rho(\alpha)$ of `I`'s and `II`'s.

  There is a relation between the sequence for α and that for 2^α. If we let $((\alpha,\zeta))$ be `I` or `II` depending on where the result of removing from $\mathcal{T}\alpha$ all cardinals of rank less than ζ is a win for `I` or for `II`, then $((\alpha,\zeta)) = \mathtt{II} \rightarrow ((2^\alpha,\zeta)) = \mathtt{I}$.

  In general, how much information about a tree can one code by this sequence of `I`'s and `II`'s?

- But there is yet more we can do. The extensional quotient of $\mathcal{T}(\kappa)$ is a $BFEXT$, a wellfounded set picture. If κ is a cardinal of infinite rank this BFEXT is of infinite rank, since the rank of the extensional quotient

> is the same as the rank of the original tree. Now assume AxCount$_{\leq}$ or something of that nature, in order to ensure that $\rho(\mathcal{T}(|V|))$ is infinite. Then there will be cardinals in $\mathcal{T}(|V|)$ of infinite strongly cantorian rank, and their extensional quotients will be of strongly cantorian rank. We have to do a little bit of work to ensure that their carrier sets are likewise strongly cantorian. (We can show that any $BFEXT$ of rank ω has a countable carrier set and is therefore strongly cantorian. It'll be harder in general but even the rank ω case serves to make the point.) Once we have established that, Rieger-Bernays permutation constructions will then give us actual wellfounded sets isomorphic to these set pictures ($BFEXTS$). And these wellfounded sets are defined using parameters from the attic.

For the last item to give us wellfounded sets of large transfinite rank with attic provenance we will need the following

Lemma 15 *Every $BFEXT$ of strongly cantorian rank has strongly cantorian carrier set.*

Proof: All in good time! ■

Of course there is no reason to suppose that sets definable with attic-parameters in this way cannot be defined in other ways, but equally there is no reason to suppose that they can.

12.5 NCI finite

Is $|V|$ indecomposible under these circs?

Well, the set of decomposible cardinals is closed under $+$, as follows

Suppose $a+b$ and $c+d$ are decomposible. Then $(a+b)+(c+d)$ is decomposible. If it weren't we would have

$$(a+b)+(c+d) = (a+b) \vee (a+b)+(c+d) = (c+d)$$

but neither disjunct can be true, since each asserts the identity of a decomposible cardinal to an indecomposible cardinal.

So if NCI is finite there is a largest decomposible cardinal. Call it δ. Then no γ can be incomparable with δ, lest $\delta+\gamma$ be a larger decomposible cardinal. Nothing to say that $\delta \neq |V|$ of course.

> Randall (22/xii/24) points out that in his models $|V|$ is decomposible: "A litter and its complement both have cardinalities distinct from that of the universe." he sez.

Suppose $|V|$ is the sum of two smaller cardinals α and β, both indecomposible. If $|V|$ is decomposible and NCI is finite then $|V|$ is a sum of two cardinals in this way.

Suppose we have split V into two smaller pieces A and B. Notice (and this is standard but it will matter) that neither A nor B injects into the other. If A injected into B then we would have

$$2 \cdot |V| \quad = \quad |V| \quad = \quad |A|+|B| \quad \leq \quad 2 \cdot |B| \quad \leq \quad 2 \cdot |V|$$

giving $2 \cdot |B| = 2 \cdot |V|$ which (since we can divide by 2) would give $|B| = |V|$.

In fact it is standard that in these circumstances (by appeal to Bernstein's Lemma) we have

$$\alpha \not\leq \beta \not\leq \alpha \geq^* |V| \leq^* \beta$$

In what follows we will be appealing to this without ffurther explanation.

It is a consequence of Bowler-Forster that if $|V|$ is decomposible then the permutations that move fewer than $|V|$ things generate the full symmetric group on V. Let G be the group generated by $\{\pi : |\{x : \pi(x) \neq x\}| < |V|\}$, the set of permutations of small support.

Remark 57 *If $|V|$ is decomposible then G is the whole of Symm(V).*

Proof:

Bowler-Forster [**?**] show that every permutation in Symm(V) is a product of involutions, so it will suffice to show that every involution is in G.

Let π be an involution. We partition π (thought of as a set of transpositions) into three pieces. One piece is a permutation that swaps things in A; another piece is a permutation that swaps things in B. Finally there is a piece that contains those pairs that swap something in A with something in B. Now we know from the foregoing that $|A|$ and $|B|$ are incomparable, so this third piece – which encodes a bijection between a subset of A and a subset of B – must be smaller than either ... and this makes it therefore a permutation of small support. We have now expressed π as the product of three disjoint involutions, all of them permutations of small support and therefore in G.

■

Remark 58
Suppose $|V| = \alpha + \beta$ with $\alpha, \beta < |V|$, and let $\mathcal{A} = \{A : |A| = \alpha \ \wedge \ |V \setminus A| = \beta\}$ Suppose further that α is indecomposible and that $\beta = 2 \cdot \beta$.

Then $\mathcal{A}$ is closed under binary intersections.

(This is ridiculously strong)

Proof:

V is the union of disjoint A and B with $|A| = \alpha$ and $|B| = \beta$. Let π be a permutation; then V is also the union of π“A and π“B, disjoint. Call them A' and B'. We want to show that the set $\mathcal{A}$ of such A' is closed under binary $\cap$.

Let's first try the intersection of two of them, to start the ball rolling.

$A = (A \cap A') \cup (A \setminus A')$. $|A'| = \alpha$ is indecomposible, whence

$$|A \cap A'| = \alpha \ \vee \ |A \setminus A'| = \alpha.$$

Now $|A \setminus A'| = \alpha$ is not possible beco's $(A \setminus A') \subseteq B'$ and

$$|B'| = \beta \not\geq \alpha = |A \setminus A'|$$

gives a contradiction. So we must have $|A \cap A'| = \alpha$ as desired.

For $(A \cap A') \in \mathcal{A}$ we also need $V \setminus (A \cap A')$ to be of size β. Now $V \setminus (A \cap A') = (B \cup B')$ so we want $|B \cup B'| = \beta$. Now clearly $|B \cup B'| \leq \beta + \beta$ so the assumption $\beta = 2 \cdot \beta$ will do nicely.

■

Now we need to think about getting members of $\mathcal{A}$ with special properties. Suppose $A \in \mathcal{A}$ and π is any involution. We will show how to obtain a subset of A that is an $\mathcal{A}$-set that is a union of π-cycles.

$A = \{a \in A : \pi(a) \in A\} \cup \{a \in A : \pi(a) \notin A\}$. Since $|A|$ is indecomposible, one of these two sets must be of size $|A|$. It cannot be $\{a \in A : \pi(a) \notin A\}$ beco's π injects $\{a \in A : \pi(a) \notin A\}$ into B and we would have $|A| \leq |B|$ which is *strengstens verboten*. So A has a $\mathcal{A}$-flavoured subset which is closed under π.

We can generalise this. Suppose $A \in \mathcal{A}$ and σ is a permutation of order n, say. We split A into
$\{a \in A : \sigma(a) \notin A\}$;
$\{a \in A : \sigma(a) \in A \wedge \sigma^2(a) \notin A\}$;
$\{a \in A : \sigma(a) \in A \wedge \sigma^2(a) \in A \wedge \sigma^3(a) \notin A\}$;
$\vdots$
$\{a \in A : \bigwedge_{i<n} \sigma^i(a) \in A\}$

There are n of these sets, and by indecomposibility precisely one of them must be of size $|A|$. Clearly that one must be the last, namely $\{a \in A : \bigwedge_{i<n} \sigma^i(a) \in A\}$, so $\{a \in A : \bigwedge_{i<n} \sigma^i(a) \in A\}$ is an $\mathcal{A}$-set that is a union of σ-cycles.

Admittedly this works only for permutations of finite order, but it's fun anyway.
What happens if σ is of infinite order? We consider the partition of A into the two pieces $\{a \in A : \sigma(a) \in A\}$ and $\{a \in A : \sigma(a) \notin A\}$. As before, we cannot have $\{a \in A : \sigma(a) \in A\}$ of size $|A|$ since σ injects it into $V \setminus A$, so we have $|\{a \in A : \sigma(a) \in A\}| = |A|$. However that doesn't seem to be enuff to get it to be closed under σ. Perhaps i need to try a little harder

Now the property of having-a-partition-into-pairs is 2-stratifiable, so if x has it so does $\pi``x$ for any permutation $\pi$. Observe that if $A$ and $A'$ are two members of $\mathcal{A}$ then there is a permutation $\pi$ s.t. $\pi``A = A'$. This is for the trivial reason that if $|x| = |y|$ and $|V \setminus x| = |V \setminus y|$ then there is a permutation π with $\pi``x = y$. So i can copy partitions over from any $\mathcal{A}$-set to any other $\mathcal{A}$-set. So, for every n, any $\mathcal{A}$-set has a partition into n-tuples. In fact this construction gives us a wee bit more, since the n-cycles can come equipped with a circular ordering, so the partition can be thought of as a permutation of degree n.

Consider the function $|x| \mapsto |\iota``\bigcup x|$. Well, it's not single-valued but if NCI is finite then there is a distinguished value, namely the sum of all its values (I don't think the inf works.) This is monotone decreasing and so has a fixed point. What can we say about the greatest fixed point? Perhaps there are possibilities.

Well, here's one. Start with $|V|$. At each stage you have a cardinal. If you can find x in that cardinal such that $|\iota``\bigcup x| < |x|$ pick that $|\iota``\bigcup x|$ to be your next cardinal. You can't pick an infinite descending chain so you must reach a fixed point.

One thing i have never properly investigated in this context – in all these years – is the lattice of equivalence classes of sets under the relation "x and y map onto each other". The quotient is a poset, with the obvious partial ordering $\leq^*$ where $[x] \leq^* [y]$ iff $(\exists f)(f : y \twoheadrightarrow x)$. The quotient is actually a lattice, and it is probably worth spelling out the details.

If x and y both map onto z, then they both map onto $x \sqcup z$. So, since NCI is finite, we can obtain the glb of $[x]$ and $[y]$ just by forming a finite disjoint union. So it looks as if the glb in this lattice is the same as in the cardinal lattice. What is the lub? It might be smaller than $|x| = |y|$ of course ($\leq^*$ contains more ordered pairs) but we can take the glb of all the upper bounds for $[x]$ and $[y]$.

Fix a cardinal a. Think about the set of all cardinals b s.t $a =^* b$ – *i.e.*, any two things of size a and b map onto each other. It's closed under $+$; is it closed under $\wedge$? It would be nice, but i can't see how to prove it. But actually it's fairly easy to see that it won't be. Think about the cardinals of sets that unions of countably many finite sets. One can easily imagine how two minimal uncountable ones could map onto each toher, but their glb will be $\aleph_0$.

Consider the cardinal ideal I of those cardinals that have only finitely many infinite cardinals below them. Clearly closed under $+$; i don't see any reason why it should be closed under $\cdot$. It doesn't contain any Dedekind cardinals. Does this ideal I have a top element? (it certainly can't have more than one maximal element). If τ is a top element then $\tau \cdot n = \tau$ for any $n \in \mathbb{N}$. Notice that if I has a maximal finite antichain then it will have a top element. Furthermore, if it has a maximal finite antichain then we can rerun inside it the old proof of mine that $n = 2n$ if NCI is finite.

What if it has an infinite antichain? Let's recall the Minimal Bad Sequence construction, taking care not to use DC. Must there be a minimal α that is the first member of an infinite antichain? Certainly! – because if there are none then there are infinitely many below any one. So there are minimal bad finite sequences of arbitrary length.

"There are only finitely many cardinals below α" sounds like the thing one ought to be able to prove by induction on $<_c$ but of course one can't. However if there are any counterexamples they are all $<_c$-minimal. OK, so the set of them forms an antichain in NC. Is this antichain finite? Possibly – it might even be empty. The problem is that there is no reason to suppose it is a maximal antichain.

Let α and β be two such minimal cardinals. Think about the things below them. They are all in I so are idemmultiple and what with one thing and another they will have binary infima.

Remark. $(\forall \alpha \in I)(\alpha = 2\alpha \to (\forall \beta < \alpha)(\beta = 2\beta))$

Proof:

Sse $\alpha = 2\alpha$, and $\beta < \alpha$. Then, for all $n \in \mathbb{N}$, $n \cdot \beta \leq n \cdot \alpha \leq \alpha$. So $\{n \cdot \beta : n \in \mathbb{N}\}$ is a set of cardinals below α and must be finite. But, but Truss-Sierpinski-Tarski, this entails $\beta = 2\beta$. ■

A converse would be nice, but i haven't found a proof so far. Suppose $\alpha < 2\alpha$ but $\beta = 2\beta$ for all $\beta < \alpha$. α cannot be a sum of smaller cardinals since any such sum/product β satisfies $\beta = 2\beta$. Nor can it be bounded above by such a sum or product, by the preceding result. If there are n cardinals $\beta_1 \cdots \beta_n$ below α then their sum is going to be below $n\alpha$. Suppose now we have n disjoint sets $A_i : 1 \leq i \leq n$, each of size α, and each copy A_i has a subset coloured in that is of size β_i. But observe that α is amorphous! There is plenty of uncoloured space in each copy – α-much of it in fact – enough for all the coloured bits to be moved into one copy. So there is a unique maximal $\beta < \alpha$.

However i see no reason why this should lead anywhere. α could be the cardinality of the socks.

All this looks like fun, but it doesn't really amount to a whole hell of a lot.

My abortive proof of the infinitude of NCI is an interesting cautionary tale. Assume NCI finite. Then the following good things happen: $n = 2n$ holds for all infinite cardinals, and NCI itself is a finite poset which is actually a distributive lattice - sups and infs exist - and sup is simply $+$. Since NCI is infinite every sequence $a, a^2, a^3, a^4 ...$ is eventually constant, so call this eventually constant value a^∞. Consider the map $a \mapsto a^\infty$. This is a lattice homomorphism, so the image is also a distributive lattice. The image of course is precisely the set of infinite a such that $a = a^2$. This set is of course a subset of the original lattice ... but it's not a sublattice! The info operation is honest but the sup isn't! Two idempotents $a = a^2$ and $b = b^2$ have a sup in the original lattice (it's just $a + b$) and they have a sup in the quotient lattice - but its $a \cdot b$ not $a + b$!

So i don't get a proof that NCI is infinite. However it does give a slightly different take on why $(\forall a)(a = a^2)$ implies AC. If $(\forall a)(a = a^2)$ then consider a, b and $a + b$ and use Bernstein's lemma. It will tell you that a and b are *-comparable.

But i'll try again....

What other homomorphisms are there? Send α to the largest cardinal that is the size of a union of $T\alpha$-many finite sets. This is idempotent.

12.5.1 How many socks?

Let S be a union of countably many pairs, and assume $|S| = |S| + |S|$. (This last happens automatically if NCI is finite.)

We have two functions π_L and π_R: $S \hookrightarrow S$ where $\pi_L``S \cup \pi_R``S = S$ and $\pi_L``S \cap \pi_R``S = \emptyset$. Thus every sock s can be thought of as the ordered pair $\langle \pi_L(s), \pi_R(s) \rangle$. (Not every ordered pair of socks is a sock, tho').

There is a quasi-order on the socks, beco's the socks come in countably many pairs. We want to refine this quasiorder into a total order. What do we do with the pair $\{s_1, s_2\}$? We exploit the fact that we can extend the quasiorder to ordered pairs of socks and ask which of $\langle \pi_L(s_1), \pi_R(s_1) \rangle$ and $\langle \pi_L(s_2), \pi_R(s_2) \rangle$ comes first. We iterate until we reach a fixed point. Is this fixed point antisymmetric? Suppose we have been unable to split the pair $\{a, b\}$, and let us suppose it is the first one we cannot split. This must mean there are two pairs $\{u, v\}$ and $\{x, y\}$ with $a = \langle u, x \rangle$ and $b = \langle v, y \rangle$. Our pair $\{a, b\}$ now represents a bijection between these two pairs. It does not give us a choice from either of them, but it has reduced the task of choosing from two pairs to a task of choosing only from one. Now we look at the second unsplit pair, and so on, getting more and more bijections between pairs. Notice that we don't have to worry about the possibility of a being the pair $\langle x, x\rangle$ and b being the pair $\langle y, y\rangle$ (in which case the pair $\{x, y\}$ would have contained no information(*)) beco's the set of first components is π_L"S and the set of second components is π_R"S and these two are disjoint. Nor do we have to worry that a might be $\langle x, y\rangle$ and b be $\langle y, x\rangle$ beco's nothing can be both a first component and a second component.

The idea is that eventually we will build a family of commuting bijections, so with one choice from the first pair we will be able to wellorder the whole of S. The major problem with this is that since every sock is a component of precisely one ordered pair, no pair of socks lands in the range of more than one bijection! It may be that with a bit more work we can get round this, perhaps by using more than one pair of mappings, so that we can prove: Sse $|S| + |S| = |S|$ and S is a union of countably many pairs, then S is countable. (This would presumably also prove that if $|S| \cdot n = |S|$ and S is a union of countably many unordered n-tuples, then S is countable. That would be nice!!)

But for now let's assume not only that every sock is an ordered pair of socks but that every ordered pair of socks is another sock, in other words $|S| = |S|\times|S|$. What now? This time we can use pairing "in the other direction" as well. If we want to separate a from b we can compare $\{a\} \times S$ and $\{b\} \times S$ lexicographically.

Now think of the first unsplit pair, which is $\{a, b\}$, and let $\{x, y\}$ be any other unsplit pair. Think about the four ordered pairs in $\{a, b\} \times \{x, y\}$. They can't belong to a quadruple co's there are no quadruples, and they must come in two pairs $\{\langle a, x\rangle, \langle b, y\rangle\}$ and $\{\langle a, y\rangle, \langle b, x\rangle\}$ (without loss of generality) and **one of these pairs comes first**! This pair is simply the graph of a bijection between $\{a, b\}$ and $\{x, y\}$. That way we have reduced the problem of choosing from $\{x, y\}$ to the problem of choosing from $\{a, b\}$.

Pretty, isn't it?! Now how about things that come in bundles larger than two? Let S be a union of countably many unordered k-tuples, and do the same. This time we reason not about the first surviving pair, but the first surviving j-bundle, where j is the maximal size of surviving bundles. Let A be the first surviving j-bundle and let B an arbitrary other j-bundle. $A \times B$ must be split into j-bundles. None of the bundles can be i-bundles with $i < j$ beco's we would have been able to use that to split A or B. In each bundle $\subseteq A \times B$ each member of A must be the first component of precisely one ordered pair and each member of B must be the second component of precisely one ordered pair. In other words, each bundles is the graph of a bijection – as in the case of the socks.

So we can match up all the j-tuples in such a way that one single choice suffices to order them all. Then we work on the next size down. So only finitely many choices needed. This is the correct proof of the allegation in the yellow book: the proof there is fallacious.

Smuggle in the expression 'indiscrete category' here

Can we do the same if S is a union of countably many finite sets without any bound on the size of the finite sets?

This time let's not assume that every ordered pair of socks is a sock, but that every ordered pair of distinct socks is a sock, and that every sock is an ordered pair of distinct socks. (This addresses the concerns above at *) This time there may well be no maximal size of surviving bundles, so we cannot use the useful boldface trick of last time to get bijections – though we might sometimes be lucky and get bijections or at least constraints on bijections: if $A \times B$ gets split we get a constraint on a bijection: the earliest bundle to be included in it represents a constraint. Also, a bijection or constraint-on-a-bijection between A and A', together with a bijection or constraint-on-a-bijection between B and B' will lift to a bijection or constraint-on-a-bijection between $A \times B$ and $A' \times B'$.

This time we look at surviving bundles of *minimal* size. Just as in the original development, with π_L and π_R we can say that a j-bundle can only be a bijection between two j-bundles. Now it becomes clear that it could really be worth trying very hard to show that in that development there really is enuff info to obtain bijections between all the pairs, because if it works there, it might work here. If it did, we could reduce the problem of splitting all j-bundles to the problem of splitting one. Then we use the fact that bijections and constraints on bijections can be lifted to cartesian products and hope that we can then attack larger bundles.

I am deeply pessimistic about this. Even supposing that we can exploit the fact that everything is an ordered pair to build up bijections between all surviving j-bundles, where j is minimal, and that we can (well, we obviously can) use this to build up bijections between cartesian products, i don't see any reason why there shouldn't be infinitely many surviving bundles of every size. For each p, we might be able to build bijections between all the p-bundles, but they don't interfere helpfully at all.

So the best we can hope is that we hang onto the finite bound in the assumption, and weaken the assumption to $|S| = |S| \cdot n$.

March 2009: i now think that this method will show that if $|S| = |S|^2$ and S has a totally ordered partition into pieces of bounded finite size then S is totally ordered.

THEOREM 22 *If NCI is finite, $\langle NCI, \leq \rangle$ is a complete distributive lattice, and $a \vee b = a + b$.*

Proof: Observe that if $a \leq c$ and $b \leq c$ then $a + b \leq c + c = c$, so $a + b$ really is $a \vee b$. This makes $\langle NCI, \leq \rangle$ into a complete poset, so $a \wedge b$ is defined. All that remains to be shown is distributivity.

It is clear that $c \wedge a + c \wedge b \leq c \wedge (a + b)$. A set that is a union of a piece that embeds into both C and A, and a piece that embeds into both C and B embeds into both $C \sqcup C$ (which is C) and into $A \sqcup B$.

For the other direction – $c \wedge (a + b) \ \leq \ c \wedge a + c \wedge b$ – we reason as follows. Consider subsets of $A \sqcup B$ of size $\leq c$. Such a subset $D \subseteq A \sqcup B$ comes in two parts: $D \cap A$ and $D \cap B$, and thereby defines two cardinals: $|D \cap A|$ and $|D \cap B|$. There are only finitely many such pairs of cardinals so for each such pair pick a D and ensure that they are all disjoint. Then take the union of all the $D \cap A$.

It will be of size $c \wedge a$. And the union of all the $D \cap N$ will be of size $c \wedge b$. But then the union of all the D will clearly be of size $(c \wedge a) \vee (c \wedge b)$. But the union of all the D is obviously the largest things thing that can be embedded in both C and $A \sqcup B$, and is therefore of size $c \wedge (a \vee b)$.

■

I suspect there are general reasons why NC should be a distributive lattice if it is a lattice at all, but in this case we can exploit $a = 2 \cdot a$.

Now that we know that $\langle NCI, \leq \rangle$ is a complete distributive lattice consider the function $f : NCI \to NCI$ defined by $f(a) = \bigvee\{b : b \not\geq a\}$. If $a \leq a'$ then $\{b : b \not\geq a\} \subseteq \{b : b \not\geq a'\}$ whence $f(a) = \bigvee\{b : b \not\geq a\} \leq \bigvee\{b : b \not\geq a'\} = f(a')$. So $a \leq a' \to f(a) \leq f(a')$. (Can we have $a \leq f(a)$? I don't see why not...)

Now start with an arbitrary cardinal a and consider $\langle f^n(a) : n \in \mathbb{N} \rangle$. This sequence can take only finitely many values, so it must repeat. Any loop must be an antichain, because of the monotonicity. Suppose it is $\{a, f(a), f^2(a) \ldots f^{(n-1)}(a), f^n(a) = a\}$ with $n > 2$.

But then $(f(a) + f(f(a)) + \ldots f^n(a))$ is a sum of things $\not\geq a$ and so must be $\leq f(a)$, so $f(f(a)) \leq f(a)$ contradicting the fact that we have an antichain.

Thus the antichain must be of width 2 at most. It could be a singleton.

Now, it doesn't have to be an antichain. It could be a chain ending at a fixed point!

Suppose $a = f(b) \ \wedge \ b = f(a)$ is such an antichain. What happens above a and b? Suppose $c > a$. If $c \not\geq b$ we have $c \leq f(b) = a$ so we must have $c \geq b$. Thus $c > a \to c \geq b$ and $c > b \to c \geq a$ analogously. So everything above either a or b must be above $a \vee b$ which is therefore a **pinchpoint**. It could be $|V|$ of course...

How about analogously defining $g(a)$ to be $\bigwedge\{b : b \not\leq a\}$?

Let α be a cardinal with a unique successor α^+. That is to say, anything $> \alpha$ is $\geq \alpha^+$. Now suppose there are cardinals incomparable with α. This makes the following definition sensible. Set

$$\alpha^- = \bigwedge\{\beta : \alpha \not\leq \beta\}.$$

By distributivity (after all, $\langle NCI, \leq \rangle$ is a complete distributive lattice)

$$\alpha \vee \bigwedge\{\beta : \alpha \not\leq \beta \not\leq \alpha\} = \bigwedge\{\alpha \vee \beta : \alpha \not\leq \beta \not\leq \alpha\}$$

This cardinal must be $\geq \alpha^+$ since it is an inf of things all $> \alpha$. But if $\alpha \vee$ splat is bigger than α, splat must be bigger than α or at least incomparable with it. It can't be bigger than α (it is the inf of thing incomparable with it) so it must be incomparable with it. So α^- is incomparable with α.

This proves that if α has a unique successor, and is not a pinch-point, there is a unique minimal thing incomparable with it.

(Let's try the dual of this. Suppose as before that α is a cardinal with a unique predecessor α^-. That is to say, anything incomparable with α is $\leq \alpha^+$. Now suppose there are cardinals incomparable with α. This makes the following definition sensible. This time

$$\alpha^+ = \bigvee\{\beta : \alpha \not\leq \beta\}.$$

By distributivity

$$\alpha \wedge \bigvee\{\beta : \alpha \not\leq \beta\} = \bigvee\{\alpha \wedge \beta : \alpha \not\leq \beta \not\leq \alpha\}$$

etc)

12.5.2 The Oberwolfach Cardinal

At the Oberwolfach meeting in 1987 John Truss and I had a look at the old question of whether or not NCI can be proved to be infinite and we briefly thought we had proved it. If NCI is finite there is a $*$-unique $*$-maximal cardinal α s.t $\alpha^2 \not\leq_* \alpha$.

Assume NCI finite ... now read on ...

Suppose α is *-maximal so that $\alpha^2 \not\leq_* \alpha$. We will show that it is *-unique. (We may also have to consider a $\leq$-maximal version.)

Suppose α and β are both $\leq_*$-maximal with this property, and are *-incomparable, so α, $\beta, < \alpha+\beta$. Therefore, by *-maximality, $(\alpha+\beta)^2 \leq_* (\alpha+\beta)$. Therefore, by Bernstein's lemma, α and β are *-comparable.

Therefore, if α is *-maximal so that $\alpha^2 \not\leq_* \alpha$ then it is unique with this property: it is the *-maximum α such that $\alpha^2 \not\leq_* \alpha$.

Now let β be any cardinal such that $\beta \not\leq_* \alpha$. Then $\alpha <_* \beta + \alpha$. Therefore, by maximality of α, we have $(\beta+\alpha)^2 \leq_* \beta+\alpha$ and we invoke Bernstein's lemma again to infer $\alpha \leq_* \beta$. So α is a *-pinch-point: $(\forall\beta)(\beta \leq^* \alpha \vee \alpha \leq^* \beta)$.

Notice that this has an an immediate corollorary that $\alpha >^*$ every aleph. It's *-comparable with every alpeh but cannot be an aleph itself, so it maps onto every aleph.

Next we show that α^2 is a *-successor of α. Suppose b and c are two cardinals $>^* \alpha$. We must have $(b + c)^2 \leq^* b + c$ so by Bernstein's lemma b and c are $*$-comparable. (this fact is worth noting on its own account!)

Next suppose $\alpha <^* \beta <^* \alpha^2$. This gives $\alpha^2 \leq^* \beta^2 \leq^* \beta \leq \alpha^2$. So β and α^2 are *-equivalent, whence α is *-adjacent to α^2.

Suppose α is a maximal lower bound (wrt $\leq^*$) for two cardinals $b \neq c$. By maximality of α we have $b^2 \leq^* b$ and $c^2 \leq^* c$. We also have $\alpha^2 \leq^* b^2$ and $\alpha^2 \leq^* c^2$. So $\alpha^2 \leq^* b$ and $\alpha^2 \leq^* c$. So α^2 is also a $\leq^*$-lower bound for $\{b, c\}$, and $\alpha <^* \alpha^2$ contradicting the assumption that α was a $\leq^*$-maximal lower bound for $\{b, c\}$.

So $(\forall b, c >^* \alpha)((\alpha <^* \alpha^2 \leq^* b, c) \wedge (b \leq^* c \vee c \leq^* b))$

Let's try to get a contradiction

Definition 16 *Define f on NCI by $f`\alpha =_{df} \ \Sigma\{\beta : 2^\beta \leq \alpha\}$.*

Evidently $f`\alpha \leq \alpha$. We want $f`\alpha < \alpha$ to make f pressing-down and interesting. In fact we can prove something stronger.

Lemma 16 $f(\alpha) \leq \alpha \not\leq_* f`\alpha$.

Proof: Let A_α be $\{\beta : 2^\beta \leq \alpha\}$. With α free we will show by induction on n that no subset of A_α with n members has a supremum $\geq \alpha$.

When $n = 1$ this is trivial.

Suppose it proved for $n = k$ and let $X \cup \{\beta\}$ be a $k+1$-membered subset of A_α whose supremum is $\geq_* \alpha$. Let χ be ΣX. Suppose *per impossibile* that $\chi + \beta \geq_* \alpha$. Then

$$\chi + \beta \geq_* \alpha \geq 2^\beta = 2^{\beta+\beta} = 2^\beta \cdot 2^\beta$$

Now use Bernstein's lemma:

$$\chi \geq_* 2^\beta \vee \beta \geq_* 2^\beta$$

so

$$\chi \geq_* 2^\beta$$

whence $\chi \geq \beta$ and $\chi + \beta \leq_* \chi + \chi = \chi$. But $\chi \not\geq_* \alpha$ by induction hypothesis. ■

We note that $f`\alpha$ is defined as long as $\alpha \geq 2^{\aleph_0}$.

We will eventually obtain a contradiction by considering f-chains. Let $F(\alpha, n, \beta)$ say that $\beta = f^n(\alpha)$. By induction on 'n' we have $(\forall\alpha\beta)(F(\alpha, n, \beta) \longleftrightarrow F(T\alpha, Tn, T\beta))$ as long as $\alpha, \beta \leq T|V|$.

Because NCI is finite and f is pressing-down, every f-chain is finite. Let $G(\alpha)$ be the largest n such that $(\exists\beta)(F(\alpha, n, \beta)$. We must check that $G(T\alpha) = TG(\alpha)$. Consider the β such that $F(\alpha, G(\alpha), \beta)$. We have $F(T\alpha, TG(\alpha), T\beta)$. Now $\beta \not\geq 2^{\aleph_0}$ (since $f(\beta)$ is not defined) and the continuum is cantorian so $f(T\beta)$ is not defined either. So if we do f $TG(\alpha)$ times to $T\alpha$ we obtain something we cannot do f to. So $G(T\alpha) = TG(\alpha)$.

To obtain a contradiction it will suffice to show that $G(T|V|) = G(T^2|V|)+1$. That might not be possible. We note that $T|V| > f(T|V|) \geq T^2|V|$. (Draw a ladder.)

Now attempt to build a bijection, leaving out $T|V|$ to get the parity argument. Pair $f(T|V|)$ with $T^2|V|$ and, once you've paired x with y, pair $f(x)$ with $f(y)$. That is to say, we endeavour to pair off $f^{n+1}(T|V|)$ with $f^n(T^2|V|)$. Since we know $f^{n+1}(T|V|) \geq f^n(T^2|V|)$ this process can come adrift (the two arms of the ladder run out at different times) only if we reach an n such that $f^{n+1}(T|V|)$ is big enuff to feed to f but $f^n(T^2|V|)$ isn't. But if $f^{n+1}(T|V|) \geq 2^{\aleph_0}$ then also $T(f^{n+1}(T|V|)) \geq 2^{\aleph_0}$.

All this shows is that this n isn't cantorian. Bugger.

A bit of fun

Assume NCI finite as usual. Let $A_n := \{\alpha \in NC : \aleph_n \leq \alpha \not\geq \aleph_{n+1}\}$. Since NCI is a distributive lattice we can show that each A_n is a sublattice, with a top element and a bottom element and is closed under $\times$.

We can do something clever by exploiting theorem **??** to show that the map $\alpha \mapsto \alpha + \aleph_{n+1} : A_n \to A_{n+1}$ must be an *injection*. What about a map coming down? consider $\alpha \mapsto \bigvee\{\beta \in A_n : \beta \leq \alpha\}$. I think this is a right-inverse to the last map. (miniexercise: check this) It wouldn't be onto by any chance would it? No reason to suppose that. But at least we show that NCI is the union of a family of finite distributive lattices, with a sequence of retracts....

leftovers

Now suppose $\alpha^2 = \alpha$. Then $2^{f`\alpha} \leq \alpha$

$$2^{f`\alpha} = \prod_{2^\beta \leq \alpha} 2^\beta$$

so $2^{f`\alpha}$ is a product of things $\leq \alpha$ and so is $\leq \alpha^n$ which is α

We ought to be able to prove something like this. Let α be a cardinal of infinite rank. Let $[\alpha]_0$ be $\{\alpha\}$ and let $[\alpha]_{n+1}$ be $\{\beta : 2^\beta \in [\alpha]_n\}$. Let $\oplus_0$ be $+$ and $\kappa \oplus_{n+1} \mu =_{df} \; 2^{(\gamma \oplus_n \zeta)}$ where $\kappa = 2^\gamma$ and $\mu = 2^\zeta$. It would be nice to show by induction on n that

$\rho(\alpha) \geq n+4 \to (\forall k \leq n)([\alpha]_k$ is closed under $\oplus_{n-k})$

Unfortunately this doesn't seem to work. Suppose $2^{\aleph_{17}} = \aleph_{\omega+1}$, $2^{\aleph_\omega} = \aleph_{\omega+3}$. $\aleph_\omega^{\aleph_{17}} > \aleph_\omega$ *to be continued*

12.5.3 α of infinite rank or $2^{T\alpha} \leq \alpha$

Does $2^{T\alpha} \leq \alpha$ have the same consequences for α (not being an aleph, for example) as $2^{T\alpha} = \alpha$? Well, it certainly doesn't if $\alpha \in \mathbb{N}$ for then we can have $n > 2^{Tn}$ but n is the cardinal of a wellordered set. So the conjecture should be something like: if α is infinite, or if AxCount$_\leq$, then $2^{T\alpha} \leq \alpha$ has the same consequences for α as $2^{T\alpha} = \alpha$?

The idea is this: use the singleton function, given $|x| > |\mathcal{P}(x)|$, to get a setlike bijection (which will – obviously – not be a set) between x and $\mathcal{P}(x)$ so that $\langle\langle x\rangle\rangle \simeq \langle\langle \mathcal{P}(x)\rangle\rangle$ and thus $\langle\langle x\rangle\rangle$ is a model *glissant* of *TSTI*. So what we need, given $\alpha > 2^{T\alpha}$, is that there should be x and y such that

$$\alpha = x + y$$

$$2^{T\alpha} = x + Ty$$

x and Ty are both odd or both even, since their sum is even. Either way α is even. Then whenever we have a thing A of size α we can partition $A = A_1 \sqcup A_2$, $\mathcal{P}(A) = B_1 \sqcup B_2$ with maps $f : B_1 \to A_1$ and $g : \iota``A_2 \to B_2$ with f a set $\iota^{-1}g$ a set. We use this to construct a bijection $h : A \longleftrightarrow \mathcal{P}(A)$ by

$h = f \cup \iota^{-1}g$. We would like this to be setlike. If it is we have shown that M_A and $M_{\mathcal{P}(A)}$ are isomorphic.

Now we do know that if $\alpha \in \mathbb{N}$ we have no hope of partitioning x in this way to get a setlike bijection, so either (i) the construction of x and y must depend on α being infinite, or (ii) the fact that the bijection constructed is setlike must depend of α being infinite, or on AxCount$_\leq$, or something.

Now (i) doesn't seem possible. It is true that a parity argument shows that α would have to be even but i don't see any way of excluding the possibility of finite solutions to this pair of equations.

So it is probably (ii) and we have to think about strong axiom would be available to make the partition have the desired property. It's worth pointing out that as long as α is infinite there are such x and y, for set $x = 2^{T\alpha}$ and $y = \alpha - 2^{T\alpha}$ (unless $\alpha = 2^{T\alpha}$ in which case there is nothing to prove!) For we want

$$2^{T\alpha} = 2^{T\alpha} + T(\alpha - 2^{T\alpha})$$

This will follow from

$$2^{T\alpha} = 2^{T\alpha} + T(\alpha)$$

which will follow from

$$2^{T\alpha} = 2^{T\alpha} + 2^{T(\alpha)}$$

which follows from

$$\alpha = \alpha + 1$$

But if $2^{T\alpha} \leq \alpha$ we must have

$$2^{2^{2^{T^3\alpha}}} \leq \alpha$$

so α must be dedekind infinite as desired.

[*HOLE Tidy this up*]

1. Can we show that α of infinite rank $\to$ α not $\beth_n$ of any aleph? Assuming AC_{wo} then $\beth_\alpha$ is defined for some $\alpha > T\alpha$ so in such a case, no.

2. If AxCount$_\leq$ fails, then $\beth_n$ will be a counterexample for some n.

3. AxCount$_\leq$ $\longleftrightarrow$ $(\Box\forall x(\mathcal{P}(x) \subseteq x \ \to\ x$ not wellordered$))$?

   Suppose $2^{T\alpha} \leq \alpha$. Then α is infinite. Suppose it is an aleph. Let Φ_α‘β be $\{\beta, 2^\beta \ldots\}$ as far as the powers remain below α. Can we do anything with this?

4. AxCount$_\leq$ $\to (2^{T|x|} \leq |x| \to M_x \models Amb)$? AxCount$_\leq$ is needed to prove $(2^{T|x|} \leq |x| \to M_x \models Amb$ because $2^{Tn} < n$ can happen otherwise and this would give a model of $Amb + \neg$ AxInf.

5. AxCount$_\leq$ $\to$ $(2^{T|x|} \leq |x| \to M_x \models Amb)$? Assume AC_{wo}. So all $\beth$ numbers exist. Now for some $\alpha \in On$ with $\alpha > T\alpha$ we will have the corresponding $\beth$ number $\beth_\alpha$ with $2^{T\beth_\alpha} < \beth_\alpha$. This cannot give rise to a model of NF + AxCount$_\leq$, for in any such we can prove "$|V|$ is not a

> $\beth$ number" But since we can use Ehrenfeucht- Mostowski to get models of KF containing $2^{T\alpha} \leq \alpha$ without any additional assumptions, we know that $2^{T\alpha} \leq \alpha$ has no strong consequences in a stratified context.

If (2) is to work, we want to be sure that (1) works only for $\alpha \notin \mathbb{N}$. If it were to work for all finite $n \geq Tn$ then for any $n \in \mathbb{N}$ we would have x and y such that

$$x + Ty = 2^{Tn}$$

$$x + y = n$$

Now clearly $x + Ty$ and $x + y$ are congruent mod 2, and one of them is a power of 2, so the other is at least even. So n is even. But if $2^{Tn} \leq n$ then certainly $2^{(Tn+1)} \leq (n + 1)$ and $n + 1$ would have to be even as well.

So far so good!

That takes care of (1). How about (2)? Well, this is just the old problem of showing that s-b works for setlike injections to give setlike bijections, and there seems no reason why it should. It is quite clear that h will lift once, but there seems no reason to suppose it will lift twice. Of course in general we cannot expect to be able to derive interesting consequences from $2^{T\alpha} \leq \alpha$ because this can happen in KF with no knobs on.

We have seen that $\text{AxCount}_{\leq} \rightarrow |V|$ is not a $\beth$ number, and that if α is of infinite rank then it is not an aleph. Can we show that if α is of infinite rank then α is not a $\beth$ number?

Every now and then one of my part II set theory supervisees asks me "I know what ω is, it's the length of the positive integers. What is ω_1 the length of?" And i always feel, when i reply "the set of all countable ordinals in their natural order" that i am giving a trick answer. And i suspect i am too, because they usually don't seem very satisfied. The witness is not the one one would obtain by transformation of a constructive proof – unless it is of higher type, where all the countable ordinals are elements – so we get $\aleph_n$ at type n. Therefore no proof of existence of $\aleph_n$ uniform in n, and no stratified proof of existence of $\aleph_\omega$. The only proof is by induction on n.

This certainly seems to be the situation in NF anyway.

Indeed even if we do have all $\aleph_n$, we cannot construct an $\aleph_\omega$ of the same type without AC. (Coret: all stratified replacement provable in Zermelo, and no $\aleph_\omega$ in Z)

$(\forall n)(\aleph_n$ exists) stratified but has no stratified proof.

Is it true that whenever $\mathrm{T\mathbb{Z}T} \vdash F(t)$ for all terms t then there is a uniform proof in the arithmetic of T$\mathbb{Z}$T that such proofs exist? Clearly the arithmetic of T$\mathbb{Z}$T is typically ambiguous: the T function is an isomorphism between the naturals of level n and the naturals at level $n + 1$.

I remember now why i was so concerned about finding a nice set of large finite size. Consider the claim that there is a function β such that $(\forall n)$ every non-empty n-symmetric set has an (at worst) $\beta(n)$-symmetric member. This sounds desirable, at least, though it is *prima facie* an even stronger assertion than the one that T$\mathbb{Z}$T has a term model. Now consider the finite cardinals, all of which are 2-symmetric. We are now stuck with having to produce, for each finite cardinal k, a $\leq \beta(2)$-symmetric set of size k. If we now use hereditarily finite sets we run up against the fact that m-symmetric hereditarily finite sets are bounded in size, and so for k large enuff we are not going to be able to find a hereditarily finite set of rank β‘2 and size k. The obvious thing to do is to reach for the initial seg of $\mathbb{N}$ bounded by k, but this is an object of higher type. What does the proof look like that that the natural numbers below n are a set of size Tn? One way we could hack round this is if we have an algebraic version of "definable with n alternating blocks of quantifiers" (after all, the notion of n-symmetric set is an algebraic version of set-abstract-with-sole-free-vbl-of-type-n) for then we seek instead function β, γ such that $(\forall n_1, n_2)$ every non-empty n_1-symmetric set definable with n_2 alternating blocks of quantifiers has an (at worst) β‘$\langle n_1, n_2\rangle$-symmetric member definable with γ‘$\langle n_1, n_2\rangle$ alternating blocks of quantifiers.

Finding large sets disjoint from their power sets can be useful. Suppose we wanted to prove the consistency of $NF_3 + \Phi(\aleph_0) \in \mathbb{N}$. We work in $NF + \neg\mathrm{AxCount}_{\leq}$ and fix on α some finite beth number $> 2^{T\alpha}$. We cannot use **??** here because this only works for internal permutations, but if we can find $x \in \alpha$ disjoint from $\mathcal{P}(x)$ then we can extend the 1-setlike bijection $x \longleftrightarrow \mathcal{P}(x)$ to a 1-setlike permutation of the universe, which gives rise to a model of NF_3 in which α is the size of the new universe. Mind you, if $a \notin a$ then $\mathcal{P}(B(a))$ is disjoint from its power set and of size $|V|$. But in any case $a \notin a \rightarrow B$‘$a \cap \mathcal{P}(B(a)) = \Lambda$ so every cardinal contains a set disjoint from its power set.

The point about finding sets disjoint from their power sets is this. If we have a bijection between a part of x and a part of $\mathcal{P}(x)$ then this will extend to a permutation of the universe. If $x \cap \mathcal{P}(x) = \Lambda$, then the permutation is an involution, which makes life much easier.

12.6 How many sets are there of any given size?

Definition 17 *A* **cardinal ideal** *is a set closed under $\subseteq$ and equipollence.*

For α a cardinal, write I_α for $\{x : |x| \leq \alpha\}$ and I^*_α for $\{x : |x| \leq^* \alpha\}$.

For I a cardinal ideal, the cardinal ideal TI is the set of all x the same size as a set of singletons in I. (overloading of 'T')

For I a cardinal ideal, let I^* be the set of surjective images of things in I.

I^* is another cardinal ideal, $I \subseteq I^*$ and the inclusion may be proper.

This next little lemma is the result of an idea of Nathan Bowler's.

Lemma 17 *(Nathan Bowler)*

For all cardinal ideals I,

$$|I^*| \leq^* |I|.$$

Proof:

Send each $X \in I$ to fst“X. (snd“X would do just as well). Clearly this gives us members of I^* as values: we just have to check that everything in I^* is obtained in this way. Yes, because if $x \in I^*$ is f“y for some $y \in I$, then $f \restriction y$ is also in I: every function is the same size as its domain! ■

Or, again:

If f is a function defined on a member X of I then fst is a bijection between f and X – which is in I, so $f \in I$. So anything in I^* is the range of a function which – considered as a set of ordered pairs – is a member of I. So let F be the function defined on I as follows:

$$F(X) := \texttt{if } X \text{ is a function } \texttt{then } X\text{“}I \texttt{ else } \emptyset.$$

then every member of I^* is a value of F. So F maps I onto I^*. ■

Remark 59

If $\beta \geq \alpha$ then $|I_\beta| \geq 2^{T\alpha}$

For B in β and A in α there is a surjection $f : B \twoheadrightarrow A$. Now $\{f^{-1}\text{“}A' : A' \subseteq A\}$ is a subset of I_β and it is the same size as $\mathcal{P}(A)$.

■

What Tarski's argument shows in an NF context is that any cardinal ideal the same size as ι“V cannot contain all wellorderable sets.

We collect some nice results about sizes of cardinal ideals, but only some. Cardinal ideals defined by excluded-minor properties have sizes that seem very hard to pin down. How does one get a handle on the number of Dedekind-finite sets for example?

Remark 60

Let I and J be cardinal ideals with $|J| = T|V|$.
Let K be the cardinal ideal $\{\bigcup x : x \in TI \wedge x \subseteq J\}$.
Then $|K| \leq^* |I|$.

Proof: Assume $|J| = T|V|$. Then we have the following

$$\iota\text{“}I =^{(1)} \iota\text{“}\mathcal{P}_I(V) \simeq^{(2)} \mathcal{P}_I(\iota\text{“}V) \simeq^{(3)} \mathcal{P}_I(J) \twoheadrightarrow^{(4)} \iota\text{“}K$$

(1) holds beco's I is a cardinal ideal;
(2) holds by redistributing iotas;
(3) holds by assumption on J;
(4) holds by definition of K.

So (peeling off the iotas) $|K| \leq^* |I|$. ■

At each step a bit more detail would not go amiss...

Can we weaken the assumption “$|J| = T|V|$” to $|J| \leq^* T|V|$? That would enable us to bound the sizes of the C_n (see below).

In the case of interest below I is the set of countable sets, so it's OK.

Corollary 11 *There is a surjection from the set of wellordered sets to the collection of sets that are wellordered unions of finite sets.*

Proof:

Let I be the ideal of wellorderable sets and J the ideal of finite sets.

■

So, if V really is a wellordered union of finite sets, then every set you can think of (being a subset of V) is a wellordered union of finite sets and is therefore in the range of this mapping from the set of wellordered sets, so the collection of wellordered sets maps onto V. That sounds terribly implausible to me.

This looks like something worth making an effort for. Can we show that there is no surjection from the set of wellorderable sets onto V?

How many countably infinite sets are there? How many wellorderings of length ω?

(i) Tarski has a theorem that every set has more wellorderable subsets than singleton subsets. This works in NF. Since no wellordered set maps onto V this tells us that there are more sets that do not map onto V than there are singletons:

$|\{x : |x| \not\geq^* |V|\}| > T|V|$

But we can do much better than that.

(ii) Nathan showed that the collection of sets that are surjective images of $\iota``V$ is itself a surjective image of $\iota``V$. We supply his proof below, section 12.7

He also showed that there are precisely $T|V|$ finite sets.

Can we connect this with the question of whether or not V is a union of a wellordered family of finite sets? Is $WFIN$ smaller than V?

Remark 61

(i) $|\{x : |x| = |V|\}| = |V|$

(ii) $|\{x : |x| < |V|\}| = |V|$.

Proof:

(i) This is beco's $\{V \times x : x \in V\}$ is a $|V|$-sized set of things of size V.

(ii) Do we have $|\mathcal{P}(x)| = |V| \to |x| = |V|$? Clearly not[2]. So there is x with $|x| < |V|$ and $|\mathcal{P}(x)| = |V|$. So $\mathcal{P}(x)$ is a $|V|$-sized set of things smaller than V. So there are $|V|$-many things smaller than V. ■

The argument for (ii) proves a bit more. Suppose $|\mathcal{P}(x)| = |V|$. Then this argument shows that there are at least $|V|$-many things of size $\leq |x|$.

Warning! I no longer think i have a refutation of $|\mathcal{P}(x)| = |V| \to |x| = |V|$.

Remark 62

(1) $(\forall \alpha, \beta \in NC)(\alpha \leq \beta \to |\alpha| \leq |\beta|)$;

(2) $(\forall \alpha, \beta \in NC)(\alpha \leq^* \beta \to |\alpha| \leq^* |\beta|)$;

(3) $(\forall \alpha, \beta \in NC)(\alpha \leq^* \beta \to |\{x : |x| \leq^* \alpha\}| \leq |\{x : |x| \leq^* \beta\}|)$;

(4) $(\forall \alpha, \beta \in NC)(\alpha \leq^* \beta \to |\{x : |x| \leq^* \alpha\}| \leq^* |\{x : |x| \leq^* \beta\}|)$.

ems weaker than (3)

Proof:

1. Suppose $\alpha < \beta$ are cardinals. Fix $A \in \alpha$ and $B \in \beta$ with $A \subset B$. Without loss of generality we can take B to be included in a moiety. This means that there are the same number of things in α disjoint from B as there are things in α. (Details for the suspicious. If M is the moiety disjoint from B and π a bijection $V \longleftrightarrow M$ then, for any $A' \in \alpha$, $\pi``A'$ is a member of $\alpha$ disjoint from $B$, and the function $A' \mapsto \pi``A'$ is injective.) Now let $A' \in \alpha$ be disjoint from B. We send it to $(B \setminus A) \cup A'$, which is a member of β. This map $A' \mapsto (B \setminus A) \cup A'$ too is injective. Composing these two injections sends α into β. This proves $\alpha \leq \beta \to |\alpha| \leq |\beta|$.

2. Suppose $\alpha \leq^* \beta$. If f is a function defined on a member B of β then `fst` is a bijection between f and B – which is in β, so $f \in \beta$. So anything in α is the range of a function which – considered as a set of ordered pairs – is a member of β.

   So fix A an arbitrary set of size α, and let F be the function defined on β as follows:

   $$F(f) := \texttt{if } f \text{ is a function with } |f``\beta| = \alpha \texttt{ then } f``\beta \texttt{ else } A.$$

   then every member of α is a value of F. So F maps β onto α.

3. If $B \twoheadrightarrow A$ then the set of things that B maps onto is a superset of the set of things that A maps onto.

■

4. We can refine (2) into a proof that if $\alpha \leq^* \beta$ then $|\{x : |x| \leq^* \alpha\}| \leq^* |\{x : |x| \leq^* \beta\}|$. The F that we need can be defined as: Consider the function

   $$\begin{aligned} F(f) := {} & \texttt{if } f \text{ is a function with } |f``\{x : |x| \leq^* \beta\}| \leq^* \alpha \\ & \texttt{then } f``\{x : |x| \leq^* \beta\} \\ & \texttt{else } \emptyset. \end{aligned}$$

   Notice that F has no parameters. That is to say, we have a **canonical** construction that gives us, for all cardinals $\alpha \leq^* \beta$, a map

   $$F_{\alpha,\beta} : \{x : |x| \leq^* \beta\} \twoheadrightarrow \{x : |x| \leq^* \alpha\}.$$

   Do the $F_{\alpha,\beta}$ commute? I bet they don't.

■

Some remarks.

[2]Should say more about this!! Need some actual examples.

Clearly $|\alpha + \beta| \geq |\alpha| \cdot |\beta|$

Chop V into a pair of moieties, A and B. Since A is a moiety, it has as many subsets of size α as does V, namely $|\alpha|$; B similarly. So that gives us $|\alpha| \cdot |\beta|$ sets of size $\alpha + \beta$, and there are lots more to come. It would be nice to have an upper bound as well as a lower bound.

There are $|\alpha|$ things of size α and $|\beta|$ things of size β so there are $|\alpha| \cdot |\beta|$ pairs p of a thing of size α with a thing of size β. If such a pair $p = \langle a, b\rangle$ gives a set $a \cup b$ of size $\alpha + \beta$ send p to that thing. If not, send it to one such set of size $\alpha + \beta$ designated as dustbin. This gives a surjection onto the set of sets of size $\alpha + \beta$. So $|\alpha + \beta| \leq^* |\alpha| \cdot |\beta|$.

We seem to have proved:

$$|\alpha| \cdot |\beta| \quad \leq \quad |\alpha + \beta| \quad \leq^* \quad |\alpha| \cdot |\beta|.$$

The same argument will show that if $\alpha = 2 \cdot \alpha$ then $|\alpha|^2 = |\alpha|$. And indeed, if $\alpha + \beta = \alpha$, then $|\alpha| = |\alpha| \cdot |\beta|$.

Here's another proof of (1)

Suppose $\alpha < \beta$ are cardinals. Fix a moiety M. Clearly M has the same number of α-sized subsets as V does, so if we can find an injection from $\mathcal{P}_\alpha(M)$ (the set of α-sized subsets of M) into β we will be done. Now the moiety $V \setminus M$ will contain a set c of size $\beta - \alpha$. We have to be careful here: a set c is of size $\beta - \alpha$ if its union with a disjoint set of size α is of size β. $\alpha < \beta$ so there are such sets c, and $V \setminus M$ is a moiety and so has subsets of all sizes. But then the function from $\mathcal{P}_\alpha(M)$ defined by $a \mapsto a \cup c$ is injective and all its values are sets of size β.

We know that the sizes of cardinals start at $T|V|$ and stay that way for finite cardinals at least, and eventually reach $|V|$. Naturally one wonders at what point the size of a cardinal (as a set) flips to $|V|$. How many things are there of size $T|V|$? Of course it is at least $T|V|$ but I have the feeling that it is *precisely* $T|V|$, but i can't now remember where this feeling comes from.

And how many countably infinite sets are there? At least $T|V|$. But also $\leq^* T|V|$. Precisely $T|V|$?

I noticed years ago that if x injects into its complement, so does $\mathcal{P}(x)$. After all, if x injects into $V \setminus x$, $\mathcal{P}(x)$ injects into $\mathcal{P}(V \setminus x)$, which is a subset of $V \setminus \mathcal{P}(x)$.

But actually the same works for other lifts. If x and y both inject into their complements, so does $x \times y$. We'd better prove this. If x injects into $V \setminus x$ and y injects into $V \setminus y$ then $x \times y$ injects into $(V \setminus x) \times (V \setminus y)$ which is a subset of $V \setminus (x \times y)$.

But what kind of ill-brought-up set does *not* inject into its complement one might ask? Some things of size $|V|$ of course. But if you are smaller than V and still do not embed in your complement then you are one piece of a partition of V into two smaller pieces. Now suppose X^2 is one piece of a partition of V into two smaller pieces. Then X^2 does not inject into its complement, so neither does X. Does this mean that if $\alpha^2, \beta < |V|$ with $\alpha^2+\beta = |V|$ then $\alpha+\beta = |V|$? It looks like it but we have to be careful. The point is that the property of being smaller than your complement is not obviously preserved under equinumerosity.

Even if $\alpha^2 + \beta = |V|$ it might be the case that whenever $|A| = \alpha$ then $|V \setminus (A \times A)| = |V|$. The fly in the ointment is that – for all we know – it might be that there are sets of size α^2 whose complements are of size β with $\beta < |V|$ but whenever $|A| = \alpha$ then $|V \setminus (A \times A)| = |V|$.

Another cute fact i've just noticed, which will have to be fitted in somehow.

Remark 63
Let α be a cardinal such that $\alpha = \alpha^2 \geq^* |V|$;
then there are $|V|$-many sets of size α.

Proof: Let α be as in the statement of the remark, and let A be a set with $|A| = \alpha$. For each $A' \subseteq A$ we have $\alpha \leq |A \times A'| \leq \alpha^2$ whence $|A \times A'| = \alpha$. There are $|V|$-many such A' (beco's $\alpha \geq^* |V|$ so $|\mathcal{P}(A)| = |V|$)) so there are $|V|$-many sets of size α. ■

I think this can be refined. Let $2^{T\beta} = |V|$ and $|A| = \alpha \cdot \beta = \alpha \geq^* |V|$, and $\beta = |B|$. For each $B' \subseteq B$ we have $\alpha \leq |A \times B'| \leq \alpha \cdot \beta = \alpha$. Each set $A \times B'$ is of size α and there are $|V|$-many of them beco's B has $|V|$-many subsets, so there are $|V|$-many things of size α.

However we don't know that there are any such α other than $|V|$ itself. Of course what is really going on in this proof is the following. Suppose A is a set of size α and I is the cardinal ideal $\{x : |x \times A| = \alpha\}$. Then there are at least $|I|$-many things of size α.

It would be nice to be able to prove that if X maps onto V then there are $|V|$-many things of size $|X|$.

12.6.1 The smallest σ-Ring and an old Question of Boffa's

Asaf tells me that in Gitik's model the smallest σ-ring containing all singletons is actually the whole universe. He derives this claim from the fact that in Gitik's model every set is a union of countably many smaller sets. I say: that relies on the order relation $<_{NC}$ on cardinals being wellfounded. He says: no, beco's of the V_αs. I might have to write out a proof of that.

Consider the recursive datatype $\mathcal{C}$ generated by the countable (ie countable or finite) sets as founders, and containing Y whenever there is a surjection $f : Y \twoheadrightarrow X$ where X is a $\mathcal{C}$-set and the fibre f^{-1}"$\{x\}$ is a $\mathcal{C}$-set for every $x \in X$.

C_0 = set of countable sets, C_α = countable unions of sets in $\bigcup_{\beta<\alpha} C_\beta$. The closure set is C_∞. Observe that each C_α is a cardinal ideal.

The question is: can we have $C_\infty = V$? one way to exclude this possibility is to bound the size of the C_αs somehow; perhaps one could show that ι"V maps onto each C_α.

Every ω-sequence S of sets can be coded up as a single set $K(S) = X$ such that $S(0) = \texttt{fst}(X)$ and thereafter $S(n) = \texttt{fst}(\texttt{snd}^n(X))$.

This gives us $f_0 : \iota``V \twoheadrightarrow C_0$ by $f_0(\{x\}) = (K^{-1}(x))``\mathbb{N}$

Thereafter we can set

$f_{n+1}\{x\} = \bigcup f_n``\iota``x$

... which is stratified but inhomogeneous. So we can define it for concrete n but cannot iterate transfinitely. $f_n : \iota``V \twoheadrightarrow C_n$.

To be more concrete about it: we have two bijections θ_1 and θ_2 with $\theta_1``V \sqcup \theta_2``V = V$. $\langle x, y\rangle$ is usually $\theta_1``x \cup \theta_2``y$. But we can do better than this. We can encode an ω-sequence $\langle x_0, x_1, x_2 \ldots\rangle$ as

$$\theta_1``x_0 \cup \theta_2``(\theta_1``x_1 \cup \theta_2``(\theta_1``x_2 \cup \theta_2``(\ldots$$

or, avoiding the unbounded nesting (since we can):

$$\theta_1``x_0 \cup \theta_2``(\theta_1``x_1) \cup (\theta_2)^2``(\theta_1``x_2) \cup \ldots (\theta_2)^n``(\theta_1``x_n) \ldots$$

By this means we can encode an ω-sequence of things at the same type as the things in the sequence.

Notice that every set encodes an ω-sequence in this way.

Consider the function $X \mapsto$ set of ω-sequences-from-X. It's $\subseteq$-monotone. (Best check this allegation!), and the GFP is V. It would be nice to have a steer on the size of the LFP – or its rank. We can reach the LFP by starting with the set of all those sequences whose every component has size 1 at most.

We might have to be careful. If we only stop when we reach a singleton (on the grounds that a ctbl union of finite sets might not be countable) we have to be sure that if we decode a finite set as a sequence then it is a sequence of singletons, and that might not be true. We could just stop descending once we reach finite sets, but that looks a bit odd. Let us call this set S_∞.

... or we could decide to just start with those ω-sequences that are everywhere singletons or empty, and then close under taking ω-sequences. Now it's no longer true that the GFP is V but that doesn't matter.

We can probably use a modification of Jech's argument to show that everything in S_∞ has rank $< \omega_2$. There is an obvious projection from $S_\infty \to C_\infty$. However there is no reason to suppose that it is surjective.

How can we exploit Jech's construction in a model in which every limit ordinal has cofinality ω? Instead of HC we consider the rectype of ω-sequences of ω-sequences of ... There will be a surjection fromn this family onto On. Or will there? Does this need AC_ω (see the worries about certificates above).

Anyway the idea now is to use a trick like that i used in LIS to show that you can embed $H_{\aleph_1}$ into $\mathbb{R}$. All you need is a set that is as big as the set of countable sequences from itself. However one such set is $\iota^5``V$, and we surely don't expect S_∞ to embed into anything that small. The point is that in LIS trick you start from nothing. Here you start from the collection of things that are unions of countably many finite sets. This is a surjective image of FIN^ω which is of size $T|V|$.

And once we have got the LFP we need to explain the connection with C_∞.

Stop burbling, Forster

A key observation of course is that, for all α, the map that sends a countable subset X of C_α to $\{\bigcup X\}$ is a surjection from $\mathcal{P}_{\aleph_1}(C_\alpha)$ onto $\iota``C_{\alpha+1}$.

Next we show that

REMARK 64 $|C_0| \leq^* |\iota``V|$.

Proof: Let $\{X_i : i \in \mathbb{N}\}$ be a partition of V into $\aleph_0$ moieties, and let χ_n be a bijection between V and X_n.

Then we can encode any sequence $f : \mathbb{N} \to V$ as the singleton

$$K(f) := \{\bigcup\{\chi_n``(f(n)) : n \in \mathbb{N}\}\}.$$

K is evidently a bijection between $\iota``V$ and the set $\mathbb{N} \to V$. Clearly any singleton is the result of encoding some – unique – f or other. Thus the map

$$\{x\} \mapsto K(\{x\})``\mathbb{N}$$

is a surjection from $\iota``V$ to C_0, the set of countable sets. ■

This is probably a corollary of remark 60

I think we can actually do better than this.

Let $\mathbb{P}$ be a partition of V into moieties, equipped with a function π such that, for each $p \in \mathbb{P}$, $\pi(p)$ is a bijection between V and p. Need a picture, really

Now suppose X is a set the same size as $\mathbb{P}$, with σ a bijection $X \longleftrightarrow \mathbb{P}$. Consider the singleton

$$\{\{(\pi(\sigma(x)))``x : x \in X\}\}$$

Notice that we can recover X from this singleton. Any $y \in \{(\pi(\sigma(x)))``x : x \in X\}$ is a subset of a unique $p \in \mathbb{P}$. $(\pi(p))^{-1}``y$ is now a member of X.

This gives us a map from $\iota``V$ onto the set of things of size $\leq |\mathbb{P}|$.

However all this gives us is a recasting of Nathan's proof that the set of surjective images of $\iota``V$ is itself a surjective image of $\iota``V$.

We will need the following

REMARK 65 *Any surjective image of a set in C_α is in C_α.*

Proof:

Clearly a surjective image of a countable set is countable. If $X \in C_\alpha$ then $X = \bigcup_{i\in\mathbb{N}} X_i$ where all the X_i are in C_β with $\beta < \alpha$. For any function f evidently $f``X = \bigcup_{i\in\mathbb{N}} f``X_i$, and the $f``X_i$ are all in C_β with $\beta < \alpha$ by induction hypothesis. ■

We ought to be able to prove that $|C_0| = T|V|$ precisely. Then we will be able to use lemma 60 to prove that $|C_n| = T|V|$ for all $n \in \mathbb{N}$. I doubt very much if that is sufficient to prove that $|C_\omega| = T|V|$.

All this is OK so far. This is where it starts to go wrong.

Mistake!

$|C_\infty| \leq^* T|V|$ and $C_\infty \neq V$.

Attempted proof.

We observed in remark 64 that $|C_0| \leq^* T|V|$. We now claim the following chain of inequalities.

$$|\iota``C_1| \;\leq^*\; |\mathcal{P}_{\aleph_1}(C_0)| \;\leq^{*(1)}\; |\mathcal{P}_{\aleph_1}(\iota``V)| \;=^{(2)}\; T|\mathcal{P}_{\aleph_1}(V)| \;=\; T|C_0| \;\leq^*\; T^2|V|$$

(1) This is where the mistake is. One would think that this star-inequality follows from $|C_0| \leq^* T|V|$, but we have to be careful. The problem is that, altho' $h``x$ is – indeed – a countable subset of C_0, we cannot be sure that every countable subset of C_0 is an h-image.

(2) Take the T outside.
so

$$|C_1| \leq^* T|V|$$

and the analogous argument will work for any α, so we have shown

$$|C_\alpha| \leq^* T|V| \to |C_{\alpha+1}| \leq^* T|V|$$

Notice that this construction is canonical: if we start with a surjection $\iota``V \twoheadrightarrow C_0$ we can recursively give later surjections in terms of it. How do we prove that there is a surjection from $\iota``V$ to C_λ, given, for each $\alpha < \lambda$, a surjection $\iota``V$ to C_α? The details deserve to be spelled out.

Let us write 'C' for $\{C_\alpha : \alpha < \lambda\}$. Let f be the function that sends each singleton $\{x\}$ to the first C_α in C such that $x \in C_\alpha$, or to C_0 if there is no such α. Thus we have $f : \iota``V \twoheadrightarrow C$. Also, the canonical nature of the construction-so-far of the surjections means that we have a function $g$ such that, for each $c \in C$, $g(c)$ is a surjection $\iota``V \twoheadrightarrow c$.

Now consider $\iota``V \times \iota``V$ and define a map

$$\langle\{x\}, \{y\}\rangle \mapsto g(f(\{x\}), \{y\})$$

This sends every ordered pair of singletons to something in the union $\bigcup C$ which is of course C_λ. Thus we can extend the canonical sequence of surjections at limit stages.

Finally this shows that $\iota``V$ can be mapped onto C_∞. ■

Deep breath. Let's try again. This is the plan.

First show that $|C_0| = T|V|$. Then use remark 60 to power an induction over countable ordinals. We need to be quite clear about what we are doing. First we establish an explicit bijection between C_0 and $\iota``V$. Then we check that the proof of remark 60 is effective. That way we can give explicit bijections between $C_n$ and $\iota``V$ by recursion on n.

What about limit ordinals? Here we trade on something that it would do no harm to spell out anyway. For any countable ordinal α, there is a function F_α that, for any $\beta < \alpha$, provides a bijection between I_β and $\mathbb{N}$. (The existence of F_{ω_1} requires AC, of course.) Let's have a proof of this. Since α is countable, there is a bijection $F : I_\alpha \longleftrightarrow \mathbb{N}$. To obtain a bijection $I_\beta \longleftrightarrow \mathbb{N}$ reflect that $F``I_\beta \subseteq \mathbb{N}$ and $F``I_\beta$ is infinite so it is in bijection with $\mathbb{N}$, and this bijection can be found beco's the proof of Cantor-Bernstein is effective.

A similar argument shows that, for any $\beta < \omega_1$, there is a system of fundamental sequences. Whether the system is Schmidt-coherent is another question! Presumably it is, or can be arranged to be.

Therefore, if the above strategy works, we can show, for any countable ordinal α, that $|C_\alpha| \leq T|V|$. The would mean that if $C_\infty = V$ then the closure ordinal is not countable.

However I can now reveal that that actually wasn't Boffa's original problem. The original version was with "countable" replaced by "wellordered". It is not clear that the analogous proof will go through, because it is not clear that the set of wellordered sets is a surjective image of the set of all singletons. However it will go through if we replace "countable" by "is a surjective image of $\iota``V$". Thus to be pedantic, say:

> An S_0 set is a surjective image of $\iota``V$. An $S_{\alpha+1}$-set is a set of the form $\bigcup X$ where $X \subseteq S_\alpha$ and $X$ is a surjective image of $\iota``V$. Take unions at limits, and let S_∞ be the union of all the S_α.

Then S_∞ is a surjective image of $\iota``V$ [why??] and therefore $S_\infty \neq V$.

We can prove by induction on the ordinals that

Remark 66

$$(\forall\kappa)((\exists x \in C_\alpha)(|x| = \kappa) \to (\exists x \in C_{T\alpha})(|x| = T\kappa)) \tag{12.1}$$

$$(\forall x)(\forall\alpha)(x \in C_\alpha \longleftrightarrow \iota``x \in C_{T\alpha}) \tag{12.2}$$

Proof:

We note first that all the C_α are closed under equinumerosity. This we prove by induction on α. If $x \in C_\alpha$ and $|y| = |x|$ then there is a bijection π between x and y. If $x = \bigcup_{i\in\mathbb{N}} x_i$ – so that $\{x_i : i \in \mathbb{N}\}$ is a certificate that $x \in C_\alpha$ – then $y = \bigcup_{i\in\mathbb{N}} \pi``x_i$ so that $\{\pi``x_i : i \in \mathbb{N}\}$ is a certificate that $y \in C_\alpha$.

Now we can prove 12.1 by induction on α. Assume 12.1 for ordinals below α.

Suppose $x \in C_\alpha$. Then there is a certificate $\{x_i : i \in \mathbb{N}\}$ with
(1) $x_i \in C_{\alpha_i}$ for each i;
(2) $\alpha = sup\{\alpha_i : i \in \mathbb{N}\}$.

Then – by induction hypothesis – for each α_i we have $\iota``x_i \in C_{\alpha_{Ti}}$. (Here we need the fact that all the $C_\alpha$ are closed under equinumerosity.) So $\iota``x \in C_{T\alpha}$.

So we have proved that if C_α contains a set of size $|x|$ then $C_{T\alpha}$ contains a set of size $|\iota``x|$ – indeed by the equinumerosity lemma it will contain $\iota``x$ itself.

For the other direction we want to show that if $C_{T\alpha}$ contains a set of size $|x|$ then C_α contains a set of size $T^{-1}|x|$. This is where the gap is! After all, if $cf(\Omega) = \omega$ then some $C_{T\alpha}$ might contain a set not the size of a set of singletons even tho' every smaller set *is* the size of a set of singletons. It seems that what might happen is that $C_\infty = C_\alpha$ and $C_{T\alpha}$ is the first level to contain sets that are not the same size as any set of singletons.

■

Let us say an S set is a surjective image of $\iota``V$.

How many sets are there that are unions of S-many finite sets? We have to be careful what we mean by this: $V = \bigcup \iota``V$ and so is a union of an $S$ set of singletons! We are interested in those sets that are the ranges of functions $\iota^2``V \to V$. Let us call this set S^*. Then

$$\iota``S^* \twoheadrightarrow (\iota^2``V \to S) \subseteq \mathcal{P}_{T^2|V|}(S) \;\; ?? \;\; \mathcal{P}_{T^2|V|}(\iota``V) \simeq \iota``\mathcal{P}_{T^2|V|}(V) \twoheadrightarrow \iota^2``V$$

The problem comes with the stage flagged by the question mark. One wants these two sets to be the same size but it's not clear that they are.

However some smaller cases work. Let FIN be the set of finite sets, C the set of countable sets and C^* the set of sets that are unions of countably many finite sets.

$$\iota``C^* \twoheadrightarrow (\mathbb{N} \to FIN) \subseteq \mathcal{P}_{\aleph_1}(FIN) \simeq \mathcal{P}_{\aleph_1}(\iota``V) \simeq \iota``(\mathcal{P}_{\aleph_1}(V)) \twoheadrightarrow \iota^2``V$$

so $|C^*| \leq^* T|V|$.

No, hang on, one of those inequalities is the wrong way round.

Notice that this is not a trivial corollary of Nathan's result. If x is cantorian then it is certainly a surjective image of $\iota``V$. It's not obvious that a union of countably many finite sets is a surjective image of $\iota``V$ nor *a foriori* cantorian, even if AxCount holds. Is a surjective image of a cantorian set cantorian? Not unless Axcount. Is a surjective image of a strongly cantorian set strongly cantorian? Yes: think about the power sets.

This last point seems to be to worth making a fuss about. Suppose $stcan(X)$, and $f : X \twoheadrightarrow Y$. Then the map $y \mapsto f^{-1}``y$ injects $\mathcal{P}(Y)$ into $\mathcal{P}(X)$, and $stcan(\mathcal{P}(X))$. One would like to be able to do it more directly, by inducing f to work somehow on $\iota\restriction X$ to give $\iota\restriction Y$, so that $\iota\restriction Y$ is obtained a a surjective inage of $\iota\restriction X$.

What are we to do (in a stratified way!) with a pair $\langle x, \{x\}\rangle$?

Suppose $f : X \twoheadrightarrow Y$. Then $g = f^\iota$ maps $\iota``X \twoheadrightarrow \iota``Y$. Declare $h(\langle a, \{b\}\rangle) = \langle f(a), g(\{b\})\rangle$. This is OK beco's f and g are both sets. Then $\iota\restriction Y = h``(\iota\restriction X)$.

Worth checking that the same sort of behaviour is exhibited by sets for which $\iota^n \restriction x$ exists. See stratificationmodn.tex

Probably worth recording that can(X) and $|X| = |Y|$ implies can(Y) (not that i ever doubted it) but it's not entirely straightforward. Suppose $f : X \longleftrightarrow Y$ is a bijection, and $g : X \longleftrightarrow \iota``X$ is a bijection. Then the composition

$$f^{\iota} \cdot g^{-1} \cdot f^{-1}$$

maps Y 1-1 onto $\iota``Y$.

Going back a bit. Suppose $f : X \twoheadrightarrow Y$ is a bijection, and write g for $\iota \restriction X$.
Then send $y \in Y$ to any $f^{-1}(y)$ and send that to $g \cdot f^{-1}(y)$ then that goes to $f^{\iota} \cdot g \cdot f^{-1} y$ which is OK.

12.6.2 Small Sets

"small" = "cannot be mapped onto V"
I claim that the set of small sets maps onto V; the set of small sets is not small.
Suppose it were. Then every set of small sets is small, so the power set of a small set is small, whence

$$(\forall A)(|\mathcal{P}(A)| \geq^* |V| \to |A| \geq^* |V|)$$

So, substituting $\mathcal{P}(B)$ for A one obtains

$$(\forall B)(|\mathcal{P}^2(B)| \geq^* |V| \to |\mathcal{P}(B)| \geq^* |V|$$

whence

$$(\forall B)(|\mathcal{P}^2(B)| \geq^* |V| \to |B| \geq^* |V|$$

and, for each concrete n,

$$(\forall B)(|\mathcal{P}^n(B)| \geq^* |V| \to |B| \geq^* |V|$$

whence
Now $|A| \geq^* |V|$ implies $|\mathcal{P}(V)| \leq |\mathcal{P}(A)|$ so we infer

$$(\forall A)(|\mathcal{P}(A)| \geq^* |V| \to |\mathcal{P}(A)| = |V|)$$

Now $|\mathcal{P}(A)| = |V|$) certainly implies $|\mathcal{P}(A)| \geq^* |V|$ so we have proved

$$(\forall A)(|\mathcal{P}(A)| = |V| \to |A| \geq^* |V|)$$

Now consider the case where A is $\mathcal{P}(B)$. This gives

$$(\forall B)(|\mathcal{P}(\mathcal{P}(B))| = |V| \to |\mathcal{P}(B)| \geq^* |V|)$$

and the RHS implies $|\mathcal{P}(B)| = |V|$) so we have proved

$$(\forall B)(|\mathcal{P}(\mathcal{P}(B))| = |V| \to |B| = |V|)$$

Ah! $(\forall B)(|\mathcal{P}(\mathcal{P}(B))| = |V| \to |B| = |V|)$ is equivalent to

$$(\forall \beta)(2^{2^\beta} = |V| \to \beta = T^2|V|)$$

and that is clearly not true. So the set of small sets is not small.

We need $\alpha \leq |\alpha|$. Then we would have been able to show that $|S|$ was the supremum of all the small cardinals.

All that looks rather sus.

12.6.3 Union of a low Set of low Sets

Must it be low? (A low set is a set the same size as a wellfounded set). Suppose $\{X_w : w \in W\}$ is a low set of low sets, where the index set W is wellfounded. The obvious thing to do is the following.

First off, observe that without loss of generality W can be taken to be a set of singletons beco's (at least if we are in NF) every wellfounded set is the size of a set of singleton^k for any concrete k.

For each $w \in W$ pick a wellfounded set Y_w which is in bijection with X_w, and consider the cartesian product $Y_w \times w$. This is wellfounded, is a bijective copy of X_w and these products are all distinct. So consider the union $\bigcup_{w \in W} Y_w \times w$. This maps onto $\bigcup\{X_w : w \in W\}$. Then we take power sets.

What have we used? Annoyingly, quite a lot.

12.6.4 How many Dedekind-finite sets are there?

From Nathan's work we know that there are $T|V|$-many (inductively) finite sets, but how many *Dedekind*-finite sets?

Any Dedekind-finite set x gives us a function f that picks from an ω-sequence a member of that sequence that is not in x. Any wellordering of length ω is a countable set, so any such f is a function whose domain is a set of countable sets; there are $T|V|$ countable sets, so f is of size $T|V|$. (Every function is the same size as its domain). How many such f are there? A: $|T|V||$, which (I seem to remember) is $T|V|$. So there are at most $T|V|$ Dedekind-finite sets. But there are at least that many, since there are that many inductively finite sets, and every inductively finite set is Dedekind-finite.

Saying that there are no more Dedekind-finite sets than there are sets the same size as ι"V may not sound like much, but it isn't completely trivial. After all, there is no reason to suppose that every D-finite set is the same size as a set of singletons.

12.7 A message from Nathan Bowler: a construction showing there aren't all that many sets x such that $\mathbf{AC}_{|x|}$ and $|x| \leq^* T|V|$

We'll be interested in encoding fragments of information about various sets; a fragment of information about a set x will be given by a specification of which elements of another set w are contained in x. The set w will be thought of as a window through which this fragment of information may be seen. The set w must be guaranteed to be small in the following slightly technical sense:

DEFINITION 18 *A* window *is a set w together with a surjection $\iota``V \twoheadrightarrow w$. Normally, we'll refer to the window as w, without mentioning the surjection.*

Let W be the set of all windows.

The first thing to notice about W is that it is only as big as $T|V|$. To see that $|W| \leq T|V|$, observe that the map $W \to \iota``V$ given by

$$\iota``V \xrightarrow{\phi} w \mapsto \{\{\langle a, b\rangle | a \in \phi(\{b\})\}\}$$

is injective. So there aren't too many windows.

A fragment of information about x which might be seen through a window w is given by a subset of w; the subset $x \cap w$.

This suggests the notion of *view*:

DEFINITION 19 *A* view *is a pair $\langle w, s\rangle$, where w is a window and $s \subseteq w$. A, the* album, *is the set of all views.*

Once more, the first thing to notice is that there aren't too many views. In fact,

$$|A| \leq |W| \cdot |\mathcal{P}(\iota``V)| = T|V| \cdot T|V| = T|V|.$$

Later, I'll need notions capturing the idea that one view is more panoramic than another, or that a view matches a particular set. Here are the relevant definitions:

DEFINITION 20
Let $v = \langle w, s\rangle$ and $v' = \langle w', s'\rangle$ be views and let x be a set. Then $v \leq v'$ iff $w \subseteq w'$ and $s = w \cap s'$, and $M(x, v)$ iff $x \cap w = s$.

Note that if $v \leq v'$ and $M(x, v')$ then also $M(x, v)$.

Now we can define the function which will give our coding, and another which will witness its injectivity.
Let $i\colon V \to \mathcal{P}(A)$; $X \mapsto \{v \in A : (\exists x \in X) M(x, v)\}$.
Let $j\colon \mathcal{P}(A) \to V$; $Y \mapsto \{x : (\exists v \in Y) M(x, v) \wedge ((\forall v' \in Y) v \leq v' \to M(x, v'))\}$.

THEOREM 23 $(\forall X) j(i(X)) \subseteq X$.

Proof:

Let $x \in j(i(X))$, and choose $v = \langle w, s\rangle \in i(X)$ such that $M(x, v)$ and $(\forall v' \in i(X))v \leq v' \to M(x, v')$. Choose $x' \in X$ such that $M(x', v)$. Pick any element a of x', and let $v' = (w \cup \{a\}, s \cup \{a\})$. Then $M(x', v')$ and so $v' \in i(X)$, and trivially $v \leq v'$. Thus $M(x, v')$, and so $a \in x$. A similar argument shows that any a which isn't in x' also isn't in x. Therefore $x = x' \in X$. ■

Theorem 24
Let $\alpha = |X| \geq 2$ satisfy AC_α and $\alpha \leq^* T|V|$.
Then $X \subseteq j(i(X))$.

Proof:

Let $x \in X$. Using AC_α, we can find a function $a : X \setminus \{x\} \to V$ such that for any $x' \in X \setminus \{x\}$ we have $a(x') \in x$ `XOR` x'.

Let w be the image of the function a; since $\alpha \leq^* T|V|$, X can be given the structure of a window, and therefore so can w.

Let $v = \langle w, w \cap x\rangle$. Clearly $M(x, v)$, and so $v \in i(X)$. Now suppose we have any other $v' \in i(X)$ such that $v \leq v'$. Choose $x' \in X$ such that $M(x', v')$. Since $v \leq v'$, we also have $M(x', v)$. If $x' \neq x$ then $a(x') \in (w \cap x)$ `XOR` $(w \cap x') = \emptyset$, which is a contradiction. Thus $x' = x$ and so $M(x, v')$. Thus $x \in j(i(X))$, as required. ■

Corollary 12 *Let $\alpha \geq 1$ satisfy AC_α and $\alpha \leq^* T|V|$. Then $|\alpha| = T|V|$.*

Proof:

By the last two theorems, for any $X \in \alpha$ we have $j(i(X)) = X$. Therefore i is an injection $\alpha \hookrightarrow \mathcal{P}(A)$ and so $|\alpha| \leq |\mathcal{P}(A)| \leq |\mathcal{P}(\iota``V)| = |\iota``V| = T|V|$. For any set X of size α, $- \times X$ is an injection from $\iota``V$ to α, so $T|V| \leq |\alpha|$. Thus $|\alpha| = T|V|$. ■

Corollary 13 *For each positive natural number n, $|n| = T|V|$.*

12.7.1 A construction showing that $|\{S : |S| \leq^* T|V|\}| \leq^* T|V|$

by

Nathan Bowler, April 16, 2021

The idea is that we can interpret any set C of pairs as the function from $\iota``V$ to $V$ sending $\{x\}$ to $\{s : \langle x, s\rangle \in C\}$. Since this operation is type-raising, it gives a surjective map from $\iota``V$ to the set of all such functions, and thus also to the set of their images.

More formally, we define a function $\phi : \iota``V \to V$ by

$$\phi : \{C\} \mapsto \{\{s : \langle x, s\rangle \in C\} : x \in V\}$$

It suffices to show that $\{S : |S| \leq^* T|V|\}$ is the image of ϕ, since then ϕ witnesses the claim in the title.

First of all, for any $C \in V$, $\phi(\{C\})$ is in $\{S : |S| \leq^* T|V|\}$ since we can define a surjective function $\iota``V \to \phi(C)$ by $\{x\} \mapsto \{s : \langle x, s\rangle \in C\}$.

Secondly, for any S with $|S| \leq^* T|V|$, let $F : \iota``V \twoheadrightarrow S$ be a surjective function witnessing this. Let $C := \{\langle x, s\rangle : s \in F(\{x\})\}$. Then

$$\phi(\{C\}) = \{\{s : s \in F(\{x\})\} : x \in V\} = \{F(\{x\}) : x \in V\} = F``(\iota``V) = S$$

and so S is in the image of ϕ.

We can squeeze a little more out of this. Write S for $\{x : |x| \leq^* T|V|\}$ Clearly $S \in S$ and S is downward closed so it extends its own power set. I think this means that S is precisely the same size as $\iota``V$. I don't see that ...

12.8 Retraceable You

There is yet another way in which one can strengthen Cantor's theorem. If F and G are subfunctors of $\mathcal{P}$ – or perhaps merely increasing functions on the complete lattice $\langle V, \subseteq\rangle$ – one can sometimes prove

$$|G(x)| \not\leq |F(x)|. \tag{12.3}$$

The strengthenings of Cantor's theorem mentioned so far fall under this form by taking F to be the identity. These strengthenings too can be phrased as assertions that a relation (to wit: $\{\langle x, y\rangle : |G(x)| \leq |F(y)|\}$) is irreflexive, and one can then even wonder if such a relation is wellfounded.

One could go mad worrying about wellfoundedness of these relations, but there is perhaps something to be gained from considering what sorts of natural conditions enable one to prove $|G(x)| \not\leq |F(x)|$. I'm not trying to drive myself or the reader mad: i am introducing this extra complication because it takes us to the more general situation that Conway was interested in analysing.

Let me tell the story the way it was told to me – or at least as i find it in my 1975 notebook.

12.8.1 A theorem of Specker

E. Specker: Verallgemeinerte Kontinuumshypothese und Auswahlaxiom, Archiv der Mathematik **5** (1954), 332–337.

Ernst Specker was (he died in dec 2011) a Swiss combinatorist and logician who did a lot of interesting work in set theory – particularly NF. He also proved a number of results in cardinal arithmetic without choice, specifically the following. If α and β are cardinals we say α adj β if there is no cardinal strictly between them. (Thus CH is the assertion $\aleph_0$ adj $2^{\aleph_0}$.) Then if α adj 2^α adj 2^{2^α} then 2^α is an aleph. (An aleph is the cardinal of a wellorderable set. When i last heard it was still an open question whether or not α adj 2^α implies that

α is an aleph!). One of the lemmas he proved *en route* to this result was the following:

THEOREM 25 $\alpha > 5 \rightarrow 2^{\alpha} \not\leq \alpha^2$.

This is an instance of formula 12.8: take $G(x)$ to be $x \times x$ and $F(x)$ to be $\mathcal{P}(x)$. Let's see Specker's proof.
Proof:
We will restrict attention to the case where α is not finite. Let X be a set whose size is a counterexample to the theorem. so that $f : \mathcal{P}(X) \hookrightarrow X \times X$. The idea is to use f to build a long wellordering of members of X, and show how to extend this so that X can be shown to have wellordered subsets of arbitrarily large cardinality.

We note that there is a bijection (the "herringbone map") uniform in α between $A \times A$ and A, where $A = \{\beta \in On : \beta < \alpha\}$.

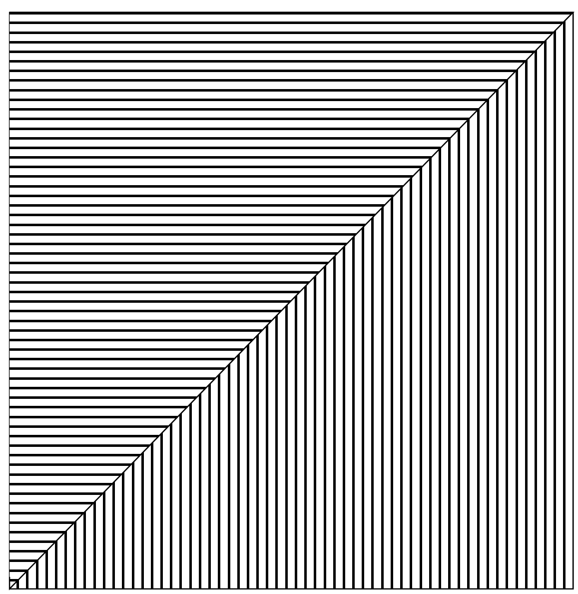


Figure 12.1: $|\alpha^2| = |\alpha|$

Let's call it 'h' for *h*erringbone so that h_β is the canonical bijection taking pairs of ordinals below β to ordinals below β.

Let M_β be a wellordered subset of X equipped with a wellordering, so that $M_\beta = \{m_\gamma : \gamma < \beta\}$. We will construct a M_β for all β. The induction step at limit β will be to take the unions of all M_γ with $\gamma < \beta$. For the successor step we procede as follows.

Restrict f to that part of $\mathcal{P}(M_\beta)$ whose image under f is included in $M_\beta \times M_\beta$. That is, consider $f \restriction (f^{-1}``(M_\beta \times M_\beta) \cap \mathcal{P}(M_\beta)$, or f_β for short. Compose this with h_β so that we now have a map $h_\beta \circ f_\beta$ sending (some!) subsets of M_β to elements of M_β.

$$\text{Let } N =: \{x \in M_\beta : x \notin (f_\beta)^{-1} \circ {h_\beta}^{-1}(x)\}.$$

Clearly N is not going to be in the domain of f_β! So

$$f(N) \in (X \times X \setminus (M_\beta \times M_\beta)).$$

We now set

$$m_\beta =: \texttt{if fst}(f(N)) \notin M_\beta \texttt{ then fst}(f(N)) \texttt{ else snd}(f(N)).$$

Remember $f(N) \notin M_\beta \times M_\beta$ so at least one of the two components is not in M_β.

■

Conway observes that the same strategy will work on any F and G to show $|F(x)| \not\leq |G(x)|$ as long as the following conditions are satisfied.

1. F and G are $\subseteq$-monotone.
2. There should be a function Ψ so that if f is a bijection between a subset of $F(x)$ and a subset of $G(x)$, then $\Psi(f) \in (F(x) \setminus (f^{-1}``G(x)))$. We say F is **diagonalisable over** G.
3. G is **retraceable**. This is, given $x \in G(Y) \setminus G(Z)$ we can produce $h(x) \in (Y \setminus Z)$.
4. If x is wellordered, so is $G(x)$.

For example – as we have seen – $\lambda x.x \times x$ is retraceable.

12.9 Kirmayer on moieties

(Kirmayer: Proc. AMS **83** (dec 1981) p 774)

Recall (this notation is not in Kirmayer) A moiety of a set is an infinite co-infinite subset. Let $\mathfrak{M}(X)$ be the set of moieties of X.

THEOREM 26 *Kirmayer's first theorem*
Suppose X has a moiety. Then $|X| \not\geq^* |\mathfrak{M}(X)|$

Proof:
Suppose $f : X \to \mathfrak{M}(X)$. We will show f is not onto. If $\{x \in X : x \notin f(x)\}$ is a moiety we get the usual paradox. So $\{x \in X : x \notin f(x)\}$ is not a moiety. Set

$$g(x) =: \begin{cases} f(x) & \text{if } \{x \in X : x \notin f(x)\} \text{ is finite} \\ X \setminus f(x) & \text{if } \{x \in X : x \notin f(x)\} \text{ is infinite.} \end{cases}$$

and

$$R =: \begin{cases} \{x \in X : x \notin f(x)\} & \text{if } \{x \in X : x \notin f(x)\} \text{ is finite} \\ X \setminus \{x \in X : x \notin f(x)\} & \text{if } \{x \in X : x \notin f(x)\} \text{ is infinite.} \end{cases}$$

Either way g is a surjection $X \to \mathfrak{M}(X)$, R is finite, and $(\forall x \in X)(x \in R \longleftrightarrow x \notin g(x))$.

Let $a \in X \setminus R$, and let $T(a) =: \{x \in X : a \in g(x)\}$. Now $(R \cup T(a)) \setminus \{a\}$ is a moiety. g is onto, so there is b such that $g(b) = (R \cup T(a)) \setminus \{a\}$. Then $b \in R \longleftrightarrow b \notin R$. So g is not onto, and f was not onto either.

[*HOLE Does this work if 'moiety' means "the same size as its complement wrt X?"*]

Theorem 27 *Kirmayer's second theorem*

If X is infinite there is no map from X onto the set of its infinite subsets.

Proof: Suppose f is a map from X to the set of its infinite subsets. Then $\{x \in X : x \notin f(x)\}$ is a moiety. [*HOLE why?*]

12.9.1 My attempt at proving Kirmayer's second theorem

We will be making much use of the adjective 'small'. It will denote any property obeying the following.

1. Every subset or surjective image of a small set is small;
2. if X is small then $X \cup \{x\}$ is small too.

(I seem to have got away so far without assuming that the union of two small sets is small). Y is a **co-small** subset of a nonsmall set X if $X \setminus Y$ is small. A subset of X that is neither small nor co-small is a **moiety**. **co-small** and **moiety** are dual: every co-small set meets every moiety.

Suppose X is not small and $Y \subseteq X$ is a moiety. If $x \notin Y$, $Y \cup \{x\}$ is a moiety, and if $x \in Y$ then $X \setminus \{x\}$ is also a moiety so there are at least $|X|$-many distinct moieties. By the same token the set of moieties containing a–or not containing a for that matter – are alike not small. That is, as long as X has any moieties at all, which it mightn't.

Theorem 28 *Let f be a map $X \to \mathcal{P}(X)$. Then there is a moiety or small subset of X not in the range of f.*

Proof: It will be helpful to use the language of permutation models and always have in mind the structure $\langle X, \in_f \rangle$, where "$x \in_f y$" means $x \in f(y)$. Thus the set $\{x \in X : (\forall y \in X)(x \notin f(y) \vee y \notin f(x))\}$ is not in the range of f, beco's it is $\{x : \neg(x \in^2 x)\}$ in the sense of $\langle X, \in_f \rangle$. Let's call it D, for Double Russell.

Let us assume, with a view to obtaining a contradiction, that every subset of X is a value of f unless it is co-small. D must now be co-small. So the set of x such that $\langle X, \in_f\rangle \models x \not\in^2 x$ is co-small.

We want to find a, b, such that $f(a)$ and $f(b)$ are complementary moieties (that is, $f(a) = X \setminus f(b)$) and a and b are both in D. For then $a \in_f a$ and $b \in_f b$ are both impossible, since both a and b are in D. But then we must have $a \in_f b \in_f a$ which is also impossible and for the same reasons. This contradiction will establish that there are things f misses that are not co-small.

Suppose we cannot find such a and b. Then for every moiety M, $X \setminus D$ either contains a code for M (that is to say, an x such that $f(x) = M$) or a code for $X \setminus M$. Fix $c \in X$ and a moiety C (it won't matter which they are) and set:

$$g(x) =: \begin{cases} C & \text{if } f(x) \text{ is not a moiety;} \\ f(x) & \text{if } a \not\in f(x); \\ X \setminus f(x) & \text{if } a \in f(x) \end{cases}$$

g now maps $X \setminus D$ onto the set of moieties of $X \setminus \{a\}$. If X is not small, neither is $X \setminus \{a\}$, so the set of moieties of $X \setminus \{a\}$ is not small, so $X \setminus D$ wasn't small. But it was.

Now this is not the end of the story, as I have assumed that X has moieties. In the trade, infinite sets that cannot be split into two disjoint infinite pieces are called *amorphous*. Let us pinch this word for use here: a nonsmall set that is not the union of two disjoint nonsmall sets is henceforth **amorphous**. It remains to exclude the possibility that X is an amorphous set with a map f onto the set $S(X)$ of its small subsets. Notice that the set $S(X)$ of small subsets of an amorphous set is not itself amorphous: $S(X)$ is not small, beco's it maps onto X. Fix $a \in X$, and think about $\{Y \in S(X) : a \in Y\}$ and $\{Y \in S(X) : a \not\in Y\}$. Each maps onto the other, and both map onto X so they are not small.

To complete the proof, notice that if $f : X \to S(X)$ is onto, then $f^{-1}``\{Y \in S(X) : a \in Y\}$ and $f^{-1}``\{Y \in S(X) : a \not\in Y\}$ are two disjoint nonsmall subsets of X.

■

12.10 Stuff to fit in

Theorem 29 *No X can be the same size as the set of its wellordered subsets.*

Proof: Suppose there were an X the same size as the set of its wellordered subsets, and that π is a bijection between X and the set of its wellordered subsets. Consider the binary structure whose domain is X and binary relation $x\,E\,y$ iff $x \in \pi(y)$. Think about the set of those $x \in X$ such that $\langle V, \in_\pi\rangle \models x$ is a Von Neumann ordinal. This cannot be a set of $\langle V, \in_\pi\rangle$ and so is not a value of π. But it is wellordered and so must be a value of π.

■

There is an alternative proof, which is the one Tarski originally gave:

Let $\langle I, \subseteq\rangle$ be a downward-closed sub-poset of $\mathcal{P}(X)$ closed under insertion. (That is to say, if $x \in I$ and $y \in X$ then $x \cup \{y\} \in I$.) Let π be a bijection $X \to I$. We will exhibit a wellordered subset of X that is not in I.

Consider the following inductively defined family of elements of I, called $\mathcal{X}$.

- The empty set is in $\mathcal{X}$
- If y is in $\mathcal{X}$ so is $y \cup \{\pi^{-1}\{u \in y : u \notin \pi(u)\}\}$.
- If $\mathcal{I}$ is a subset of $\mathcal{X}$ wellordered by $\subseteq$, then $\bigcup \mathcal{I} \in \mathcal{X}$, as long as $\mathcal{I} \subseteq I$.

We want to know that $y \cup \{\pi^{-1}\{u \in y : u \notin \pi(u)\}\}$ is distinct from y. Let $\{u \in y : u \notin \pi(u)\}$ be a for short. Suppose $\pi^{-1}(a)$ is in y. Then we have (subst $\pi^{-1}(a)$ for u)

$$\pi^{-1}(a) \in a \longleftrightarrow \pi^{-1}(a) \notin \pi(\pi^{-1}(a))$$

This is Crabbé's paradox. Therefore $y \neq y \cup \{\pi^{-1}\{u \in y : u \notin \pi(u)\}\}$ as desired.

By induction, every member of $\mathcal{X}$ is wellorderable, and $\mathcal{X}$ itself is wellordered by inclusion. Now $\bigcup \mathcal{X}$ is wellordered, being a union of a nested set of wellordered sets. It therefore follows that $\bigcup \mathcal{X}$ is not in I, for otherwise $\bigcup \mathcal{X} \cup \{\pi^{-1}\{u \in \bigcup \mathcal{X} : u \notin \pi(u)\}\}$ would be in $I \cap \mathcal{X}$ and would be bigger. So there is a wellordered subset of X that is not in I.

Actually i don't think this original proof is of any interest.

The general idea seems to be:

(i) find a concept of smallness

(ii) Find a paradoxical set which is small

(iii) Deduce that there is a small set not in the range of $f : X \to \mathcal{P}(X)$.

EG, Tarski's result is: small = wellordered; paradoxical set = set of VN ordinals.

Is there a Cantor theorem for wellfounded sets? Some thing that says that a set has more wellfounded subsets than members? No: think of a Quine atom.

But there *is* something with that flavour...

$$(\forall A, X)(\mathcal{P}(A) \subseteq A. \to .\neg\exists f : (X \cap A) \twoheadrightarrow \mathcal{P}(X) \cap A)$$

Suppose $f : (X \cap A) \twoheadrightarrow \mathcal{P}(X) \cap A$. Consider $\{x \in X \cap A : x \notin f(x)\}$. All its members are members of A, so it is a subset of A and therefore a member of A, so it's in $\mathcal{P}(X) \cap A$ and must be in the range of f. Consider an $x \in X$ such that $f(x) = \{x \in X : x \notin f(x)\}$. We get a Cantor-style contradiction as usual. ■

Later (march 2025) it occurs to me that this last paragraph may be connected to Button-Hurkens. Must check, tho' at first blush it just looks like Cantor's theorem for wellfounded sets. And will have to be modified for NF.

I insert a note from march 2024...

12.10.1 Note on two Articles by Hurkens and Button

> Schönberg once said that "there is still plenty of good music waiting to be written in the key of C major". In the spirit of this remark I offer a piece of elementary set-theoretic combinatorics that nobody seems to have noticed before. Striking tho' it is, it could have been proved a hundred years ago

In [2] Hurkens proved that the relation $\mathcal{P}(x \cap y) \subseteq y$ is wellfounded, just like it says on the tin. (Of course this is in a context where we are not assuming the axiom of foundation: banish all such tho'rts for the duration of this note) Thinking about this I noticed that what Hurkens showed could be expressed as

Every nonempty class of power sets has an $\in$-minimal member

or as

$\in$ *restricted to power sets is wellfounded.*

I felt quite pleased with this version (I still do) but Hurkens' result can be refined still further. Button – working on *level theory* and quite independently of Hurkens or me – proved the following. He calls 'potent' any set that contains all subsets of any of its members: a potent set is one that is $\subseteq$-downward-closed. Clearly any power set is potent, but there are plenty of potent sets that aren't power sets, and Button shows us a strengthing of Hurkens' result that uses this extra freedom. Lemma 3.5 of [1] states

Remark 67 $\in$ *restricted to potent sets is wellfounded.*

Proof:

(What follows is my proof not his).

Let $\mathcal{X}$ be a set of potent sets with no $\in$-minimal element.

We will show that $\mathcal{X}$ is empty.

Suppose not; we will prove by $\in$-induction that every wellfounded set belongs to everything in $\mathcal{X}$. Suppose A is a set such that, for all $a \in A$, a belongs to everything in $\mathcal{X}$. Let Y be an arbitrary member of $\mathcal{X}$, and let X be a member of $\mathcal{X}$ that is also a member of Y. (There is such, by the assumption that $\mathcal{X}$ has no $\in$-minimal element). Then $(\forall a \in A)(a \in X)$, which is to say, $A \subseteq X$. But $X \in Y$ and Y is potent, so all subsets of X are also in Y, so in particular $A \in Y$ as desired. But Y was an arbitrary member of X.

This proves by $\in$-induction on the wellfounded sets that they all belong to everything in $\mathcal{X}$. But then $\bigcap \mathcal{X}$ must be a proper class, which is impossible unless $\mathcal{X}$ is empty. ■

Now for the refinement. Let ϕ be any one-place predicate and let us weaken the definition of 'potent' to say that x is ϕ-**potent** if $(\forall y, z)(\phi(z) \wedge z \subseteq y \in x \to z \in x)$. Now run the proof of remark 1. This time we need to make the assumption that $\phi(A)$. So we have proved by $\in$-induction on the set H_ϕ of things that are hereditarily ϕ that they all belong to everything in the set $\mathcal{X}$ putatively lacking an $\in$-minimal element.

At this point one of two things happens, depending on whether or not H_ϕ is paradoxical.

- If H_ϕ is paradoxical, then $\mathcal{X}$ must be empty, and every nonempty set of ϕ-potent sets has an $\in$-minimal element; we get an exact analogue of Button's lemma: $\in$ *restricted to ϕ-potent sets is wellfounded.* This happens for example if ϕ is "wellorderable" or "wellfounded".

- If H_ϕ is not paradoxical, then we still know that every member of $\mathcal{X} \supseteq H_\phi$. This tells us that the collection of ϕ-potent sets that miss at least one set in H_ϕ is wellfounded – every nonempty subset of it has an $\in$-minimal element.

That second possibility sounds like an excluded-subset characterisation of something . . . I think i'd better stop!

Tim says

Let A be the intersection of all ϕ-potent sets (which are themselves ψ for some further condition). Let D be the subset of A comprising just A's non self membered sets. Now D isn't in A, so there's a ϕ-potent set X (which is also ψ) such that $D \notin X$ but $D \subseteq X$. Now if D is ψ then X is $\in$-minimal as requested. But if D isn't ϕ . . . nothing interesting happens?

I think this is a bit different than what you've suggested. You said we need to assume A is ϕ, but I think the key is whether D is. And we need this extra step of building D, as we can't assume that $\in$ is well founded.

Tonny writes

Hello everyone,

Nice subject!

I would formulate and prove the refinement of Remark 1 as follows:

Let X be a class of ϕ-potent sets with no $\in$-minimal element. If X is non-empty, then its intersection A is a set. We will show that for each subset z of A, if $\phi(z)$ then z is in A.

Proof: : Let z be a subset of A with $\phi(z)$. We have to show that for each $x \in X$, z is in x. Since x is not $\in$-minimal, there is some $y \in x$ with $y \in X$. So A is a subset of y.

Now z is a subset of y with $\phi(z)$, so z is in x. ■

This result implies that each z that is hereditarily ϕ must be a member of the set A, so the class H_ϕ of all these z is in fact a subset of A that can be defined with just Separation.

I think that $\in$-induction on H_ϕ is allowed since H_ϕ is a set. It is a subset of the set D of all $x \in A$ with $x \notin x$.

To give an example, let $\phi(z)$ be "z is finite" and X the singleton $\{A\}$ where A is a non-wellfounded set whose members are A itself and all hereditarily finite (wellfounded) sets.

Then the intersection of X is A and the set D is H_ϕ, which does not satisfy ϕ: it is infinite.

Greetings,

Tonny Hurkens

Dear Thomas and Tim,

Indeed: I try to define hereditarily ϕ and prove results on it, just using Separation.

To formulate this more carefully, step 1 is to define the set B of all z that hereditarily satisfy "$\phi(z)$ and $z \in A$".

Step 2 is to prove that set B is in fact H_ϕ: the least CLASS C such that ϕ is a subclass of the CLASS $F(C)$ of all sets z with: if z is a subset of C, then z is in C.

Now the result that I proved earlier in fact expresses that ϕ is a subclass of the class $F(A)$.

A very general fact is that the intersection of $F(A)$ and $F(C)$ is a subclass of $F(A \cap C)$.

So the least class C with ϕ subclass of $F(C)$ is in fact the least subclass/subset C of A with ϕ subclass of $F(C)$.

The di Giorgi view reminds us that facts about cardinal relations between subfunctors of the power set are just facts about the consistency of certain set theories!

Cantor's theorem sez that $|x| < |\mathcal{P}(X)|$. Of the many ways of generalising this result, i shall concentrate on two. One can ask for which subfunctors of $\mathcal{P}$ one can prove the obvious analogue. One can also note that Cantor's theorem is equivalent to the assertion that the relation $|\mathcal{P}(x)| \leq |y|$ is irreflexive. In fact one can prove that it is wellfounded. The analogues of Cantor's theorem we will prove will of course also be castable in the form "the relation $|F(y)| \leq |x|$ is irreflexive" and one can wonder whether these strengthenings of Cantor's theorem can themselves be strengthened to assertions that the relations appearing in these versions are wellfounded as well as being irreflexive. (The Sierpinski-Hartogs theorem is used to show that "$2^\alpha \leq \beta$" is wellfounded. Analogues of it might be useful.)

Let us contemplate a few subfunctors and what is known about them. There are analogues of Cantor's theorem for the function sending x to the set of all its wellorderable subsets, and the set of its transitive subsets. There is no analogue for the function sending x to the set of all its finite subsets. This might suggest that the availability of a Cantor-like theorem depends on the function not having finite character, but then one reflects that there is no Cantor theorem for the function sending x to the set of all its countable subsets, nor indeed the set of subsets of size κ for any *fixed* κ. Indeed in ZF one can construct fixed points for all these functions. The key seems to be that if the function has *bounded* character then one can prove in ZF that there is a fixed point. If it has unbounded character one can derive a paradox. The slightly disquieting feature is that the available proofs of Cantor-like theorems do not all seem to be the same.

(The meaning of Hartogs' theorem seems to be that 'wellordered' does not have bounded character)

It would be nice to see more clearly for which f one can find fixed points in ZF, and for which fs one can prove Cantor-like theorems.

Propositions to consider:

The book sez: Let us say I is a *notion of smallness* if

1. Any subset of an I thing is also I
2. Any union of I-many I-sets is I (if $f : X \to Y$ is onto, and Y is small, and for all $y \in Y$, f^{-1}"$\{y\}$ is small, then X is small.)
3. V is not I

Could also consider:

I must be nonprincipal and contain all singletons! Closed under bijective copies.

surjective image of smalls are small, or (weaker) Not mapping onto V.

If you have as many small subsets as subsets then you are small;

closed under unions of small chains;

The union of a wellordered number of small sets is small.

The set of all small sets is small

The power set of a small set is small.

If X is not small, there is a map from X onto V where the preimage of every singleton is small.

$\in$ restricted to small sets should be wellfounded.

Is there a notion of small such that for every x either x has as many small subsets as subsets (in which case x is small) or has as many small subsets as singletons (in which case it isn't)? This is stratified! Sounds a bit like GCH,

So consider the operation $G =: \lambda S.\{x : |\mathcal{P}(x)| = |(\mathcal{P}(X)) \cap S|\}$.

I can't see any reason why $G(S)$ should be downward closed if S is (and we will need this) so redefine G:

$$G =: \lambda S.\{x : (\forall x' \subseteq x)(|\mathcal{P}(x')| = |(\mathcal{P}(x')) \cap S|)\}.$$

Or we could even try the much weaker

$$G =: \lambda S.\{x : (\exists x' \supseteq x)(|\mathcal{P}(x')| = |(\mathcal{P}(x')) \cap S|)\}.$$

Anyway: here is something to think about. We have a notion of smallness, and we keep on making it weaker and weaker by iterating some homogeneous operation. We start off with something that isn't self-membered, like finite. We might reach something trivial like V, which *is* self-membered. Now we can't ask for the first stage at which it becomes self-membered, but for any self-membered stage S, we can enquire about the stage at which S becomes small

Consider Boffa's set: the least nonempty set closed under wellordered unions. This is a special case.

A message from Jeff Egger

Boolos JPL v 26 pp237-9

Let X, Y be sets. Then $\mathcal{P}(X)$ and $\mathcal{P}(Y)$ are, as you well know, complete boolean algebras. Moreover if f is a function $X \to Y$ then $j(f)^{-1} : \mathcal{P}(Y) \to \mathcal{P}(X)$ is a homomorphism of complete boolean algebras. In particular, it preserves *all* meets and *all* joins. (I remember proving this as a first-year undergraduate exercise.) Because $j(f)^{-1}$ preserves all meets, it has a left adjoint $\exists_f : \mathcal{P}(X) \to \mathcal{P}(Y)$ and because it preserves all joins, it has a right adjoint $\forall_f : \mathcal{P}(X) \to \mathcal{P}(Y)$.

Now $\exists_f$ turns out to be the same as the direct-image map:

$$(\exists_f)(A) = \{f(a) : a \in A\}$$
$$= \{b \in Y : (\exists a \in A)(f(a) = b)\}$$
$$= \{b \in Y : (\exists a \in X)(f(a) = b \wedge a \in A)\}$$

Why have I written $\exists_f$ in terms of such a complicated formula? Because it's my mnemonic device for remembering the formula for $\forall_f$!

$$(\forall_f)(A) = \{b \in Y : (\forall a \in X)(f(a) = b \to a \in A)\}$$

The point of this is that there are (at least) three "powerset functors". Unfortunately there is no standard convention for naming or notating them: I use **Sub** to denote the (contravariant, i.e. $Set^{op} \to Set$) functor

$$x \mapsto \mathcal{P}(x) \qquad f \mapsto f^{-1}$$

$\exists P$ for the functor

$$x \mapsto \mathcal{P}(x) \qquad f \mapsto \exists_f$$

and $\forall P$ for the functor

$$x \mapsto \mathcal{P}(x) \qquad f \mapsto \forall_f$$

Every topos has an analogue for each of Sub, $\exists P$ and $\forall P$.

Sub is regarded as important and comparatively well-understood; $\exists P$ is regarded as important and comparatively not well-understood; $\forall P$ is regarded as unimportant and not understood at all. In fact, people do tend to refer to $\exists P$ as "the" covariant powerset functor, despite the fact that $\forall P$ also fits that description.

Now a subfunctor of $\exists P$ (which is what I am studying) consists of $Q(x) \subset \mathcal{P}(x)$ for every set x

SUCH THAT

$A \in Q(x) \to \exists_f(A) \in Q(y)$ whenever $f : x \to y$ is a function.

TTFN, Jeff.

The extension of Q must be closed under hom (not subsets) eg Kfinite

A message from Greg Kirmayer

From t.forster@dpmms.cam.ac.uk Thu Apr 27 11:07:09 2000

Greg, despite my retraction i now think i can prove that the number of small sets is large in relation to the set of singletons. I'm glad i took up this line of thought beco's i am now satisfied that i *really* understand the theorem of Tarski about the set of wellordered subsets of a set. Here goes:

Suppose there is a bijection π between a set X and the set $S(X)$ of its small subsets. Then the structure $\langle X, \in \circ \pi \rangle$ is a model for some sort of set theory. The collection of things that are Von Neumann ordinals of this structure cannot be coded in it. So that is a wellordered subset of X that is not small. So this is what Tarski proved: if X is the same size as $S(X)$, it has a wellordered subset that is not small. Specifically, since we can take small to be wellordered, no X is the same size as the set of its small subsets.

Applied to the NF case this shows that there can be no bijection between the set of singletons and the set of small sets, where here small means NF-small, not mapping onto V. Not terribly surprising, but better than nothing. I'll have to check what happens if we assume a surjection from the singletons to the small sets rather than a bijection.

From gkirmayer@cmpmail.com Fri Apr 28 18:38:23 2000

Thomas,

I think Zermelo showed that if $F : \mathcal{P}(X) \to X$ then there is a unique subset W of X and well-ordering $<$ of W such that $F\{y : y < x\} = x$ for all $x \in W$, and $FW \in W$.

Suppose now that $f : P_1(X) \to P(X)$ is injective. Let a be an element of X. Define $F : P(X) \to P_1(X)$ by $F(Y) = \{y\}$ if $f\{y\} = Y$, and $F(Y) = \{a\}$ otherwise. Let W and $<$ be the sets as above in Zermelo's theorem. Then $F(W) = F\{y : y < F(W)\}$. W and $\{y; y < F(W)\}$ are different because FW is in the first and not the second. Since f is injective at least one of them is not in the range of f (if $f\{y\} = W$ then $y = F(W) = F\{y : y < F(W)\}$ and thus $\{y : y < F(W)\}$ is not in the range of f).

As you can see this argument does not need that the range of f is downward closed or closed under the addition of singletons. The argument you sent me did not require this either. As to whether the above paragraph can be attributed to Zermelo, I do not know. I first learned about the above corollary of Zermelo's theorem from a paper by Kanamori in the September 1997 issue of the Bulletin of Symbolic Logic. Kanamori's paper has some historical information which might be of interest to you.

Best Wishes,
Greg

A message from Daniel Mahler

From mahler@cyc.com Thu May 11 18:31:44 2000

External motivation is certainly helpful: I dug up and looked at the Reynolds paper last night. it is "Polymorphism is not set theoretic". It looks like the models should exist in NF and/or relatives. The proof consists of showing that if system F has a set theoretic mode then the operation $\lambda x.2^{2^x}$ has an least fixed point A meaning that $A = 2^{2^A}$ which is a contradiction in classical set theory. The proof however can be extended to any covariant type constructor expressible in system F. I believe this is the origin of the Girard-Reynolds correspondence between types in F and initial algebras. It has been a while since I have looked at categorical semantics but I believe the essence of the paper is that for a category to provide a "set theoretic" model of F, it must have a full cartesian closed subcategory which has initial fixed points for all "representable" covariant functors. A sufficient condition for this for the subcategory to have an initial object and directed colimits. In more set theoretic language this is more or less equivalent to a class of sets, containing the empty set, closed under function spaces, finite products and (I think) directed unions of classes of sets. If I am right about directed cocompletess, and directed unions, then everything should be fine since directed unions of classes can be obtained by taking the intersection of all upperbounds in V. I am a little nervous that I have imported some classical intuitions into the above though. I have Randall's book and saw some issues about the regarding the singleton constructor. I think the correspondence between directed colimits and directed unions assumes certain "obvious" isomorphisms.

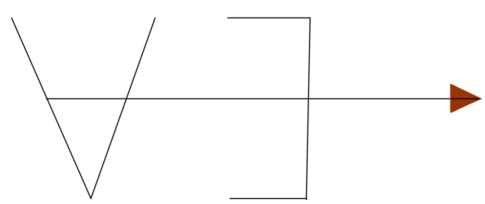
At any rate, my statements should be taken with a grain of salt: it has been a long time since I have looked at any categorical type theory seriously, and I am new to NF.

Daniel

Chapter 13

The Universal-Existential Problem

(One day i am going to write a novel in which the world is being taken over by a nasty yank megacorporation which peddles pseudopsychotherapeutic bullshit. It will be called *Universal Existential* and its logo looks something like



Its mission statement will contain a promise to free the world from *Angst*.)

Reading list for Diamant on the Universal-Existential problem in T$\mathbb{Z}$T.

P. Rouvelas, Increasing sentences in Simple Type Theory. Ann. Pure Appl. Logic, Vol 168, No 10, p. 1902–1926, 2017

P. Rouvelas. Partial type-shifting automorphisms. Logique et Analyse, Vol 60, No 238, p. 167–177, 2017

P. Rouvelas. Decreasing sentences in Simple Type Theory. Math. Logic Quart., Vol. 63, Issue 5, p. 342–363, 2017

Zachiri McKenzie, Anuj Dawar and Thomas Forster Decidable Fragments of the Simple Theory of Types with Infinity and NF Notre Dame J. Formal Logic 58 Number 3 (2017), 433–451.

Panagiotis Rouvelas Decidability results for stratified four-quantifier sentences

Throughout this discussion we will try to keep to the cute mnemonic habit – due to Quine – of writing a typical universal-existential sentence with the initial – universally quantified – variables as $\vec{y}$ ('y' for $y$ouniversal) and the existentially quantified variables as $\vec{x}$ – for E$x$istential). That was so that we can talk about y variables and x variables.

13.1 Stuff to be put in the right place

I've just noticed that $\forall\vec{y}\exists!x\phi(\vec{y},x)$ is actually $\forall^*\exists^*$. It's the conjunction

$$\forall\vec{y}\exists x\phi(\vec{y},x)\ \wedge(\forall\vec{y})(\forall x,x')((\phi(\vec{y},x)\wedge\phi(\vec{y},x'))\to x=x')$$

Zachiri, Diamant...

I've been thinking again about how to embed the canonical model of TST (with empty bottom level) into an arbitrary model $\mathfrak{M}$ of T$\mathbb{Z}$T in such a way as to include any given tuple of elements of $\mathfrak{M}$ in the embedded substructure. If we don't care about the behaviour of the inclusion embedding then this is a piece of cake. If we want the inclusion embeddding to be somehow nice then we have to take care.

I was sure i had written this out somewhere but i can't find it, so i'll have to work it out from scratch! I am for ever saying that the only people who understand the wheel are the poor buggers who keep on reinventing it so i am hoist by my own petard. Oh well, here goes.

The idea is to show that if $\mathfrak{M}\models$ T$\mathbb{Z}$T and $\vec{a}$ is a finite tuple of things in $\mathfrak{M}$, then there is a substructure of $\mathfrak{M}$ containing all the $\vec{a}$ which is an isomorphic copy of the canonical model of TST, the model with empty bottom type, and the inclusion embedding preserves the boolean operations. Let's start by considering the case of a single a by itself.

> There is a nice observation which Arran Fernandez made to me over lunch at Clare Hall once, when i was describing this problem to him. He shows how if you have n sets at level l of a model of T$\mathbb{Z}$T then you can find $n-1$ things at level $l-1$ that distinguish them. I'll stick (my doctored version of) his proof on the end of this as an appendix.

Let's describe how to embed the canonical model into an arbitrary model $\mathfrak{M}$ of T$\mathbb{Z}$T, starting at any level of $\mathfrak{M}$. We can worry later about incorporating designated inhabitants of $\mathfrak{M}$.

Send the empty set at the bottom (level 0) of the canonical model to the empty set of level l of $\mathfrak{M}$. Then send the empty set of level 1 of the canonical model to the empty set of level $l+1$ of $\mathfrak{M}$, and send V of level 1 of the canonical model to V of level $l+1$ of $\mathfrak{M}$. (This V in the canonical model is of course really $\{\emptyset\}$!) We now seek a subalgebra of level $l+2$ of $\mathfrak{M}$ with four elements. We seek a partition of level $l+1$ of $\mathfrak{M}$ into two pieces, one of which contains V and the other contains $\emptyset$. Call these pieces a and $\overline{a}$. Our subalgebra of level $l+2$ contains V and $\emptyset$ and a and $\overline{a}$. Naturally this can be done in infinitely many ways.

Next we seek a partition of level $l+2$ of $\mathfrak{M}$ into *four* pieces, one of which contains $\emptyset$, another contains V, a third contains a and the last one contains $\overline{a}$. These four pieces are the atoms of a boolean subalgebra of $l+3$. Again such a partition can be found in infinitely many ways, so we have lots of freedom of manœuvre.

And so on! We get a substructure of $\mathfrak{M}$ which is an isomorphic copy of the canonical model. The embedding preserves boolean operations ($\cap$ and $\cup$, $\emptyset$ and V and complementation, but not singleton, or B). And of course it's not transitive.

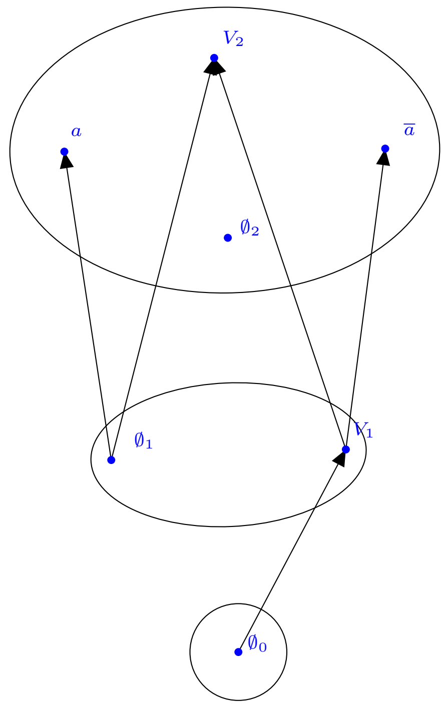
The idea is that, what with all this freedom of movement we have, that we can in this way incorporate into a copy of the canonical model any desired inhabitant of $\mathfrak{M}$, if we start low enough.

> Can we tweak the construction to preserve B as well...? Not clear. At level $l+3$ we have to ensure that every piece p of the partition-into-four-pieces has the property that either everything in p contains $\emptyset$ or nothing in p contains $\emptyset$, and either everything in p contains V or nothing in p contains V. That way $B(\emptyset)$ and $B(V)$ are both unions of pieces in the partition, and so are in the subalgebra.

Now to think about how we can control this construction to incorporate any given inhabitant of $\mathfrak{M}$. At stage one, one can incorporate $\emptyset$ and V (of level $l+1$). At stage 2 one can additionally incorporate (at level $l+2$) any a such that $\emptyset \in a \longleftrightarrow V \notin a$. (Let us say that such an a is *good*.. At stage 3 one can additionally incorporate (into level $l+3$) any a' which is contains some but not all of $\emptyset$, V, a, $\overline{a}$ where a is good. (Let us say that such an a' is *good*$'$, and that the collection of good$'$ sets obtained from a good set in this way is a good$'$ family).. At stage 3 one can additionally incorporate any set a that is a union of some but not all of a good$'$ family.

But of course what we want to do is start from a designated member a at some level of $\mathfrak{M}$, and hope to be able to incorporate it into a substructure that is an isomorphic copy of the canonical model of TST.

If a is V or $\emptyset$ at the level it lives on, we're happy. But, if it's neither, the substructure we are trying to build will contain at least *four* things from the level at which a lives, namely V, $\emptyset$, a and $\overline{a}$. The level below that level (let us say that a lives at level l) will contain at least $\emptyset$ and V, so if one of a and $\overline{a}$ contains one of V and $\emptyset$ (of level $l-1$ of course) then we are OK. The substructure we want will have empty bottom level; its level 1 will contain $\emptyset$, its level 2 will contain $\emptyset$ and V (by which we mean $\emptyset$ and V of level $l-1$ of $\mathfrak{M}$). Its level 3 will contain $\emptyset$ and V and a and $\overline{a}$. Suppose without serious loss of generality that $\emptyset \in a$ and $V \notin a$. Then we get the following picture:



We need to think a little bit about how we label the 16 points in the ellipse that sits above the top ellipse in this picture. The top element is going to be labelled with (the infinite set) V_3 not (the finite set) $\{a, \overline{a}, V_2, \emptyset_2\}$. We need to think about injective homomorphisms of boolean algebras.

However if one of a and $\overline{a}$ contains *both* V and $\emptyset$ then we still have work to do. Or rather, we need some luck. What sort of luck are we hoping for? Well, if there are $b \in a$ and $b' \in \overline{a}$ which can be distinguished in the way we were trying to distinguish a and $\overline{a}$ then we are in luck. We'd be in luck if $\emptyset \in b$ but $\emptyset \notin b'$... beco's then we can build a substructure (whose level 0 is empty) where level 1 contains the empty set and level 2 contains $\emptyset$ and V, level 3 contains $\emptyset$ and V (of course) and b and $\overline{b}$. Level 4 will contain eight things.

But perhaps a is closed under complementation! Or not closed under complementation but still have the bad feature than whenever b and $\overline{b}$ both belong to a then one of them contains both V and $\emptyset$! It is at this point that the multiplication of possibilities starts to make my head spin and i realise that i have to pass this problem on to brains that are younger and more nimble than mine.

13.2 Distinguishing between sets by points, a note from Arran Fernandez

Duplicates material on p 355

Arran Fernandez
20 May 2016

Abstract

For any collection of n distinct sets, at most $n-1$ points are sufficient to distinguish between the sets in such a way that each set contains a different selection of the points.

Theorem 30 *Let $S_1, S_2 \ldots, S_n$ be a collection of n distinct sets. Then we can choose elements $a_1, a_2, \ldots, a_{n-1}$ such that any two S_i can be distinguished by looking at which of the a_k they contain.*

More formally: $\forall$ sets $S_1, S_2, \ldots, S_n$ with $S_i \neq S_j$ for all $i \neq j$,
$\exists A = \{a_1, a_2, \ldots, a_{n-1}\} \subseteq \bigcup\limits_{i \leq n} S_i$ *for $i \neq j$, $S_i \cap A \neq S_j \cap A$.*

Proof: .

We proceed by induction on n. The base case $n = 2$ is trivial: for two distinct sets S_1 and S_2 , one of $S_1 \setminus S_2$ and $S_2 \setminus S_1$ must be non-empty, so let a_1 be any element of one of these sets.

Assume therefore that we have sets $S_1, \ldots S_n$ for some fixed $n \geq 2$ and a set $A = \{a_1, a_2 \ldots, a_{n-1}\} \subseteq \bigcup\limits_{i \leq n} S_i$ of size $n-1$ such that $S_i \cap A \neq S_j \cap A$ for all distinct $i, j \leq n$.

Let S_{n+1} be a new set distinct from all the other S_i, and let B be $S_{n+1} \cap A$. Now we have a set $B \subseteq A$ and, by our assumption, there can be at most one $i \leq n$ such that $S_i \cap A = B$. If there is no such i, then the $S_i \cap A$ are pairwise distinct for $i = 1, \ldots, n, n+1$ and we are done.

So let us assume there is such an i. Without loss of generality, say $i = 1$, so that $S_1 \cap A = B = S_{n+1} \cap A$. Now S_1 and S_{n+1} are distinct sets, so (as in the $n = 2$ case) there exists an element a_n which is in one of them but not the other. Let $A' = A \cup a_n$; this is a set of size n. Clearly the sets $S_1 \cap A_0, \ldots, S_n \cap A_0$ are pairwise distinct, since $S_1 \cap A, \ldots, S_n \cap A$ are. Also $S_{n+1} \cap A_0$ is distinct from $S_1 \cap A_0$ (via the element a_n) and from each of $S_2 \cap A_0, \ldots, S_n \cap A_0$ (since $S_{n+1} \cap A = B$ is distinct from each of $S_2 \cap A \ldots S_n \cap A$). Thus we have a set $A' = \{a_1 \ldots, a_{n-1}, a_n\} \subseteq \bigcup_{i \leq n+1} S_i$ of size n which distinguishes all the sets $S_1 \cap A_0 \ldots S_n \cap A_0$, $S_{n+1} \cap A_0$ from each other, and the induction is complete.
■

It's just struck me (it should've struck me earlier) that the inclusion map from the substructure that Arran builds preserves $=$ and $\in$ but doesn't preserve $\subseteq$ and it can't. Suppose at some level i consider a lot of values of B. To distinguish any two such objects i need two things, to witness the two facts

$B(a) \not\subseteq B(b)$ and $B(b) \not\subseteq B(a)$. I think there is no nice parsimonious way of reusing witnesses ...

Does Arran's argument show that in NF (and possibly some weaker systems) every concretely finite set has a concretely finite superset which is extensional? Perhaps not. When you add $n-1$ elements to discriminate your original n points, at the next stage you have to discriminate between $2n-1$ rather than $n-1$ so the process might not converge. Now, for two concrete naturals $n < m$ there is a $\forall^*\$$*formulathatsaysthatanyywith*—y— = n*thereisanextensional*x $\supseteq y$ with $|x| \leq m$. I can't see any of those $\forall^*\exists^*$ formulæ being theorems (or even consistent...?) but can one omit the type of their negations?

To be clearer about it.... For each n consider the type

$$E_n = \{E_{n,m} : m \in \mathbb{N}\}$$

where $E_{n,m}$ says there is a set x with $|x| = n$ and no superset of x of size $\leq m$ is extensional.

Can we find a model omitting every E_n?

For every $2 \leq n < k$ there is a $\forall^*\exists^*$ formula $E(n,k)$ that says that any set of size n has an extensional superset of size k. How many of these $E(n,k)$ are provable? How many are consistent? How many of the consistent ones can be proved consistent by permutation models?

Well, $E(n, n+1)$ is always false, because we can always find n pairwise disjoint sets. $E(n,m)$ and $E(m,k)$ imply $E(n,k)$. Also if $n < m < k$ then $E(m,k) \rightarrow E(n,k)$: if you want to expand an n-set to an extensional k-set just pump it up to an m-set and invoke $E(m,k)$. So perhaps we should write '$E(n,m)$' rather for "every set with $\leq n$ members has an extensional superset with no more than m members". Makes it more obvious that it's increasing in first argument.

This does raise the possibility of a fast-growing function. If it is consistent wrt NF that every finite set has a finite superset.... This has some arithmetical consequences. If X is extensional then $x \mapsto x \cap X$ injects X into $\mathcal{P}(X)$ giving us $|x| \leq |\mathcal{P}(X)|$ (and in fact $|x| < |\mathcal{P}(X)|$ since equality is impossible for finite sets). So if every finite set has a finite extensional superset then $(\forall n \in \mathbb{N})(\exists m \geq n)(m < 2^{Tm})$. If $m < 2^{Tm}$ does m contain an extensional set? It's a bit like asking whether every such m contains a transitive set?

13.2.1 Does every finite set have an extensional finite superset?

No!

A reply from Farmer S on StackExchange (doctored by me) ...

We will construct a set $p = \{a, b\}$ with no extensional finite superset. First define sets n^* for $n \in \mathbb{N}$ recursively by:

> $0^* = \emptyset$, $1^* = \{0^*\}$, $2^* = \{1^*\}$, $3^* = \{0^*, 2^*\}$, $4^* = \{1^*, 3^*\}$, etc, so that

> $(2n+1)^* = \{0^*, 2^*, \ldots, (2n)^*\}$, and $(2n+2)^* = \{1^*, 3^*, \ldots, (2n+1)^*\}$.

Then let
$a = \{2n^* : n \in \mathbb{N}\}$,
$b = \{(2n+1)^* : n \in \mathbb{N}\}$ and finally
$p = \{a, b\}$.

The idea is that p will be a finite set with no finite extensional superset.

Suppose that $p \subseteq q$ and q is finite and extensional. There must be some $n^* \in q$, in order to distinguish between a and b. Let N be largest such that N^* is in q. If $N = 2n+1$ then $N^* \cap q = a \cap q$, but then q is not extensional. Likewise with y if $N = 2n$. Contradiction.

F's construction works "locally" in any model of NF to show that for every concrete n there is a pair p that has no extensional superset of size $\leq n$.

I think the idea is to prove by induction that if the pair p contains n^* and some m^* for $m < n$ then any extensional superset $q \supset p$ must contain all of 1^* $\ldots n^*$.

Certainly true for $n = 3$.

Now suppose true for n, and suppose the pair to contain $(n+1)^*$ and m^* for some $m \leq n$. To distinguish these two we have to add some $k \leq n$. But now we have two stars in our set, and we can invoke the induction hypothesis. I don't quite see how to get n^* but something like this will work.

■

Presumably it's also true that for any n and k we can find a collection of n things with no extensional superset of size $< k$.

Notice that this proof can be carried out in NF0, since all the n^* (for n concrete) are NF0 terms. However the original construction, of the countable sets a and b, cannot be carried out in NF0. So it may be consistent with NF0 – or even NF – that every finite set has a finite extensional superset.

On the face of it there is a connection between "every finite set has a finite extensional superset" and a $\forall^*\exists^*$ scheme that has things like "every set of size n has an extensional superset of size k"(where $k > n$ both concrete). However this scheme never gets started beco's of the finite cases arising from F's counterexample. However it might be the case that the term model for NF0 satisfies "every finite set has a finite extensional superset" and it might even be consistent with NF. Why? Well, it has sort-of AE character and NF is supposed to welcome AE sentences true in the term model for NF0. May be stretching it a bit tho'.

Presumably every model of NFC has a permutation model in which we can construct F's counterexample. It might be worth checking how much of the axiom of counting one needs to construct a counterexample. What are the invariant consequences of "there is a finite set with no finite extensional superset". Might it be that F's counterexample is in some sense essential? That any counterexample will resemble it closely enuff to imply Counting?

The idea is to build a permutation model containing two sets `Odd` and `Even`. So i think we consider the inhomogeneous function F that takes a toset $\langle X, \subseteq_X \rangle$ and returns the toset of (carrier sets of) proper initial segments ordered by $\subseteq$. Then we want a permutation model containing a fixed point for F^2. Well, *actually* not *exactly*. We also need the inhomogeneous function H that takes a toset $\langle X, \subseteq_X \rangle$ and returns the toset of (carrier sets of) *nonempty* proper initial segments ordered by $\subseteq$. Then we want a permutation model containing a fixed point for $F \cdot G$. The von Neumann ω is of course a fixed point for F. Recall that the existence of the von Neumann ω implies the axiom of counting; i would expect that the existence of F's counterexample, too, implies Counting ... and by the same method.

To be precise. Consider the function that sends each member n of `Odd` to $\{\{i\}\}$ where i is the $\subseteq$-largest thing in `Odd` that is a member of a member of n. The graph of this function is a set, co's it has a stratified definition. And it is $\iota^2 \restriction$`Odd`. That will give us Counting. So the axiom of counting is equivalent to the possible existence of F's counterexample.

If you like, you can think of this construction as an implementttaion of $\mathbb{N}$ somewhat in the style of von Neumann. Every even number is the set of all odd numbers below it and every odd number is the set of all even numbers below it.

In NF0..?

We'd be looking for something like this. Take the rank of a NF0 term to be the depth of nesting of Bs. Then show that if t_1 and t_2 are distinct terms then there is a witness to their symmetric difference that is of lower rank than either of them. The term model is extensional, so this isn't an absurd thing to be looking for.

[Here's a simple illustration. We want to show that $\{B(x), B(y)\}$ can always be extended to an extensional superset. Quite how we do it will depend on relations between x and y; in some cases $\{B(x), B(y)\}$ is already extensional. One case where it isn't is when $x \in x \in y \in y \in x$. We consider the two terms $B(x)$ and $B(y)$. Then we add the two elements $B(x) \setminus \{B(y)\}$ and $B(y) \setminus \{B(x)\}$. The resulting four-element set is extensional. Of course this isn't a proof or anything like it, but it may be a straw in the wind.]

The symmetric difference of two terms is just another term and i think every term has an inhabitant of lower rank.

Here's the interesting possibility. Suppose some scheme of axioms like "every set with n members has an extensional superset of size k" holds in the term model for NF0. Then the universal-existential conjecture will tell us that it's consistent with NF, at no cost. But then we run up against Farmer S's fact and refute the axiom of counting. But one has the very strong feeling that the universal-existential scheme has nothing to do with unstratified cardinal arithmetic.

So i suspect that what will happen is that the term model for NF0 satisfies "every finite set has a finite extensional superset" but does not satisfy any of the $\forall^*\exists^*$ sentences "every set of size n has an extensional superset of size k" (for concrete n and k). Then the scheme is not required by the $\forall^*\exists^*$ conjecture part 5 to be provably consistent by permutations wrt any invariant extension of NF, so everything will be in order.

So my guess is that "there is a pair with no finite extensional superset" is not a theorem of NF, but that any model of NFC has a permutation model in which Farmer's construction can be run.

In Feb 2023 i was trying to reconstruct this in conversation with Diamant. It now occcurs to me (as i look up the discussion from back then) that the singleton of the von Neumann ordinal ω is a finite set with no finite extensional superset. Not quite! (tho' that may have been Farmer S's point of departure) that singleton is (vacuously!) extensional itself – all singletons are.

13.3 A jumble of versions of the clever proof

THEOREM 31 *Every $\forall^*\exists^*$ sentence true in arbitrarily large finitely generated model of TST is true in all infinite models of TST.*

Proof: The key is to show that every model of TST can be obtained as a direct limit of finitely generated models of TST. The hard part is to find the correct embedding.

Let $\mathfrak{M}$ be a model of TST. We will be interested in finite subthingies characterised as follows. Pick finitely many elements $x_1 \ldots x_k$ from level 0 of $\mathfrak{M}$; they will be level 0 of the finite subthingie. Then take a partition of level 1 of $\mathfrak{M}$ for which the x_i form a selection set (a "transversal"). The pieces of this partition are the atoms of a boolean algebra that is to be level 1 of the finite subthingie. That gives us level 1 of the subthingie. To obtain level 2 we find a partition of level 2 of $\mathfrak{M}$ such that the carrier set of the boolean algebra we have just constructed (which is level 1 of the subthingie) is a selection set for it. The pieces of this partition are the atoms of a boolean algebra that is to be level 2 of the finite subthingie. Thereafter one obtains level $n+1$ as a boolean alegbra whose atoms are the pieces of a partition of level $n+1$ of $\mathfrak{M}$ for which level n of the subthingie is a transversal.

There is, at each stage, an opportunity to choose a partition, so this process generates not *one* subthingie from the finitely many elements $x_1 \ldots x_k$ from level 0 of $\mathfrak{M}$, but infinitely many. This means that the family of subthingies has not only a partial order structure but also a topology. Choosing n things from level 0 does not determine a single finite subthingie, co's you have a degree of freedom at each step (when you add a new level). It's a kind of product topology, where each finite initial segment (a model of TST_k with n things at level 0) determines an open set: the set of its upward extensions.

Is the obvious inclusion embedding an example of what Richard calls an almost-$\forall$ embedding?

The long-term aim is to take a direct limit, and we want this direct limit to be $\mathfrak{M}$ itself, so we must check that every element of $\mathfrak{M}$ can be inserted into a subthingie somehow.

Clearly any finite set of elements of level 0 of $\mathfrak{M}$ can be put into a finite subthingie, but what about higher levels? We prove by induction on n that every finite collection of things of level n can be found in some finite subthingie or other.

The induction step works as follows. We have a subthingie $\mathfrak{M}_1$ and we want to expand it to a subthingie $\mathfrak{M}_2$ that at level $n+1$ contains finitely many things $x_1 \ldots x_k$. To do this we have to refine the partition of V_n that is the set of atoms that $\mathfrak{M}_1$ has at level $n+1$ so that every x_i is a union of pieces of the refined partition. There are only finitely many x_i so any refinement that does the job has only finitely many pieces. Identify such a refinement, and pick a transversal for it that refines the set which is level n of $\mathfrak{M}_1$. This transversal is a finite set of things of level n, and we can appeal to the induction hypothesis. ■

Next we ask, suppose at each level from 2 onwards, instead of picking a partition of level n of $\mathfrak{M}$ to be the set of atoms of the boolean algebra at level n, we simply take B"level $n-2$ to be a set of generators for the boolean algebra of level n? We lose a degree of freedom but we get better behaviour of the embedding, since this ensures that it preserves B. Can we still ensure that every element of $\mathfrak{M}$ appears in the direct product?

Unfortunately the answer to this can be easily shown to be 'no' since, for the answer to be 'yes', one would have to be able to express every element of level n of $\mathfrak{M}$ – for n as big as you please – as a $\{B, \cup, \cap, \setminus, V\}$-word in the finitely many elements chosen to be level 0 of the subthingie and the elements of the partition that are to be level 1. That is clearly not going to happen . . . unless $\mathfrak{M}$ is infinite, of course.

This proof is essentially the correct general version of the proof in the book where the same result is claimed only for countable models. This proof is more general and easier to follow. The converse problem remains: can we show that every $\forall^*\exists^*$ sentence true in even one model of T$\mathbb{Z}$T is true in the term model for T$\mathbb{Z}$T0?

The latest wheeze is to show that $\Pi_3^{B,\iota}$ things generalise downward to the term model for T$\mathbb{Z}$T0. Suppose $(\forall \vec{z})(\exists \vec{x})(\forall \vec{y})\phi$ is true in some model $\mathfrak{M}$ of T$\mathbb{Z}$T0. Then, however we instantiate the $\vec{z}$ to T$\mathbb{Z}$T0 terms, we hope that the resulting $\Sigma_2^{B,\iota}$ formula with parameters holds in the term model. So we hope that witnesses to the $\vec{x}$ variables can be found in the term model.

So consider $(\exists \vec{x})(\forall \vec{y})\phi$ where ϕ now contains parameters (from the term model) Let's process ϕ. . . . First we put it into CNF, and then we extract (pull to the front) all the atomic subformulæ that contain only x variables. These are not bound by the y quantifiers and do not need to be within their scope. ϕ now looks says "either the $\vec{x}$ are related to each other like this and they are related to the ys like thus-and-so, or . . . , and so on with finitely many mutually exclusive disjuncts. Since $\mathfrak{M}$ believes this to be the case, it believes precisely one of these can hold. So $\mathfrak{M}$ says that there are these $\vec{x}$ and they are related to each other like thus-and-so, and there are finitely many clauses we have to satisfy, all of them looking like $(\forall \vec{y})D$ where D is a disjunction of atomics and negatomics. We now have to select from the term model things suitable to be witnesses to the $\vec{x}$. The wriggle room comes from the fact that we don't have to literally satisfy all the $(\forall\vec{y})D$ clauses, but only all the substitution instances obtained by instantiating the $\vec{y}$ to T$\mathbb{Z}$T0 terms.

If you try this you find (at least i found) that mostly i could make do with NF_2 words but it's easy to see that that won't work in general.

There is some further processing we can do to the conjuncts/disjunctions inside ϕ... leave at the front of the formula the universal quantifiers over the xs of lowest type and import everything else. Then (inside that formula) leave outside the quantifiers over x variables of next lowest type and import everything else. The result is that each conjunct/disjunction ends up looking like

$$(\forall\vec{x}_1)(D_1 \vee (\forall\vec{x}_2)(D_2 \vee (\forall\vec{x}_3)(D_3 \vee \ldots)))$$

We will show that if Φ is universal-existential then $\Phi \to \Phi^+$ holds in every model of TST with at least n atoms, where n is finite and depends only on Φ. That will suffice to establish that $\Phi \to \Phi^*$ is a theorem of T$\mathbb{Z}$T.

We know of old that when dealing with universal-existential sentences we need concern ourselves only with those Φ that are of the form $(\forall\vec{y})(\psi(\vec{y}) \to (\exists\vec{x})\theta(\vec{x},\vec{y}))$ where ψ is a conjunction of atomics and negatomics, and θ is quantifier-free; all universal-existential sentences considered below will be assumed to be of this form.

We want to prove that $\mathfrak{M}$ (a model of TST) satisfies $\Phi \to \Phi^*$. We assume that $\mathfrak{M} \models \Phi$ and that the variables in Φ of lowest level are of level 0. We want to infer that $\mathfrak{M} \models \Phi^*$. The new idea is that it is not neccessary to find a particularly clever type-raising injection that deals with all Φ; it isn't even necessary to find an h for each Φ. Our h will depend on the instantiations of the y variables in Φ. Probably worth being clear about this. We want to show

$$\forall\vec{y}\exists\vec{x}\phi \to \forall\vec{y}\exists\vec{x}\phi^+.$$

We do this by UG on 'ϕ'. So: fix ϕ we want to prove

$$\forall\vec{y}\exists\vec{x}\phi \to \forall\vec{y}\exists\vec{x}\phi^+.$$

So we assume $\forall\vec{y}\exists\vec{x}\phi$ and aspire to prove $\forall\vec{y}\exists\vec{x}\phi^+$. We then prove this by UG on '$\vec{y}$'.

We require of our injection h that it lift types and that it respect $\in$: $(\forall u, v)(u \in v \longleftrightarrow h(u) \in h(v))$. For this it is necessary and sufficient that $h(v)$ always be a (not neccessarily proper) superset of $h``v$, with the property that $h(v) \setminus h``v$ be disjoint from the range of h.

So, let $\vec{y}$ – elements of $\mathfrak{M}$ – be some tuple of instances of the '$\forall\vec{y}$' in Φ^*. Clearly if we can find a type-raising injection $h : \mathfrak{M} \hookrightarrow \mathfrak{M}^*$ with the feature that every y in our tuple is a value of h then we are home and hosed.

To start with, things are comparatively straightforward. For reasons which will become clear (they may be clear already) h is going to have to be setlike, and the best way of doing that is to ensure that it is definable. So, for each $y_1 \ldots y_n$, (where $y_1 \ldots y_n$ are the y objects of level 1 in Φ^* (the lowest level)) we designate a thing of level 0 to serve as $h^{-1}(y_i)$ *and we give it a name* – 'a_i', say. This will ensure that h is definable with parameters (the various ys and the a_i) and is therefore setlike. We are now in a position to announce what h does to things in level 0: it sends a_i to y_i and sends everything else of level 0 to its singleton (or anything definable – it really doesn't matter as long as it's an injection).

That was painless. Suspiciously easy, you might think! Thereafter how do we define h on (things of) level $n+1$ – on the assumption that we have defined it on (things of) level n? Well, there are various y objects of level $n+2$ that have to be values of this h. So y is h of something. . . but of what? Here the clue is that h is an $\in$-homomorphism. This tells us that $h(v)$ is always a superset of $h``v$. What do we know about $h(v) \setminus h``v$? We have already remarked that it mustn't contain any values of h. So, if y is to be h of anything it must be h of $h^{-1}``(y \cap h``V)$. If y is at level $n+2$ then $h^{-1}``(y \cap h``V)$ is of level $n+1$, so that reveals to us h of at least *some* things of level $n+1$. (Notice that for this to work we absolutely need to ensure that h remains setlike at each stage, and this is why we want it to be definable.) The other elements u of level $n+1$ can be sent to $h``u$, but of course any superset of $h``u$ obtained by adjoining *non*values of h will do – as long as the h that results thereby is setlike.

This extra flexibility in constructing h seems to be of no use to us, and with our fairly limited aims it isn't, admittedly. However, we might be trying to (upwardly) preserve formulæ in $\forall^*\exists^*\Gamma$ for some class more demanding than just the quantifier-free formulæ, and in such an endeavour the extra flexibility might turn out to be very useful indeed.

I hope it is now clear how to show that $\forall^*\exists^*$ sentences generalise upward in all sufficiently large models of TST. Let Φ be any universal-existential sentence as above, and fix a sufficiently large model $\mathfrak{M} \models$ TST. For any tuple of ys instantiating $\psi(\vec{y})$ we devise an injection h as in the above construction. Now invoke Φ in $\mathfrak{M}$, obtaining witnesses to the x variables, and apply h to all those witnesses. These will be witnesses to the x variables in Φ^*.

■

Notice that this does not (or at least does not obviously) resolve the question of whether or not T$\mathbb{Z}$T decides all $\forall^*\exists^*$ sentences. It does mean that every $\forall^*\exists^*$ is either true in cofinitely many finitely generated models of TST or is false in cofinitely many finitely generated models of TST. We know that every model of TST is elementarily equivalent to a countable model and that every countable model is a direct limit (colimit) of all finitely generated models, but there does seem to be the possibility that there could be a $\forall^*\exists^*$ sentence that is false in cofinitely many finitely generated models of TST but nevertheless true in some (but not all) models with an infinite bottom level.

13.4 Injections and Surjections

All a jumble at the moment

Zachiri,

Thanks for this. You have started me thinking, and reminded me of old tho'rts...

Cast your mind back to the proof you showed me on wednesday. You have a big model of TST, and a family of points in it, and you want to find a small model of TST and an injection from the small model into the big model which hits all those points. It's easy if the family is extensional, so the idea is to plump up the family to an extensional one, You show how to do that. Fine

I have two tho'rts on this

(i) I recall having had the same idea myself once, but i got stuck, beco's what i was trying was too ambitious. Suppose the big model is a model of T$\mathbb{Z}$T! How can you be sure that the downward propagation ever terminates? There's no reason why it should, but might it happen, if you are very clever in your choice of witnesses to symmetric difference, that it eventually hits the empty set. Suppose one had an E*A* sentence such that, whatever witnesses one chose, and however one propagated downward, one never reached the empty set. Wouldn't there be something really weird going on?

(ii) Your downward propagation idea is fine. Tickety-boo. However, i think one can do something even better. Recall that every level of your model of TST is a boolean algebra. So, when you propagate, add enuff stuff to ensure that at any one level of the extended family, the things at that level form a sub-boolean-algebra of that Level. As far as i can see, this is entirely painless. And what does it get for us? Presumably it means that: whatever we could prove for sentences of the form $\exists\vec{y}\forall\vec{x}\phi$ where ϕ is quantifier-free, we can now prove for such formulae where phi is allowed to contain $\cap$, $\cup$, $\setminus$ and $\subseteq$.

Is that not so?

On Nov 1 2013, Zachiri McKenzie wrote:

Dear Anuj (cc'ed Thomas),

I hope that this finds you both well!

It is Friday afternoon and perhaps a good time to make a summary of where we are at:

So far we have shown that every $\exists^*\forall^*$ sentence is either true in finitely many finitely generated models or cofinitely many finitely generated models. Moreover, if an $\exists^*\forall^*$ sentence is true in any 'infinitely generated model' (model with an infinite base) then it is true in cofinitely many finitely generated models. This has the following consequences:

> * Every pseudo-finite model of TST satisfies the same EA sentences and this set of sentences is decidable (I suppose we already knew the latter).
>
> * The set of EA sentences true in any model of TST must be contained in the set of EA sentences true in the pseudo-finite models.

Thomas has also proved the following: Any AE sentence that is true in some model of T$\mathbb{Z}$T is true in the term model of T$\mathbb{Z}$T0.

Therefore, what we would like to do is show that the term model of T$\mathbb{Z}$T0 only satisfies the AE sentences true in the pseudo-finite models of TST...

Very best wishes,

Zach.

5/xii/2013

I've been thinking some more about these recent tho'rts of Zachiri's. Here is my take on them.

We have in our left hand a large model $\mathfrak{M}$ of TST, one with an infinite bottom level. (To keep things simple, but large-finite might come later). We want to establish that $\mathfrak{M} \models (\forall \vec{x})(\exists \vec{y})\phi(\vec{x}, \vec{y})$, where ϕ belongs to some syntactic class Γ.

To this end we point to a tuple of things in $\mathfrak{M}$ and think of them as inputs $\vec{x}$ to ϕ, and hope to find a tuple $\vec{y}$. The strategy for doing this involves finding a smaller model $\mathfrak{M}'$ (one that satisfies $(\forall \vec{x})(\exists \vec{y})\phi)\vec{x}, \vec{y}))$ plus an injection $h : \mathfrak{M}' \to \mathfrak{M}$, where h does two things.

(i) everything in our tuple must be in the range of h; and

(ii) h preserves all formulæ in Γ.

Then we copy our tuple down into $\mathfrak{M}'$ (using the fact that everything in the tuple is hit by h); then we find witnesses to the $\vec{y}$ inside $\mathfrak{M}'$, and then we copy them upstairs. Job done.

That, as i understand it, is Zachiri's Cunning Plan. And here is my take.

We have our tuple of $\vec{x}$ in $\mathfrak{M}$. The idea is to use these elements to build a substructure of $\mathfrak{M}$. We start at the top level of $\mathfrak{M}$ at which elements from $\vec{x}$ appear. This top level is a boolean algebra, and we consider the subalgebra generated by those top-level members of $\vec{x}$. The atoms of this algebra constitute a partition of this top level, and we add to the $\vec{x}$s of the next level down a representative from each element off the partition, and we carry on downwards until we have reached the bottom level of $\mathfrak{M}$ at which $\vec{x}$s appear. Now comes the clever bit. The boolean subalgebra we have at this level is still only finite, and it has only finitely many atoms. So we can find a partition of the same size as this partition-into-atoms-of-the-partition which is mapped onto it by a permutation, and such that each element of the image of the partition under this permutation contains a hereditarily finite set. We now continue our downward march, but this ruse has ensured that we eventually reach the empty set. The substructure we have thus constructed is a copy of the canonical model of TST with empty bottom level, with a twist in the middle induced by the permutation.

So M' is just the canonical model of TST with empty level 0. Now copy the $\vec{x}$ down and find $\vec{y}$ and copy them back up. But what formulae does our h preserve? Not just atomic formulae, but also all $\bigcup$, $\bigcap$, $\setminus$, $\emptyset$ and $\subseteq$.

The permutation of course doesn't change anything, so we seem to have proved:

Any $\forall^*\exists^*\Gamma$ sentence true in arbitrarily large finitely generated models of TST is true in all infinitely generated models, where Γ is the language containing not just $=$ and $\in$ but also $\bigcup$, $\bigcap$, $\setminus$, $\emptyset$ and $\subseteq$.

How does this sound?

We seem to need the permutation to get round the possibility that the downward propagation doesn't reliably seem to reach the empty set. But perhaps we can show that there is always a way of propagating downwards so as to reach the empty set.

13.4.1 Embedding the canonical model into every model

The aim is to show that not only can we always embed the canonical model in any $\mathfrak{M} \models$ TST, but we can ensure that given finitely many things $m_1 \ldots m_n$ in $\mathfrak{M}$ then we can arrange for them all to be in the range of the embedding. Or (to put it another way) in any model of TST any finite subset generates a substructure (not transitive!) that is a copy of the canonical model. (Not sure what this will do, but never mind).

Key observation (thank you Arran Fernandez!) is that whenever we have a set A of sets, with a set $D \subseteq \bigcup A$ of discriminators (which is to say that whenever $a \neq b \in A$ then $((a \texttt{ XOR } b) \cap D) \neq \emptyset$ – at least whenever a and b live at the same level) then, for any $a \notin A$, the set $A \cup \{a\}$ has a set of discriminators obtained by adding at most one new element to D.

Let's break off to prove this observation of Arran's. This is his proof.

Distinguishing between sets by points

Arran Fernandez Duplicates material on p 345

20 May 2016

Abstract

For any collection of n distinct sets, at most $n-1$ points are sufficient to distinguish between the sets in such a way that each set contains a different selection of the points.

Theorem 1.

Let $S_1, S_2 \ldots, S_n$ be a collection of n distinct sets. Then we can choose elements $a_1, a_2, \ldots, a_{n-1}$ such that any two S_i can be distinguished by looking at which of the a_k they contain. More formally: $\forall$ sets $S_1, S_2, \ldots, S_n$ with $S_i \neq S_j$ for all $i \neq j$, $\exists A = \{a_1, a_2, \ldots, a_{n-1}\} \subseteq \bigcup_{i \leq n} S_i$ for $i \neq j$, $S_i \cap A \neq S_j \cap A$.

Proof: .

We proceed by induction on n. The base case $n = 2$ is trivial: for two distinct sets S_1 and S_2 , one of $S_1 \setminus S_2$ and $S_2 \setminus S_1$ must be non-empty, so let a_1 be any element of one of these sets.

Assume therefore that we have sets $S_1, \ldots S_n$ for some fixed $n \geq 2$ and a set $A = \{a_1, a_2 \ldots, a_{n-1}\} \subseteq \bigcup_{i \leq n} S_i$ of size $n-1$ such that $S_i \cap A \neq S_j \cap A$ for all distinct $i, j \leq n$.

Let S_{n+1} be a new set distinct from all the other S_i, and let B be $S_{n+1} \cap A$. Now we have a set $B \subseteq A$ and, by our assumption, there can be at most one $i \leq n$ such that $S_i \cap A = B$. If there is no such i, then the $S_i \cap A$ are pairwise distinct for $i = 1, \ldots, n, n+1$ and we are done.

So let us assume there is such an i. Without loss of generality, say $i = 1$, so that $S_1 \cap A = B = S_{n+1} \cap A$. Now S_1 and S_{n+1} are distinct sets, so (as in the $n = 2$ case) there exists an element a_n which is in one of them but not the other. Let $A' = A \cup a_n$; this is a set of size n. Clearly the sets $S_1 \cap A_0, ..., S_n \cap A_0$ are pairwise distinct, since $S_1 \cap A, ..., S_n \cap A$ are. Also $S_{n+1} \cap A_0$ is distinct from $S_1 \cap A_0$ (via the element a_n) and from each of $S_2 \cap A_0, ..., S_n \cap A_0$ (since $S_{n+1} \cap A = B$ is distinct from each of $S_2 \cap A \ldots S_n \cap A$). Thus we have a set $A' = \{a_1 \ldots, a_{n-1}, a_n\} \subseteq \bigcup_{i \leq n+1} S_i$ of size n which distinguishes all the sets $S_1 \cap A_0 \ldots S_n \cap A_0$, $S_{n+1} \cap A_0$ from each other, and the induction is complete. ■

As Fernandez says, this means that, since any two distinct sets can always be distinguished by any one element of the symmetric difference, we can prove by induction that $n \in \mathbb{N}$ distinct sets can always be distinguished by $n - 1$ suitably selected members of their union.

This means that we can add new elements to our original stock of chosen elements of $\mathfrak{M}$, descending, and eventually we will be down to a single discr-minator, and then none. So we have a substructure of $\mathfrak{M}$ which contains all our chosen elements, and is extensional, and finite; however it is not yet (an isomorphic copy of) the canonical model with empty bottom level. And it's certainly not transitive! We now close under ... what exactly? Any level of $\mathfrak{M}$ is a boolean algebra under $\subseteq$, $\emptyset$, $\vee$ etc so – working upwards from the lowest level that our activities have populated – we

> (i) expand each level-of-our-construction to a sub-boolean-algebra of that level of $\mathfrak{M}$.
>
> (ii) We then populate the next level up with all subsets of the level we have just processed, and
>
> (iii) we add to the level *two* steps up, $B(x)$ for all x that we have constructed.

The result is an (intransitive) copy of the canonical model, which is a substructure of $\mathfrak{M}$ closed under the boolean operations, ι and B. Being thus closed, it is a substructure elementary for more than just $\in$.

This surely proves *something*, but what?

I think of it as a proof of concept. It shows that any finite set of elements of $\mathfrak{M}$ can be expanded into a copy of the canonical model. The challenge is to see if there is a way of tweaking Arran's construction into a construction that results not only in a substructure that is a copy of the canonical model but one that is embedded in $\mathfrak{M}$ in a nice way.

OK, what are we trying to do? We want to show that certain kinds of $\forall^*\exists^*$ sentences hold in all models of T$\mathbb{Z}$T as long as they hold in certain finitely generated models. So, working in some model $\mathfrak{M} \models$ T$\mathbb{Z}$T, we pick up a tuple instantiating the y variables of our formula $\forall\vec{y}\exists\vec{x}\phi(\vec{y},\vec{x})$

We extend this tuple into a substructure of $\mathfrak{M}$ that is a copy of some finitely generated model. We want the embedding to be nice, somehow. We then appeal to the fact that $\forall\vec{y}\exists\vec{x}\phi(\vec{y},\vec{x})$ holds in the finitely generated model, so there are witnesses to $\phi(\vec{y},\vec{x})$ and we then hope that the embedding is nice enuff for those witness to remain witnesses when we climb back up into $\mathfrak{M}$.

So we need to think very carefully about how we expand the tuple of instances of $\vec{y}$ to a finitely generated model.

Now: do we actually need Arran's observation? Why don't we think of our tuple as living in the lowest possible level? Well, even if we are happy to have atoms at bottom level we still have to have enough of them to distinguish between things higher up. What Arran's trick means is that, for any n, we can take the model into which we expand the tuple to be the finitely generated model with n atoms.

I think this will show that any $\forall^*\exists^*$ sentence true in infinitely many finitely generated models is a theorem of T$\mathbb{Z}$T. I think we already knew that; the significance of this trick is that if we can control the embeddings we can show that same for $\forall^*\exists^*\Gamma$ sentences over a slightly more complicated Γ.

One complication we have to keep track of is this. We consider a particular $\forall^*\exists^*$ sentence. For every n, each tuple $\vec{y}$ can be expanded to a copy of the model of TST with n atoms, but there is no reason to suppose that all these tuples appear at the same level. However for each formula there is a bound on the number of levels we have to consider. We are free to consider embeddings into higher levels but we don't have to.

SO: what we seem to be able to prove is the following. We are working in some model $\mathfrak{M} \models$ T$\mathbb{Z}$T. Fix some $\forall\vec{y}\exists\vec{x}\phi$ and some n. Then for any choice of $\vec{y}$ we can find a model of TST with n atoms into which we have embedded $\vec{y}$. We don't know at what level of this model the $\vec{y}$ appear, but we do know that there is some constant k (depending on the length of $\vec{y}$) such that we know it will be no higher than that. So, if we know that $\forall\vec{y}\exists\vec{x}\phi$ is provable at the first k levels of TST-with-n-atoms then we capture all tuples $\vec{y}$ and we know that $\forall\vec{y}\exists\vec{x}\phi$ holds in the model $\mathfrak{M}$ in which we are working. But $\mathfrak{M}$ was an arbitrary model of T$\mathbb{Z}$T so it's a theorem of T$\mathbb{Z}$T.

So we seem to have a proof of the following:

REMARK 68
For every quantifier-free ϕ and for every concrete n there is k such that if $\forall\vec{y}\exists\vec{x}\phi$ is true at the first k levels of the model with n atoms then $T\mathbb{Z}T \models \forall\vec{y}\exists\vec{x}\phi$

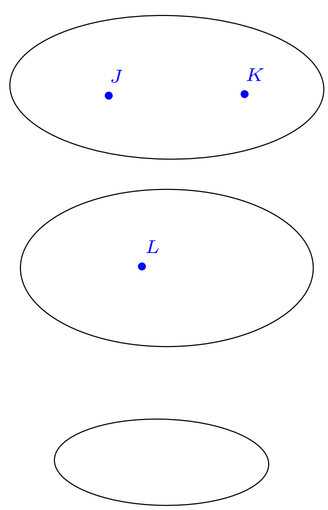
So the next thought is that if we construct the embeddings cleverly enough then we can relax the 'quantifier-free' condition on ϕ. It seems pretty clear that we can preserve the boolean operations. Can we get preserve all $\forall$ predicates?

That means preserving B. That would require our generated substructure to be closed under B. Now *that* propagates *upwards*. . . !

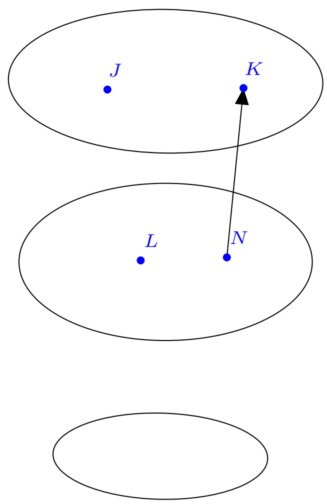
Let's think a bit about how this might work. Suppose the highest level inhabited by members of our tuple $\vec{y}$ is l. We populate V_{l-1} with elements to discriminate between them. Then we do V_{l-2}. Then we have to put Bs of all those V_{l-2} things into V_l. Now comes the crunch. We have to discriminate all these level l things; does this spawn an uncontrollable multitude of new inhabitants of V_{l-1}? Take the easy cases first. We need to discriminate between $B(a)$ and $B(b)$. So we need to find a thing in V_{l-1} that contains one of a and b but not the other. Have we got one already? No reason why we should; bugger.

But let's not give up *just yet*. Suppose a is there beco's $a \in x \setminus y$ and b is there beco's $b \in u \setminus v$. We want something like $x \setminus v$ to contain a but not b .... But it's clear that nothing like that can work. It would be OK if each level of the substructure contained all singletons of things in the next lower level of he substructure, but that involves some upward propagation too, and it's not clear that the processes will converge.

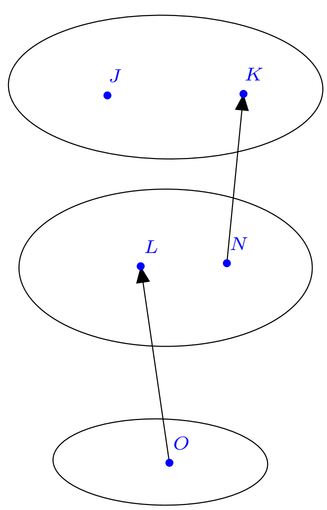
Let us draw a picture of this process, starting with three objects J, K and L



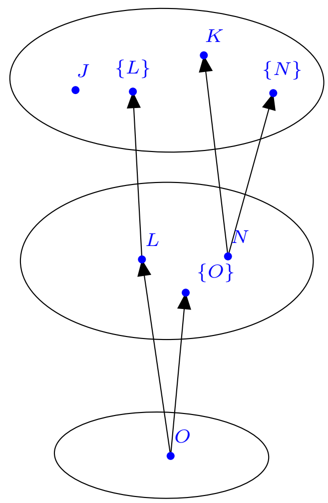
Now we add a new thing on the level below J and K to discriminate between them. Without loss of generality $N \in K \setminus J$.



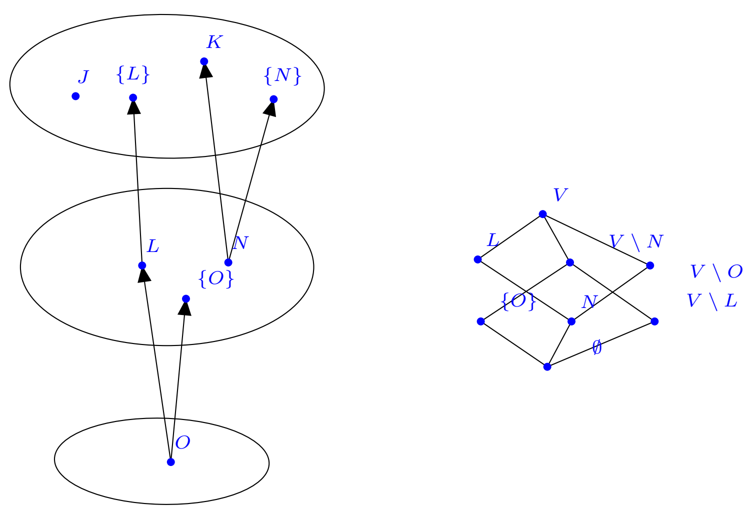
and we add O at the next level down to be a member of $L \setminus N$.



Then we add singletons:



Now we plump out the middle level to a boolean algebra:



Observe that some of the atoms of this b.a. are not singletons but i don't think that matters. Every element of the substructure has a singleton and that singleton is an atom. But – hang on – the structure we have is no longer a model of extensionality. We need to have *three* things in the level below to tell all distinct things apart.... Then it will never stop! So we can't do ι!!

13.4.2 Surjections again

If $\mathfrak{M}$ and $\mathfrak{N}$ are two natural models of simple type theory, with f a surjection from the bottom type of $\mathfrak{M}$ onto the bottom type of $\mathfrak{N}$, then we can lift f successively to surjections from the nth level of $\mathfrak{M}$ onto the nth level of $\mathfrak{N}$ by the obvious recursion: $f(x) =: f``x$. This shows that $\mathfrak{N}$ is a homomorphic image of $\mathfrak{M}$. This implies ambiguity for positive formulæ. Let us say a formula is *stable* if it is preserved both ways. Then $x = \emptyset$ is stable. Let us say a term t is stable if $x = t$ is stable. Then $\{t_1 \ldots t_n\}$ is stable if the t_i are stable.

Observe that every model of TST is dual, and the dual of a positive formula is a special kind of negative formula, where we have $\notin$ and never $\in$, but $=$ and never $\neq$. So f will preserve any conjunction of disjunctions of positive formulæ and duals of positive formulæ.

Now consider a "basic" $\forall^*\exists^*$ sentence in the bigger model. It says that, for all $\vec{x}$ if the things in the tuple are related in a certain way [conj of atomics and negatomics] , then we can add a lot of stuff to obtain a larger tuple related in some way. It has an antecedent and a consequent.

We want to see how much of such a basic AE sentence we can recover by using only stable AE basic sentences. Such sentences either have negative antecedents and positive consequents or positive antecedents and negative consequents

Any antecedent is a conjunction of a purely positive antecedent and a purely negative one. These two conjuncts can be thought of as the antecedents of a purely positive basic AE fmla and a purely negative one. Then we look up all the stable basic AE sentences with those consequences, and infer the consequences. Unfortunately that doesn't do very much for us.

This tells us that ambiguity can be proved to hold in T$\mathbb{Z}$T for sentences that are equivalent both to a positive formula and to a dual positive formula.

13.4.3 Boolean injections

Any surjection $f : A \twoheadrightarrow B$ lifts to a surjection $\mathcal{P}(A) \twoheadrightarrow \mathcal{P}(B)$ and so on up. Just send $A' \subseteq A$ to $f``A'$. But it also gives an injective homomorphism $\mathcal{P}(B) \hookrightarrow \mathcal{P}(A)$ by $B' \subseteq B$ goes to $f^{-1}``B'$.

This means that given $f : A \twoheadrightarrow B$ there is a family of injective maps i_n from the levels of the natural model $\langle\langle B\rangle\rangle$ to the levels of the natural model $\langle\langle A\rangle\rangle$. (The nonzero levels that is!) These are boolean homomorphisms, but do they cohere to form a morphism between the models? That is, does it preserve $\in$?

I think the answer is 'yes'. (Must verify by hand). Of course f has to be setlike. But in the setting we are interested in, it *is* setlike. Let $\mathfrak{M}$ be an arbitrary model of TST, and f an (internal) surjection from level 1 onto level 0. So let's do this thing, slowly.

Let $\mathfrak{M} = \langle V_0, V_1 \ldots \rangle$ be a model of TST. Let $f \subseteq V_1 \times \iota``V_0$ be a surjection $V_1 \twoheadrightarrow V_0$. As usual $\mathfrak{M}^+$ is $\mathfrak{M}$ shorn of its bottom level and with the surviving levels relabelled. We can think of f as a surjection from level 0 of $\mathfrak{M}^+$ to level 0 of $\mathfrak{M}$ and, for each $n > 0$, it lifts to a surjection from level n of $\mathfrak{M}^+$ to level n of $\mathfrak{M}$, which me way as well also notate 'f', since no confusion will arise. Since surjections $h : A \twoheadrightarrow B$ always give a boolean injection $x \mapsto h^{-1}``x$ from $\mathcal{P}(B) \hookrightarrow \mathcal{P}(A)$ these fs will give injections from $\mathfrak{M}$ back to $\mathfrak{M}^+$.

So what does this injection preserve? Not much, really; certainly not enough. It doesn't preserve B or ι. If we start with an injection instead of a surjection – so that we have a chance of preserving singletons – then at each level we have to have a nonprincipal ultrafilter up our sleeve.

13.4.4 Earlier Stuff

Let $A \subseteq B$ be sets. There is a surjection $\mathcal{P}(B) \twoheadrightarrow \mathcal{P}(A)$ defined by $x \mapsto x \cap A$. And any surjection lifts in the obvious way so we have an injection $\mathcal{P}^2(A) \hookrightarrow \mathcal{P}^2(B)$ by $i : X \mapsto \{y \subseteq B : y \cap A \in X\}$.

Things to check.

1. it sends generators to generators. $i(B(a)) = \{y \subseteq B : y \cap A \in B(a)\} = \{y \subseteq B : a \in y \cap A\} = \{y \subseteq B : a \in y \cap A\}$. But, since $a \in A$, $a \in y$ iff $a \in y \cap A$, so this is $\{y \subseteq B : a \in y\}$ which is $B(a)$ in the sense of B.

2. It doesn't preserve singletons or sets of singletons so it doesn't interact well with extraction of models.

3. It preserves $\in^2$. Sse $a \in^2 i(X)$. This is $a \in^2 \{y \subseteq B : y \cap A \in X\}$. So there is $y \subseteq B$ with $y \cap A \in X$ and $a \in y$. But again, since $a \in A$, $a \in y$ iff $a \in y \cap A$. So this is equivalent to $a \in^2 X$.

Now let's think about the surjection from $\mathcal{P}(B) \twoheadrightarrow \mathcal{P}(A)$. It would be nice if we can cook up a right inverse. For $x \subset A$, what sort of things get sent to x? Only supersets of x. Only the empty subset of B gets sent to the empty subset of A, but (the whole of) B gets sent to the whole of A. So if we want a right inverse we have to find some extra stuff to add to x to get what we want.

Now let f be any boolean homomorphism $\mathcal{P}(B) \to \mathcal{P}(B \setminus A)$. It will turn out that if the kernel of the homomorphism contains all singletons then the injection we eventually build will preserve singletons. But let's not make any assumptions just yet.

The map $x \subseteq A \mapsto x \cup f(x)$ is now a right-inverse to the surjection $\mathcal{P}(B) \twoheadrightarrow \mathcal{P}(A)$.

Let us now overload 'i' to mean the identity on A, $x \mapsto x \cup f(x)$ on $\mathcal{P}(A)$, and i on $\mathcal{P}^2(A)$. Is i an $\in$-isomorphism?

Sse $x \in A$ and $y \subseteq A$. Then $i(x) \in i(y)$ iff $x \in y \cup f(y)$ but $f(y) \cap A = \emptyset$ so this is just $x \in y$.

Now sse $x \subseteq A$ and $y \subseteq \mathcal{P}(A)$. Then $i(x) \in i(y)$ iff $x \cup f(x) \in \{z \subseteq B : z \cap A \in y\}$. This is $x \cup f(x) \subseteq B$ and $(x \cup f(x)) \cap A \in y$. Now of course $(x \cup f(x)) \cap A = x$ so this reduces to $x \in y$ as desired.

Now can we lift i on the second type to i on the fourth type?

For this we want i (at the third level) to be a right-inverse for the surjection arising from the x-goes-to-$x \cup f(x)$-injection at the second level. Let's call this surjection h. We want:

$$h(i(X)) = X.$$

Now

$$\begin{aligned} h(i(X)) &= \{x \subseteq A : i(x) \in i(X)\} \\ &= \{x \subseteq A : x \cup f(x) \in i(X)\} \\ &= \{x \subseteq A : x \cup f(x) \in \{y \subseteq B : y \cap A \in X\}\} \\ &= \{x \subseteq A : (x \cup f(x)) \cap A \in X\} \\ &= \{x \subseteq A : x \in X\} \\ &= X \end{aligned}$$

Does this respect $\in$??

Let us now write down what i at the fourth level is. Actually i suspect that before we can do this intelligibly we'd better generalise all this to the case where B is not a superset of A but where there is an injection from A into B.

So let's start all over again. We have two sets of atoms, A and B, with $i : A \hookrightarrow B$. We'll agree to start counting the types of our variables so that A and B are of type 1, and i_n accepts inputs of level n.

This injection induces a surjection $h : \mathcal{P}(B) \twoheadrightarrow \mathcal{P}(A)$. $h(x) := \{a \in A : i(a) \in x\}$. This in turn induces an injection $i : \mathcal{P}^2(A) \hookrightarrow \mathcal{P}^2(B)$ by $i(x_3) = \{y_2 : h(y_2) \in x_3\}$ or, in other words, $i(x_3) = \{y_2 : \{a \in A : i(a) \in y_2\} \in x_3\}$.

Now let $f : \mathcal{P}(A) \to \mathcal{P}(B \setminus A)$ be a boolean algebra homomorphism. set $i(x_1) := i``x_1 \cup f(x_1)$

[at some point rerun the proof that this i on the first three levels is still an $\in$-isomorphism]

To get the dfn of i_4 just copy the dfn of i_3:

$$i(x_4) = \{y_3 : \{x \in \mathcal{P}(A) : i_2(x) \in y_3\} \in x_4\}.$$

$i_2(x)$ is $i``x \cup f(x)$ so this is

$$i(x_4) = \{y_3 : \{x \in \mathcal{P}(A) : (i``x \cup f(x)) \in y_3\} \in x_4\}.$$

Now we want to simplify $i(x_3) \in i(x_4)$

$$\{y_2 : \{a \in A : i(a) \in y_2\} \in x_3\} \in \{y_3 : \{x \in \mathcal{P}(A) : (i``x \cup f(x)) \in y_3\} \in x_4\}$$

$$\{w \in \mathcal{P}(A) : (i``w \cup f(w)) \in \{y_2 : \{a \in A : i(a) \in y_2\} \in x_3\}\} \in x_4$$

Now $(i``w \cup f(w)) \in \{y_2 : \{a \in A : i(a) \in y_2\} \in x_3\}$ is just
$\{a \in A : i(a) \in (i``w \cup f(w))\} \in x_3\}$
so we can simplify to

$$\{w \in \mathcal{P}(A) : \{a \in A : i(a) \in (i``w \cup f(w))\} \in x_3\} \in x_4$$

Now $\{a \in A : i(a) \in (i``w \cup f(w))\}$ is just w, so this becomes

$$\{w \in \mathcal{P}(A) : w \in x_3\} \in x_4$$

and $\{w \in \mathcal{P}(A) : w \in x_3\}$ is obviously just x_3 so we get extensionality as desired.

So we've got *something*!! (Not sure what!!)

i_3 doesn't send singletons to singletons. i_3 sends x to the set of *all* y such that $i^{-1}``(y \cap i``A) \in x$ not just some of them. That's how i_3 sends V to V. We could have sent x to $\{y \subseteq i``A : i^{-1}``y \in x\}$ but then it wouldn't send V to V. So we want to send x to some z s.t

$$\{y \subseteq i``A : i^{-1}``y \in x\} \subseteq z \subseteq \{y : i^{-1}``(y \cap i``A) \in x\}.$$

So we have to "inflate" $\{y \subseteq i``A : i^{-1}``y \in x\}$ with some quantity that is the empty set for singletons and is the whole of $V_3 \setminus i``V_2$ for V. Clearly we need another boolean homomorphism killing all singletons! But beware: once we have such a thing, can we be confident that the revised version of i_3 will preserve B?

We certainly want injective boolean homomorphisms from level n to level $n+1$. Any surjection $A \twoheadrightarrow B$ gives rise to an injective boolean homomorphism from $\mathcal{P}(B)$ to $\mathcal{P}(A)$. But how do we lift it up a level? We have to have a smooth way of obtaining a surjective boolean homomorphism from $\mathcal{P}(A)$ to $\mathcal{P}(B)$ from an injective boolean homomorphism from $\mathcal{P}(B)$ to $\mathcal{P}(A)$.

13.4.5 Can there be a $\forall$-elementary embedding $\mathfrak{M} \hookrightarrow \mathfrak{M}^+$?

> This section needs radical revision. First we must establish that for an embedding to be $\forall$-elementary it is necessary and sufficient that it should also preserve B and ι. We prove this by considering a formula in prenex normal form, with the matrix in CNF so we can import the universal quantifier so it is applied to disjunctions of atomics and negatomics: to wit, things like $(\forall x)(u \in x \vee v \notin x \vee x = y \vee x \in z)$ which of course is $\{y\} \cup B`u \cup \overline{Bv} = V$. Then we have the sad duty of showing that any B-and-singleton-preserving boolean homomorphism will force there to be a nonprincipal prime ideal $\subseteq V_2$ which blows away any hope of showing that the ambiguity we might get from this doesn't just drop out of the infinitude of the model of TST.

A $\forall$-elementary map is one that preserves formulæ of the form $(\forall x)\Phi$ where x is the sole bound variable. (these are sometimes called "1-embeddings" by model theorists). I here consider the task of building a $\forall$-elementary map h

from a model $\mathfrak{M}$ of simple type theory into $\chi`\mathfrak{M}$, (When $\mathfrak{M}$ is a model of simple type theory $\chi`\mathfrak{M}$ is the result of truncating the bottom type and relabelling the new bottom type – which had been 1 – as 0). We will trade on the fact that for an embedding $h : \mathfrak{M} \to \chi`\mathfrak{M}$ to be $\forall$-elementary is sufficient (because in type theory we need consider only *stratified* $\forall$-formulæ) that it should respect B, ι and the boolean operations. At the time of writing it is not known whether there can be such an embedding or not. Any model with one must at least have infinitely many elements of type 0.

Why should any NF-ist care? Two reasons.

(i) it is a natural subcase of full ambiguity.

(ii) finding a method for constructing $\forall$-elementary embedding $M \to \chi`M$ when $\mathfrak{M}$ has infinitely many elements at type 0 would prove conjecture 2, that NF decides all stratified $\forall_2$ sentences.

We can think of constructing a $\forall$-elementary embedding $\mathfrak{M} \to \chi`\mathfrak{M}$ as building a series of maps $h_i : \mathfrak{M}_i \to \mathfrak{M}_{i+1}$ where $\mathfrak{M}_i$ is the ith level of $\mathfrak{M}$. We shall try to construct these maps h_i so that they can be coded inside $\mathfrak{M}$ in the usual way. The precise nature of this coding is not important: what *does* matter is that the image of a set in the embedding will be a *set* of the model if h_i is coded in the model. (h *of* an element of the model must be an element of the model, but if h is not coded in the model there is no reason to suppose that *the image of x in h* is an element of the model) in general, so we shall want h to be *setlike*. h_0 can be any old map $M_0 \to M_1$ that is 1-1. If the only thing h_1 had to do was respect $\in$, (that is, if we were content merely to preserve quantifier-free sentences) we would set $h_1`x =_{df} h_0``x$, and indeed the idea survives in part in this more complicated context. As it is, h_1 must be a map $\mathfrak{M}_1 \to \mathfrak{M}_2$ which also respects the boolean operations and the singleton operator ι, i.e., we must have $h_1`\{x\} = \{h_0`x\}$. The requirement that h_1 respect the boolean operations means that in particular $h`V_1 = V_2$.

We can construct h_1 if we have a nonprincipal prime ideal on the boolean algebra M_1. If x is in the ideal $h_1`x$ is to be $h_0``x$. If not, then $V_1 \setminus x$ is in the ideal and we set $h_1`x =_{df} V_2 \setminus h_0``(V_1 \setminus x)$. The ideal must be nonprincipal because otherwise some singletons might be "large", would not get sent to singletons and thus ι would not be respected.

It is only when we reach $\mathfrak{M}_n$ with $n \geq 2$ that we have to consider the remaining operation B. For each n, $\mathfrak{M}_{n+2}$ is a complete boolean algebra, and it is generated by the $B`x$, for $x$ in $M_n$. On this important fact will turn the rest of the construction. Thus every object in $\mathfrak{M}_{n+2}$ can be regarded as an (in some cases infinitary) word in the generators $B`x_i$. We may as well fix now a notation which we will need later: g_n of a word (at type n) is simply the same word in generators $B`h`x$ instead of $B`x$. $g_n$ thus preserves $B$ and the boolean operations, tho' not necessarily $\iota$. For a lot of $x$, $g_n`x$ is what we want $h_n`x$ to be. For example if $x$ is a finite boolean combination of the $B`x$, then $h_n`x$ *must* be $g_n`x$ in order for h_n to respect B. However if x is an infinitary word $h_n`x$ need not be taken to be $g_n`x$, and indeed in some cases (when x is a singleton for example) *cannot*, for $g_n`x$ will be infinite as we shall see, and $h_n$ of a singleton must be a singleton, for $\iota$ must be preserved. For singletons, and indeed finite sets $x$ in general $h_n`x$ must be $h_{n-1}``x$. The apparent conflict with the need to preserve B causes no problem as long as $\mathfrak{M}_n$ is infinite, for then no singleton is a finitary word in the $B`x$, and it is only finitary first-order properties we have to preserve. Finally the empty set (universe) at each level must be sent to the empty set (universe) at the next type. Thus V_n gets sent neither to $h_{n-1}``V_n$ nor to $g_n`V_n$, but to something bigger than either of these. $h_n`x$ must always extend $h_{n-1}``x$ in order for the family of h_i to respect $\in$. Small things x, like $\emptyset$, get sent to $h_{n-1}``x$, but bigger things $x$ get sent to $h_{n-1}``x \cup$ *something*, with the something depending on x. Let us call this something the "inflator" of x, since it is what we have to inflate $h_{n-1}``x$ by to get $h_n`x$. To be explicit,

DEFINITION 21 $\texttt{infl}(x) = h_n`x \setminus h_{n-1}``x$

First we show that if we are to succeed in constructing h_n at all then `infl` must be a boolean algebra homomorphism.

PROPOSITION 5 $x \subseteq y \to \texttt{infl}`x \subseteq \texttt{infl}`y$

Proof:

Suppose *per impossibile* that we could find x, y such that $x \subseteq y \wedge \texttt{infl}`x \not\subseteq \texttt{infl}`y$. Then there is z such that

$z \in \texttt{infl}`x \wedge z \notin \texttt{infl}`y$

Now $z \notin \texttt{infl}`y$ is $z \notin (h_n`y \setminus h_{n-1}``y$ and similarly x, whence

$z \in h_n`x \wedge z \notin h_{n-1}``x \wedge (z \in h_n`y \vee z \notin h_{n-1}``y)$

Now $z \in h_n`x$ so $z \in h_n`y$ since h_n respects $\subseteq$. So the first disjunct is impossible, and we conclude $z \in h_{n-1}``y$. But since $z$ is in the range of $h_{n-1}$ it must be $h_{n-1}`w$ for some $w$. But then $h_{n-1}`w \in h_n`x$ so $w \in x$ and $h_{n-1}`w \in h_{n-1}``x$ contradicting $z \notin h_{n-1}``x$. ■

PROPOSITION 6 $\texttt{infl}(x \cap y) = \texttt{infl}`x \cap \texttt{infl}`y$

Proof:

$\texttt{infl}`(x \cap y) =$
$h_n`(x \cap y) \setminus h_{n-1}``(x \cap y) =$
$h_n`x \cap h_n`y \cap \setminus h_{n-1}``x \cap \setminus h_{n-1}``y =$
$(h_n`x \cap \setminus h_{n-1}``x) \cap (h_n`y \cap \setminus h_{n-1}``y)$
$= \texttt{infl}`x \cap \texttt{infl}`y$

■

PROPOSITION 7 *$\texttt{infl}(V \setminus x)$ and $\texttt{infl}(x)$ are complements in $h_n`V_n \setminus h_{n-1}``V_n$.*

Proof:

They are disjoint since they are included in $h_n`(V \setminus x)$ and $h_n`x$ respectively which are disjoint by $\forall$-elementarity of h_n. $\texttt{infl}(x \cup \texttt{infl}(V \setminus x)$ is

$$(h_n(V \setminus x) \setminus h_{n-1}``(V \setminus x)) \cup (h_n`x \setminus h_{n-1}``x).$$

Now since $h_n(V \setminus x)$ and h_n‘x are disjoint we can rearrange this to

$$(h_n(V \setminus x) \cup h_n\text{‘}x) \setminus (h_{n-1}\text{“}(V \setminus x) \cup\ h_{n-1}\text{“}x)$$

which is

$$V_{n+1} \setminus h_{n-1}\text{“}V_n$$

■

Thus `infl` is a boolean algebra homomorphism. Let I be the kernel. We will use the notation $[w]_I$ (the subscript I usually omitted) to mean that $w \in \mathfrak{M}_n$ and $[w]_I$ is the element of $\mathfrak{M}_n/I$ to which w belongs.

Remark 69 *There is in each element of $\mathfrak{M}_n/I$ at most one object x such that h_n‘$x = g_n$‘x*

Proof:

Suppose we had x, y such that

g_n‘$x = h_n$‘x, g_n‘$y = h_n$‘y, x `XOR` $y \in I$

h_n‘$(x$ `XOR` $y) = h_{n-1}$“$(x$ `XOR` $y)$ since x `XOR` y is small. But h_n and g_n both commute with boolean operations so h_n‘$(x$ `XOR` $y) =\ g_n$‘$(x$ `XOR` $y)$. We conclude

h_{n-1}“$(x$ `XOR` $y) = g_n$‘$(x$ `XOR` $y)$.

We shall now show that these two objects are of impossibly different sizes. The first object is bounded in size by $\mathfrak{M}_{n-1}$. To ascertain the size of the second we think of $(x$ `XOR` $y)$ as a union of singletons z.

g_n‘$(x$ `XOR` $y)$ as a union of g_n‘singletons$z$. What is such a g_n‘ singleton z? Garbled Well, z is an intersection of things B‘$u \cap -B$‘v so g‘z is B‘h_{n-2}‘$u \cap -B$‘h_{n-2}‘v where the u and the v between them exhaust $\mathfrak{M}_{n-2}$.

Thus each member of g_n‘z must have as members h_{n-2}‘u

…not have as members h_{n-2}‘v.

This was enough to determine the member of z uniquely, as u and v exhausted $\mathfrak{M}_{n-2}$ but there are now more generators in $\mathfrak{M}_{n-1}$ ($|\mathfrak{M}_{n-1}|$) of them in fact) and so $|\mathfrak{M}_n|$ possibilities for members of g_n‘z. Thus h_{n-1}“$(x$ `XOR` $y)$ and g_n‘$(x$ `XOR` $y)$ are of impossibly different sizes as promised.

■

From this we can conclude that each equivalence class in $\mathfrak{M}_n/I$ contains at most one x such that g_n‘$x = h_n$‘x and infer the important

Corollary 14 *Distinct finitary words are sent to distinct members of $\mathfrak{M}_n/I$.*

So far we have been trying to deduce information about h from the fact that it is $\forall$-elementary . If conversely we are using this knowledge to build a $\forall$-embedding this shows that at the very least we will need to find a quotient algebra $\mathfrak{M}_n/I$ of M_n. If we can find an order-preserving set of representatives to get a subalgebra of $\mathfrak{M}_n$, then, given $a \in M_n/I$ we compute h_n‘x for $x \in a$ by

$\texttt{infl}(x) =_{df} (g_n`a_x) - (h_{n-1}``a_x)$ where a_x is the representative from a. If all we have is an $\mathfrak{M}_n/I$ without such a set of representatives we know that all members of any a $\in \mathfrak{M}_n/I$ have the same inflator, but we do not know what that inflator is, and therefore have no obvious means of constructing h_n for members of a.

Thus to construct a $\forall$-elementary embedding by this method we must find an ideal I in $\mathfrak{M}_n$ which is non-principal (because singletons must be preserved) and contains no finitary words in the generators $B`x$, and such that $\mathfrak{M}_n/I$ has an order-preserving set of representatives.

Obvious questions are

(i) Can we ever do this? and

(ii) Is there a converse?

Distinct generators must be sent to distinct members of $\mathfrak{M}_n/I$. So if an element a of $\mathfrak{M}_n/I$ contains a generator (or, *a fortiori*) a finitary word in those generators, then that generator (or word) must be the chosen representative, and we know what h_n does to members of a. This is because h must respect B and finitary boolean algebra operations, so for finitary words w we know $h_n`w = g_n`w$. We have seen above that no quotient class can contain more than one x such that $g_n`x = h_n`x$, and so can contain at most one finitary word.

Now consider b, an element of $\mathfrak{M}_n/I$ which contains no finitary words. What is b_x, the representative of b, to be? We have some guidance in this from the consideration that the set of representatives is to be order-preserving, and so if b contains a word W which is $\subseteq$ infinitely many finitary words W_i, then $b \leq [W_i]$ for each i, and the chosen b_x must $\subseteq$ the representatives of the $[W_i]$ which will be W_i of course. Thus $b_x \subseteq W$. So if b contains any infinitary intersections of finitary words, b_x must be (included in) the intersection of all those infinitary intersections. Dually if b contained elements that were infinitary unions of finitary words.

13.5 The direct limit construction

There is an old idea that i have never written about. Start with the canonical model of TST with empty bottom type. Define f by picking, for each i, an injection $f_i : T_i \hookrightarrow T_{i+1}$ satisfying $x \in y$ iff $f(x) \in f(y)$. This gives a direct limit. We define $\in$ on the direct limit in the obvious way. There is an obvious profinite family of direct limits with an obvious topology. There is of course also a logical ("Stone") topology as well. This pair of topologies reminds me of the pair of topologies on the family of all permutation models. These two topologies seem to take no notice of each other in exactly the way the two topologies on the space of permutation models take no notice of one another.

13.5.1 Try to show that $\forall^*\exists^*$ sentences generalise *downwards* in models of T$\mathbb{Z}$T

Without loss of generality we can suppose every $\forall^*\exists^*$ sentence is of the form

$$(\forall \vec{y})(\psi(\vec{y}) \to \bigvee_{i \in I} \phi(\vec{x}, \vec{y})) \tag{1}$$

where ψ and the ϕ_i are all conjunctions of atomics and negatomics.

So suppose $(\forall \vec{y})(\psi(\vec{y}) \to \bigvee_{i \in I} \phi(\vec{x}, \vec{y}))$ holds at level 1, in the sense that the variable(s) of lowest level are of level 1. We want to show that it holds one level down.

That is to say we want to know that if we pick up a tuple of ys (one level down, as it were) we can find a tuple of xs related to the ys in the right way. The idea is to "copy the ys up" and then use the fact that our AE sentence holds one level up to find xs one level up which we can then copy down.

To do this of course we need a type-raising injection h that respects $\in$, and we want the xs that we obtain one level up to be in the range of h so we can copy them back down. There are lots of such injections, fortunately for us. How are we to extend an $\in$-preserving injection h up one level? If h is to be an $\in$-isomorphism we must have $h(x) \supseteq h``x$, and this is sufficient. For each $x$ we have to pick something $\texttt{infl}(x)$ (the *inflator* of $x$ from definition 21) so that $h(x) = h``x \cup \texttt{infl}(x)$. As far as i can see the only constraint on the inflator of x is that it must be disjoint from the range of h. Well, we also have the minor constraint on inflators that h has to be injective, to `inflator` cannot be just any function raising types by 1.

The key fact is that we are free to use different injections h for different tuples $\vec{y}$: our choice of h is not determined solely by the AE sentence we are trying to generalise down. In fact we will build our injection h bit-by-bit as we ascend through the levels. This is worth making a fuss about. In trying to prove the universal-existential conjecture one might think that one has to find a uniformly definable type-raising injection which, for all $\mathfrak{M}$, injects $\mathfrak{M}$ into $\mathfrak{M}^*$ in a way that preserves all universal-existential sentences. I don't know if there is such a definable injection, but in any case we don't need one: it would be sufficient to have a family of injections, one for each universal-existential sentence. Indeed, one can choose a different injection for each instantiation of the y variables to elements of $\mathfrak{M}$.

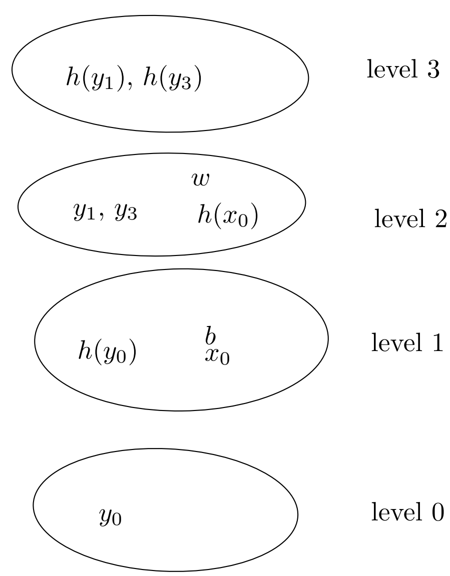
So we have our handful $\vec{y}$ of input objects and we want to find things related to them in certain ways. Well, we whack our objects with h (whatever h turns out to be) and invoke our assumption that our AE sentence holds one level up. So there are things one level up that are related to $h``$(our handful) in the right way; all we have to do is ensure that all these things are values of h, so we can copy them down a level and they become witnesses to the existential quantifiers.

So there is a witness w to the $x$istential quantifier; hang onto that fact. We want this thing w to be h of something. So what is it h of? If it is $h(u)$ then $h(u) = h``u \cup a$, some $a$. So $w$ must be $h$ of $h^{-1}``(w \cap h``V)$. There are two difficulties here. (i) we need h to be setlike, and (ii) (worse!) h for things of *that* level has already been defined! We are going to have to create h by means of a priority construction.

So: let $\mathfrak{M}$ be a terminal segment of a model of T$\mathbb{Z}$T. We want a $\in$-preserving injetion $\mathfrak{M} \hookrightarrow \mathfrak{M}^+$. I think there is no cost attached to taking h_0 (from level 0 of $\mathfrak{M}$ to level 1 of $\mathfrak{M}$ aka level 0 of $\mathfrak{M}^+$) to be ι.

Start by considering the case there there is only one x variable. I think this will turn out to be less of an oversimplification than one might think, because the construction in the general case will deal with the witnesses to the $x$istential quantifiers by recursion on the levels to which they belong.

So we have fixed $\mathfrak{M} \models$ T$\mathbb{Z}$T and an AE formula $\forall\vec{y}\exists\vec{x}\Phi(\vec{x}\vec{y})$; Without loss of generality we can take the y vbl of lowest level to be of level 0 and the x variable of lowest level to be of level 1. We want $\forall\vec{y}\exists\vec{x}\Phi(\vec{x}\vec{y})$ to be true, but all we are told is that it is true one level up.



We want to find x_0 such that y_0 is (or is not) a member of it, and such that it is (or is not) a member of y_2 and y_3. (We don't have to worry about ys of the same level as x_0.) What we do know is that there is w such that $h(y_0)$ is (or is not) a member of it, and such that it is (or is not) a member of $h(y_2)$ and $h(y_3)$. What we want to do is doctor h so that this w (or at any rate at least one of these w) is h of something. But of course doctoring h so that w is in the range of h has the potential to alter $h(y_1)$ and $h(y_3)$.

Well, what is $h(y_1)$? It is a judiciously chosen superset of $h``y_1$. So we need to know how to find $h$ of members of $y_1$. Members of $y_1$ are things of level 1, and $h$ of a thing $a$ of level 1 is a judiciously chosen superset of $h``a$. But h on level 0 is just *iota*. so $h(a)$ is a judiciously chosen superset of ι"a. Now we want w to be h of some object b of level 1. So w has to be a judiciously chosen superset of ι"b, namely $(\iota``b) \cup c$, where $c$ contains no singletons. And $b$ must be $\iota^{-1}``(w \cap \iota``V)$ (which i suppose is just $\iota^{-1}``w$).

So alter that part of h that sends level 1 into level 2 (which i suppose we could call $h_{1\hookrightarrow 2}$) by deciding that $h(b)$ is no longer the old $h(b)$ but is now w. This changes only one ordered pair in $h_{1\hookrightarrow 2}$) but of course propagates to $h_{2\hookrightarrow 3}$, and changes infinitely many pairs there, and of course that can mean that the new $h(y_1)$ is not the same as the old $h(y_1)$. And that again means that our w may have become useless. There will be a new w of course, but there's nothing to say that we won't have exactly the same problem all over again. The challenge is to have designed h in such a way that when we tweak it we don't suddenly find we need a new w. Or better still, seek an h that doesn't cause us to alter w in the first place.

And how do we do *that*?!

But perhaps, like Wrong Way Norris[1], our path is in the correct direction but has the wrong sense. What we should be doing is trying to prove the following.

> Let $\mathfrak{M} \models \mathrm{T}\mathbb{Z}\mathrm{T}$. Let $y_1 \ldots y_n$ be n distinct elements of $\mathfrak{M}$. Then we can find an injection $h : \mathfrak{M} \hookrightarrow \mathfrak{M}$ that preserves $\in$ and raises types by 1, such that every y_i is a value of h.

This actually sounds quite plausible!

If we can prove it, then we can use it to prove the universal existential conjecture (or at least that universal-existential sentences generalise upwards) as follows.

As usual it suffices to consider formulæ of the kind $(\forall\vec{y})(\psi(\vec{y}) \rightarrow (\exists\vec{x})(\phi(\vec{y},\vec{x})))$ where ψ and ϕ are quantifier-free and ψ is a conjunction of atomics and negatomics. We want to show that, whenever $\mathfrak{M}$ is a model of $\mathrm{T}\mathbb{Z}\mathrm{T}$, $\mathfrak{M} \models (\forall\vec{y})(\psi(\vec{y}) \rightarrow (\exists\vec{x})(\phi(\vec{y},\vec{x})))$

So fix $\mathfrak{M} \models \mathrm{T}\mathbb{Z}\mathrm{T}$ and fix a tuple $y_1 \ldots y_n$ of things in $\mathfrak{M}$ instantiating $\psi(\vec{y})$. Use the conjecture to obtain a type-raising h defined on a terminal segment of $\mathfrak{M}$ and elements $y'_1 \ldots y'_n$ of $\mathfrak{M}$ such that $h(y'_i) = y_i$ for all $1 \leq i \leq n$. Then we assume that $(\forall\vec{y})(\psi(\vec{y}) \rightarrow (\exists\vec{x})(\phi(\vec{y},\vec{x})))$ holds one level down to obtain witnesses for the $x$istential variables. We then whack those witnesses with h to obtain witnesses for the instance of the unshifted version of $(\forall\vec{y})(\psi(\vec{y}) \rightarrow (\exists\vec{x})(\phi(\vec{y},\vec{x})))$.

Now this seems to be just the idea that Zachiri had in 2013! So why should it be any different this time?

Clearly we are going to construct h by recursion on levels. h^{-1} can be defined on y objects of the lowest level with complete freedom, as far as i can see at the moment.

Thereafter we are trying to inject level n into level $n+1$. If y is of level $n+1$ what is $h^{-1}(y)$ to be? y is to be thought of as $h``A \cup B$ where $A$ is $y \cap h``V$ and B is disjoint from $h``V$. Then $h^{-1}(y)$ is $h^{-1}``A$. So, given y we have to identify

[1] http://www.montypython.net/scripts/emigration.php

A and B. A is clearly controlled by what is in y. Things at level $n+1$ that are not y objects we don't care about, but we do have to define h on all the other things are level n, the things that aren't h^{-1} of y objects at level $n+1$. I think these remaining things at level n can be sent to their images in h (i.e., null inflators). Notice that for this h has to be setlike. Does this construction create a setlike h? I think the answer is 'yes' beco's the h we construct can be definable with the y objects as parameters.

Don't we prove somewhere that an $\in$-loop cannot consist entirely of finite sets? (yes: it's lemma 15 of Bowler-Forster). Is there an AE version of this allegation?

The natural assertion "Every bottomless set contains V" is $\forall^*\exists^*\forall^*$ which is the wrong way round.

13.6 The Conjectures

Conjecture 1 *Is it the case that every $\forall^1\exists^*$ sentence consistent with NF_2 has a permutation model.*

Conjecture 2 *Every $\forall^*\exists^*$ sentence refutable in NF is refutable already in NF0.*

Conjecture 3 *$NF0$ decides all stratified $\forall^*\exists^*$ sentences.*

Conjecture 4 *Any term model for NF and any model for NF in which all sets are symmetric satisfies every $\forall^*\exists^*$ sentence consistent with NF0.*

Conjecture 5 *All unstratified $\forall^*\exists^*$ sentences are either decided by NF or can be proved consistent by permutations relative to any invariant extension of NF.*

Conjecture 6 *Let us say a* Henkin *sentence is a branching quantifier sentence where every prefix is $\forall^*\exists^*$. Then $T\mathbb{Z}T$ has a model satisfying all consistent Henkin sentences.*

We cannot strengthen this last conjecture to "T$\mathbb{Z}$T decides all Henkin formulæ" beco's there is a Henkin formula that says there is an external tsau. And that is true in some models of TST but not all!

Throughout this discussion we will try to keep to the cute mnemonic habit – due to Quine – of writing a typical universal-existential sentence with the initial – universally quantified – variables as $\vec{y}$ ('y' for $y$ouniversal) and the existentially quantified variables as $\vec{x}$ – for E*x*istential). That was so we can talk about y variables and x variables.

In earlier versions, conjecture 3 used to be "NF_2 decides all stratified $\forall^*\exists^*$ sentences.

It is known that the term model for NF0 satifies all consistent $\forall^*\exists^*$ sentences consistent with NF0[2]. Putting this together with conjecture 2 suggests that *NF* might have a model satisfying all the $\forall^*\exists^*$ sentences consistent with *NF*. (In fact we conjecture that a term model for *NF* would be such a model). At the very least it suggests that the class of $\forall^*\exists^*$ sentences consistent with *NF* is closed under conjunction. This also suggests that if conjecture 2 is correct then whenever ϕ is a consistent $\forall^*\exists^*$ sentence consistent with *NF* then $\{\pi : \phi^\pi\}$ belongs to some class Γ of sets of permutations that is closed under intersection. Is Γ nicely defined in terms of a natural topology on the symmetric group on V? It clearly can't mean "open" in the usual topology.

13.7 A note on the first two conjectures

The background to these conjectures is that NF0 proves all $\exists^*$ sentences consistent with LPC, and one naturally wants to speculate about what happens with formulæ with more quantifiers.

Notice that "every superset of a self-membered set is self-membered" is a $\forall^2\exists^1$ sentence consistent with NF_2 (it's true in the term model) that is not consistent with NF0, so we cannot strengthen 'NF0' to 'NF_2' in conjecture 2.

Every $\forall^*\exists^*$ sentence has a canonical normal form. If we take the disjunction of all possible conjunctions of atomic and negatomic formulæ built up from all the x and y variables by means of $\in$ and $=$, then any $\forall^*\exists^*$ sentence can be put in the form $(\forall\vec{y})(\exists\vec{x})$ followed by a disjunction of some of those conjunctions.

Let us assume this done. Now suppose we had started with a $\forall^1\exists^*$ sentence, and put it into this normal form. There is only one y variable, and every value that it takes either is or is not a member of itself, so we know that if our $\forall^1\exists^*$ sentence is to be satisfiable at all then at least one of its disjuncts must be a conjunction containing the atomic conjunct '$y \in y$' and at least one of its disjuncts must be a conjunction containing the negatomic conjunct '$y \notin y$'. This is because (since V is a set) 'y' might be interpreted by something that is a member of itself, and (since $\emptyset$ is a set) 'y' might be interpreted by something that is not a member of itself.

Anything else is going to be false in all models of any theory in which we can prove the existence of V and Λ. Also, this seems to be about all we can do in the way of weeding out formulæ that are not going to be satisfiable. Notice that this line of talk relies only on things we can prove in NF_2. Hence conjecture 1.

Now let us consider $\forall^2\exists^*$ sentences. We now have to consider not just the two formulæ '$y \in y$ and '$y \notin y$' but the 32 conjunctions we get by assigning truth values to '$y_1 \in y_1$', '$y_1 \in y_2$', '$y_2 \in y_1$', '$y_2 \in y_2$' and '$y_1 = y_2$'.

[2]Where did i prove this FFS!!!

Now in any set theory in which we can find objects satisfying, for example, $t_1 \in t_1 \wedge t_1 \notin t_2 \wedge t_2 \notin t_1 \wedge t_2 \in t_2$ we can argue that if a $\forall^2\exists^*$ is to be satisfiable at all then at least one of its disjuncts must be a conjunction containing $y_1 \in y_1 \wedge y_1 \notin y_2 \wedge y_2 \notin y_1 \wedge y_2 \in y_2$, because otherwise it could be falsified in any model by interpreting each 'y_i' by t_i. Such a theory is NF0. As before, this seems to be the only thing we can do to weed out formulæ that are not going to be satisfiable, so the corresponding conjecture for $\forall^2\exists^*$ sentences will be that every $\forall^2\exists^*$ sentence refutable in *NF* is refutable in NF0. As it happens, NF0 proves every consistent $\exists^*$ sentence so we do not need to reach for more complicated theories when considering $\forall^3\exists^*$ sentences. This is why conjecture 2 takes the form that it does.

We can prove that every $\forall^*\exists^*$ sentence consistent with NF0 is true in the term model of NF0. (I tho'rt there is a proof in the book somewhere but i can't find it!!). What about NF_2? There is a complication with NF_2, namely that the term model doesn't satisfy the $\exists^*$ sentence $(\exists x_1 x_2)(x_1 \in x_1 \notin x_2 \in x_2 \notin x_1)$. So it isn't true that the term model for NF_2 satisfies every consistent $\forall^*\exists^*$ sentence. (I think it proves that, given two self membered sets, one is a member of the other)

OTOH, we do get this:

Remark 70
The term model for NF_2 satisfies every $\forall^*\exists^1$ sentence consistent with NF_2.

Proof:
Let $(\forall\vec{y})(\exists x)\Phi$ be a $\forall^*\exists^1$ sentence consistent with NF_2. Then for every vector $\vec{t}$ of terms there is an x such that Φ, so all we have to do is establish that such a witness can be found among the terms.

$(\forall\vec{y})(\exists x)\Phi$ is satisfiable, so fix a model in which it is true. (It doesn't matter which one, as the term model is unique, and embeds in all models). Express Φ in DNF, and fix a tuple $\vec{t}$ of terms. One of the disjuncts is true. Truth of this disjunct tells us that there is a witness x which has certain ts as members, is distinct from certain other ts (if there is a clause requiring it to be equal to one of the ts then we are done) lacks certain other ts, and belongs to a final t. This last simplification arises beco's a finite conjunction of things like $x \in t$ and $x \notin t$ is equivalent to a single expression of that form, complements and intersections of ts being ts. If this final t is a low set then the witness is already a term. If it isn't, then we are looking inside a cofinite set for a set satisfying conditions each of which exclude only a moiety of sets. So there must be a witness. ■

Something analogous holds for all basic CO models. The term model for NF_2 is the hereditarily finite-or-cofinite sets, least fixed point version. This needs to be nailed down.

In particular this holds for $(\forall x)(x \in x \to (\forall y)(y = x \setminus \{x\} \to y \in x))$
$(\forall x)(x \in x \to (\forall y)((\exists z)(z \in y \longleftrightarrow \neg(z \in x \wedge z \neq x) \vee y \in x))$

Now the same argument won't work for $\forall^*\exists^2$ sentences consistent with NF_2, since that could commit us to finding two witnesses x_1 and x_2 satisfying $x_1 \in$

$x_1 \notin x_2 \in x_2 \notin x_1$. In these circumstances x_1 and x_2 both have to be cofinite, and if x_2 and x_2 are cofinite, one is a member of the other: if $V \setminus \{a_1 \cdots a_n\}$ and $V \setminus \{b_1 \cdots b_n\}$ are members of each other then $V \setminus \{b_1 \cdots b_n\}$ must be one of the a_i and $V \setminus \{a_1 \cdots a_n\}$ must be one of the b_i, contradicting the fact that the subformula relation on terms is wellfounded.

What about extending this to $\forall^*\exists^*$ sentences?

Every $\forall^*\exists^*$ sentence is a conjunction of things of the form

$$(\forall \vec{y})(A(\vec{y}) \to (\exists \vec{x})(B(\vec{x}, \vec{y})))$$

where A is a conjunction of $\in$ and $\notin$ between the $\vec{y}$ and in B all atomics involve at least one x.

The point is that if there is more than one y we can get A to describe a finite structure that is not a substructure of the term model for NF_2, which means that any $\forall^*\exists^*$ sentence built up using that A is trivially true in the term model for NF_2.

But we might be working our way back. Suppose $(\forall \vec{y})(A(\vec{y}) \to (\exists \vec{x})(B(\vec{x}, \vec{y})))$ is a $\forall^*\exists^*$ sentence refutable in NF_2. Then A must describe a substructure of ...

13.8 A note on Conjecture 2 and Conjecture 3

The **finitely generated** models of TSTO are those whose type 0 has only finitely many atoms.

A **partition** $\mathbb{P}$ of a set X is a subset of $\mathcal{P}(X)$ such that $\bigcup \mathbb{P} = X$ and the members of $\mathbb{P}$ are pairwise disjoint.

If $\mathbb{P}_1$ and $\mathbb{P}_2$ are two partitions of the same set we say $\mathbb{P}_1$ **refines** $\mathbb{P}_2$ if every piece of $\mathbb{P}_1$ is a subset of a piece of $\mathbb{P}_2$.

A subset $X' \subseteq X$ **crosses** another subset $p \subseteq X$ if $X' \cap p$ and $X' \setminus p$ are both nonempty. (That is to say, X' is not in the field of sets generated by $\mathbb{P}$ if X' crosses a piece of $\mathbb{P}$).

We first prove that every countable model of TSTO is a direct limit of all the finitely generated models of TSTO. (The "all" is important.)

We do this by induction on the number of types. For reasons which will become clear we will regard the finitely generated models as starting with a base type T_1 with 2^n elements and a boolean algebra structure rather than starting with a base type T_0 with n elements and no structure. In effect we forget about the bottom type. So the thing we are going to prove by induction on k is that every countable model of $\mathrm{TST}O_k$ is a direct limit of **all** finitely generated models of $\mathrm{TST}O_k$.

For the base case we prove that every countable atomic boolean algebra $\mathcal{B}$ there is a family $\mathcal{B}_i : i \in \mathbb{N}$ of subalgebras of $\mathcal{B}$ where $\mathcal{B}_i$ has i atoms, where the inclusion map is a boolean homomorphism and the union $\bigcup_{i \in \mathbb{N}} \mathcal{B}_i$ is $\mathcal{B}$.

We obtain $\mathcal{B}_{i+1}$ from $\mathcal{B}_i$ by splitting one of the i atoms into two, effectively adding two new atoms. To decide which atom to split, and how to split it, depends on how we wellorder $\mathcal{B}$. We have a fixed wellordering of $\mathcal{B}$ to order type ω. At stage 0 we consider $\mathcal{B}_0$ which of course is just the two element boolean algebra containing the top element and the bottom element. We make x_1 an atom and set $\mathcal{B}_1$ to be the four element boolean algebra with x_1 and $V \setminus x_1$ as atoms.

Thereafter at any stage we have two things in hand:

(i) a most-recently-constructed algebra $\mathcal{B}_i$ and

(ii) an x_k which is to be an element of an algebra soon to be constructed. (Notice that i and k are not assumed to be the same! In general i is likely to be much bigger than k.)

The set of atoms of $\mathcal{B}_i$ that we have is simply a partition of the atoms of $\mathcal{B}$ into i pieces. At stage k we consider x_k. x_k will be a superset of some atoms and disjoint from others. These we do nothing to. The remaining atoms it crosses. The atoms are ordered by the canonical worder of $\mathcal{B}$. Suppose for example x_k crosses five of the i atoms of $\mathcal{B}_i$, to wit: c, d, e, f, g in order. Then we obtain succesively $\mathcal{B}_{i+1}$ by splitting c into $c \cap x_k$ and $c \setminus x_k$; then $\mathcal{B}_{i+2}$ by splitting d into $d \cap x_k$ and $d \setminus x_k$; then $\mathcal{B}_{i+3}$ by splitting e into $e \cap x_k$ and $e \setminus x_k$; then $\mathcal{B}_{i+4}$ by splitting f into $f \cap x_k$ and $f \setminus x_k$; and finally $\mathcal{B}_{i+5}$ by splitting g into $g \cap x_k$ and $g \setminus x_k$.

What has this achieved? We now have constructed our sequence of subalgebras as far as $\mathcal{B}_{i+5}$ and we have ensured that x_k is in the direct limit. By iterating this we will eventually ensure that every element of $\mathcal{B}$ appears, so the direct limit of the sequence of subalgebras generated in this way is $\mathcal{B}$.

The induction step is similar but messier.

Let $\mathcal{B}$ be a countable atomic boolean algebra which is the union of $\langle \mathcal{B}_n : n < \omega \rangle$: a $\subseteq$-nested sequence of finite subalgebras of $\mathcal{B}$. Let $\mathcal{B}^+$ be a countable atomic subalgebra of $\mathcal{P}(\mathcal{B})$ containing all singletons. Then there is a sequence $\langle \mathbb{P}_i : i < \omega \rangle$ of finite partitions of $\mathcal{B}$ such that $\mathbb{P}_0$ is the trivial partition with only one piece and for each $i \geq 1$,

$\mathbb{P}_i$ refines $\mathbb{P}_{i-1}$. $\mathbb{P}_i \subseteq \mathcal{B}^+$
$\mathcal{B}_i$ is a selection set for $\mathbb{P}_i$.

Further, if we let $\mathcal{B}_i^+$ be the subalgebra of $\mathcal{B}^+$ whose atoms are the pieces of $\mathbb{P}_i$ (so that $\mathcal{P}(\mathcal{B}_i) \simeq \mathcal{B}_i^+$) then the union of the $\mathcal{B}_i^+$ is $\mathcal{B}^+$. As before we have a well-ordering $\langle x_n : n < \omega \rangle$ of $\mathcal{B}^+$.

We construct the sequence of partitions by recursion. As noted above, $\mathbb{P}_0$ is the trivial partition with only one piece. Thereafter we procede as follows.

Suppose we have constructed partitions up to $\mathbb{P}_{i-1}$, and we have x_j in hand, where x_j is the first element of $\mathcal{B}^+$ (in the sense of the canonical ordering) not already a finite union of pieces of $\mathbb{P}_{i-1}$. We seek a refinement $\mathbb{P}_i$ of $\mathbb{P}_{i-1}$ such that each piece of $\mathbb{P}_i$ contains precisely one element of $\mathcal{B}_i$ and such that x_j is a union of pieces of $\mathbb{P}_i$.

How are we to subdivide the pieces of $\mathbb{P}_{i-1}$ to get pieces of $\mathbb{P}_i$? Clearly whenever x_j extends, or is disjoint from, a piece of $\mathbb{P}_{i-1}$ then we do not need to subdivide that piece in order to get pieces for $\mathbb{P}_i$ such that x_j is a union of some of them. However, if x_j crosses a piece p of $\mathbb{P}_{i-1}$ we need to take steps. There are other things that may cause us to subdivide p and that is the need to ensure that every member of $\mathbb{P}_i$ contains precisely one element of $\mathcal{B}_i$. If p meets x_j and $p \cap x_j$ contains elements from $\mathcal{B}_i \setminus \mathcal{B}_{i-1}$ then we can partition p into pieces each of which contains precisely one element of $\mathcal{B}_i \setminus \mathcal{B}_{i-1}$. Naturally if we can do this for every piece that meets x_j or its complement success is assured.

However there remains the possibility that p crosses x_j but that $p \cap x_j$ contains *no* elements from $\mathcal{B}_i \setminus \mathcal{B}_{i-1}$. This is grave, because then there is no means of partitioning p into pieces each of which contains precisely one element of $\mathcal{B}_i$ and whose union is $p \cap x_j$ (and this of course excludes the possibility of refining $\mathbb{P}_{i-1}$ into a partition every piece of which contains precisely one element of $\mathcal{B}_i$ and such that x_j is a union of pieces).

This means that in these circumstances we have to lower our ambitions. It has turned out to be too much to expect x_j to be a union of pieces of $\mathbb{P}_i$ but we can expect to be able to make it a union of pieces of $\mathbb{P}_{i+k}$ for some finite k. That will be sufficient, because that way every x_j will get used up eventually, but can it be done? We have to go on throwing in elements of $\mathcal{B}_i$, $\mathcal{B}_{i+1}$, $\mathcal{B}_{i+2} \ldots \mathcal{B}_{i+k}$, until $p \cap x_j$ and $p \setminus x_j$ both meet $\mathcal{B}_{i+k}$. But this must happen sooner or later because $\mathcal{B}$ is a union of all the $\mathcal{B}_i$ so any subset of $\mathcal{B}$ (such as $p \cap x_j$) must meet cofinitely many of them.

So, to sum up, the step from $\mathbb{P}_{i-1}$ to $\mathbb{P}_i$ is made with an x_j in mind. If we can refine $\mathbb{P}_{i-1}$ in such a way that every piece of the new partition contains precisely one element of $\mathcal{B}_i$ and x_j is a union of the new pieces, well and good. Set $\mathbb{P}_i$ to be the new partition and worry next about x_{j+1}. If we cannot do this, we can at least refine $\mathbb{P}_{i-1}$ in such a way that every piece of the new partition contains precisely one element of $\mathcal{B}_i$, and we call that $\mathbb{P}_i$. We then attempt the same, starting this time with $\mathbb{P}_i$ and continuing to worry about x_j.

$\blacksquare$

Every countable model of TST is a direct limit of **all** finitely generated models of TST.

Let $\mathfrak{M}$, a countable model of simple type theory, have as its domain a family $\langle \mathcal{B}_n : n < \omega \rangle$ of countable atomic boolean algebras, where $\mathcal{B}_{n+1}$ is a countable atomic subalgebra of $\mathcal{P}(\mathcal{B}_n)$. Let $\mathcal{B}_1$ be a union of an ω-sequence $\langle \mathcal{B}_1^i : i < \omega \rangle$. We then invoke the induction step above repeatedly to obtain, for each n, families $\langle \mathcal{B}_n^i : i < \omega \rangle$ of subalgebras and $\langle \mathbb{P}_n^i : i < \omega \rangle$ of partitions as above. Now for each $i < \omega$ consider the structures $\langle \langle \mathcal{B}_n^i : n < \omega \rangle, \in \rangle$. We have constructed the $\mathcal{B}_n^i$ so that $\mathcal{B}_n^{i+1}$ is an atomic boolean algebra whose atoms are elements of a partition for which $\mathcal{B}_n^i$ is a selection set. Thus, if we want to turn the $\langle \mathcal{B}_n^i : n < \omega \rangle$ into a model of simple type theory the obvious membership relation to take is $\in$ itself. They are models of simple type theory without the axiom of infinity, and by construction their direct limit is pointwise the nth type of M, so the direct limit is M as desired. $\blacksquare$[3]

[3]This suggests that the obvious product topology on the space of countable models of TST might be useful ...

There is an obvious modification for T$\mathbb{Z}$T. Every countable model of TST is a direct limit of all finitely generated models of TST and so is certainly a direct limit of $\mathfrak{M}_1$, $\mathfrak{M}_2 \ldots \mathfrak{M}_n$ where $\mathfrak{M}_1$ is the canonical model where T_0 has one element and $\mathfrak{M}_{n+1}$ is the result of deleting the bottom type off $\mathfrak{M}_n$ and relabelling. Now let $\mathfrak{N}$ be an arbitrary countable model of T$\mathbb{Z}$T, and consider a terminal segment of it. We have just shown that this terminal segment is a direct limit of the $\mathfrak{M}_n$. It is a simple exercise to extend this network of embeddings downwards ...

This tells us that every countable model of T$\mathbb{Z}$T is a direct limit of an ω^* sequence of copies of $\mathfrak{M}_1$.

The missing link is a proof of the assertion that there is no universal-existential sentence (in the language of boolean algebra or perhaps set theory) which has infinite models but no finite models.

The intention is that once we have this we wrap up the proof as follows.

Let ϕ be a existential-universal sentence with an infinite model. Therefore it has a countable model. Then $\neg\phi$ cannot be true in arbitrarily large finitely generated models because otherwise $\neg\phi$ would be true in all countable models. So ϕ is true in all suff large finitely generated models, say all models with at least n atoms.

If $\neg\phi$ has an infinite model so does the expression "$\neg\phi\wedge$ there are at least n atoms". (Indeed they have the same infinite models!) But this expression has no finite models. But unless ϕ is true in all countable models, "$\neg\phi\wedge$ there are at least n atoms" is an example of a universal-existential sentence with an infinite model but no finite models.

So if there is no universal-existential sentence (in which language?) which has infinite models but no finite models then TST decides all universal-existential sentences.

13.8.1 Some other observations that might turn out to be helpful

First prove that if $(\forall\vec{y} \in V_\omega)(\exists\vec{x})\Phi(\vec{x},\vec{y})$ is a universal-existential sentence consistent with TST then its type-free version is true in some transitive wellfounded model of *KF*. Next prove that if $(\forall\vec{y} \in V_\omega)(\exists\vec{x})\Phi(\vec{x},\vec{y})$ is a universal-existential sentence true in some transitive wellfounded model of *KF*then it is true in V_ω. (This ought to be true beco's every transitive wellfounded model of *KF* is an end-extension of V_ω – also rud functions increase rank by only a finite amount may 1998). Then we argue that if $(\forall\vec{y} \in V_\omega)(\exists\vec{x})\Phi(\vec{x},\vec{y})$ is a stratified universal-existential sentence true in V_ω then its typed version has a finitely generated model.

Do we mean true at one type or true at all types?

Suppose $(\forall\vec{y} \in V_\omega)(\exists\vec{x})\Phi(\vec{x},\vec{y})$. Assume that Φ is stratified and is in disjunctive normal form.

Since Φ is stratified there is a stratification of its variables. This suggests an obvious conjecture. If 'u' is of type n why not restrict the quantifier binding 'u'

to V_n and hope the result to be true? How might this go wrong? One obvious way is exemplified by the following formula:

$$(\forall y)(\exists x_1 \ldots x_{10^{10}})(\bigwedge_{1 \leq i < j \leq 10^{10}} (x_i \neq x_j \wedge (y \in x_i \wedge y \in x_j)))$$

This is only going to be true at sufficiently high types. What we have to establish is that this is the *only* way things can go wrong.

First, by reasoning in ZF or Zermelo plus foundation, we argue that every universal-existential sentence true in V is true in V_ω.

Pause briefly to think about the graph of Φ, by which i mean the digraph whose vertices are variables with a directed edge from 'y' to 'x' if '$y \in x$' occurs somewhere. If this digraph has no loops involving 'x' variables we procede as follows, making use of the obvious rank function on 'x' variables which is available in these circumstances.

Instantiate all the 'y' variables to names of individual hereditarily finite sets. We can import the existential quantifiers past the disjunctions so that each disjunct is now a string of existential quantifiers outside a conjunction of atomics and negatomics. At least one of these disjunctions is true: grab it. We now want to find witnesses for – *instantiate* – the 'x' variables bound by the existential quantifiers leading that disjunct, and we want to find these inside V_ω. (Notice that at this stage we can assume there are no positive occurrences of '$=$' within this disjunct, because '$(\exists u)(\exists v)(\ldots u = v \ldots)$ can be rewritten to remove one of the two variables.) Some of these variables "point to" 'y' variables in the sense that there is a directed edge from them to one or more 'y' variables. Such 'x' variables must be instantiated by hereditarily finite sets if they can be instantiated at all, and we know they can be so instantiated because we are assuming that $(\forall \vec{y} \in V_\omega)(\exists \vec{x})\Phi(\vec{x}, \vec{y})$. So instantiate them all simultaneously with a tuple of witnesses in virtue of which we knew that particular instance of $(\forall \vec{y} \in V_\omega)(\exists \vec{x})\Phi(\vec{x}, \vec{y})$.

Now to instantiate the remaining 'x' variables. A witness for this sort of variable must have certain given things as members and certain other things not as members, so why not simply take it to be the set of things that it has to have as its members? Because we might end up thereby instantiating both 'x_1' and 'x_2' to $\{a_1, a_2, \{\emptyset\}\}$, say, while elsewhere in the formula we are trying to make $x_1 \neq x_2$ true, so sometimes we have to add silly elements to things to make them different.

We then continue by recursion on the rank of 'x' variables.

This tells us that any true stratified universal-existential expression in the language of set theory is true in V_ω.

In this connection it may be worth noting that every model of *TSTO* $\mathcal{P}$-extends every finitely generated model, so any $\Sigma_1^{\mathcal{P}}$ sentence true in even one finitely generated model is true in all infinitely generated models.

It is almost certainly time to use the theorem of Ramsey that says that there is a decision procedure to establish whether or not an arbitrary Π_1 sentence has an infinite model. Ramsey claims a generalisation to Σ_2 formulæ.

The following remark should be properly spruced up and included in `TZTstuff.tex`.

13.8.2 An old theorem, revived

We need to distinguish between the Lévy hierarchy and the $\mathcal{P}$ hierarchy. The Lévy hierarchy is the usual one used in the ZF world, where restricted quantifiers don't count, and the $\mathcal{P}$ hierarchy is where restricted quantifiers $\forall x \subseteq y$ and $\exists x \subseteq y$ are allowed.

I have dusted off old notes on ambiguity of $\Sigma_1^{L\acute{e}vy}$ sentences. I think $\mathrm{T}\mathbb{Z}\mathrm{T} \vdash Amb(\Sigma_1^{L\acute{e}vy})$. In fact i suspect that the proof i am about to give might prove something slightly stronger, namely that $\mathrm{T}\mathbb{Z}\mathrm{T}$ proves every consistent $\Sigma_1^{L\acute{e}vy}$ sentence in the language of $\mathrm{T}\mathbb{Z}\mathrm{T}$.

> I am not quite sure what one means by *consistent* in this context but an examination of the construction will reveal it. Notice that we cannot work the same trick in NF, beco's the existence of a Quine atom is $\Sigma_1^{L\acute{e}vy}$ and is certainly not a theorem of NF.

Remark 71 *$T\mathbb{Z}T \vdash Amb(\Sigma_1^{L\acute{e}vy})$*

Proof:

We start by observing that $\Sigma_1^{L\acute{e}vy}$ formulæ generalise upward through end-extensions. This helps with the ambiguity question beco's $\mathfrak{M}^*$ can be thought of as an end-extension of $\mathfrak{M}$. Any injection from the bottom level of $\mathfrak{M}$ to level 1 of $\mathfrak{M}$ lifts to injections from level n to level $n+1$ for all larger n. These injections embody an injection from $\mathfrak{M}$ into $\mathfrak{M}^*$. Since every subset of a value of this injection is itself a value of this injection it preserves $\Delta_1^{L\acute{e}vy}$ formulæ and therefore $\Sigma_1^{L\acute{e}vy}$ formulæ upward.

The hard part is to show that $\Sigma_1^{L\acute{e}vy}$ formulæ generalise *downwards* as well, in the sense that $\mathrm{T}\mathbb{Z}\mathrm{T} \vdash \phi^* \to \phi$ for ϕ a $\Sigma_1^{L\acute{e}vy}$ formula.

This hard part falls into two cases, depending on whether or not the axiom of infinity holds.

- We claim that all models of $\mathrm{T}\mathbb{Z}\mathrm{T} + \neg\mathrm{AxInf}$ satisfy $\mathrm{Amb}(\Sigma_1^{L\acute{e}vy})$.

Since $\Sigma_1^{L\acute{e}vy}$ sentences generalise upwards then, for any $\Sigma_1^{L\acute{e}vy}$ sentence Φ, either it is false in all finitely generated models or there is an n such that it is true in all models bigger than n. This n is standard if the Gödel number of Φ is standard[4]

- We know that $\Sigma_1^{L\acute{e}vy}$ sentences generalise upward; we need to show that if we have AxInf then they also generalise downward.

In fact we will claim something stronger.

[4] Actually, looking at this again while trying to sell it to Randall. . . Is it really true? It is reminding me of the glitch in my erroneous proof of Tennenbaum: if your metatheory T is fibbing then it might tell you, of a nonterminating computation whose gnumber is concrete, that it halts – in a nonstandard number of steps! Now, if $\mathfrak{M} \models \mathrm{T}\mathbb{Z}\mathrm{T} + \neg\mathrm{AxInf}$, then $|M|$ is non-standard finite, so it is bigger than all the n. This shows that *all* models of $\mathrm{T}\mathbb{Z}\mathrm{T} + \neg\mathrm{AxInf}$ satisfy the *same* $\Sigma_1^{L\acute{e}vy}$ sentences.

LEMMA 18 *Suppose ϕ is a $\Sigma_1^{Lévy}$ formula and we can prove in the arithmetic of $T\mathbb{Z}T$ that ϕ is consistent. Then $T\mathbb{Z}T \vdash \phi$.*

Proof:

Perhaps we want to strengthen the assumption to "T$\mathbb{Z}$T $\vdash Con(\mathrm{T}\mathbb{Z}\mathrm{T}) \rightarrow Con(\mathrm{T}\mathbb{Z}\mathrm{T} + \phi)$."

Then, by appealing to the Completeness theorem in T$\mathbb{Z}$T, we argue that ϕ must have a transitive model. The model in which we are conducting this argument will be an end-extension of this model, and so will also believe ϕ. But the arithmetic of T$\mathbb{Z}$T is ambiguous, so this argument can be run at any level.

If that doesn't work, we will need a fall-back. How about this? Let $\mathfrak{M}$ be a model of T$\mathbb{Z}$T. So suppose some $\Sigma_1^{Lévy}$ formula A is true at some level of $\mathfrak{M}$. Then the arithmetic of $\mathfrak{M}$ at some higher level is cognizant of this fact and knows Con(A). But arithmetic is ambiguous, as we showed in remark **??**, so Con((A) is true in the arithmetic much lower down – as far down as we like. Now – since we have AxInf – we can prove the completeness theorem (and as low down as we like). So: in $\mathfrak{M}$ (as low down as we like) there is a model of A, and an $\in$-model at that, and a suitable terminal segment of $\mathfrak{M}$ will be an end-extension of it. Now $\Sigma_1^{Lévy}$ sentences are preserved under end-extensions, so $\mathfrak{M} \models A$ – as low down as we like.

■

The Devil – as always – is in the detail. When T$\mathbb{Z}$T proves there is a model of A. . . what is this model? It's a set with a binary relation on it? What we actually want is a substructure of the model in which we are working, whose membership relation is the membership relation of the model. Can we be sure of getting that?]

There now follows my earlier discussion from years ago.

The next step is to show that if $Th(\mathfrak{M}) \vdash Con(\phi)$ where ϕ is $\Sigma_1^{Lévy}$ in the language of T$\mathbb{Z}$T then M contains an $\in$-model of ϕ.

First we prove in the arithmetic of $Th(\mathfrak{M})$ that $\phi + Ext$ has a model $\mathfrak{N}$ in the integers. The elements of this model have a type discipline in a natural way, and only finitely many types are mentioned. We construct an $\in$-model $\mathfrak{N}'$ essentially by a Mostowski collapse as follows: the elements of minimal (internal) type are the same as they were in $\mathfrak{N}$, namely particular integers at (external) type k, or whatever. The objects of (internal) type 1 in $\mathfrak{N}'$ are to be the appropriate sets of things of (internal) type 0, and these will of course be of (external) type $k+1$. And so on, for finitely many types. Thus

$$\mathrm{T}\mathbb{Z}\mathrm{T}+\ \mathrm{AxInf} \vdash Con(\phi)\ \rightarrow\ \mathrm{T}\mathbb{Z}\mathrm{T}+\ \mathrm{AxInf} \vdash \phi \text{ for } \phi \in \Sigma_1^{Lévy}$$

in slang T$\mathbb{Z}$T+ AxInf reflects $\Sigma_1^{Lévy}$ sentences.

Next we need a converse. Suppose $\phi \in \Sigma_1^{Lévy}$ is true at some level of $\mathfrak{M}$. Therefore ϕ has a model and this model can in fact be coded by a set of $\mathfrak{M}$. Therefore $Th(\mathfrak{M})$ knows that $\phi + Ext$ is a consistent theory. This allegation is expressible in the arithmetic of $\mathfrak{M}$ and so $Th(\mathfrak{M}) \vdash Con(\Phi)$.

Note that this construction cannot work for $\Sigma_1^{\mathcal{P}}$! $\Sigma_1^{\mathcal{P}}$ formulæ would be a good thing to think about next. What is the status of Amb($\Sigma_1^{\mathcal{P}}$)?

13.9 Conjecture 5: finding permutation models

Given a $\forall^*\exists^*$ sentence S, import all the $\exists$'s and export all the $\forall$'s. The result is a formula with $\forall\vec{y}$ outside a conjunction of implications each of the form

$$Y \to (\exists\vec{x})(\phi(\vec{x},\vec{y}))$$

where ϕ is a boolean combination of atomics and negatomics each one containing an x variable, and Y is of the form

$$(\bigwedge_{\langle i,j\rangle\in J\subseteq I^2} y_i\, R\, y_j),$$

where the 'R' is either '$\in$' or '$\notin$'. The disjunction of all the Ys must be valid, since every consistent $\exists^*$ formula of LPC is a theorem of *NF*. We can now export the conjunctions, and this shows that S is a conjunction of things of the form

$$(\forall\vec{y})((\bigwedge_{\langle i,j\rangle\in J\subseteq I^2} y_i\, R\, y_j) \to (\exists\vec{x})(\phi(\vec{x},\vec{y}))$$

Now a conjunction of two formulæ of this form is another formula of this form. This means that without loss of generality we need consider only formulæ of this form.

Can we restrict attention even further to $\forall^*\exists^*$ sentences of this form where the consequent is the existential closure of a **conjunction** of atomics and negatomics rather than a boolean combinations? Sadly, no. Consider $y_1 \in y_1$ and $y_2 \notin y_2$. There is something in y_1 `XOR` y_2 but is it in $y_1 \setminus y_2$ or in $y_2 \setminus y_1$? No reason to suppose either. But perhaps if we supply more information, about whether or not $y_1 \in y_2$ and $y_2 \in y_1$ then we might be able to cut down to a single disjunct.

[This problem is nothing to do with these things being unstratified: the same happens with $y_1 \in y_2 \wedge y_3 \notin y_2$. There is either something in $y_1 \setminus y_3$ or something in $y_3 \setminus y_1$ but we don't know which.]

That this is not true is shown by the following case.

$$(\forall y_1 y_2)(y_1 \notin y_1 \wedge y_2 \in y_2 \wedge y_2 \in y_1 \wedge y_1 \in y_2 \to (\exists x)(x \in y_1 \wedge x \notin y_2) \vee (\exists x)(x \notin y_1 \wedge x \in y_2))$$

This is provable beco's of extensionality, but neither

$$(\forall y_1 y_2)(y_1 \notin y_1 \wedge y_2 \in y_2 \wedge y_2 \in y_1 \wedge y_1 \in y_2 \to (\exists x)(x \notin y_1 \wedge x \in y_2))$$

nor

$$(\forall y_1 y_2)(y_1 \notin y_1 \wedge y_2 \in y_2 \wedge y_2 \in y_1 \wedge y_1 \in y_2 \to (\exists x)(x \in y_1 \wedge x \notin y_2))$$

are provable because we can find y_1 and y_2 satisfying the antecedent with $y_1 \subseteq y_2$ and y_1 and y_2 satisfying the antecedent with $y_2 \subseteq y_1$. Try $y_2 := \overline{BV}$; $y_1 := \{\overline{BV}\}$ for the first case and $y_2 := \overline{BV}$; $y_1 := \overline{BV} \cup \{V\}$ for the second.

Anyway the idea now is that all we have to do to prove the $\forall^*\exists^*$ conjecture is to show how to get a permutation model of anything of the form

$$(\forall\vec{y})(\Big(\bigwedge_{\langle i,j\rangle\in J\subseteq I^2} y_i \in y_j) \to (\exists\vec{x})(\phi(\vec{x},\vec{y})\Big)$$

as long as it's consistent with NF0. But is it not the case that every $\forall^*\exists^*$ sentence consistent with *NF*0 is true in the term model for *NF*0?

That suggests considering only permutations that leave NF0 terms alone, since they have witnesses anyway!

We can't just move things that aren't NF0 terms, since being an *NF*O term is not stratified, so we have to move things are not "sufficiently like" NF0 terms. The idea is that anything sufficiently like an NF0 term will satisfy the $\forall^*\exists^*$ formula we have in mind at any one time, where "sufficiently alike" depends on the formula in question. So we consider only those permutations that, say, swap with their complements those things that are not *NF*0 terms of rank at most k for some concrete k.

Illustrate this by thinking about the assertion that there are no Boffa atoms. What witness is there?

There is something very odd about the case

$(\forall y_1 y_2)(y_1 \not\in y_1 \wedge y_2 \in y_2 \wedge y_2 \in y_1 \wedge y_1 \in y_2 \to (\exists x)(x \in y_1 \wedge x \not\in y_2) \vee (\exists x)(x \not\in y_1 \wedge x \in y_2))$

The point is that this is true not because of the behaviour of NF0 terms, but because of extensionality and classical logic. There is no reason to suppose that the witnesses will be easy to find.

13.10 Positive results obtained by permutations

Let's have a section on $\forall^1\exists^*$ sentences. The point is that the difficulty with extensionality (no definable witnesses) cannot occur if there is only one $y$universally quantified variable.

Is it the case that every $\forall^1\exists^*$ sentence consistent with NF_2 has a permutation model? This is conjecture 1.

Is the set of $\forall^1\exists^*$ sentence consistent with NF_2 closed under conjunction? The set of $\forall^1\exists^*$ sentences is closed under conjunction

Many of these are published, and collected in Forster [1991]. Here are some new ones.

13.10.1 The size of a self-membered set is not a concrete natural

Boffa has made some progress on this front. He has proved that, if the axiom of counting holds, there is a permutation π such that in V^π there is no self-membered finite set. A little adjustment strengthens the conclusion and weakens the assumption slightly.

Remark 72 *If NF+AxCount$_\leq$ is consistent so is NF+AxCount$_\leq$ + "Every self-membered set maps onto $\mathbb{N}$".*

Proof: Let X be the collection of sets that do not map onto $\mathbb{N}$. If x is such a set, then the set of $n \in \mathbb{N}$ such that $\{n\} \times V$ meets x is finite, and will have a last member. Add 1 to this last member to get a number we will call n_x. n_x has the feature that $(\forall m \geq n_x)((x \cap (\{m\} \times V)) = \Lambda)$. Tn_x is the same type as x and so the permutation

$$\prod_{x \in X} (x, \langle Tn_x, x \rangle)$$

is a set. Notice that if $x \in X$ then $\tau`x$ is infinite and not equal to x.

Now suppose $x \in \tau`x$. To prove that in $V^\tau$ every self-membered set is infinite it will suffice to show that $\tau`x$ is infinite. We will assume AxCount$_\leq$ and prove that $\tau`x$ has a countable partition.

If x is fixed then x is infinite so $\tau`x$ (which is x) is infinite as desired. If x is not fixed there are two cases to consider.

(i) $x \in X$. Then $\tau`x$ is infinite by construction.

(ii) $\tau`x \in X$. Then $x = \langle Tn_{\tau`x}, \tau`x \rangle$. But also $x \in \tau`x$ so $\langle Tn_{\tau`x}, \tau`x \rangle \in \tau`x$. Now $n_{\tau`x}$ has been chosen to be so large that no ordered pair $\langle m, y \rangle$ is a member of $\tau`x$ for any $\geq n_{\tau`x}$. So, to get a contradiction all we need is $Tn_{\tau`x} \geq n_{\tau`x}$. The simplest way to get this is to assume AxCount$_\leq$.

■

(Originally Boffa had taken n_x to be the *first* n such that $\{n\} \times V$ does not meet x. That way he needs the whole of the axiom of counting.) Friederike Körner and i both noticed that to make this proof work it is sufficient to have a (set) function $f : \mathbb{N} \to \mathbb{N}$ such that $(\forall n)(f(Tn) \geq n)$. I propose to call such functions **Körner functions**. If we have such a function we swap x (when x is finite) with $\langle f(Tn_x), x \rangle$ instead of $\langle (Tn_x), x \rangle$. Indeed in those circumstances we can do something even better.

Remark 73 *If there is a function $f : \mathbb{N} \to \mathbb{N}$ such that $(\forall n)(f(Tn) \geq n)$ then (letting π be the permutation*

$$\prod_{|x| \in \mathbb{N}} (\langle f(Tn_x), x \rangle, x)$$

that swaps x with $\langle f(Tn_x), x \rangle$ for x finite) we find that in V^π the membership relation restricted to finite sets is wellfounded.

Proof:

Suppose $V^\pi \models x \in y \wedge |x| \in \mathbb{N} \wedge |y| \in \mathbb{N}$. Then $\pi(x)$ and $\pi(y)$ are both finite and $x \in \pi(y)$. We will show $n_{\pi(x)} < n_{\pi(y)}$. Since $\pi(x)$ is finite, x must be $\langle f(Tn_{\pi(x)}), \pi(x) \rangle$. But then, since $x \in \pi(y)$, the first component of x must be less than $n_{\pi(y)}$, so $f(Tn_{\pi(x)}) < n_{\pi(y)}$. But we have $n_{\pi(x)} < f(Tn_{\pi(x)})$ by choice of f so $n_{\pi(x)} < n_{\pi(y)}$ as desired. ■

(In fact we can swap x and $\langle x, f(Tn_x)\rangle$ as long as x does not map onto $\mathbb{N}$. So we can set $\pi := \prod(x, \langle x, f(Tn_x)\rangle)$ taking $(x, \langle x, f(Tn_x)\rangle)$ to be the identity if n_x is undefined.)

Friederike Körner then showed that it is consistent relative to *NF* that there should be $n \in \mathbb{N}$ such that for all greater m we have $m < Tm$, and that means there is such an f, namely $\lambda x.(\text{if } x < n \text{ then } n \text{ else } x)$. Let us call natural numbers k such that $(\forall n \in \mathbb{N})((n+k) < T(n+k))$ **Körner numbers**.

The significance of Körner numbers is that if there is a Körner number then there is a Körner function, a function $f : \mathbb{N} \to \mathbb{N}$ such that $(\forall n \in \mathbb{N})(n \leq f\text{‘}Tn)$. The existence of such a function commuting with T is of course equivalent to AxCount$_{\leq}$, but this is weaker, and implies that there is a permutation model in which $\in\restriction FIN$ is wellfounded (which indeed was how we found it!). Given the desire to find cardinal arithmétic equivalents for all modalised sentences it is natural to try to find a converse ...

The following result uses ideas of Boffa-Pétry from []

Remark 74

If NF is consistent so is NF + “No strongly cantorian set is self-membered”.

Proof:

For $\alpha \in T\text{“}NO$ set

- $F(\alpha, x) = \{u \in x : (\exists y)(u = \langle V, T^{-1}\alpha, y\rangle)\}$
- $\mu(x)$ = the least $\alpha \in T\text{“}NO$ such that $F(\alpha, x) = \emptyset$ if there is one, $= V$ otherwise.

Note the following:

1. $stcan(x) \to (\exists\alpha \in T\text{“}NO)(F(\alpha, x) = \emptyset)$;
2. If $stcan(x)$ then $\mu(x)$ is a strongly cantorian ordinal;
3. For all x, $(\forall y)(\langle V, T^{-1}(\mu(x)), y\rangle \notin x)$.

Then set
$$\pi = \prod_{x \notin |V|} (x, \langle V, \mu(x), x\rangle)$$

If we alter the definition of μ so it picks up the sup of the nonempty Fs rather than the first empty F we have to be sure that (ii) remains true. It will be true if only strongly cantorian ordinals can have strongly cantorian cofinality. But perhaps that's not even plausible....

I now think that – assuming that this works at all – it establishes that $\in$ restricted to strongly cantorian sets is wellfounded. To that end, suppose V^π believes that x is a member of y and both are strongly cantorian. We will show that $\mu(\pi(x)) < \mu(\pi(y))$.

So $\pi(x)$ and $\pi(y)$ are both strongly cantorian and therefore cannot be nasty ordered triples. So it is x and y that are the nasty triples, and we must have $x = \langle V, \mu(\pi(x)), \pi(x)\rangle$ and $y = \langle V, \mu(\pi(y)), \pi(y)\rangle$

We also have $x \in \pi(y)$, which is to say that the triple $x = \langle V, \mu(\pi(x)), \pi(x)\rangle$ is one of the triples in $\pi(y)$.

We want $\mu(\pi(x)) < \mu(\pi(y))$. $\pi(x)$ is strongly cantorian, so $\mu(\pi(x))$ is a strongly cantorian ordinal.

This will work as long as $cf(\Omega)$ is not strongly cantorian. In fact i suspect that it will show that membership restricted to small sets is wellfounded as long as $cf(\Omega)$ is not small. ■

I think we have to modi
definition of $\mu(x)$ to b
sup of nonempty Fs r
than the first nonempt
...

So let's try to generalise the Boffa-Pétry construction

For $\alpha \in T``NO$ set

- $F(\alpha, x) := \{u \in x : (\exists y)(u = \langle V, T^{-1}\alpha, y\rangle)\}$
- $\mu(x)$:=sup$\{\alpha + 1 \in T``NO : F(\alpha, x) = \emptyset\}$ if this sup is defined, $= V$ otherwise.

Note the following:

1. If x is small then $\mu(x)$ is not V;
2. For all x, $(\forall y)(\langle V, T^{-1}(\mu(x)), y\rangle \notin x)$.

Then set

$$\pi = \prod_{x \notin |V|} (x, \langle V, \mu(x), x\rangle)$$

(Perhaps we don't need to swap everything smaller than V: it may be that swapping only small things will do; but we shall see.)

We shall attempt to show that, in V^π, $\in$ restricted to small sets is wellfounded. So let x and y be such that V^π believes $x \in y$ and that both x and y are small. We will (we hope) infer from this that $\mu(\pi(x)) < \mu(\pi(y))$.

Assuming that smallness is a property preserved under surjection we know that V^σ believes x to be small iff $\sigma(x)$ was small in V. So in this context we infer that $\pi(y)$ and $\pi(x)$ are both small and so cannot be nasty ordered triples. So it is x and y that are the nasty triples, and we must have $x = \langle V, \mu(\pi(x)), \pi(x)\rangle$ and $y = \langle V, \mu(\pi(y)), \pi(y)\rangle$

We also have $x \in \pi(y)$, which is to say that the triple $x = \langle V, \mu(\pi(x)), \pi(x)\rangle$ is one of the triples in $\pi(y)$. from which $\mu(\pi(x)) < \mu(\pi(y))$ is immediate ■

So we seem to have shown that:

if $cf(\Omega)$ is not small, then $\Diamond(\in\restriction$small sets is wellfounded).

There doesn't seem to be anything special about the choice of Ω here

It's worth remembering that in Boffa's original construction μ picks up the first empty F rather than the sup of the nonempty Fs. I thought this was wasteful but actually the difference between his definition and my modification of it is the same as the difference between the definition of grundyrank on a wellfounded structure and the definition of rank, so it might be something natural and meaningful.

H I A T U S

It seems that we should be able to do better than this. Suppose there is a function $f : NO \to NO$ such that $(\forall\alpha)(f(T\alpha) \geq \alpha)$. Let α_x be the first ordinal that is bigger than every ordinal in $\texttt{fst}``x$. α_x is defined as long as x is small in the sense of not being mappable onto a cofinal subset of NO. Then let π be the permutation that swaps x with $\langle f`(T\alpha_x), x\rangle$ for x small then in V^π the membership relation restricted to small sets is wellfounded.

Can we tweak André's proof to show that Con(NF) $\to$ Con(NF+ $\in\restriction stcan$ is wellfounded)?

What can we say about the idea that there is an $f : NO \to NO$ such that $f(T\alpha) \geq \alpha$? Suppose there is such a function, and let X be a cofinal subset of $T``NO$. Then $f``X$ is a cofinal subset of NO so $cf(NO) \leq cf(T``NO) = T(cf(NO))$. For the other direction sse, to take a straightforward case, that $cf(NO) = \omega$. To get such an f (try it!) we would need AxCount$_\leq$.

Boffa has a conjecture that

Conjecture 7 *It is consistent with NF that* $(\forall x)(x \in x \to |x| = |V|)$

The dual of this is $(\forall x)(x \notin x \to |V \setminus x| = |V|)$. Now if these two hold simultaneously we infer $(\forall x)(|x| = |V| \vee |V \setminus x| = |V|)$. This is stratified and so is certainly not going to be provably consistent by means of permutations. It is known that there are models of ZF in which the real line can be split into two smaller pieces. Richard Kaye's idea for a counterexample to $(\forall x)(|x| = |V| \vee |V \setminus x| = |V|)$ is $\{y : |y| < |V|\}$. In view of what follows we should also consider $\{y : |y| \not\geq^* |V|\}$.

If we think of Bernstein's lemma, all it tells us is that $(\forall x)(|x| = |V| \vee |V \setminus x| \geq_* |V|)$.

If $|x| \not\geq_* |V|$ we say that x is **small** and if $|V \setminus x| \not\geq_* |V|$ we say x is **co-small**. By Bernstein's lemma a set cannot be simultaneously co-small and small. (Beware: not everything the same size as a co-small set is co-small: every co-small set is of size $|V|$ but not *vice versa*. However, nothing the size of a co-small set is small.)

This suggests that we might be able to tackle a weaker version by permutations, namely:

Conjecture 8 *NF*$\vdash \Diamond(\forall x)(x \in x \to |x| \geq_* |V|)$

We can make a small amount of progress with this version of the conjecture.
\begin{digression}

Some remarks on Quine pairs

In what follows we will be using ordered pairs in the style of Quine. That is to say, we set $\langle x, y\rangle = \theta_1``x \cup \theta_2``y$, where θ_1 and θ_2 are homogeneous bijections between V and two other sets $\theta_1``V$ and $\theta_2``V$ such that $\theta_1``V = -\theta_2``V$. Quine actually provides two such functions θ_1 and θ_2 but we do not need to know anything more about them than i have just said. $\texttt{fst}(x)$ is the first component of the ordered pair x.

> The advantage Quine pairs are usually supposed to have is that they ensure the "$x = \langle y, z\rangle$" is homogeneous. There are other advantages as well. If we need a disjoint union function $x \sqcup y$ then $\langle x, y\rangle$ would do. $\langle V, \subseteq, -\ldots\rangle$ is a boolean algebra, and so is $V \times V$. The Quine pairing function is actually an **isomorphism** between $V \times V$ and V. Thus, $V \setminus \langle x, y\rangle = \langle V \setminus x, V \setminus y\rangle$, $\langle x \cap y, z\rangle = \langle x, z\rangle \cap \langle y, z\rangle$, and so on. Some of this will be useful in what follows.
>
> Of course this is less attractive in the context of *ZF*, but similar results hold. One should also think about the smallest number of types with which one can define the two theta functions. Is now the time to go back and look at Joel Friedman Some set-theoretical partition theorems suggested by the structure of Spinoza's God. SYNTHESE v 27 (1974) pp 199-210

`\end{digression}`

REMARK 75
If

$$(\forall x)(x \in x \to |x| \geq_* |V|)$$

is consistent with NF, so is

$$(\forall x)(x \in x \to |x| \geq_* |V|) \land (\forall x)(|V \setminus x| \not\geq_* |V| \to x \in x)$$

Proof:
The two conjuncts are duals of each other, so one is consistent iff the other is. So let us start with a model V satisfying

$$(\forall x)(|V \setminus x| \not\geq_* |V| \to x \in x)$$

We want to swap every small set x with $\langle V \setminus \texttt{fst}``x, x\rangle$ but to do this we must check that if $x$ is small then $\langle V \setminus \texttt{fst}``x, x\rangle$ isn't (otherwise we would have to swap that with $\langle V \setminus \texttt{fst}``\langle V \setminus \texttt{fst}``x, x\rangle, \langle V \setminus \texttt{fst}``x, x\rangle\rangle$ and the definition would not be consistent.) We will show that if $x$ is small $\langle V \setminus \texttt{fst}``x, x\rangle$ is not small, and *vice versa*.

Suppose x is small. $\langle V \setminus \texttt{fst}``x, x\rangle$ is a superset of $\theta_1``(V \setminus \texttt{fst}``x)$. Now $\texttt{fst}``x$ is a surjective image of a small set and is therefore small. Therefore $V \setminus \texttt{fst}``x$ is a co-small set, and $\theta_1``(V \setminus \texttt{fst}``x)$, being the same size as a co-small set, is at least not small, so its superset $\langle V \setminus \texttt{fst}``x, x\rangle$ is not small either.

For the converse suppose $\langle V \setminus \texttt{fst}``x, x\rangle$ is small. If $\langle V \setminus \texttt{fst}``x, x\rangle$ is small, then so is its subset $\theta_1``(V \setminus \texttt{fst}``x)$. But if $\theta_1``(V \setminus \texttt{fst}``x)$ does not map onto V neither does $V \setminus \texttt{fst}``x$. So $\texttt{fst}``x$, being the complement of a small set, is co-small. But if $\texttt{fst}``x$ is co-small, x cannot be small.

(If we were to try to prove an analogous result with "small" meaning "smaller than V", this is where the proof would break down. We cannot show that if x is smaller than V then $\langle V \setminus \texttt{fst}``x, x\rangle$ isn't. As far as we know $x$ could be smaller than $V$ but $\texttt{fst}``x$ could be the whole of V. Well, that last bit isn't true but we still have to be careful.)

Now we can safely set

$$\pi = \prod_{|x| \not\geq_* |V|} (x, \langle V \setminus \mathtt{fst}``x, x\rangle)$$

We will verify the two conjuncts separately.

$$V^\pi \models (\forall x)(|x| \not\geq_* |V| \to x \notin x)$$

This is

$$V \models (\forall x)(|x| \not\geq_* |V| \to \pi`x \notin x)$$

We procede by a case analysis:

Surely we mean '$x \notin \pi(x)$'...?

- If $x = \pi`x$ then x was not small, because all small things are moved. Therefore the antecedent is false and the conditional is true.
- If $x \neq \pi(x)$ and x is small, then $\pi(x) = \langle V \setminus \mathtt{fst}``x, x\rangle$. Since $x$ is small, $\mathtt{fst}``x$ (which is a surjective image of x) is also small, so $V \setminus \mathtt{fst}``x$ is co-small, and therefore – by hypothesis – a member of itself. Therefore $\langle V \setminus \mathtt{fst}``x, x\rangle \notin x$, which is to say $\pi(x) \notin x$.
- If $x \neq \pi(x)$ and x is not small, then the antecedent is false and the conditional is true.

We also want the dual to hold in V^π, as it did in V. So we want

$$V^\pi \models (\forall x)(|V \setminus x| \not\geq_* |V| \to x \in x)$$

This is

$$V \models (\forall x)(|V \setminus \pi(x)| \not\geq_* |V| \to x \in \pi(x))$$

As before, we do a case analysis.

- If x is fixed, the result is true because it was true in the base model by hypothesis.
- If x is small, then $\pi(x) = \langle V \setminus \mathtt{fst}``x, x\rangle$. This is $\theta_1``(V \setminus \mathtt{fst}``x) \cup \theta_2``x$, so $V \setminus \pi(x) = \theta_1``(\mathtt{fst}``x) \cup \theta_2``(V \setminus x)$. But if $x$ is small, $V \setminus x$ is co-small, and so $\theta_2``(V \setminus x)$ – being the same size as a co-small set – cannot be small. So its superset $\theta_1``(\mathtt{fst}``x) \cup \theta_2``(V \setminus x)$ isn't small either. But $\theta_1``(\mathtt{fst}``x) \cup \theta_2``(V \setminus x)$ is $V \setminus \pi(x)$. Therefore $V \setminus \pi(x)$ is not small so the antecedent is false, and the conditional true.
- If x is not small, it is $\pi(y)$ for some small set y. So $\pi(x)$ is small, and so $V \setminus \pi(x)$ is co-small and the antecedent is false.

■

Another observation in the same style is the following:

REMARK 76
If there is a wellfounded set X such that $\mathcal{P}_\kappa(X) \subseteq X$ then there is a permutation model in which $\in$ restricted to sets without partitions of size κ is wellfounded.

Proof: (κ actually has to satisfy the extra condition: $\alpha \leq^* \kappa \to \alpha \leq \kappa$, but κ will be an aleph in all current applications – for the moment at least.) Let π be the product

$$\prod_{|x| \not\leq^* \kappa} (x, \langle V \setminus x, (\texttt{snd}``x \cap X)\rangle)$$

of the transpositions $(x, \langle V \setminus x, (\texttt{snd}``x \cap X)\rangle)$ over all x without partitions of size κ.

Let such sets be "κ-small", at least for the duration of this proof. This is basically a Boffa permutation (as in remark 72). However, there is a slight wrinkle. With Boffa's original permutation much use was silently made of the fact that the second components of the ordered pairs in the story were *large*, being natural numbers. This ensured that whenever π moved x, then $\pi(x)$ was large iff x was small. This was essential to the plot, and remains essential here. Now $\texttt{snd}``x \cap X$ is small if x is, so in order to achieve "whenever π moves x, then $\pi(x)$ is large iff x is small" we need to do something to the $\texttt{fst}$ element of the pair it make *it* large instead. This is what complementation is doing.

Let's just check this. If x is κ-small then $\pi(x)$ is an ordered pair one of whose components is $V \setminus x$ wot ain't nohow κ-small, so $\pi(x)$ is not κ-small. Now suppose $\pi(x) \neq x$ and $\pi(x)$ is not small. Then it is $\langle V \setminus x, \texttt{snd}``x \cap X\rangle$. By design of π, this object can only have been moved from x, so x was κ-small.

Suppose V^π thinks that that $x \in y$ and both are κ-small. This last tells us – as we have seen – that y must be $\langle V \setminus \pi(y), (\texttt{snd}``\pi(y) \cap X)\rangle$, and $x$ must be $\langle V \setminus \pi(x), (\texttt{snd}``\pi(x) \cap X)\rangle$. Now $x \in \pi(y)$ so $\texttt{snd}(x) \in \texttt{snd}``\pi(y)$. Now $\texttt{snd}(x) = \texttt{snd}``\pi(x) \cap X$ so $\texttt{snd}(x)$ is at least a subset of X, and it's κ-small because it's a subset of $\texttt{snd}``\pi(x)$ which is a surjective image of $\pi(x)$ which is $\kappa$-small. So it's a $\kappa$-small subset of $X$ and is therefore a member of $X$, since $\mathcal{P}_\kappa(X) \subseteq X$. So $\texttt{snd}(x)$ is a member of both $\texttt{snd}``\pi(y)$ and X, so it's a member of $\texttt{snd}``\pi(y) \cap X$, which is $\texttt{snd}(y)$ so $\texttt{snd}(x) \in \texttt{snd}(y)$.

Thus we have shown that: whenever V^π thinks that $x \in y$ and both x and y are κ-small, then $\texttt{snd}(x) \in \texttt{snd}(y)$, and we also know that both of these things are in X. In other words, if we let K be the set of things that V^π believes to be κ-small, then $\texttt{snd}$ is a homomorphism from $\langle K, \in_\pi\rangle$ to $\langle X, \in\rangle$. X is wellfounded by assumption, so $\langle K, \in_\pi\rangle$ must be too.

■

It might be worth considering an indexed family of permutation models generated as follows. Given an X as above (minus the wellfoundedness condition) let π_X be the permutation defined as above. Order them according to the partial order on the X's. The result is a Kripke model of something-or-other.

It would be very nice to have a converse to remark 76.

My version of Boffa's conjecture is: co-small implies self-membered. (A special case of) the universal-existential conjecture is: self-membered implies meets everything in the sublattice generated by the values of B. The conjunction of these two implies that every co-small set meets everything in the sublattice generated by the values of B. This we know to be true.

Can we spice this up to lattices generated by free bases for V?

How many bases are there? How big are they? How big are their elements?

Given any basis i can swap any element with its complement, so the number of bases is at least two-to-the size of any basis.

The $\forall^*\exists^*$ conjecture implies that if $x \in x$ then x meets every element of the standard basis. How about every element of every basis? Doesn't that sound a bit like "Every self-membered set generates $\langle V, \subseteq, -\rangle$"?

I claim the following

1. "Every self-membered set generates $\langle V, \subseteq, -\rangle$" is $\forall^*\exists^*$;
2. If α is the size of a generating set then $T|V| \leq 2^\alpha$;
3. If $2^\alpha = T|V|$ then there is a basis of size α.

The first is easy to check. It is $(\forall y \in y)(\forall y_1 y_2)(\exists x \in y)(y_1 \in x \longleftrightarrow y_2 \notin x \vee y_1 = y_2)$. The following generalisation of item (i) merits attention: $x \in x \to x \cap \mathcal{P}(x)$ generates $\mathcal{P}(x)$. It's not $\forall^*\exists^*$ but it's natural.

(ii) Follows beco's every singleton is an intersection of basis elements and complements of basis elements.

(iii) Sse $F : \iota``V \longleftrightarrow \mathcal{P}(X)$ is a bijection. Each singleton $\{y\}$ corresponds to a subset $X'$ of $X$, and we deem that $\{y\}$ is the intersection of the basis elements belonging to $X'$ and the complements of the basis elements in $X \setminus X'$. So $f`x$ must be $\bigcup\{y \in \iota``V : x \in F(y)\}$. Then $f``X$ is a basis.

small$(x) \to x \notin x$; Hsmall$(x) \to WF(x)$; $\in$small is wellfounded.

13.10.2 Bases for the irregular sets

Something about this in coret.tex

A set is **irregular** iff it meets all its members. A basis for the irregular sets is a set that meets every irregular set. Some of the theorems we have proved can be expressed as facts about bases. Membership restricted to finite sets being wellfounded is the same as the infinite sets forming a basis. Can the uncountable sets form a basis? We shall see! However the set of co-small sets isn't big enuff to be a basis. If X is irregular so is $B``X$, and no member of $B``X$ is co-small!

Still, there is a large gap between the set of uncountable sets and the set of co-small sets.

13.10.3 Membership restricted to ideals and their filters

Need to fit Button-Hurkens into here somewhere, and take this section by the scruff of the neck.

Button-Hurkens: If X is a bottomless (aka irregular) set of ideals, then $\bigcap X$ contains all wellfounded sets.

History seems to lead us thus. We start off with a notion of smallness (like *finite*) and notice that no small set seems to be a member of itself. We then conjecture that $\in$ restricted to small sets is wellfounded, and finally that R (defined by $R(x, y)$ iff x and y are both small or co-small and $x \in y \longleftrightarrow y$ is small) is wellfounded. But it's no good if the ideal of small sets is prime:

REMARK 77

Let I be a prime ideal and consider the relation $x\,R\,y$ defined as $x \in y \longleftrightarrow y \in I$. Then R is not wellfounded.

Proof:

In those circumstances $\in\restriction I$ is wellfounded and $\notin\restriction I$ is wellfounded. Find somehow sets a and b such that $a \notin a \cup b$ and $b \in a \cap b$. (This is easy to arrange: set $a := \overline{B\Lambda}$; $b := B(V)$.) Then $a \notin a$ so $a \in I$ and $b\,R\,a$ 'cos $b \in a$. $b \in b$ so $b \notin I$ and $a\,R\,b$ 'cos $a \notin b$. Then R is not wellfounded. ■

(Notice that this refutation uses the NF0 axiom, so we might get away with the following relation over Church-Oswald models of NF_2 might be wellfounded (at least when the $k$oding function is nice): $x \in_{new} y \longleftrightarrow (\texttt{snd}(k^{(-1)}(y)) = 0)$.)

There are two steps involved:

(i) Move from "$\in$ restricted to I has no loops of diameter 1, 2 ..." to "$\in$ restricted to I is wellfounded"

(ii) to Move from "$\in$ restricted to I is wellfounded" to "R is wellfounded".

How difficult are these? Where $I = FIN$, (i) seems clear enuff. How about (ii)? Perhaps the permutation making $\in\restriction FIN$ wellfounded (or some variant of it) will also make this other relation wellfounded.

Try the following permutation: if x is finite, swap x with $\langle f(Tn_x), x\rangle$; if x is cofinite swap x with $\langle f(Tn_{V\setminus x}), V \setminus x\rangle$. (I think we will need a sort-of rank function that sends x to $n_{V\setminus x}$ if x is finite and to $n_{V\setminus x}$ if n is cofinite. Call this n'_x)

We want $V^\pi \models$"R is wellfounded". Now $V^\pi \models\ x\,R\,y$ iff
$\pi(x)$ and $\pi(y)$ are both finite-or-cofinite and $x \in \pi(y) \longleftrightarrow \pi(y)$ is finite.
Want to show that if $V^\pi \models\ x\ R\ y$ then $n'_{\pi(x)} < n'_{\pi(y)}$.
case 1: $\pi(y)$ is finite. Then $y = \langle f(Tn_{\pi(y)}), \pi(y)\rangle$.

1. Case 1a $\pi(x)$ is finite. Then x must be $\langle f(Tn_{\pi(x)}), \pi(x)\rangle$. But then, since $x \in \pi(y)$, the first component of x must be less than $n_{\pi(y)}$, so $f(Tn_{\pi(x)}) < n_{\pi(y)}$. But we have $n_{\pi(x)} < f(Tn_{\pi(x)})$ by choice of f so $n_{\pi(x)} < n_{\pi(y)}$ as desired.

2. case 1b $\pi(x)$ is cofinite. Then x must be $\langle f(Tn_{V\setminus\pi(x)}), V \setminus \pi(x)\rangle$. But then, since $x \in \pi(y)$, the first component of x must be less than $n_{\pi(y)}$, which is to say $f(Tn_{V\setminus\pi(x)}) < n_{\pi(y)}$ and therefore (by choice of f) $n_{V\setminus\pi(x)} < n_{\pi(y)}$.

case 2 $\pi(y)$ is cofinite. Then $y = \langle f(Tn_{V\setminus\pi(y)}), V \setminus \pi(y)\rangle$.

Case 2a $\pi(x)$ is finite. Then x must be $\langle f(Tn_{\pi(x)}), \pi(x)\rangle$. But then, since $x \in \pi(y)$, the first component of x must be less than err......

. . . will get to the bottom of this.

At any rate (when $I = FIN$) the assertion that R has no loops is a $\forall^*\exists^*$ scheme. For example here is the subscheme that says there are no loops of diameter 2.

$$\forall\vec{x}\forall\vec{y}\bigwedge_{i,j}(x_i = -\{y_1 \ldots y_n\} \to y_j \neq \{x_1 \ldots x_m\})$$

13.10.4 a bit of duplication here

Why does Boffa's permutation work? The reason is that there is a set X with a wellfounded relation on it, and a map which accepts a bounded subset of X and returns a bound. So here's an idea. Force with the following family. Let X satisfy $\mathcal{P}(X) \subseteq X$ (tho' perhaps i mean $\mathcal{P}_\alpha(X)$ for some α – wait and see!). Let A be the set of things x so small that any map from x to X has bounded range. Remember that in Boffa's original treatment X was the set of naturals and it was very important that naturals *qua* sets, are very big. To preserve this feature we will deal not with members of X but with members of X **labelled** to be big. A **widget** is a pair $\langle x, V\rangle$ with $x \in X$. Let Y be a set of widgets. Then $\bigvee Y$ is $\langle\bigcup \texttt{fst}``Y, V\rangle$. ("Peel off the labels, take the sup, put a label on again"). Then consider the permutation

$$\prod_{x\in A}(x,\ \langle x, \bigvee((X \times V) \cap \texttt{snd}``x)\rangle)$$

This is not enuff to show that comparatively small things are not self-membered, but if we force over all such X we might end up with a model in which: $\in\restriction\{x : (\exists y)(WF(y) \wedge |y| = |x|)\}$ is wellfounded. I see no reason why this should not be true. I have actually managed to show that every model of ZF is the wellfounded part of a model of NF_2 in which the membership relation restricted to low sets is wellfounded.

Maybe we should start from below and have a large wellfounded set X . . .

Suppose H_κ were a set. Label its elements as above to get widgets. Let A be the set of things x such that no map $x \to H_\kappa$ is unbounded. Consider the permutation

$$\prod_{x\in A}(x, \langle x, \bigvee((X \times V) \cap \texttt{snd}``x)\rangle)$$

Isn't this remark 76?

13.11 Some provable special cases or weak versions

The two following results are already in print:

Remark 78
Every $\forall^*\exists^*$ sentence consistent with NF0 is true in the term model for NF0.

We should show that this holds for branching-quantifier formulæ all of whose quantifier prefixes are $\forall^*\exists^*$.

But this is immediate – the same proof works!

I think we should be able to prove that every stratified $\forall^*\exists^*$ sentence consistent with NF_2 is true in the term model for NF_2. In fact it's quite a nice question how much we can weaken "stratified".

Remark 79
Every countable binary structure can be embedded in the term model for $NF0$.

Think of this last remark as saying that every $\exists^\infty$ expression consistent with NF0 is true in the term model.

There are also these two very similar lemmas on term models

Remark 80
Let M be the (NF-)term model from some model N of NF, and suppose M is extensional.
Let '$(\exists\vec{y})(\Phi(\vec{x},\vec{y}))$' be weakly stratified and suppose that '$(\forall\vec{x})(\exists\vec{y})\Phi(\vec{x},\vec{y})$' is true in N.
Then it is true in M.

Proof:

Assume the hypotheses. $(\exists\vec{y})(\Phi(t,\vec{y}))$ for any choice $\vec{t}$ of terms. We now want to be sure that witnesses for the $\vec{y}$ can be found in M. To do this, consider $\{\vec{y}$: $\Phi(t,\vec{y})\}$. This is a term if we can stratify the $\vec{y}$, as the matrix will be stratified since the t_i (being closed terms) can be given any type. M is an extensional substructure of N, and so there must be such a witness in M. ■

And now the second theorem.

Remark 81 *Let $\mathfrak{N}$ be a model of some subsystem T of NF extending $NF\forall^*$, and $\mathfrak{M}$ be the T-term model from $\mathfrak{N}$, with $\mathfrak{M}$ extensional.*
Let '$\exists\vec{y}\Phi(\vec{x},\vec{y})$' be weakly stratified with 'Φ' quantifier-free. Suppose

$$\mathfrak{N} \models \forall\vec{x}\exists\vec{y}\Phi(\vec{x},\vec{y})$$

then

$$\mathfrak{M} \models \forall\vec{x}\exists\vec{y}\Phi(\vec{x},\vec{y})$$

Proof:

Assume the hypotheses.

We start counting the $\vec{y}$ at y_0. Then for each $\vec{t} \in M$, $N \models \exists\vec{y}\Phi(\vec{t},\vec{y})$ and the question is, can these $\vec{y}$ be found inside $\mathfrak{M}$? Consider $\{y_0 : \exists y_1 \ldots y_n \Phi(\vec{t},\vec{y})\}$. Now since '$\Phi$' is quantifier-free, this thing is actually an $NF\forall^*$ term over the $\vec{t}$ and therefore certainly a T-term and is in $\mathfrak{M}$. We also know that it is nonempty in $\mathfrak{N}$ and therefore nonempty in $\mathfrak{M}$ since $\mathfrak{M}$ is extensional. Therefore, for some m_0 in $\mathfrak{M}$, $\exists y_1 \ldots y_n \Phi(\vec{t}, m_0, y_1 \ldots y_n)$ and the task now is to find witnesses for the $y_1 \ldots y_n$ in $\mathfrak{M}$. This is the same problem as before, but with one fewer y-variable to deal with. So we have a proof by induction on the length of '$\vec{y}$'. ■

13.12 Some Consequences of Conjecture 1

If WF(x) we do not expect there to be a $y = x \cup \{y\}$. This gives an axiom

$$A_\omega : \quad (\forall xy)(WF(x) \to y \neq x \cup \{y\})$$

which is $\forall_4$ or something horrid anyway. Are there $\forall_2$ versions obtained by thinking about loops?

$$A_1 : \quad (\forall xy)(x \notin x \to y \neq x \cup \{y\})$$

This is stronger (antecedent weaker) – perhaps *much* stronger. It's like the version that is true in the term model of NF_2 but not INF, but weaker. That was "every superset of a self-membered set is self-membered". This one *is* true in the term model of *NF*O – think about the least rank of a counterexample.

If so, then perhaps we should consider the other finite versions:

$$A_n : \quad (\forall xy)(x \notin^{\leq n} x \to y \neq x \cup \{y\})$$

which get weaker as n gets larger, and they're all $\forall^*\exists^1$. Natural to ask if they are all true in ther term model of NF0. I bet they are.

It would be nice to prove A_ω by $\in$-induction but of course we can't. We would be able to if we could show that for any $y \in y$, the set $\{x : x \cup \{y\} \neq y\}$ is fat. It isn't of course, but might the assertion that it is be $\forall^*\exists^*$? Sadly no. ("Tooo many quantifiers") So the universal-existential conjecture doesn't imply A_ω by $\in$-induction.

Let's check. The following formula asserts that $\{x : x \cup \{y\} \neq y\}$ is fat:
$\mathcal{P}(\{x : x \cup \{y\} \neq y\}) \subseteq \{x : x \cup \{y\} \neq y\}$
$(\forall z)(z \subseteq \{x : x \cup \{y\} \neq y\}) \to z \in \{x : x \cup \{y\} \neq y\})$
$(\forall z)(z \subseteq \{x : x \cup \{y\} \neq y\}) \to z \cup \{y\} \neq y\})$
$(\forall z)((\forall w \in z)(w \cup \{y\} \neq y\}) \to z \cup \{y\} \neq y\})$

Reflect that $\{x : x \cup \{y\} = y\}$ is always a set! ($x \cup \{y\} = y$ is weakly stratified!)

...so the assertion no well-founded set is capped-off by y is $\forall^*\exists^*\forall^*$

I used to think that one consequence of conjecture 1 is that $\{x : x \in x\}$ is an upper set in $\langle V, \subseteq \rangle$. However, this can be refuted by considering $V \setminus B(V)$ (which is selfmembered) and its superset $(V \setminus B(V)) \cup \{V\}$ (which isn't).

The scheme of assertions: "$x \in x \to (y \texttt{ XOR } x)$ is finite $\to y \in y$" is $\forall^*\exists^*$ but doesn't (despite what i initially tho'rt) come under the conjecture because –

altho' consistent with NF_2, it's not consistent with NF0, and for similar reasons. There is an interesting formula that comes out of this, tho'. "$x \in x \to y$ `XOR` x is finite $\to y \in y$" would follow from "$\{x : x \in x\}$ is an upper set in $\langle V, \subseteq\rangle$" and $(\forall x)(\forall y)(x \in x \ \wedge \ y \in x \to (x \setminus \{y\}) \in (x \setminus \{y\}))$ which is $\forall^*\exists^*$ too. This second is equivalent to the conjunction of

$$(\forall x \forall y)(x \in x \ \wedge \ y \in x \ \to (x \setminus \{y\}) \in x))$$

$$(\forall x \forall y)(x \in x \ \wedge \ y \in x \ \to (x \setminus \{y\}) \neq y)$$

(This is because the conjunction of these two implies that if $x \in x$ and $y \in x$ then $x \setminus \{y\} \in x \setminus \{y\}$. Then of course we can use them any standard number of times to conclude that if $x \in x$ and $y \subseteq x$ with $x \setminus y$ standardly finite, then $y \in y$ too. The we want to know that $\{x : x \in x\}$ is an upper set to infer that if $x \in x$ and x `XOR` y is standardly finite, then $y \in y$.)

As noted, we can forget about the first (try $x := B(V)$ and $y := V$), but the second is interesting. It is an assertion that there are no generalised Quine antiatoms. The dual assertion, that there are no generalised Quine atoms, is

$$(\forall x)(\forall y)((x \cup \{y\}) = y \ \to \ (y \in x \vee x \in x))$$

Actually we can simplify this a bit. The '$y \in x$' in the consequent implies the other disjunct in the consequent, so this is really

$$(\forall x \forall y)((x \cup \{y\}) = y \ \to \ x \in x)$$

Notice that in the case where $x = \Lambda$ this becomes the assertion that there are no Quine atoms.

This admits generalisation, and in two ways.

If $x \cup \{y\} = y$ we say that y **caps** x. Only self-membered sets can be capped, and even then the cap is unique.

1. For some x we can find y such that $y \setminus \{y\} = x$. But for any x there should be at most one such y. This is $\forall^*\exists^*$ and presumably true in all term models but don't quote me on that.

   $$(\forall y_1 \in y_1)(\forall y_2 \in y_2)(y_1 \setminus \{y_1\} = y_2 \setminus \{y_2\} \to y_1 = y_2)$$

   which is

   $$(\forall y_1 \in y_1)(\forall y_2 \in y_2)((\forall z)(z \in y_1 \setminus \{y_1\} \longleftrightarrow z \in y_2 \setminus \{y_2\}) \to y_1 = y_2)$$

   which is $\forall^2\exists^1$.

   We dislike counterexamples to this for the same reason that we dislike Quine atoms: there is no recursive way of telling them apart. (in fact the nonexistence of Quine atoms is a special case)

What about the situation where $x_1 \setminus \{y_1\} = x_2 \setminus \{y_2\}$.

This might be perfectly innocent with all four objects different. But funny things start to happen if enough of them are self membered or members of each other. (Trouble is: the number of cases is huge!)

let's try classifying them like this.

$$(\forall x_1 x_2 y_1 y_2)(x_1 \setminus \{y_1\} = x_2 \setminus \{y_2\} \wedge \Phi(x_1, x_2, y_1, y_2) \rightarrow x_1 = x_2)$$

where Φ is a boolean combination of atomics in the language $\mathcal{L}$('x_1', 'x_2', 'y_1', 'y_2', $=$, $\in$).

These are all universal-existential. If ϕ is stratifed then the whole formula is stratified and not interesting. We assume Φ contains $y_1 \in x_1$ and $y_2 \in x_2$.

I think what i was trying to get at was the following generalisation.

We have a set X (which started off being finite) with the graph of $\in$ restricted to X. We are then given some equation between words in the members of X with operations like singleton, union and difference. (The equation must be $\forall^*$) and invited to infer an equation between two members of X. This conditional is $\forall^*\exists^*$ and should be consistent according to the universal-existential conjecture.

E D I T B E L O W H E R E

But we can claim more than this in a $\forall^*\exists^*$ way.

$$(\forall X)(\forall z_1 z_2)((\forall z_1 z_2 \in X)(((z_1 \setminus x) = (z_2 \setminus x)) \rightarrow z_1 = z_2)$$

Notice that the assertion at the start of this paragraph (that if $x \cup \{y\} = y$ and $x \cup \{z\} = z$ then $y = z$) is the special case where $X = \{y, z\}$. We might need to insert into the displayed formula a condition like $z \in X \rightarrow z \cap x$ not self-membered, beco's of course if $x \in x$ we might be able to "cap" x in more than one way. (Check this!) As it stands it's not true: $X := V$ is a counterexample, and so is an initial segment of WF. But we should be able to recover something. After all: this condition is just: $\in | \restriction X$ is extensional plus a little bit extra. There might be other examples too. One could take X to be inductively defined by $\{V \setminus X\} \in X$ and $y \subseteq X \rightarrow y \cup \{X\} \in X$. If one inserts a condition that $\in \restriction X$ is strongly illfounded then one could require that X be empty. But this is no longer $\forall^*\exists^*$.

A S F A R A S H E R E

2. Is there an infinite family of analogues of this where the conclusion is $x \in^n x$? If you can obtain y from x by inserting y into the transitive

closure of x n levels down then $x \in^n x$? Doesn't seem to be $\forall^*\exists^*$ tho'. The key might be to look at the dual, namely

$$\forall x \forall y\ ((x \setminus \{y\}) = y\ \rightarrow\ x \notin x)$$

Do not make the mistake i made of assuming that $(x \setminus \{y\}) = y$ is the same as $(y \cup \{y\}) = x$ ...'cos $y \in y$ is a possibility! We would need to look at

$$(\forall x \forall y)((y \notin y \wedge (y \cup \{y\}) = x) \rightarrow x \notin x)$$

13.13 Some $\forall^*\exists^*$ sentences true in all term models

There is a lemma (see lemma 80 and lemma 81) that covers 1- 4^k below, though in fact we can at present use it to prove only that 4^k must hold in DEF, permutation models for the others not being forthcoming at present. In fact we can show by other methods that 1-3 hold in DEF and SYMM (that 2 is true in DEF was proved directly by Boffa [1]).

1 All $x \in x$ are infinite (which is a scheme)

2 $\bigcup x \subseteq x \in\ x \rightarrow\ x = V$

3 $x \in^n x \rightarrow\ \bigcup^n x = V$

4^n $(\forall x)(x \neq \iota^n(x))$

Observe that [3] and [4] are stratifiable-mod-n.

Item 3

About [3] one can say the following. Let x be co-small (a small set is one that doesn't map onto V). Then x meets every set that is not small. So it meets every B-word (as it were!).

We can do better than this, for if y is small, the set of its supersets isn't, and so x contains a superset of y. If y isn't small, nor is $\mathcal{P}(y)$ and so x contains a subset of y. Let's abbreviate this to $F(x)$.

So $F(x)$ means "x meets every non-small set"? I don't know what i meant here...

So we have co-small$(x) \rightarrow F(x)$. Can we interpolate $x \in x$ into this conditional? Perhaps with the help of the universal-existential conjection we can get $F(x)$ to imply $x \in x$ and even *vice versa*.

item 1

Friederike has solved 1. She has shown that it is consistent relative to *NF* that the membership relation restricted to sets without a countable partition is wellfounded. After seeing her model i proved a similar result about symmetric sets (proposition **??** below).

First we consider direct proofs that some of the things we want must be true in DEF or SYMM. Propositions 8 to **??** below are best seen as statements about the behaviour of the substructure $SYMM^M$ of an arbitrary model $\mathfrak{M}$ of NF. There is no very satisfactory way of representing these as first-order theorems of NF.

Proposition 8 *For all symmetric sets x, $(x \in x \ \rightarrow\ (\exists y \in x)(y \notin y))$*

Proof:

Suppose not and that

$$x \in x$$

is a symmetric set such that $(\forall y \in x)(y \in y)$. Let x be n-symmetric and k be some power of $2 > n$ where x is n-symmetric. Then $x \in x$ implies – since x is $\leq k$-symmetric – that $(j^k c)(x) \in (j^k c)(x)$.

Now for a useful *factoid* which we are going to use repeatedly: for any u and v and for any permutation f we have $u \in (j`f)`v$ iff $f^{-1}`u \in v$. In fact in the only cases we are going to use it on here f is an involution so we can forget about the -1. Using the factoid we infer

$$(j^{k-1}`c) \circ (j^k`c)`x \in x$$

Now, by hypothesis everything in x is self-membered so we infer

$$(j^{k-1}`c) \circ (j^k`c)`x \in (j^{k-1}`c) \circ (j^k`c)`x.$$

. Now we use the factoid again to rearrange this to:

$$(j^{k-2}`c) \circ (j^{k-1}`c) \circ (j^{k-1}`c) \circ (j^k`c)`x \in x.$$

The k-1`s cancel, since $j^n`c$, like c, is of order 2 for any n, giving

$$(j^{k-2}`c) \circ (j^k`c)`x \in x.$$

Now consider the sequence of the three displayed formulæ. They are all of the form $W_x \in x$. To get from the first to the second (and to get from the second to the third) we first appealed to the fact that everything in x is self-membered, and then to the factoid to infer that some other W_x was a member of x. The reader is invited to think for a minute or so about what happens when we repeat this process, bearing in mind that important simplifications can be made when we exploit the fact that complementation commutes with every permutation that is j of something, and that if π and σ commute so do $j`\pi$ and $j`\sigma$. Thus an easy induction tells us that all $j^n`c$, $j^k`c$ commute with each other. This enables us to tidy up the W_x satisfactorily. Consider the following picture (i have written numbers in hex to make it prettier):

$$\begin{array}{ccccccccccccccccc}
 & & & & & & & & & & & & & & & & 10 \\
 & & & & & & & & & & & & & & & F & 10 \\
 & & & & & & & & & & & & & & E & & 10 \\
 & & & & & & & & & & & & & D & E & F & 10 \\
 & & & & & & & & & & & & C & & & & 10 \\
 & & & & & & & & & & & B & C & & & F & 10 \\
 & & & & & & & & & & A & & C & & E & & 10 \\
 & & & & & & & & & 9 & A & B & C & D & E & F & 10 \\
 & & & & & & & & 8 & & & & & & & & 10 \\
 & & & & & & & 7 & 8 & & & & & & & F & 10 \\
 & & & & & & 6 & & 8 & & & & & & E & & 10 \\
 & & & & & 5 & 6 & 7 & 8 & & & & & D & E & F & 10 \\
 & & & & 4 & & & & 8 & & & & C & & & & 10 \\
 & & & 3 & 4 & & & 7 & 8 & & & B & C & & & F & 10 \\
 & & 2 & & 4 & & 6 & & 8 & & A & & C & & E & & 10 \\
 & 1 & 2 & 3 & 4 & 5 & 6 & 7 & 8 & 9 & A & B & C & D & E & F & 10 \\
0 & & & & & & & & & & & & & & & & 10
\end{array}$$

(Get each row from its predecessor by "subtract 1 pointwise and take symmetric difference"). Think of the elements of each row as the indices on j that appear in the prefix W_x referred to above. I have talked through the construction of the first three rows (with $k = 16$). The picture makes it plain to the eye that if k is a power of 2 we end up with $(j^k`c)(V \setminus x) \in x$. Now $(j^k`c)`(V \setminus x) = (V \setminus x)$ since x is $\leq k$-symmetric so $V \setminus x \in x$ and (since all members of x are self-membered) $V \setminus x \in V \setminus x$ which cannot be simultaneously true.

(This is actually a digitised picture of a Sierpinski sponge, tho' this probably does not matter!)

■

This assertion considered in the next proposition is actually a consequence of the $\forall^*\exists^*$ expression $(\forall x)(x \in x \to (\forall y)(\exists z \in x)(y \notin z))$. Can we prove that this is true in the symmetric sets?

Proposition 9 : *$(\forall x \in SYMM)(x \in x. \to .(\exists y)(y \in x \land x \notin y))$*

Proof:

Suppose x is m-symmetric and belongs to all its members. Then, for any permutation τ, and any $n \geq m$, $x \in x$ iff

$(j^n`\tau)`x \in (j^{n+1}`\tau)`x$

iff

$(j^n`\tau)`x \in x$ $(= (j^{n+1}`\tau)`x$ because x is $\leq n$-symmetric) iff

(i) $x \in (j^n`\tau)`x$ iff $(j^{n-1}`\tau)^{-1}`x \in x$ whence

(ii) $x \in (j^{n-1}`\tau)^{-1}`x$ since x belongs to all its members.

We now repeat the line of reasoning that led us from (i) to (ii), decreasing exponents on j at each step until

$x \in \tau^{-1}x$ (n odd) or $x \in \tau`x$ (n even).

But τ was arbitrary, and it is easy enough, given x, to devise a permutation τ so that $x \in \tau`x \wedge x \in \tau^{-1}`x$. ■

I proved proposition **??** after Friederike Körner produced a construction of a model of *NF* in which the membership relation restricted to finite sets is wellfounded. It is sensible to ask if this can be proved for larger sets too. Let us say I is a *notion of smallness* if

1. Any subset of an I thing is also I
2. Any union of I-many I-sets is I
3. V is not I

Finiteness is a notion of smallness, so is dedekind-finiteness. There's not a great deal more! In particular **smallness** (as in "can't be mapped onto the universe") isn't a notion of smallness. (We should perhaps consider here Boffa's question about the sequence: W_1 = set of wellorderable sets, W_{i+1} = sumsets of wellordered subsets of W_i. This isn't *directly* applicable here because the natural application would be: W_{i+1} = sumsets of W_i subsets of the set of all wellordered sets. However, if W_∞ is not V we can prove proposition **??** for W_∞ too.)

Finally we should note that the first list approximant to the branching quantifier formula saying that there is an (external) antimorphism is true in the definable or symmetric sets.

13.14 Strengthening the conjecture

We can't extend this to formulæ with bounded quantifiers because the assertion "There is a dense linear order" is Σ_1.

Some $\forall^*\exists^*$ sentences are theorems of *NF* beco's they are consequences of extensionality. In these cases we cannot expect to be able to prove the formula in a nice way by witnessing the existential quantifiers with terms. We don't have this problem with $\exists^*\forall^*$ expressions, so perhaps we should strengthen the conjecture to:

For every stratified $\exists^*\forall^*$ sentence either it is provable in SF or if it isn't its negation is a theorem of *NF*.

So look at the ways in which wee could fail to prove a given $\exists^*\forall^*$ sentence ...

One might have hoped that one could have developed NF0 as a `PROLOG` theory with the expectation that whenever NF0 proves a universal-existential sentence the witnesses to the x variables can be found as words in the y variables. The following $\forall^3\exists^1$ example shows that this is doomed.

$$(\forall y_1 y_2 y_3)((y_1 \in y_2 \wedge y_3 \notin y_2) \to (\exists x)(x \in y_1 \longleftrightarrow x \notin y_3))$$

Idea:

(i) Show that if NF0 proves something existential-universal it exhibits a witness.

(ii) Use a `PROLOG`-style treatment. An attempt to prove an existential-universal assertion corresponds to an attempt to make the youniversal vbls into constants and to instantiate the xistential vbls with closed terms. ivo (i) this is sufficient.

(iii) Transform a failure to NF0-prove your existential-universal assertion into an NF0-proof of its negation.

Let's apply this to the nasty example above. We fail to find a closed term to do for 'x' in

$$(\exists x)(\forall y_1 y_2 y_3)((y_1 \in y_2 \wedge y_3 \notin y_2) \to (x \in y_1 \longleftrightarrow x \notin y_3))$$

so (if this works) we expect to be able to prove its negation, namely

$$(\forall x)(\exists y_1 y_2 y_3)((y_1 \in y_2 \wedge y_3 \notin y_2) \to (x \in y_1 \longleftrightarrow x \in y_3))$$

which seems innocent enough.

$\forall^* \exists^*$ witnesses?

If we should be able to prove consistent by permutations all consistent $\forall_2$-sentences, we can ask whether there are definable skolem functions that do the business for us. For example, is there a skolem function witnessing

$$\Psi: \ (\forall y \in y)(\exists x \in \ y)(x \neq y)?$$

A good guess is that x could consistently be taken to be $y \setminus \{y\}$. Ψ is $\forall\exists$, and a natural extension of this conjecture would be that for any $\forall\exists$ sentence there is some NF_2 word (or finite disjunction of NF_2 words) we can consistently assume to uniformly provide witnesses for it. This certainly *looks* plausible for Ψ. And, pleasingly, the assertion that $y \setminus \{y\}$ works for Ψ is itself $\forall_2$, namely

$$(\forall y)(y \in y \ \to \ (y \setminus \{y\}) \in y)$$

which is

$$\Psi^*: \ \forall x \forall y \ x \in x \wedge y \notin x \ \to \ \exists z \ \ (z \in (y \ \texttt{XOR} \ (x \setminus \iota\text{`}x)))$$

But presumably in general this cannot work, because otherwise we would be committed to producing a disjunction of terms which would be candidate witnesses to the x `XOR` y if $x \neq y$, because of

$$(\forall xy)((x \in x \wedge y \notin y) \to (\exists z)(z \in x \longleftrightarrow z \notin y))$$

and this would presumably imply AC.

Let's think about this a bit. Extensionality implies that if $x \notin x$ and $y \in y$ then x `XOR` y is inhabited. But by what? Not provably by x or y beco's of $B\text{`}V$ and $\{B\text{`}V\}$. I can't see any NF0 word in x and y that can be relied upon to inhabit x `XOR` y in these circs, nor any finite set of words one of which must. It would be nice to have a proof of this fact.

13.14.1 Extending the conjecture to sentences with more blocks

If the $\forall^*\exists^*$ conjecture is true, we will have an extension of Hinnions old result on $\exists^*$ sentences. What is the appropriate extension of these conjectures to formulæ with three blocks of quantifiers? Presumably it would be to $\exists^*\forall^*\exists^*$ formulæ, keeping going the pattern of having the *innermost* block a block of existential quantifiers.

Notice that there is a $\exists^*\forall^*\exists^*$ sentence saying that there is a total ordernesting of V.

And what is the conjecture to be? Let us define Γ_n to be the set of formulæ with n blocks of quantifiers, with the innnermost existential. The strongest form of the conjecture would be to set $NF^1 := NF$; $NF^{n+1} := NF^n \cup$ all the Γ_{n+1} sentences consistent with NF^n. Finally we would hope that the complete theory which is a union of all these is consistent and has a term model.

Unfortunately there are some pretty obvious *prima facie* counterexamples. Wellfoundedness is a source of lots of hard cases for the three-quantifier case, since "x is wellfounded" is $\forall^*\exists^*\forall^*$,

1. The axiom of $\in$-determinacy is $\forall^*\exists^*\forall^*$ but is true in all term models.

2. There is a $\exists^*\forall^*\exists^*$ sentence POL that asserts that there is an antimorphism of the universe which is an involution (a polarity). "X is a partition of V" is

$$(\forall u)(\exists v \in X)((u \in v) \wedge (\forall v' \in X)(u \in v' \to v' = v)) \tag{B}$$

   $\forall^*\exists^*$. What we do is assert that and add the clause

$$(\forall yz)(\forall uv)[((\exists a \in X)(y \in a \wedge z \in a) \wedge (\exists b \in X)(u \in b \wedge v \in b)) \to$$
$$(u \in y \longleftrightarrow v \notin z)] \tag{A}$$

   It is a simple exercise using extensionality to check that (A) implies that every member of X is a pair. (If we had to assert specifically that every member of X is a pair it would cost an extra alternation of quantifiers.) POL is the conjunction of (A) and (B)

   This shows that "x is a polarity" is $\forall^*\exists^*$ No term model can contain an antimorphism. So we must hope that POL is refuted by the $\forall^*\exists^*$ scheme. But that can happen only if the existence of a polarity is $\exists^*\forall^*$.

3. "Every transitive set that is not self-membered is wellfounded" is $\forall^*\exists^*\forall^*$ but is true in all term models.

4. $x = \bigcap x$ is $\exists^*\forall^*\exists^*$ but not true in any term model.

Ad item (1). I'd like to see this spelled out.

13.14.2 Perhaps the key is to doctor the logic

There are other logics that have a notion of quantifier hierarchy that we might be able to use. The cofinite logic for example. With a two-block formula we can say something like "Every transitive set that isn't self-membered is finite": and this ought to be true in the nice models

$$(\forall_\infty y_1)((\forall_\infty x_1 \in y_1)(x_1 \subseteq_\infty y_1) \to (\forall_\infty y_2)(y_2 \in y_1))$$

(Is there a prenex normal form theorem for the logic with the cofinite quantifier?)

No, that's not what we wanted. Something more like

$$(\forall_\infty y_1)([(\forall_\infty x_1 \in y_1)(x_1 \subseteq_\infty y_1) \wedge y_1 \not\in y_1] \to (\forall_\infty y_2)(y_2 \not\in y_1))$$

(3)

$$\Phi_\infty : (\exists x)(\bigcup x \subseteq x \neq V \wedge \neg WF(x))$$

This is $\exists^*\forall^*\exists^*$, and apparently consistent with *NF*^{2}, but it is obviously pathological, e.g., it is demonstrably false in DEF and SYMM, because of Boffa's theorem that there are no definable transitive sets other than V and a smattering of hereditarily finite sets.

However it does not appear to be consistent with *NF*$^2 \cup\ \Gamma$, where Γ is the formula:

$(\forall x)$(No circles $x \in^n x \to$no ω-descending $\in$-chains starting at x)

To see this consider the formulæ

$$\Phi_n\text{: } (\forall x)(\bigcup x \subseteq x \ \wedge \ x \in^n x. \to \ x = V)$$

as n varies over the positive integers. Each Φ_n is certainly $\forall^*\exists^*$ and appears to be consistent with *NF* (no Proof to hand, but they are demonstrably true in DEF or SYMM, because of Boffa's theorem just alluded to). But in any model in which the Φ_n and Γ all hold, Φ_∞ must be false.

Now although Γ is certainly infinitary it is in some sense $\forall_2$ in $L_{\omega_1\omega_1}$. This suggests that we should consider cutting down the number of $\exists^*\forall^*\exists^*$ sentences we have to add to *NF*^{2} to get *NF*^{3} by defining *NF*^{2} to be not: *NF* $\cup$ all $\forall^*\exists^*$ sentences consistent with *NF*, but: *NF* $\cup$ all $\forall^\infty\exists^\infty$ sentences consistent with *NF*.

(4) $x = \bigcap x$ is a candidate pathology because

> (i) it is true of no symmetric set. (We can establish this easily enuff by asking about the least n such that there is an n-symmetric x such that $x = \bigcap x$.)
>
> (ii) it looks possible that the existence of such an x should be consistent with all the $\forall^*\exists^*$ formulæ true in all term models. $x \subseteq \bigcap x$ is $\forall^*$ (it's $(\forall z)(z \in x \to (\forall w)(w \in x \to z \in w))$). So $(\forall x)(x \subseteq \bigcap x \to x = \emptyset)$ is $\forall^*\exists^*$ and would solve our problem if it is consistent.

However, life isn't that easy. $\{y\} \subseteq \bigcap\{y\}$ is just $y \in y$. Cofinite sets tend to be members of each other. Consider $x = \{V \setminus \{\{y\}\} : y \in V\}$. Clearly $x \subseteq \bigcap x$.

$x = \bigcap x$ is equivalent to the assertion that $\in\restriction x$ is just $x \times x$ (So it follows immediately that the proper class of x such that $x \subseteq \bigcap x$ is downward closed and closed under directed unions.) In particular, $(\forall y \in x)(y \in y)$. The contrapositive of Proposition 8 tells us that any such (symmetric) x will not be a member of itself. Also by propositions 8 and 9 this is true in symmetric models. Unfortunately this does not seem to tell us any more than that $x \subseteq \bigcap x \to x \notin x$, and this does not seem to be impossible.

I keep having the feeling that if $x = \bigcap x$ then stcan(x).

13.15 Remains of some failed proofs of conjecture 3

Richard reminds me that if $\mathfrak{M}$ satisfies every $\forall^*$-consequence of T then $\mathfrak{M}$ has an extension which is a model of T. (Do we have a $\mathcal{P}$-version?). So, he says, can we show that if $\mathfrak{M} \subseteq \mathfrak{N}$, both models of NF, then $\mathfrak{M} \prec_{str(\exists^*)} \mathfrak{N}$? To do this by the method below we would want something along the following lines:

$\mathfrak{N}$ a model of *NF* is an NF-term model over some list of generators $\vec{n}$. The question is, can we represent an arbitrary model $\mathfrak{M} \subseteq \mathfrak{N}$ as a term model over some subset of $\vec{n}$ in such a way that terms have the same meaning in both models? Why might we expect this?

13.15.1 A failed proof

Suppose $(\forall\vec{x})(\exists\vec{y})(\Phi(\vec{x},\vec{y}))$, with Φ stratified, is true in the term model of NF0. We are now going to show that it is true in an arbitrary sufficiently large finite model of TST.

The proof is laborious and we spare the reader and ourselves some details. Consider first of all the x variables of lowest type. Suppose there are n of them. We want to show that any n objects from that level of any model satisfy a certain property. Consider such an n-tuple of objects in T_k. Any set of objects in T_{k-2} will generate a boolean subalgebra of T_k. Consider the minimal set $\vec{a} \subseteq T_{k-2}$ such that all elements of the n-tuple belong to the free subalgebra of T_k generated by the $B`a_i$, so that each object in the n-tuple is a unique $\cup$, $\cap$, B, $-$, word in the $\vec{a}$. As before, we expand '$y_i \in t_j$' until they have all been eliminated, and recast the matrix into DNF. As before we know that not all the disjuncts can trivially violate the theory of identity since all results of substituting NF0 words for the a_i in '$(\exists\vec{y})(\Phi(\vec{x},\vec{y}))$' are satisfiable. Fasten on one good disjunct. Look at the $\vec{y}$ of minimal type. The conditions like '$y \in t_j$' have been replaced by boolean combinations of conditions saying that such-and-such a_i are $\in$ y or not, as the case may be. Now how many conditions of this sort are there on any of these minimal y? Clearly at most as many as there are things in $\vec{a}$, so the desired witness is a member of the boolean interval $[\{\vec{a}_i : i \in I\}, -\{\vec{a}_j : j \in J\}]$ for some sets I, J of a's. I and J must be disjoint, since we know that the disjunct we are contemplating does not violate the elementary theory of =. So if there were no inequations around we would have shown that $(\exists\vec{y})(\Phi(\vec{x},\vec{y}))$. However we now have to accomodate some family of inequations $y \neq x_i$. These may exclude some more elements of $[\vec{a}_i, -\vec{a}_j]$ and we no longer know that $[\{\vec{a}_i : i \in I\}, -\{\vec{a}_j : j \in J\}]$ is infinite. What this tells us is that if we have inequations to deal with we wish $[\{\vec{a}_i : i \in I\}, -\{\vec{a}_j : j \in J\}]$ to be big enough for us to satisfy them all *and that it is only the number of inequations that we have to worry about.* If $\vec{a}$ is a proper subset of T_{k-2} then $[\{\vec{a}_i : i \in I\}, -\{\vec{a}_j : j \in J\}]$ will have many members, and $\vec{a}$ will be a proper subset of T_{k-2} if $2^{2^n} < |T_k|$.

So as long as T_k is sufficiently large in relation to the number of inequations (which is bounded by (length of $\vec{x}$ + length of $\vec{y}$)^2) we will be able to find witnesses. In short we can see:

> For all m and n there is k such that for all Φ, if $(\forall x_1 \ldots x_m)(\exists y_1 \ldots y_n)\Phi(\vec{x},\vec{y})$ is true in the term model of NF0, then $(\forall\vec{x})(\exists\vec{y})\Phi(\vec{x},\vec{y})$ is true in all models of TST where T_0 has at least k elements.

13.15.2 Another failed proof

We are trying to show that any $\exists_2$ stratified sentence true in $\mathfrak{M} \models NF0$ is witnessed by a term. Idea:

look at $\exists\vec{x} \bigwedge \forall\vec{y} \bigvee$ atomics or negatomics.

We can rewrite to get rid of equations and inequations but it won't help. We think of each of the conjuncts as a constraint on what term the witness has to be. We have the impression that such conjuncts reduce to things like "x is the complement of the singleton of something other than Λ". The point is that these say that x must be a value of some NF0 operation. If so, this is good news, because a conjunction of finitely many such conditions can be satisfied in the term model if at all.

each conjunct give rise to something like

$\vec{x} = \vec{t}(\vec{u})$ subject to finitely many exceptions $\vec{u} \neq \vec{s}$ for some NF0 terms $\vec{s}$

which we shall call a *constraint.*

For example $\forall y_0 y_1 y_2 (y_0 \in y_1 \vee y_0 \in y_2 \vee y_2 \in x \vee y_1 \in x)$

gives rise to

$x = V \setminus \{u\}$ with exception $u \neq \Lambda$

If this works we then hope that any finite set of constraints has either a solution containing a parameter (like the singleton list above) in which case it will certainly have infinitely many solutions, or it will have none at all, in which case it wasn't true in M in the first place.

later

Actually it seems that we have to use $NF\forall^1$ words for this

13.15.3 A third failed proof

Proposition 10 *: NF decides all stratified $\forall^*\exists$ sentences.*

In fact we will prove something significantly stronger. Let us descend to simple type theory for a while, and accordingly impose type subscripts on our variables. We will show that every wff

$(\forall\vec{x}_0)(\exists y_2)(\forall z_1)\Phi(\vec{x}_0, y_2, z_1)$ with $\Phi(\vec{x}_0, y_2, z_1)$ quantifier-free is true in all sufficiently large finite models of simple type theory.

I shall not provide a proof in full, for it makes use of tricks that we cannot use to show that all stratified $\forall_2$ sentences are decided by simple type theory.

First we note that it makes no difference whether the initial quantifier $(\forall\vec{x}_0)$ in

$$(\forall\vec{x}_0)(\exists y_2)(\forall z_1)\Phi(\vec{x}_0, y_2, z_1)$$

is $\exists$ or $\forall$, since all n-tuples will satisfy the matrix if any do. Next we notice that each object x_0 of type 0 gives rise to an object $(B`x_0)$ of type 2 and the subalgebra of the boolean algebra $\langle T_2, \subseteq\rangle$ generated by these elements is free. Having it in mind to make use of this we invent a one-place predicate g on objects of type 2, whose intended reading is "is a member of a set of free generators for $\langle T_2, \subseteq\rangle$". We now rewrite

$$(\forall\vec{x}_0)(\exists y_2)(\forall z_1)\Phi(\vec{x}_0, y_2, z_1)$$

as

$$(\forall\vec{x}_2)(g(\vec{x}_2) \rightarrow (\exists y_2)(\forall z_1)\Phi(\vec{x}_0, y_2, z_1))$$

by replacing "$x_0 \in\ x_1$" by "$x_1 \in\ x_2 \wedge g(x_2)$" where the x_2 are secretly the various $B`\vec{x}_0$. Next we show that the innermost quantifier – $(\forall z_1)$ – can be assimilated into the matrix to result in an expression

$$\text{A}\ (\forall x_2)(g(x) \rightarrow (\exists y_2)\Psi(x_2, y_2))$$

in the language of boolean algebras with the added primitive g, where Ψ is quantifier-free. Finally some elementary manipulations in boolean algebra will show that if we furnish g with this interpretation then any sentence like A above with any models at all is true in all sufficiently large finite free boolean algebras. I am grateful to Peter Johnstone says the witnesses to the y_2 can be found among words in the x_2. *Peter says:*

can assume only one y. Restrict ourselves to combinations of

$p(\vec{x}, y) \leq q(\vec{x}y)$ without loss of generality

$p = \bigwedge \vec{x}\&\neg\vec{x}, y$ $q = \bigcup \vec{x}\&$[illegible] y occurs on only one side. so reduces to $y \leq q(\vec{x})$ or $p(\vec{x}) \leq y$

$\bigvee p_j \leq \bigwedge q_i$

set $y = V$ or Λ. Can't piece it together ...

A hard case: consider the assertion that the meet of all the $\vec{y}$ is not an atom. This is certainly satisfiable in suff big algebras, but is not true in the algebra generated by the $\vec{y}$.

13.16 stuff to fit in

Where do self-membered sets come from in NF? V is a member of itself, and we can get further self-membered sets by means of NF0 operations. The NF0 operations can give us sets that are members^2 of themselves but these sets usually turn out to be self-membered anyway. $V \in \{V\} \in V$ but then $V \in V$. So is it the case that – in the term model for NF – $x \in y \in x \to x \in x$? No, beco's of $\{V\}$. However we could try this:

$$(\forall y_1 y_2)(y_1 \in y_2 \in y_1 \to y_1 \in y_1 \vee y_2 \in y_2)$$

Obviously not, co's it's $\forall^*$. But how about the assertion that given an n-loop, one of the things in it belongs to an $n-1$-loop? I don't think that works. Consider $\iota``V$. We have $\iota``V \in \{\iota``V\} \in \iota``V$ but any self-membered member of $\iota``V$ would have to be a Quine atom.

Might that be consistent? Can we find a counterexample?

13.16.1 A Message from Franco Parlamento

Paul Studtmann writes:

Robinson's Arithmetic is complete with respect to quantifier free sentences. I am wondering whether anyone can tell me if an analog of this holds in set theory. Suppose, for instance, that the language contains two constants – one for the empty set and one for the set of finite ordinals – as well as function symbols for the basic set theoretic operations like set union, set difference, power set, pairing, etc. Is ZF (or a fragment thereof) or some other theory complete with respect to all the quantifier free sentences in the language?

If you omit the power set operator from the list and by "union" binary union is meant, then ZF, but also ZF \ Power Set Axiom and even weaker theories, are complete with respect to quantifier free sentences (equiv. atomic sentences). That can be inferred from the decidability of truth in V for existential closures of restricted purely universal formulae with no nesting of quantified variables, over the primitive language of set theory with the addition of constants for the empty set and the set of finite ordinals (as well as a unary predicate Ord(x) for "x is an ordinal") (Breban M., Ferro A., Omodeo E., Schwartz J.T. "Decision Procedures for Elementary Sublanguages of Set Theory II. Formulas involving restricted quantifiers together with ordinal, integer, map and domain notions" Comm. on Pure and Applied Mathematics XLI 221-251 (1988) - see also Ch.7 in Cantone D., Ferro A., Omodeo E "Computable Set Theory Vol 1" Oxford University Press, 1989 and my [FOM] of june 3th, 2003)

In fact the operations of (binary) union, intersection and set-difference as well as the operation of n-tuple formation have a restricted purely universal definition with no nesting of quantified variables, so that an atomic sentence which also involves them, turns out to be equivalent to a sentence which belong to the decidable class described above. Completeness follows since the proof of the decidability of the class in question, which exhibits an actual algorithm that shows that it does what it is supposed to do, can be formalized inside ZF\ Power Set Axiom (and even weaker theories).

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13.16.2 The Universal-Existential conjecture in $\mathbf{TC}_n\mathbf{T}$

Consider the formula $(\forall x)(x \neq \iota^n(x))$, which is stratifiable-mod-n. Evidently in any model of $\mathrm{TC}_n\mathrm{T}$ it is either true at all levels or false at all levels. The fact that it is also universal-existential brings the universal-existential conjecture into sharp focus. I now think that that conjecture should perhaps be that:

(i) Amb(str($\forall^*\exists^*$)) is a theorem of T$\mathbb{Z}$T and,
(ii) for each n, $\mathrm{TC}_n\mathrm{T}$ proves Amb($\forall^*\exists^*$) for sentences that are stratifiable-mod-n.

rather than that

T$\mathbb{Z}$T decides all $\forall^*\exists^*$ sentences.

Actually (ii) is not quite true. It would seem that in the two-lobe ("yin-yang") model one can have a Boffa atom in one lobe but not in the other.

And another thing... In NF we can show that there is no Boffa antiatom. A Boffa antiatom is an object b such that $(\forall x)(x \in b \longleftrightarrow b \notin x)$. The assertion that there is no Boffa antiatom is $\forall^*\exists$ and is stratifiable mod 2. We obtain a contradiction when $b = x$. This (purely logical) argument works in NF but not in $\mathrm{TC}_2\mathrm{T}$ ... but might it be a theorem of $\mathrm{TC}_2\mathrm{T}$ anyway?

We can show that you can't have a Boffa atom in one lobe and a Boffa antiatom in the other, as follows

Suppose `yin` contains a Boffa atom b and `yang` contains a Boffa antiatom b', so $(\forall x)(x \in b \longleftrightarrow b \in x)$ and $(\forall y)(y \in b' \longleftrightarrow b \notin y)$. because one obtains both $b \in b' \longleftrightarrow b' \in b$ and $b \in b' \longleftrightarrow b' \notin b$).

There doesn't seem to be any objection to having a Boffa antiatom in each lobe. However suppose there is a *unique* Boffa antiatom in each lobe. Then there is a failure of typical ambiguity. Our two Boffa antiatoms are b and b', and we can tell them apart, since $b \in b' \longleftrightarrow b' \notin b$. So one lobe contains a unique Boffa antiatom that is a member of a Boffa antiatom whereas the other lobe doesn't. But – given that NF says that there are no Boffa antiatoms – this is not to be wondered at.

Another example is $(\forall x)(x \in^n x \to \bigcup^n x = V)$. Again, this is $\forall^*\exists^*$ and is stratifiable-mod-n and it seems that, in any model of $\mathrm{TC}_n\mathrm{T}$, if it is true at one level then it is true at all. (Well, *something* like that seems to be true: if $(\forall x)(x \in^n x \to \bigcup^n x = V)$ holds at level i then $(\forall x)(x \in^n x \to \bigcup^{n+1} x = V)$ holds at level $i+1$. That's the best I can do at the moment.)

13.16.3 Nathan has made me see some things..

If $\mathfrak{M}$ is a countable model of T$\mathbb{Z}$T0 think of it as a direct limit of its finitely generated substructures, but consider only those finitely generated substructures that are generated by things that cannot be T$\mathbb{Z}$T0 words. Hereafter a *generator* is something that is not a singleton, B of anything, not the empty set not the universe, not a boolean combination etc.

I think the idea is to show that every Π_2 sentence generalises downwards to any of these guys.

I'm still trying to prove that every $\forall^*\exists^*$ sentence consistent with T$\mathbb{Z}$T0 is true in the term model. Here is something that might work. let $\mathfrak{M}$ be a countable model of T$\mathbb{Z}$T0. Then it is a direct limit of a suitable ω-chain $\langle S_i : i \in \mathbb{N}\rangle$ of finite substructures, with embeddings $\langle f_i : i \in \mathbb{N}\rangle$ from S_i into S_{i+1}. But each S_i is of course embeddable into $\mathfrak{T}$ the term model of T$\mathbb{Z}$T0. So we flesh out each S_i to a copy of $\mathfrak{T}$ and expand somehow each f_i to an injection also called f_i from $\mathfrak{T}$ into $\mathfrak{T}$. Can we do this? Yes, every countable binary structure (so, in particular, $\mathfrak{T}$) can be embedded into $\mathfrak{T}$ – and, indeed, into any cofinite subset of $\mathfrak{T}$. The hard part is to ensure that the new direct limit is the same as the old. To bring this about we have to do is ensure that every f-thread eventually lands inside an S_i. To do this we will have to exploit the fact that $\mathfrak{M}$ is a model of T$\mathbb{Z}$T0, not just any arbitrary countable structure – because the result we are trying to prove isn't true for an arbitrary countable structure! Also it has to be an argument that exploits the fact that $\mathfrak{M}$ is a term model of T$\mathbb{Z}$T0 rather than NF0, beco's of unstratification and the possibility of Quine atoms.

Express T$\mathbb{Z}$T0 in the language with function letters for the operations. Then it is universal-existential, which may or may not help. Let $\mathfrak{M}$ be a countable model of T$\mathbb{Z}$T0 (countability might not help, but it's not going to do any harm). Express $\mathfrak{M}$ as a direct limit of an ω-sequence $\langle \mathfrak{M}_i : i \in \mathbb{N}\rangle$ of some of its finitely generated substructures. Each $\mathfrak{M}_i$ can be embedded somehow into a copy $\mathfrak{T}_i$ of $\mathfrak{T}$ the term model of T$\mathbb{Z}$T0. We want to do this in such a way that the direct limit of the $\langle \mathcal{T}_i : i \in \mathbb{N}\rangle$ is actually $\mathfrak{M}$.

I think (check it!) that if a model $\mathfrak{M}$ of T$\mathbb{Z}$T0 is thought of as an $\mathcal{L}^{B,\iota}$ structure then it embeds into $\mathfrak{T}$ thought of as a $\mathcal{L}^{B,\iota}$ structure.

All these things i want to connect...

universal-existential sentences in T$\mathbb{Z}$T.

Also $\forall^*_\infty\exists^*_\infty$ sentences.

The way in which every countable structure embeds in the term model of NF0 in continuum many ways;

countable categorical theories;
something to do with random structures[
See quantifiertalk.tex.
Are there any models for T$\mathbb{Z}$T that are random?
Is the term model for NF0 a random structure for the theory of extensionality?
(Doesn't the existence of a universal set bugger things up?)
What about the model companion of NF?
Aren't model companions something to do with random structures?

Are co-term models a distraction?

model companions
random structures
universal-existential
zero-one
nice embeddings

Chapter 14

Some thoughts on NF0 – and a new theory: NF0^{--}

SYNOPSIS

We also need the notion of a *nice embedding*.

Definition 22 *A* **nice family** $\mathcal{F} : \mathfrak{M}_1 \hookrightarrow \mathfrak{M}_2$ *is a set of injective homomorphisms containing an injective homomorphism from the substructure of* $\mathfrak{M}_1$ *generated by the empty set to the substructure of* $\mathfrak{M}_2$ *generated by the empty set, and, for each* $f \in \mathcal{F}$ *(and any such* f *is an injection from a finitely generated substructure* $\mathfrak{M}_1'$ *of* $\mathfrak{M}_1$ *into* $\mathfrak{M}_2$ *such that...) and, for every finitely generated extension* $\mathfrak{M}_1''$ *of* $\mathfrak{M}_1'$*,* f *can be extended in infinitely many ways to an injection* $f' \in \mathcal{F}$ *sending* $\mathfrak{M}_1''$ *into* $\mathfrak{M}_2$*.*

We will need a special symbol to say that there is a nice family of maps from $\mathfrak{M}_1$ *to* $\mathfrak{M}_2$*. "*$\mathfrak{M}_1 \prec_{\text{nice}} \mathfrak{M}_2$*", say... or "*$\mathfrak{M}_1$ *embeds nicely into* $\mathfrak{M}_2$*".*

> This seems to be the same as *partial isomorphisms* as in the Barwise article[1] reviewed by Cutland, supplied to me by James and included in `forcing.tex`.

We now have the correct setting for the embedding result of Forster [**?**] which we will restate and reprove for the new context.

Theorem 32 *For every countable binary structure* $\mathfrak{M} = \langle M, R\rangle$ *and every model* $\mathfrak{N}$ *of NF0*$^{--}$ *there is a nice family of embeddings from* $\mathfrak{M}$ *to* $\mathfrak{N}$*.*

Proof:

We will prove this by refining the construction of [**?**] to obtain a construction of a nice family of embeddings.

The 1987 construction takes a countable binary structure $\mathfrak{M} = \langle M, R\rangle$ equipped with a wellordering of length ω and gives to each initial segment (or

[1] Barwise, Jon. Back and forth through infinitary logic. Studies in model theory, pp. 5–34. MAA Studies in Maths., **8**, Math. Assoc. Amer., Buffalo, N.Y., 1973. 02B25 (02H10)

more strictly, its domain) an injection into the term model for NF0. We will do something slightly more complicated. For a start our embeddings will be not into the term model for NF0 but rather into an arbitrary model $\mathfrak{N}$ of NF0^{--}. We will not be providing injections-into-$\mathfrak{N}$ from (domains of) initial segments of a fixed wellordering of M: our injections-into-$\mathfrak{N}$ will be defined on the domains of finite partial functions from M to $\mathbb{N}$. We will think of these finite partial functions as lists of ordered pairs so that we can construct the nice family of injections by primitive recursion on lists. Doubled colons is our notation for consing things onto the front of lists, so that – to take a pertinant example – $\langle x, k\rangle :: s$ is the finite map that agrees with s on its domain and additionally sends x to k. We will construct for each s an injective homomorphism i_s from $dom(s)$ to $\mathfrak{N}$, and this family of maps will be nice.

We will need an infinite supply of distinct selfmembered sets and an infinite supply of distinct non-selfmembered sets: such a supply can easily be found with the help of the B function. By lemma 19 NF0^{--} proves the existence of self-membered sets and of non-self-membered sets, so fix an arbitrary $a \in a$ and an arbitrary $b \notin b$. Let the nth *left* object be $B^n(a)$ and the nth *right* object be $B^n(b)$. All left objects are self-membered and no right objects are. The exponent gives us a convenient notion of *rank* of these left and right objects. It will be important in what follows that every value of any i_s has finite symmetric difference with a left object or a right object, since this enables us to construct each such value by finitely many applications of adjunction and subcision. It will also be important that any two left or right objects have infinite symmetric difference: $B(x)$ `XOR` $B(y)$ is infinite unless $x = y$.

For s a finite partial map $M \to \mathbb{N}$ we will construct i_s from $dom(s)$ to the term model by primitive recursion on lists.

We start with the empty map from the empty substructure (the domain of the empty partial map).

The variable 's' will range over finite partial maps $M \to \mathbb{N}$ and, for each s, i_s will be an injective homomorphism from $dom(s)$ to $\mathfrak{N}$

For the recursion (primitive recursion on lists) let us suppose we have constructed a map i_s and we want to construct $i_{\langle x,k\rangle::s}$. And we must have $i_{s'} \neq i_{s''}$ whenever $s \neq s''$.

The construction of $i_{\langle x,k\rangle::s}$ from i_s is uniform in x and k. Of course $i_{\langle x,k\rangle::s}$ will agree with i_s on $dom(s)$. During the earlier construction of i_s we will have used some left objects and some right objects. Let n_s be the least n such that the only left or right objects touched so far in the construction of i_s have indices below n. Now, given $k \in \mathbb{N}$, we want X to be a left object or a right object, depending on whether $\mathfrak{M} \models R(x, x)$ or not, and we set it to be the $(n_s + k)$th such object, or the $(n_s + k + 1)$th, if $(n_s + k)$ is odd. X is thus a left or right object, with a subscript that is even and is larger than any subscript we have seen so far.

$i_{\langle x,k\rangle::s}(x)$ will be obtained from X by adding and removing only finitely many things. We have to add things in A and delete things that are in B:

A: $\{i_s(m) : m \in dom(s) \ \wedge \mathfrak{M} \models R(m, x)\}$

B: $\{i_s(m) : m \in dom(s) \wedge \mathfrak{M} \models \neg R(m, x)\}$

C and D are harder to deal with:

C: $\{i_s(m) : m \in dom(s) \ \wedge \ \mathfrak{M} \models R(x, m)\}$

E: $\{i_s(m) : m \in dom(s) \ \wedge \ \mathfrak{M} \models \neg R(x, m)\}$

Our final choice for $i_{\langle x,k\rangle::s}(x)$ must extend A, be disjoint from B, belong to everything in C, and to nothing in E. There is no guarantee that X will do, but it's a point of departure; our first approximation to $i_{\langle x,k\rangle::s}(x)$ is $(X \setminus B) \cup A$.

For each m in $dom(s)$ let X_m be that left or right object from which $i_s(m)$ was obtained by the finite tweaking that we are about to explain. We want to control the truth-value of $i_{\langle x,k\rangle::s}(x) \in i_s(m)$. It's hard to see how to do this directly, but one thing we *can* control is the truth-value of $i_{\langle x,k\rangle::s}(x) \in X_m$, because this is the same as the truth-value of $B^{-1}X_m \in i_{\langle x,k\rangle::s}(m)$ and we can easily add or delete the various $B^{-1}(X_m)$ from $(X \setminus B) \cup A$.

Suppose for some particular m we want to arrange that $i_{\langle x,k\rangle::s}(x) \in i_s(m)$. We put $B^{-1}(X_m)$ into $i_{\langle x,k\rangle::s}(x)$. This ensures that $i_{\langle x,k\rangle::s}(x) \in X_m$. This is very nearly what we want, since the symmetric difference X_m `XOR` $i_s(m)$ is finite. Now, because we chose n_s to be larger than the subscript on any left or right object we had used so far in building i_s we can be sure that $i_{\langle x,k\rangle::s}(x)$ is not one of the finitely many things in X_m `XOR` $i_s(m)$. So $i_{\langle x,k\rangle::s}(x) \in X_m$ and $i_{\langle x,k\rangle::s}(x) \in i_s(m)$ have the same truth-value.

In the light of this, we obtain $i_{\langle x,k\rangle::s}(x)$ from our first approximation – $(X \setminus B) \cup A$ – by adding everything in $\{B^{-1}(X_m) : \mathfrak{M} \models R(x, m)\}$ and deleting everything in $\{B^{-1}(X_m) : \mathfrak{M} \models \neg R(x, m)\}$. Just a final check to ensure that this doesn't interfere with the adding and deleting we did initially, by adding everything in A and deleting everything in B: this last stage adds and deletes left-or-right objects with *odd* subscripts, whereas the initial tweaking added and deleted left-or-right objects (if any) with *even* subscripts only.

■

COROLLARY 15 *Every countable binary structure embeds into the term model of NF0*$^{--}$ *in* $2^{\aleph_0}$ *ways.*

What about arbitrary binary structures? We can create a nice family of finite injective maps from an arbitrary binary structure $\mathfrak{M}$ to any model of NF0^{--}. Of course they won't cohere becos $\mathfrak{M}$ might be uncountable. And our strategy for creating an injection for a finite substructure of $\mathfrak{M}$ gives us a different answer depending on the order in which the elements of that finite substructure are presented to the algorithm. So we think of the domains of those partial maps as finite list from M, the carrier set of $\mathfrak{M}$. And we need to correctly state the amalgamation condition that these partial injections obey. I think it is this: if i_1 and i_2 are two partial injections then if the union $i_1 \cup i_2$ of their graphs is the graph of an injection then they have a common extension.

14.1 Some messages on this subject from Albert

edited by tf

Dear Thomas and Allen,

I hope you are both doing well.

Good morning.

Â· Extensionality Â· Empty set Â· Existence of $x \cup \{y\}$ for all x and y. (adjunction)

This is adjunctive set theory with Extensionality. We can drop extensionality for essential undecidability. For an overview of the uses of adjunctive set theory, see:

Visser, Albert, "What is sequentiality?", in "New Studies in Weak Arithmetics", CSLI Lecture Notes **211**, CSLI Publications and Presses Universitaires du Pôle de Recherche et d'Enseingement Supérieur Paris-est", 2013", pp 229–269", editors Cégielski, Patrick and Cornaros, Charalampos and Dimitracopoulos, Costas"

The theory plays a fundamental role in the definition of sequential theories.

> Suppose we weaken the axiom giving us an empty set to an axiom that merely says there is a set that is not a member of itself ... is the result still essentially undecidable..?

A good question. The answer is no. Consider the graph with two elements a and b; we have arrows back and forth between a and b and a loop on b and that'ss all.

We have $a = \{b\}$ and $b = \{a, b\}$, $a \cup \{a\} = \{b, a\} = b$, $a \cup \{b\} = \{b, b\} = a$, $b \cup \{a\} = \{a, b, a\} = b$, $b \cup \{b\} = \{a, b, b\} = b$. Extensionality is obvious and a is not an element of a.

This is Harvey's insight: the theory having just $x \cup \{y\}$ and $x \setminus \{y\}$ is essentially undecidable since it is mutually interpretable with Adjunctive Set Theory (with a parameter). We can let an arbitrary element pose as the empty set.

See Theorem 3.4 of Vissersequential.pdf in assorted-paper-archive

14.2 Some Logical Results

A project worth pursuing is that of extending to the logic of the cofinite quantifier the various known results about ordinary logic and the Quine systems. We know that every $\exists^*$ sentence consistent with NF0 holds in the term model. To get a version for the cofinite quantifier we need to get straight the idea of a $\exists^*_\infty$ formula consistent with NF0.

"Being consistent" in this sense for a formula $(\exists_\infty x_1 \ldots x_n)\phi$ where ϕ is quantifier-free means the following. Suppose ϕ has n free variables. Then we invent constants whose suffixes come from $\mathbb{N}^{\leq n}$. For each sequence $c_{i_1} \ldots c_{i_n}$ where the suffix i_{k+1} is of length $k+1$ and is an end-extension of the suffix i_k, we adopt the axiom $\phi(c_{i_1} \ldots c_{i_n})$. Call this theory T. Then T is equivalent to

$(\exists_\infty x_1 \ldots x_n)\phi$ in the sense that every model of T is an expansion of a model of $(\exists_\infty x_1 \ldots x_n)\phi$ and *vice versa.*

THEOREM 33
Every $\exists^*_\infty$ formula consistent with NF0 is true in all models of NF0.

Proof:
Let $(\exists_\infty x_1 \ldots x_n)\phi$ be such a formula, and T the theory obtained from it as above. Now every axiom of T is a consistent $\exists^*$ formula, and so is true in the term model, and so is a theorem of NF0.

■

Notice that we haven't yet had to exploit the clever construction of nice embeddings. That happens next.

REMARK 82 *The term model for NF0 satisfies every $\forall^*_\infty\exists^*_\infty$ formula consistent with NF0.*

Proof:
Consider $(\forall_\infty x_1 \ldots x_n)(\exists_\infty y_1 \ldots y_k)\phi(\vec{x}, \vec{y})$. Suppose this has a model $\mathfrak{M}$. We want to show that it is true in the term model. For this it will suffice to show that if $\vec{t}$ is any tuple of terms such that $\mathfrak{M} \models (\exists_\infty y_1 \ldots y_k)\phi(\vec{t}, \vec{y})$ then there are infinitely many many *terms* s_1 such that there are infinitely many terms s_2 etc such that $\phi(\vec{t}, \vec{s})$.

The first step is to simplify $(\exists_\infty y_1 \ldots y_k)\phi(\vec{t}, \vec{y})$ to the limits of our ingenuity. We know that atomic formulæ in ϕ need never be of the form '$y_j \in\ t_i$', because any such atomic wff can be expanded until it becomes a boolean combination of atomic wffs like '$y_i = t_j$', '$y_j \in y_i$', and '$t_j \in\ y_i$'. Then we can recast the matrix into disjunctive normal form. We know that $\mathfrak{M} \models (\forall_\infty \vec{x})(\exists_\infty \vec{y})(\Phi(\vec{x}, \vec{y}))$ so there is at least one disjunct that does not trivially violate the theory of identity. This disjunct is a conjunction of things like '$y_i = t_j$', '$y_j \in y_i$', and '$t_j \in y_i$' and their negations, atomic wffs not containing any $\vec{y}$ having vanished since they are decidable.

We now have to find ways of substituting $NF0$ terms $\vec{w}$ for the $\vec{y}$ to make every conjunct in the disjunct true. To do this we return to the constructions seen in the proof of theorem 32. We construct witnesses for the $\vec{y}$ in the way we constructed values of the function i in the proof of theorem 32. Let n_0 be some fixed integer such that all the t_i that appear in our disjunct have Bs nested less deeply than n. We know of (the infinitely many witnesses that we have to find for) y_0 that they is to have certain ts as members and certain others not. For each $k \in \mathbb{N}$ we construct a word w_0 which is the $n_0 + k$th left member (if '$y_0 \in y_0$' is a conjunct) or the n_0th right object (otherwise) $\cup$ (the tuple of t_i such that '$t_i \in y_0$' is a conjunct) minus (the tuple of t_j such that '$t_j \notin y_0$' is a conjunct). From here on, we construct words w_i to be witnesses for y_i in exactly the same way as we proved theorem 32. ■

Actually we can exploit the theorem (Yasuhara?) that says that all occurrences of '=' within the scope of a '$\forall_\infty$' can be massaged away.

THEOREM 34
If $NF0 \vdash \exists\vec{x}\forall\vec{y}\phi(\vec{x},\vec{y})$ where ϕ is quantifier-free then for some tuple $\vec{t}$ of NF0 words, we have $NF0 \vdash \forall\vec{y}\phi(\vec{t},\vec{y})$.

Proof:
Let $\exists\vec{x}\forall\vec{y}\phi(\vec{x},\vec{y})$ be a $\exists^*\forall^*$ sentence, and suppose that for every tuple $\vec{t}$ of NF0 terms it is consistent that the tuple $\vec{t}$ is not a witness to the $\vec{x}$. Then the scheme

$$(\exists\vec{y})(\neg\phi(\vec{t},\vec{y})) \text{ over all tuples of terms } \vec{t} \tag{14.1}$$

is consistent.

How complicated is scheme 14.1? Well, each instance is equivalent to a disjunction of things of the form $(\exists\vec{y})(\psi(\vec{t},\vec{y}))$ where ψ is a conjunction of atomics and negatomics. What sort of atomics and negatomics? Well, equations and inequations between the ts disappear beco's they are all T or F by elementary means. Equations $y = t$ can be removed by replacing all occurrences of 'y' by 't'. What's left? Inequations $y \neq t$ and $y \in t$, $t \in y$, $y \notin t$, $t \notin y$. We attack those recursively. $y \in t$ might be $y \in t_1 \wedge y \in t_2$, in which case we recurse further. If it is $y \in t_1 \vee y \in t_2$ then the $\exists^*$ formula in which it occurs gets split into two such formulæ. If we keep on doing this we will end up with a disjunction of $\exists^*$ formulæ with terms appearing, but only in inequations or to the left of an '$\in$'. Clearly any such disjunction, if satisfiable at all, is satisfiable with the witnesses being finite tuples of terms, and is therefore true in the term model. So each instance of scheme 14.1 is true in the term model. That is to say, the term model believes $(\forall\vec{t})(\exists\vec{y})(\neg\phi(\vec{t},\vec{y}))$. So the original $\exists^*\forall^*$ sentence is not true in the term model, contradicting our assumption that $NF0 \vdash \exists\vec{x}\forall\vec{y}\phi(\vec{x},\vec{y})$.

So if NF0 proves a $\exists^*\forall^*$ sentence, there are provably witnesses that are NF0 terms. ■

By now the reader will have thought enough about extending these results to isomorphic formulæ in the language with the cofinite quantifier to have spotted that in the last para of the last proof there are of course *infinitely many* ways of satisfying such disjunctions. Accordingly I hope that later draughts of this note will contain a proof of

THEOREM 35 *If $NF0 \vdash (\exists_\infty\vec{x})(\forall_\infty\vec{y})\phi(\vec{x},\vec{y})$ where ϕ is quantifier-free then for a suitable infinity of tuples $\vec{t}$ of NF0 words, we have $NF0 \vdash (\forall_\infty\vec{y})\phi(\vec{t},\vec{y})$.*

We must think a bit about the scenario that the theorem describes. "$NF0 \vdash (\exists_\infty\vec{x})(\forall_\infty\vec{y})\phi(\vec{x},\vec{y})$" means simply that in every model of NF0 we can find infinitely many x_1 such that for each of them we can find infinitely many x_2 etc. The claim then is that, whenever this happens, we can take this network of xs to be NF0 terms.

Now suppose the claim is false, and that altho' in every model of *NF*O we can find infinitely many x_1 such that for each of them we can find infinitely many x_2 etc., we cannot take all of these witnesses to be terms.

That is to say, if we take any set of countably many terms – and think of them as t_s where s is a sequence of natural numbers of length at most the length of $\vec{x}$ – then the scheme

$$(\forall_\infty \vec{y})\phi(t_i, t_{i,j}, t_{i,j,k} \ldots \vec{y}) \text{ over all tuples of terms } \vec{t} \tag{14.2}$$

is not a theorem scheme. We wish to show that this scheme fails in the term model. So let $(\forall_\infty \vec{y})\phi(t_i, t_{i,j}, t_{i,j,k} \ldots \vec{y})$ be one of the instances that is not a theorem. Its negation is

$$(\exists_\infty \vec{y})\phi(t_i, t_{i,j}, t_{i,j,k} \ldots \vec{y})$$

and we wish to show that this is true in the term model. But this can be done by the constructions of theorem 32 and remark 82.

See section **??** of quantifiertalk.tex for a discussion of the correct generalisation of this to random/generic/countably categorical structures.

It's worth asking whether or not we can prove that every Henkin sentence consistent with *NF*0 is true in the term model for *NF*0. And of course there is the same question about T$\mathbb{Z}$T0.

But this is immediate!

14.3 More thoughts about NF0

If we add a constant symbol 'V' for the universe, and function symbols B, $\{,\}$ (for singletons) and the boolean operations $\setminus$ and $\cup$ then we can axiomatise NF0 as a $\forall^*$ theory as follows.

$(\forall xy)(x \in B(y) \longleftrightarrow y \in x))$
$(\forall xy)(x \in \{y\} \longleftrightarrow x = y))$
$(\forall xyz)(x \in y \cup z \longleftrightarrow (x \in y \vee x \in z)))$
$(\forall xy)(x \in \overline{y} \longleftrightarrow x \notin y)$
$(\forall x)(x \in V)$

and extensionality is
$(\forall xy)((x \texttt{ XOR } y) = \emptyset \to x = y)$

Do we need all the comprehension of T$\mathbb{Z}$T to make this work? It suffices that every permutation of finite support (or at least every finite product of disjoint transpositions) should be setlike. Do we get this in T$\mathbb{Z}$T0? My guess is not.

Can we generalise this to theories with richer axioms than T$\mathbb{Z}$T0? No, or at least not straightforwardly. We were able to obtain the assignment W_p by an iterative process that worked by recursion on types. This was because the characteristic axioms of T$\mathbb{Z}$T0 are type raising. At least one of the characteristic axioms of T$\mathbb{Z}$T0 is $\bigcup$, which is type-lowering.

Corollary: any $\Sigma_1^{\{B\}}$ sentence that is consistent with T$\mathbb{Z}$T is true in the term model for T$\mathbb{Z}$T0, and therefore true in every model of T$\mathbb{Z}$T0. So T$\mathbb{Z}$T0 decides all $\Sigma_1^{\{B\}}$ sentences. I think every $\forall^*\exists$ sentence is $\Pi_1^{\{B\}}$ so we will have proved at least that T$\mathbb{Z}$T decides all $\forall^*\exists$ sentences.

Thu 27/v/2025

Isabella (cc Randall),

I now think i know what is going on. Long story short... I think there may be a hitherto unknown countably categorical digraph, as follows. It has the property of "being closed under B" which is to say that, for every vertex v, there is a vertex v' such that whenever there is an edge from u to v then there is an edge from u to v' and *vice versa*. Let us say that such a v' is a **B-vertex**. Then one also wants to say that every vertex v "has finite difference" from a B-vertex v', by which i mean that: for all vertices v there is a B-vertex v' s.t. for all but finitely many vertices u there is an edge $u \to v$ iff there is an edge $u \to v'$. (This is not a first-order condition, tho' it can be expressed using the cofinite quantifier.)

I haven't quite nutted out the extra conditions one wants to make the graph countably categorical but i am keeping my fingers crossed that the above is a step in the right direction. I think the extra conditions are going to be "all possible close vertices exist" which means closure under subcision and adjunction. Also a kind of omitting types condition that prevents there being silly terms like something that is B^n of something for every n. And if we want all countable models of the theory to be isomorphic we have to rule out things like $x = B(x)$ ("Boffa atoms")

This digraph will have the property that every countable digraph embeds into it.

14.4 Friederike Körner on Model Companions of Stratified Theories: notes by Thomas Forster

Let T be a theory in the language $\mathcal{L}$ of set theory ($=$ and $\in$) with (at least) the axioms of extensionality and

$$\forall x_1 \ldots x_n \exists y (y = \{x_1, \ldots x_n\})$$

existence of unordered n-tuples.

We assume that T has an infinite model in which every transposition is setlike.

14.4.1 The set of universal consequences of T

PROPOSITION 11 *Every finite $\mathcal{L}$-structure can be isomorphically embedded in some model of T.*

Proof:

Let $\mathcal{A} = \langle A, \in_{\mathcal{A}} \rangle$ be an arbitrary finite $\mathcal{L}$-structure.[2] ($A = \{a_1 \ldots a_n\}$). Let $\mathfrak{M}$ be a model of T where every permutation of finite suport is setlike. Choose distinct $c_1 \ldots c_n \in \mathfrak{M}$ and define a permutation τ by

$$\tau(c_i) = \{c_j : \mathcal{A} \models a_i \in c_j\}$$

for $1 \leq i \leq n$. Then $\mathfrak{M}^\tau \models c_j \in c_i$ iff $\mathcal{A} \models a_j \in a_i$ (for $1 \leq i \leq n$ as before). So the function f sending each a_i to the corresponding c_i is an isomorphic embedding.

■

Corollary 16 *Any $\mathcal{L}$-structure can be isomorphically embedded in a model of T.*

Proof: Compactness ■

Definition 23 *$T_\forall$ is the set of $\forall^*$ theorems of T.*

Corollary 17 *T is an extension of LPC conservative for $\forall^*$ formulæ*

Definition 24 *A theory T is* **model-complete** *iff every embedding between models is an elementary embedding. (Equivalently, every first-order formula is equivalent to a universal formula. This notion was introduced by Abraham Robinson.)*

Definition 25 *A theory T^* in $\mathcal{L}$ is the* **model companion** *of T if $(T^*)_\forall = T_\forall$ and T^* is model-complete*

If T has a model companion at all then it is unique.

Now we are going to define the theory T^* which will turn out to be the model companion of T. Let $\gamma(x, y_1 \ldots y_n)$ be a conjunction of some of the following atomic and negatomic formulæ: $x \in x$, $x \notin x$, $x \in y_i$, $x \notin y_i$ $(1 \leq i \leq n)$ $y_i \in x$, $y_i \notin x$ $(1 \leq i \leq n)$. Now if

$$\bigwedge_{1 \leq i < j \leq n} (y_i \neq y_j \wedge x \neq y_i) \wedge \gamma(x, y_1 \ldots y_n)$$

is satisfiable[3] then

$$(\forall y_1 \ldots y_n)(\exists x) \bigwedge_{1 \leq i < j \leq n} y_i \neq y_j \rightarrow (\bigwedge_{1 \leq i \leq n} x \neq y_i \wedge \gamma(x, y_1 \ldots y_n))$$

is an axiom of T^*. T^* has no other axioms.

Proposition 12 *T^* is consistent*

[2] Something like this is in Hinnion's thesis

[3] Does she mean "consistent with T?"

Proof: Since all the axioms of T^* are $\forall^*\exists$ sentences we have grounds to hope that we can devise a model which is a union of a countable chain of models. Enumerate all the axioms of T^* as $\langle \phi_n : n \in \mathbb{N}\rangle$ in such a way that every axiom appears infinitely often.

Start with $\mathfrak{M}_0 = \langle M, R_0\rangle$ where $M = \{a_i : i \in \mathbb{N}\}$ and if $i \neq j$ then $a_i \neq a_j$. Thereafter construct $\mathfrak{M}_{n+1}$ from $\mathfrak{M}$ as follows:

Suppose ϕ_n is $(\forall y_i \ldots y_n)(\exists x)\psi(x, y_1 \ldots y_n)$. If $\mathfrak{M}_n \models \phi_n$ then $\mathfrak{M}_{n+1} = \mathfrak{M}_n$. Otherwise let $\langle a_{i_1} \ldots a_{i_k}\rangle \in \mathfrak{M}^k$ be the first k-tuple (in the lexicographic order of $\mathfrak{M}^k$) for which there is no x such that $\exists x\psi(x, a_{i_1} \ldots a_{i_k})$. Let b be the a_i of smallest index which is not in $dom(R_n) \cup rn(R_n)$. R_{n+1} is now obtained from R_n by adding enough pairs $\langle a_{i_l}, b\rangle$, $\langle b, a_{i_l}\rangle$ to make $\gamma(b, a_{i_1} \ldots a_{1_k})$ true.

$\mathfrak{M}$ is the direct limit of the $\mathfrak{M}_i$ and is a model of T^*.
[4]

PROPOSITION 13 $(T^*)_\forall = T_\forall$.

Proof:
We have to show that for every $\mathcal{L}$-structure $\mathcal{A}$ there is a model $\mathcal{A} \models T^*$ with $\mathcal{A} \subseteq \mathfrak{M}$.

Let $\mathcal{A}$ be an arbitrary $\mathcal{L}$-structure and let $\Delta_\mathcal{A}$ be the diagram of $\mathcal{A}$ in the language $\mathcal{L}_\mathcal{A}$. We claim that any finite subset of $\Delta_\mathcal{A}$ is consistent with T^*. Let Σ be a finite subset of $\Delta_\mathcal{A}$. There are only finitely many constants $c_1, \ldots c_n$ that occur in Σ. We may assume that $c_i \neq c_j$ for $1 \leq i \leq j \leq n$. Let $\gamma(c_1)$ be the conjunction of the formulæ in Σ that contain c_1 only. Choose $a_1 \in \mathfrak{M}$ such that $\mathfrak{M} \models \gamma(a_1)$. Thereafter, having chosen $a_1 \ldots a_i \in \mathfrak{M}$, let $\gamma(c_{i+1}, c_1 \ldots c_i)$ be the conjunction of those of the following formulæ that are in Σ: $c_{i+1} \in c_{i+1}$, $c_{i+1} \notin c_{i+1}$, $c_{i+1} \in c_l$, $c_l \in c_{i+1}$ $(1 \leq l \leq i)$. Since $\gamma(c_{i+1}, c_1 \ldots c_i)$ is satisfiable, $(\forall y_1 \ldots y_n)(\exists x) \bigwedge_{y_i \neq y_j} \to \bigwedge_i (x \neq y_i) \wedge \gamma(x, \vec{y}))$ is an axiom of T^* and thus there is $a_{i+1} \in \mathfrak{M}$ with $\mathfrak{M} \models \bigwedge_{k=1}^{i} a_k \neq a_{i+1} \wedge \gamma(a_{i+n}, a_1 \ldots a_i)$. Therefore $\langle \mathfrak{M}, a_1 \ldots a_n\rangle \models T^* \cup \Sigma$.

■

PROPOSITION 14 T^* *is model complete*

Proof:
Use Lindström's theorem. (See, for example, Chang and Keisler 3rd edn 3.5.8.) To do this we must show:

1. All models of T^* are infinite.

2. T^* is preserved under unions of chains.

3. T^* is α-categorical for some $\alpha \geq \aleph_0$.

[4] Why do we want each ϕ to appear infinitely often? Presumably all this is "standard model theoretic nonsense".

(1) is obvious. (2) follows from the fact that T^* has a set of $\forall^*\exists^*$ axioms. As for (3), a back-and-forth argument will show that T^* is countably categorical.

Suppose $\mathcal{A} = \langle A, \in_A\rangle$ and $\mathcal{B} = \langle B, \in_B\rangle$ are countable models of T^*. Wellorder $\mathcal{A}$ and $\mathcal{B}$ in order-type ω by $\leq_\mathcal{A}$ and $\leq_\mathcal{B}$.

Let a_0 be the $\leq_\mathcal{A}$-first element of A, and let $\gamma_0(x) = x \in x$ (if $a_0 \in_\mathcal{A} a_0$) and $x \notin x$ (otherwise). Let b_0 be the $\leq_\mathcal{B}$-first member of B that satisfies $\gamma_0()$ and set $f`a_0\ b_0$.

Now suppose we have constructed n pairs in f.

Two cases

- $n+1$ is even. Let a_{n+1} be the $\leq_\mathcal{A}$-first element not in the domain of the f-so-far. Let $\gamma(x, y_0 \ldots y_n)$ be $x \in^* x \wedge \bigwedge_{i=0}^{n} x \in^* y_i \wedge \bigwedge_{i=0}^{n} y_i \in^* x$ where the asterisks on top of the epsilons mean that they should be negated, or not, so that $\mathcal{A} \models \gamma(x, y_0 \ldots y_n)$. Since $(\forall y_1 \ldots y_n)(\exists x)(\bigwedge y_i \neq y_j \to (\bigwedge x \neq y_i \wedge \gamma(x, y_1 \ldots y_n) \in T^*$ we infer that $\mathcal{B} \models \exists x \gamma(x, b_0 \ldots b_n) \wedge \bigwedge_{i=0}^{n} x \neq b_i$. Define b_{n+1} to be the $\leq_\mathcal{B}$-first element b of $B \setminus \{b_0 \ldots b_n\}$ that satisfies $\gamma(b, b_0 \ldots b_n)$ and set $f`a_{n+1} = b_{n+1}$.
- $n+1$ is odd. Let b_{n+1} be the $\leq_\mathcal{B}$-first element not in the range of the f-so-far. …and procede as before.

■

PROPOSITION 15 *T^* is the model-completion[5] of T.*

Proof: It will be sufficient to show that T has the amalgamation property.

Let $\mathcal{A} = \langle A, \in_\mathcal{A}\rangle$, $\mathcal{B} = \langle B, \in_\mathcal{B}\rangle$ and $\mathcal{C} = \langle C, \in_\mathcal{C}\rangle$ be three disjoint models of T with $f : \mathcal{C} \hookrightarrow \mathcal{A}$ and $g : \mathcal{C} \hookrightarrow \mathcal{B}$. Define an $\mathcal{L}$-structure $\mathcal{D}$ as follows. The domain D will be $C \cup (A \setminus f``C) \cup (B \setminus g``C)$. Then, for $a, b \in D$ set $a \in_\mathcal{D} b$ iff one of the following holds:

$a, b \in C$ and $a \in_\mathcal{C} b$
$a, b \in B$ and $a \in_\mathcal{B} b$
$a, b \in A$ and $a \in_\mathcal{A} b$
[*HOLE exercise: complete this definition!!!!*]

```
From koerner@math.tu-berlin.de Fri Jun 12 15:03:12 1998

>
>   You know i have a conjecture
> that NF remains consistent if you add to it
> every $\forall^*\exists^*$ (or $\forall_2$ if
> you prefer) sentence that is consistent with
> it.    This is presumably something to do with
> NF having a model companion.
```

[5]That is to say, T^* is the model companion of T and, for any model $\mathfrak{M} \models T$, $T \cup \Delta_\mathcal{A}$ is complete.

Your question concerns the stuff in Ch2 of my thesis (do you have a copy ?, i forget).

If i recall correctly, the basic facts are

NF has a model companion, i.e. there is a theory T which has exactly the same universal consequences as *NF* (i.e. no sentences except tautologies) and is model complete. (for definitions etc. see e.g. Chang/Keisler, 3rd ed., 3.5)

The countable model of T is countably categorical and probably should be named "the countable universal homogeneous di-graph".

That is, it's the theory consists of all the sentences saying:

i)

for all finite disjoint sets I, J of points (vertices) and all finite disjoint sets K, L of points (vertices) there is a point x such that

- $x \, R \, y_i$ for all $y_i \in I$,

  $\neg(x \, R \, y_j)$ for all $y_j \in J$,

  $y_k \, R \, x$ for all $y_k \in K$ and

  $\neg(y_l \, R \, x)$ for all $y_l \in L$ and

  $x \, R \, x$

  and

- for all finite disjoint sets I, J of points (vertices) and all all finite disjoint sets K, L of points (vertices) there is a point x such that

  $x \, R \, y_i$ for all $y_i \in I$,

  $\neg(x \, R \, y_j)$ for all $y_j \in J$,

  $y_k \, R \, x$ for all $y_k \in K$ and

  $\neg(y_l \, R \, x)$ for all $y_l \in L$ and

  $\neg(x \, R \, x)$.

T admits elimination of quantifiers. All $\forall_2$-sentences which are consistent with *NF* are true in T. Unfortunately the converse is false.

Love, Friederike

THEOREM 36 *Every $\forall^*\exists^1$ sentence consistent with NF0 is true in the term model.*

Proof:

Suppose we have a model of NF0 that satisfies $(\forall\vec{x})(\exists y)\phi(\vec{x}, y)$. Fix a tuple $\vec{x}$ of things in the term model. We will show that the 'y' can be witnessed by a term.

ϕ is quantifier-free, so $\phi(\vec{x}, y)$ is a boolean combination of things like $y \in x_i$, $x_i \in y$, $y \in x_j$, $x_j \in y$ and $y = x_j$. How do we find a y satisfying those conditions? Conditions like $x_i \in y$ and $x_j \notin y$ are easy to deal with – we just put x_i into y and do not put in x_j. It's conditions like $y \in x_j$ that require ingenuity.

The key observation is that x_j is a boolean combination of singletons and principal ultrafilters, things like $B(z)$. So $y \in x_j$ becomes a boolean combination of things like $y = z$ and $y \in B(z)$. But this last is just $z \in y$, so conditions like $y \in x_j$ become boolean combinations of atomics where 'y' is never to the left of an '$\in$', so we are back in a situation where all we have to decide is which things to put in y and which to leave out. Easy-peasy! But there is one detail i have saved up for the end. One atomic that we need to consider in $y \in y$. ϕ might require $y \in y$ or it might require $y \notin y$. This is where we need the trick that enabled us to embed every countable binary structure in the term model for NF0.

Very well, the challenge is to find a y which satisfies $u_i \in y$ for various terms u_i obtained by deconstructing the x_i as above, and satisfies $u_j \notin y$ similarly. (We also have $y = u_i$ and $y \neq u_j$ but they don't cause a problem.) On the face of it all we have to do is set y to be the tuple of those u_i for which we require $u_i \in y$, and hope that none of those u_i are equal to any u_j for which we want $u_j \notin y$. This hope will be realised since – if it weren't – then $(\exists y)\phi(\vec{x}, y)$ was not true in our model for that choice of $\vec{x}$. (This is also the reason why the equations and inequations do not cause a problem) So we obtain such a y. But does it satisfy $y \notin y$, or $y \notin y$ – whichever is required? It seems pretty clear that any y obtained by this method will not be a member of itself, but we don't need this fact and we won't prove it. Instead we appeal to the device of left objects and right objects from the proof that every countable binary structure embeds into the term model for NF0. The *left objects* are $B^n(\emptyset)$ for all concrete n, and the *right objects* are $B^n(V)$ for all concrete n. No left object is a member of itself and every right object is a member of itself. The clever bit is that (more-or-less) any set that has finite symmetric difference from a left-object or from a right object has the same behaviour (a member of itself or not) as the object it is close to.

The various u_i that go into the construction of y are all terms; there are finitely many of them, so there is a concrete number n that is greater than the depth of all the terms u_i. We want $y \in y$? Very well, we take the mth right object for some $m > n$ and modify it by inserting or deleting the u_i as required. Since the things that are being inserted or deleted are of rank $< n$ none of them can be the mth right object, so the modified term remains self-membered. *Mutatis mutandis* if we want $y \notin y$.

Notice that for sufficiently large m the mth right object will serve our purpose, so there are infinitely many witnesses. (Shades of *nice embeddings*.)

■

That's all very well, but the result we wanted was for $\forall^*\exists^*$ sentences not just $\forall^*\exists^1$ sentences. I think the proof can be tweaked to get the more general result. All is the same up to the stage where we deconstruct expressions of the form $y \in x_i$. This time we have a tuple $y_1 \ldots y_k$ and we obtain witnesses by recursion. We obtain y_1 the way we obtained the sole y in the construction above. Successively, for each y_1 with $1 > 1$ we consider its relations with the x_i and with y_j for $j < i$ but *we don't worry about its relations with the y_i for*

$i > 1$.

So – for example – when considering y_2 we have to accommodate one of $y_2 \in y_1$ and $y_2 \notin y_1$. But $y_2 \in y_1$ can be expressed as a boolean combination of atomics where y_2 is never to the left of an '$\in$' – as we did above.

It's worthy of note that this construction can be iterated so as to accommodate an ω-sequence of y variables, so what we have is a proof that

THEOREM 37 *Every $\forall^*\exists^\infty$ formula of $\mathcal{L}_{\omega_1,\omega_1}$ true in even one model of NF0 is true in the term model.*

... where the '$\exists^\infty$' signifies an ω-sequence of existential quantifiers. (But there are only finitely many $\forall$ variables.)

A further development would be an analogous result for $\forall^*\exists^*$ sentences where $\forall$ means "for all but finitely many" and $\exists$ means "there infinitely many". I am expecting this to be comparatively straightforward, since it will be assured by the existence of left objects and right objects of arbitrarily high rank.

Now NF0 extends TTST (which is essentially undecidable) so it can't be decidable. And the theory of the term model of NF0 cannot be decidable either, for the same reason. But the class of $\exists^*\forall^*$ sentences true in the term model is r.e. – as follows. Suppose ϕ is $\exists^*\forall^*$ and true in the term model. Then $\neg\phi$ is $\forall^*\exists^*$ and cannot be true in any model of NF0 lest it be true in the term model, which it ain't. So NF0 $\vdash \neg\phi$.

I wonder if this might come in useful. We have a function symbol B; let's help ourselves additionally to two new binary function symbols `Ad` and `Su` for adjunction and subcision. We can then drop '$\in$' from the language since we can define $x \in y$ either as $\mathtt{Su}(y, x) \neq y$ or as $\mathtt{Ad}(y, x) = y$. In fact we probably want the biconditional

$$(\forall x, y)(\mathtt{Su}(y, x) \neq y \longleftrightarrow \mathtt{Ad}(y, x) = y)$$

as an axiom. Armed with this definition of $\in$ we can supply axioms for B and express extensionality.

So we can express NF0^{--} in a language without '$\in$'!

Does this help?

A conversation with Isabella Scott 5/vi/2025

NF0^{--} ("NF0 minus complementation") has adjunction and subcision and B. It also has extensionality but we can probably do without it.

LEMMA 19 *NF0^{--} proves both $(\exists x)(x \in x)$ and $(\exists x)(x \notin x)$.*

Proof:

For present purposes we take $(\exists x)(x = x)$ to be a logical truth.

(i) NF0^{--} $\vdash (\exists x)(x \in x)$

For any a we have $B(a) \cup \{a\} \in B(a) \cup \{a\}$ as follows.

$a \in B(a) \cup \{a\}$

$B(a) \cup \{a\} \in B(a)$ by definition of B
$B(a) \cup \{a\} \in B(a) \cup \{a\}$

(ii) $\mathrm{NF0}^{--} \vdash (\exists x)(x \notin x)$
For any a we have $B(a) \setminus \{a\} \notin B(a) \setminus \{a\}$ as follows.
$a \notin B(a) \setminus \{a\}$
$B(a) \setminus \{a\} \notin B(a)$
$B(a) \setminus \{a\} \notin B(a) \setminus \{a\}$
And of course both $B(a) \cup \{a\}$ and $B(a) \setminus \{a\}$ exist by the axioms of $\mathrm{NF0}^{--}$.
■

What about $\mathrm{NF0}^{--}$ with the cofinite quantifier. Isabella sez: perhaps it has elimination of (cofinite) quantifier. Is it *o*-minimal?

$\mathrm{NF0}^{--}$ is essentially undecidable. This is beco's adjunction-and-subcision (plus emptyset i think) is essentially undecidable.

Isabella sez the complexity of the infinitary version should be the same as the complexity of the finite version, and therefore undecidable.

Isabella wonders if the Nice Embedding Property is enuff *by itself* to make the theory interpret Robinson Arithmetic. Worth asking: do we know any other theories that have the Nice Embedding Property? (What about the canonical random graph? Doesn't every countable graph embed in it??) She sez: it's easy to find a binary structure with uncountably many 2-types, as follows. For each x we can think of $F(x, y)$ as a 1-place predicate, so in any infinite model we may have countably many such predicates and therefore uncountably many types. Any such structure can be embedded using the Nice Embedding Construction. (We hope and trust that this means that ...) every model of $\mathrm{NF0}^{--}$ has uncountably many types and so by Ryll-Nardzewski's theorem cannot be countably categorical. But we knew that anyway. Isabella says that perhaps the undecidability follows from the Nice Rmbedding Property *via* Ryll-Nardzewski's theorem. There are a few dots to join up there.

Chapter 15

The General Hierarchy

[*HOLE This chapter needs heavy editing!*]

It is an old puzzle whether or not Amb^n (as i call it) is equiconsistent with Amb. I showed that Amb^n, for any n is enough to refute AC, and Marcel gave a much simpler proof. How about trying to prove that $Amb^n \vdash Amb$ for any n?

Here is a way that might work. Think about $\mathcal{P}$-extensions. These are the extensions Kaye and I wrote about in our joint JSL paper of 1990. $\mathcal{B}$ is a $\mathcal{P}$-extension of $\mathcal{A}$ iff $\mathcal{B}$ is an end-extension of $\mathcal{A}$ in which old sets do not acquire new **subsets** (not only no new members).

Take the case $n = 2$ for ease of illustration. If we had a model of Amb^2 then we would have a model of TST that was glissant^2. (I hope it is obvious what that means!). Remind yourself of two elementary facts, and one piece of notation. $\mathfrak{M}_{-n}$ is the model obtained from $\mathfrak{M}$ by deleting the bottom n levels and relabelling everything so that the old level n is now level 0. It is not hard to check (use ι) that $\mathfrak{M}_{-1}$ is (isomorphic to) a $\mathcal{P}$-extension of $\mathfrak{M}$ whatever $\mathfrak{M}$ is. Now let $\mathfrak{M}$ be a model of TST which is glissant^2. We have

- $\mathcal{M}$ is a $\mathcal{P}$-extension of $\mathcal{M}_{-1}$ (because $\mathcal{M}$ is isomorphic to $\mathcal{M}_{-2}$ (it's glissant^2) and
- $\mathcal{M}_{-1}$ is a $\mathcal{P}$-extension of $\mathcal{M}$ (it always is).

So we have two structures each of which is (isomorphic to a) $\mathcal{P}$-extension of the other. What can we infer from this? Must they be elementarily equivalent, or what?

Of course there are similar examples in the one-sorted case.

$\mathfrak{M} \subseteq_e \mathfrak{N}$ says that $\mathfrak{N}$ is a $\mathcal{P}$-extension of $\mathfrak{M}$.

15.1 end-extension

$\subseteq_e$ is obviously transitive. Boffa has pointed out to me that CH is $\Delta_1^{\mathcal{P}}$ and independent of TST so $\subseteq_e$ lacks upper bounds (and *a fortiori* sups). Also ω-

chains do not have sups in general, for the sup of $\langle \chi^n(\mathfrak{M}) : n < \omega \rangle$ would have to be a model of Amb.

There is a related relation probably best written $\prec_e$ We might wonder whether $\prec_e$ is antisymmetrical. (We should at least be able to prove that if $\mathfrak{M} \prec_e \mathfrak{N} \prec_e \mathfrak{M}$ then $\mathfrak{M} \equiv \mathfrak{N}$ but even this i cannot see at the moment). This question of antisymmetry is intimately related to whether we allow the injection implicit in "$\mathfrak{N} \prec_e \mathfrak{M}$" to be setlike or insist on it being a set. The problem is that the relation "there is a setlike injection $x \to y$" does not seem to be antisymmetrical, and it appears to go wrong in two quite separate ways. First, there seems to be no guarantee that x and y can be split in the way required by the proof of s-b, and second, even if we can split x and y appropriately the bijection doesn't seem compelled to be setlike: it will lift once (to give a model of TST_3) but not twice! This keeps cropping up. Perhaps it is worth isolating this problem: it might be the right context for developing NF-with-classes. ML is usually overlooked, as is GB and for the same reasons. However, there might be a case for examining the halo of classes that lives around a *pair* of models, for it might help us understand $\prec_e$.

It is not clear whether or not $\prec_e$ or $\subseteq_e$ is wellfounded. It is probably worth noting that it doesn't really measure size, for very small models of TST do not go into very big ones: no non-natural-model is $\subseteq_e$ a natural one! Another topic for later development will be how $\subseteq_e$ behaves with ultraproducts, permutation models etc. We do know that every model of *NF* has a permutation model which is a proper $\mathcal{P}$-extension of it. We know also that end-extensions are never elementary so we cannot ever have $\mathfrak{M} \subseteq_e \mathfrak{M}^\kappa/U$. To the extent that $\subseteq_e$ is a bit like $\trianglelefteq$ (normal subgroup) we should be thinking about a quotient $\mathfrak{N}/\mathfrak{M}$ when $\mathfrak{M} \subseteq_e \mathfrak{N}$. Since the images of the embeddings are ideals this is a possibility[1]. The following is an obvious thing to try. $T_0^{N/M} = \mathfrak{N} \setminus \mathfrak{M}$. $T_1^{N/M} = (T_1^N)/(T_1^M)$ as b.a.s, thereafter take power sets in the sense of $\mathfrak{N}$. That way the quotient is a substructure of $\mathfrak{N}$, unlike groups, but that is only beco's of the greater expressive power of set theory. I have the impression that $\mathfrak{N}$ is the sup of $\mathfrak{N}/\mathfrak{M}$ and $\mathfrak{M}$, tho' i do not see how to prove it.

Another remark of Boffa's is that two models could be elementarily equivalent and still fail to have a common end-extension because one contains nonstandard integers and the other doesn't. But if two models have a common end-extension they must satisfy the same $\Delta_1^{\mathcal{P}}$ sentences! No converse! Thus $\prec_e$ seems to have less to do with logic than one might have expected.

15.1.1 Natural models

The study of $\prec_e$ is fairly easy in this case. If we take $\subseteq_e$ in the strong sense it is simply the study of $\leq$ on cardinals. In ZF the strong and the weak notions coincide but in *NF* they do not, and life can get quite difficult. We have a + well-defined on natural models, and it is *not* defined on arbitrary models. This

[1]Is the inclusion embedding of a normal subgroup ever elementary? Probably not in interesting cases: Any abelian group is a normal subgroup of any ultrapower of itself ...

is kin to the failure of s-b for setlike embeddings.

Question If $\mathfrak{M}$ and $\mathfrak{N}$ are both ambiguous natural models, what about $\mathfrak{M} + \mathfrak{N}$?

Question: given a consistent extension T# of TST, is there a model of *ZF* containing a natural model of T#? Presumably the answer to this is no, because we can make T assert something pathological which is irrefutable in Zermelo but not in *ZF*. One thinks of borel determinacy but that uses choice ...

An answer to this would help us know when we can safely restrict our attention to natural models.

Annoying (but possibly deep?) fact: There are no natural models of T$\mathbb{Z}$T.

(Nice models of T$\mathbb{Z}$Tare scarce. Not only are there no natural models, no-one has ever found an ω-model or a term model.)

15.1.2 Other models

Let us consider the old question of whether or not Amb^2 implies Amb. Assume Amb^2. Then we have a model M with a $tsau^2$ σ. If we try to do s-b using the obvious maps (ι and $\iota \circ \sigma^{-1}$) then we need to know that the clever split of the bottom type into two bits actually splits it into two sets of the model. A little calculation shows that what we need is that there should be x such that $\sigma(x) = V \setminus (\iota``(V \setminus \iota``x))$. This can certainly be arranged with the help of some model theory and no extra axioms, but all it gives us as an isomorphism h between the two bottom types. As usual, it will lift once (beco's x is a set) but not, apparently, twice. Actually for what it's worth we can get this far with Amb^n for an old n.

REMARK 83 *If $\prec_e$ is antisymmetric on models of TST then $Amb^n \vdash Amb$ for any concrete n.*

Proof: :

If $\mathfrak{M} \models TST$ and has a tsau^n then $\chi^n`\mathfrak{M} \prec_e \mathfrak{M}$. But $\chi`\mathfrak{M} \prec_e \chi^n`\mathfrak{M}$ holds for all $\mathfrak{M}$ anyway, so we infer $\chi`\mathfrak{M} \prec_e \mathfrak{M}$. But we always have $\mathfrak{M} \prec_e \chi`\mathfrak{M}$, so, by antisymmetry, $\chi`\mathfrak{M}$ and $\mathfrak{M}$ are isomorphic.

■

15.1.3 The NF Case

We would naturally want to consider the analogous relation on models of NF. Is it antisymmetric? If we have two models $\mathfrak{M}$ and $\mathfrak{N}$ of *NF* such that $\mathfrak{M} \prec_e \mathfrak{N} \prec_e \mathfrak{M}$, are they

1. isomorphic? or at least
2. stratimorphic? or, lowering our sights,
3. elementarily equivalent? Or at worst
4. elementarily equivalent w.r.t. stratified sentences?

We do not seem to be able to prove any of these at the moment. Discussion must split into 4 cases depending on whether or not the models are natural, and whether or not we are doing this in *NF*. It also depends on whether or not the injection mentioned in $\prec_e$ has to be a set! In the next paragraph it is allowed that it mightn't be.

> Natural models discussed in *NF*. If the injections are *sets* then they must be isomorphic. If they are merely *setlike* then we don't know a great deal. Any $\mathcal{P}(x) \subseteq x$ will give rise to such a pair of *natural* models $\mathfrak{M}$ and $\mathfrak{N}$. If x and $\mathcal{P}(x)$ are distinct sizes then of course $\mathfrak{M} \neq \mathfrak{N}$. If $\neg$AxCount$_\leq$ there can be finite $\mathcal{P}(x) \subseteq x$ so they would $\models AC$.
>
> Natural models in ZF
>
> They must be isomorphic
>
> Non-natural models.
>
> In ZF without doing any extra work we can certainly show that $\mathfrak{N}$ and $\mathfrak{M}$ satisfy the same $\Sigma_1^{\mathcal{P}}$ sentences. If we try to argue that they must satisfy the same $\Pi_2^{\mathcal{P}}$ sentences we would want to know that every witness to $\exists \vec{x}\ \phi(\vec{x}, \vec{y})$ can be found inside $\langle\langle \vec{y} \rangle\rangle$ if ϕ is $\Delta_0^{\mathcal{P}}$ but this just isn't true, as Adrian's counterexample shows: n-sized set all of whose members are infinite and all of different sizes. We might be able to construct a counterexample to (1) and (3) consisting of $\mathfrak{M}$ and $\mathfrak{N}$, each embedded in the other as the unique maximal $x = \mathcal{P}(x) \neq V$ and where $\mathfrak{M} \models \exists y = \{y\} \wedge \forall x = \mathcal{P}(x) \neq V\ y \notin x$ but $\mathfrak{N}$ doesn't. (4) looks plausible. Unfortunately the problem of constructing a stratimorphism in this case seems to be the usual problem of s-b with setlike maps.

Beware of the following trap. Suppose ϕ is $\Delta_2^{\mathcal{P}}$. Consider the obvious direct limit. If ϕ is true in M, then it is true in the direct limit. If $\neg\phi$ is true in N then it is also true in the direct limit. Therefore M and N agree on $\Delta_2^{\mathcal{P}}$ sentences. Now they are both models of $\exists V$ (in which case everything is $\Delta_2^{\mathcal{P}}$). But this is not much help. Let ψ be an arbitrary expression true in M and false in N. Then

$$M \models \forall x \exists y\ y \notin x \wedge \psi^x$$

$$N \models \forall x \exists y\ y \notin x \wedge \neg\psi^x$$

So the direct limit satisfies both. This doesn't give us a contradiction unless the direct limit doesn't contain a universal set, which it obviously doesn't.

15.2 Normal Forms

The idea is that everything is equivalent to a formula in *normal form* where all unrestricted quantifiers are out at the front and all restricted quantifiers are in the matrix. We need to be able to push restricted universal quantifiers inside unrestricted existentials (and dually). This introduces a complication.

Quantifier-pushing lemma:
if
$$(\forall x \in y)(\exists z)\Phi(x, z, y)$$
then
$$(\exists w)(\forall x \in y)(\exists z \in w)\Phi(x, z, y)$$
The usual trick for this is the axiom scheme of collection:
$$(\forall x \in A)(\exists y)\Phi(x, y, A) \ \rightarrow \ (\exists B)(\forall x \in A)(\exists y \in B)\Phi(x, y, A)$$
(which is equivalent to replacement)[2]. So we need collection to do quantifier-pushing, and this is actually ok in type theory. It is even ok in NF *as long as we are restricting attention to stratified formulæ*, since stratified collection is provable in NF – just take B to be $\{y : \exists x \in A\ \Phi(x, y, A)\}$. We do not have unstratified collection in NF for obvious reasons, so we cannot push restricted universal quantifiers inside unrestricted existentials (and dually) if the matrix is unstratified. This will mean that $\forall x \in y$ outside something $\Sigma^{\mathcal{P}}_n$ may turn out to be $\Pi^{\mathcal{P}}_{n+1}$ instead of $\Sigma^{\mathcal{P}}_n$ if the matrix is unstratified. So for the moment we shall restrict our attention to stratified formulæ. If we do restrict our attention to stratified formulæ (and we are doing type theory for the moment) we can drop the "$\wedge$, $\vee$ and limited quantifiers" closure condition (that exists in some formulations) on the levels of the G hierarchy.

So, back to Z and stratified formulæ. Coret's theorem is that we have stratified replacement in Z so can we do all this for stratified formulæ in Z? Most of it goes over without any trouble. We can even squash a block of quantifiers of unlike type: if we have a block $\exists\vec{x}$ we can squash $\vec{x}$ into one variable, by saying $\exists$ an n-tuple (or $\forall$ n-tuple) which is $\langle \ldots \iota^{n_i}{}^{\backprime}x_i \ldots \rangle$ and this is Δ_0. The way in which this is done is not uniform in the differences in the type indices, but this is neither surprising nor unfortunate, since even so we are lumping together infinitely many formulæ into one form.

In Zermelo we have stratified replacement (but not stratified collection) so consider
$$(\forall x \in y)(\exists z)\Phi(x, z, y)$$
where $\Phi(x, z, y)$ is stratified. We want an f so that $f{}^{\backprime}x$ is some nonempty subset of $\{z : \Phi(x, y, z)\}$. Then we let $w = \bigcup f``y$. (We can't just send x to the set of things of minimal rank, it isn't stratified). Now one might think we should be able to show that $\{z : \Phi(x, y, z)\}$ must meet $\mathcal{P}^n(\bigcup^k y)$, but Adrian has a nice counterexample: let $H(x, y)$ say that y is a set of infinite sets all of different sizes and ${}^{=}y\ = x$. Then
$$(\forall x < \omega)(\exists z)H(x, z)$$
but there is nothing that collects all the y, i.e., not
$$(\exists w)(\forall x < \omega)(\exists z \in w)H(x, z)$$

[2]Evidently a combination of quantifier-squashing and quantifier-pushing will eventually get any formula into normal form. The point is that truth-definitions are available for things in normal form.

This counterexample clearly shows that we cannot bound the z inside $\mathcal{P}^n(\bigcup^k y)$, which is what one might expect. It may be sheerest coincidence but in NF we have almost exactly the same problem: there doesn't seem to be any way of proving that there are infinitely many distinct infinite cardinals.

All this quantifier-pushing and squashing is pretty easy in NF and such systems if $\Phi(x, z, y)$ is stratified.

And what about quantifier-pushing and squashing for arithmetic?

$$(\forall x \leq y)(\exists z)(\Phi(x, z, y))$$

z has to be an y-tuple sending things $x \leq y$ to things z such that $(\Phi(x, z, y))$ Can we do a uniform definition of y-tuples?

It is suggestive that the one $\Sigma_1^{\mathcal{P}}$ sentence (NCI infinite) is used to show that

I think there is a global error here. I think most occurrences of '$H_{\beth_\omega}$' should be occurrences of '$\bigcup_{n<\omega} H_{\beth_n}$'.

1. stratified replacement does not prove stratified collection;
2. $H_{\beth_\omega} \not\prec_{\Sigma_1^{\mathcal{P}}} V$ even tho', for limit λ, $H_{\beth_\lambda} \prec_{\Sigma_1^{L\acute{e}vy}} V$;
3. NF does not prove all consistent^{NF} stratified $\Sigma_1^{\mathcal{P}}$ sentences.

It is worth noting that $V_{\omega+\omega} \prec_{strat} H_{\beth_\omega} \prec_{\Sigma_1^{L\acute{e}vy}} V$ so that any stratified $\Sigma_1^{L\acute{e}vy}$ sentence true in V is true in $V_{\omega+\omega}$, that is,

$$V_{\omega+\omega} \prec_{str(\Sigma_1^{L\acute{e}vy})} V$$

("str" short for "stratified") This is actually best possible beco's the assertion that there is an infinite set of infinite sets no two the same size is $\Delta_2^{L\acute{e}vy}$ and false in $V_{\omega+\omega}$ tho' true in V. Can we have

$$V_{\omega+\omega} \prec_{str(\Sigma_1^{\mathcal{P}})} V\ ?$$

Since "there is an infinite set of infinite sets no two the same size" is $str(\Sigma_1^{\mathcal{P}})$ this would imply that GCH fails below $\beth_\omega$. It would also mean no measurables, since "$\exists$ measurable" is also $str(\Sigma_1^{\mathcal{P}})$.

How about

Conjecture 9

1. *NFC proves every consistent^{NFC} stratified $\Sigma_1^{\mathcal{P}}$ sentence;*
2. *NFC proves every consistent^{NFC} $\Sigma_1^{\mathcal{P}}$ sentence;*
3. *Every consistent^{NFC} $\Sigma_1^{\mathcal{P}}$ sentence is consistent with NFC;*
4. *Every consistent^{NFC} stratified $\Sigma_1^{\mathcal{P}}$ sentence is consistent with NFC;*
5. *NF proves every consistent^{NF} stratified $\Sigma_1^{\mathcal{P}}$ sentence;*

6. *NF proves every consistent^{NF} $\Sigma_1^{\mathcal{P}}$ sentence;*

7. *Every consistent^{NF} $\Sigma_1^{\mathcal{P}}$ sentence is consistent with NF;*

8. *Every consistent^{NF} stratified $\Sigma_1^{\mathcal{P}}$ sentence is consistent with NF.*

$3 \to 7$, $4 \to 8$. We can't prove these by skolemheim. 1, 2, 5 and 6 are presumably false beco's of CH. This is a (probably) consistent^{NF} stratified $\Sigma_1^{\mathcal{P}}$ sentence that appears not to be a theorem of NF. It should be possible to find examples that are more obviously not theorems of NF, though this and "there is a nonprincipal ultrafilter" are the best i can do at the moment. 6 is obviously false, because AxCount is a consistent^{NF} $\Sigma_1^{\mathcal{P}}$ sentence. 7 simply says NFC is consistent. 8 can be true only if it is consistent w.r.t. NF that NCI should be infinite and there is a nonprincipal ultrafilter somewhere.

Existence of wellfounded extensional relations on V generalises upward in models of T$\mathbb{Z}$T, and is $\Sigma_1^{\mathcal{P}}$.

- Is Z + stratified collection equiconsistent with ZF?
- ZF is not an extension of Z conservative for Σ_1-sentences: consider "There is a model of Z". For stratified Σ_1-sentences?
- Does every stratified Σ_1 consequence of Z follow from Ext, $\bigcup x$, $P(x)$, AxInf, $\{x, y\}$ and *stratified* replacement.
- What substructures of V are there elementary for $\Pi_2^{Lévy}$ sentences?

15.2.1 remaining junk

Z really is stronger than TST + AxInf so we cannot assume that the model of TST + Inf is a model of Z, and, even if it was, we know that not every model of Z is an initial segment of a model of ZF (Martin-Friedman theorem) in the sense of being $V_{\omega+\omega}$ of the new model. The new model might be an end-extension of the old but that isn't enough to ensure that no new $\Sigma_1^{Lévy}$ sentences become true.

Develop arithmetic in Z in a stratified way (use Russell-Whitehead cardinals at some level). We then find that we can devise lots of nasty *stratified* Σ_1^{Levy} sentences, such as $Con(TST)$. This means that there is no hope of showing (in ZF) that any model of $TST + Inf$ must satisfy all stratified Σ_1^{Levy} sentences. This also shows that ZF is not an extension of Z conservative for stratified $\Sigma_1^{Lévy}$ sentences (even). So this trick cannot work.

Let the scheme E_n say there are at least n distinct objects. If ϕ is true in all sufficiently large finite models then it follows from some E_n + *whatever remaining first-order stuff all finite models have in common*, like the negation of the axiom of infinity etc., so it does *not* automatically follow that ϕ is true in all infinite models of T.

Let us say a map σ between the bottom types of two models $\mathfrak{M}$ and $\mathfrak{N}$ of TST is *setlike* if for all n, $j^n`\sigma|(T_n^{\mathfrak{M}})$ is onto $T_n^{\mathfrak{N}}$. We can have a similar notion of setlike permutations of a model of a set theory with a universal set. There are setlike maps from V onto proper subsets of V that are not sets, e.g. ι. I don't know any setlike permutations of V that are not sets. There are setlike permutations of $\mathbb{N}$, NC, NO etc. that aren't sets but they do not seem to extend to setlike permutations of V[3].

André says that you can prove omitting types if you define $\phi(\vec{x}\vec{y})$ realizes Σ [a set of fmlæ with only 'x' free] if there is some $\vec{a}$ such that

$$\phi(\vec{x}, \vec{a}) \to \bigwedge_{\sigma \in \Sigma} \sigma(\vec{x}, \vec{a})$$

15.2.2 messages from james about reflection principles

dear t, thanks for the message. here is all i can think of off the top of my head about reflection principles:

- (levy) if $|V_\theta| = \theta$, and ϕ is a Σ_1 statement with parameters from V_θ, then $(V \models \phi) \to (V_\theta \models \phi)$. In the jargon of model theory the inclusion map is a 1-embedding.

- (solovay?) if θ is supercompact, then the same holds for $\Sigma_2 \phi$.

- (reinhardt?) if θ is extendible, ditto for Σ_3. Notice that if Σ_n formulæ reflect down then

  a) Π_{n+1} formulæ reflect down

  b) Π_n formulæ go up (i think the model theorists say they are preserved) see kanamori + magidor's expository paper on large cardinals for proofs and refs relating to 1,C,3. Another approach could be to reason like this ...if j embeds V into M (not necessarily contained in V) then $j``V$ is an elementary substructure of $j'V = M$. 2) has amusing consequences e.g. the first huge < the first supercompact if both cardinals exist ('cos although huge is higher in consistency strength, the defn. of huge is Σ_2 so "there exists huge" reflects). Not sure if this is germane (but it's good stuff anyway). if A is a class of V, κ is (Π_1)-strong in A iff for all Π_1 ϕ (in a

[3] Let's try. After all, we have

$$V \xrightarrow{\iota} V$$
$$V \xleftarrow{\iota} V$$

So the S-B trick invites us to find X such that $V \setminus X = \iota``(V \setminus \iota``X)$, and look at the permutation $\iota \restriction X \cup \iota^{-1} \restriction (V \setminus X)$. As usual, it seems to lift one type but not two. Now even this much is almost certainly not possible in a term model (exercise: prove that no set abstract t satisfies $t = V \setminus \iota``(V \setminus \iota``t))$) so perhaps in term models every setlike permutation is a set ...

Conjecture: if we have permutation σ of V so that $j`\sigma$, $j^2`\sigma$ and $j^3`\sigma$ are all permutations of V, then σ is setlike. Outer automorphims of V are setlike. I do not know how to prove the existence of setlike permutations of V that are not sets, so consider the ML axiom: every setlike permutation of V is a set. Is there are nice way of restricting this to a first-order version?

language with a 1-place predicate A(x)) $\langle V, A\rangle \models \phi \to \exists j, crit`j = \kappa$, into inner model M such that $\langle M, j(A)\rangle \models \phi$ (n.b. ϕ could have parameters from V, i'm asserting that these get into M) it's easy to check that κ is Π_1 strong in $\Lambda \longleftrightarrow \forall\lambda\exists j : V \to$inner model M such that $V_\lambda \subseteq M$. The point of all this is that the Π_n strong hierarchy fit nicely into the large cardinals (between hypermeasures and woodins) and have a goodish inner model theory (at least Π_1 does..

another message

Let ϕ be Σ_1, with free variables among $x_1, \ldots x_n$.

a) κ is regular.

let $a_1, \ldots a_n \in H_\kappa$, and let $\phi(a_1, \ldots a_n)$ hold in V. By the reflection principle it holds in some V_λ where λ is chosen $>> \kappa$. By Skolemheim there is $M \prec V_\lambda$ such that $TC(a_1 \cup \ldots \cup a_n) \subseteq M$ and $|M| < \kappa$. Take the Mostowski collapse of M to N: $N \subseteq H_\kappa$, the collapses fix $a_1, \ldots a_n$, so N thinks $\phi(a_1 \ldots a_n)$. but now M does too by upwards absoluteness. does this sound plausible?

In fact why doesn't this work for singular κ as well? Answer beco's for singular κ it's not enough to be hereditarily card less than κ to be in $H_\kappa =_{\text{def}} \{x : |TC(x)| < \kappa\}$.

b) κ singular. ϕ and a's as before. As κ is limit, all the a's are in H_β for $\beta < \kappa$, β regular! H_β thinks ϕ holds so by upwards absoluteness H_κ does.

luv j.

(I asked him: is it true that every Π_2^{Levy} theorem of ZF is true in V_λ for λ limit)

let $\phi(x, y)$ say something like $y = x \cup \omega$. then your statement is false in V_ω. less trivial examples can be concocted.

This is the usual thing about Σ_0 functions vs rud functions; the former can raise rank by an infinite amount, and the latter (by an easy induction) cannot [in the sense that, if F is rud, there is n finite such that $(\forall x)(rank(F(x)) \leq rank(x) + n)$.

there is a theorem of jensen saying that if $\phi(\vec{x})$ is Σ_0 then for some rud F $\phi(\vec{x}) \longleftrightarrow F(\vec{x}) = 0$.

Chapter 16

Stratification and Proof Theory

All a bit of a jumble(!)

16.1 Parameter-free-NF

This is the fragment of NF consisting of extensionality plus all axioms of the form $(\exists x)(\forall y)(y \in x \longleftrightarrow F(y))$ where 'F' is stratifiable and has no parameters and no occurrences of 'x' in it. Let's give it a name: NFpf.

It's presumably weaker than NF, but any term model for it is a term model for NF – which would mean that NF is consistent. This suggests two projects:

(i) show that NFpf is consistent, and
(ii) show that if it is consistent then it has a term model.

The first thing is to persuade the reader that this project is not as hopelessly optimistic as it might appear.

> Ad (i), if NFpf is not consistent one needs to ask what could an inconsistency proof look like?
>
> Ad (ii) one reflects that there are two weapons: the Omitting Types theorem and Herbrand's theorem. The absence of parameters makes both of the theorems more useful.

Can we show that if NFpf is consistent then it has a term model?

It has a term model as long as there is a $T \supseteq$ NFpf that locally omits the 1-type $\{x \neq t_i : i \in \mathbb{N}\}$. T will locally omit that type as long as, whenever T proves $(\forall x)(F(x) \to x \neq t_i)$ for all i, then T proves $(\forall x)\neg F(x)$.

So: if NFpf does *not* have a term model, then the following happens. Whenever $T \supseteq$ NFpf, there is an F such that T proves $(\forall x)(F(x) \to x \neq t_i)$ for all

i, but T does not prove $(\forall x)\neg F(x)$. One wants to somehow make use of the theorem (Herbrand?) that says that, if $T \vdash (\exists x)F(x)$, then T proves a finite disjunction of things of the form $F(t)$, where the ts are T-terms. I think we do it like this.... Ask "Why does NFpf *not* prove $(\forall x)\neg F(x)$?" One wants to say that it sure as hell can't be beco's it proves $(\exists x)F(x)$ beco's by Herbrand it would then have offer us a choice of finitely many terms to be F, and it tells us that no term can be F. One *wants* to say that, but then of course NFpf might be inconsistent! So the best we can hope for is that if it is consistent then it has a term model. So it looks as if, should NFpf prove $(\forall x)(F(x) \to x \neq t_i)$ for all i, then we can at least consistently add $(\forall x)\neg F(x)$ as an axiom.

One shouldn't expect too much from this, co's all we are using is that the only axioms NFpf has are extensionality and existence of closed set abstracts, and there is no reason for that to be consistent – one might assume the existence of the Russell class. True, but we make the assumption that the theory is consistent. So we would be proving that if T is a set theory, namely (extensionality plus) some axioms giving the existence of closed set abstracts then if it is consistent then it has a term model. Is that true?

OK, here is the challenge. Suppose T is a consistent set theory whose axioms are extensionality plus a lot of axioms asserting the existence of objects $\{x : \phi(x)\}$ where ϕ has no parameters. Must T have a model wherein everything is the denotation of one of the terms $\{x : \phi(x)\}$?

Why might one expect anything like this to be true? A: Combination of Omitting Types and Herbrand's theorem. What conditions do we need to put on T to obtain such a result? One sensible thing would be for the set of T's ϕs to be closed under boolean operations.

Is it finitely axiomatisable? Presumably not unless it is inconsistent. Observe that if it has any models at all then it has only infinite models. (It proves the existence of every concrete Zermelo natural and proves that they are all distinct.) Does it prove the axiom of infinity?

If NFpf has a term model then NF is consistent: any term model for NFpf is a term model for NF.

Consider theories of the form: Extensionality + axioms saying that certain (closed, parameter-free) set abstracts exist. Some of these theories are consistent, some are inconsistent. As things stand, I know of no inconsistent theory of this kind whose inconsistency needs extensionality. Further I don't think excluded middle has any rôle to play in the paradoxes of naïve set theory. So I float the conjecture:

> *If T is a constructive theory whose nonlogical axioms are all assertions that certain (closed, parameter-free) set abstracts exist, then Con(T) $\to$ Con(T + Ext + Excluded middle)*

Getting rid of extensionality would be good, because the rules for extensionality are cut-absorbing. If we know that any inconsistency in a finite fragment of NFpf has a genuine cut-free proof we would surely be able to do something with the stratification.

[nov 2014: Michael Rathjen tells me that there are models of constructive ZF in which the collection of regular sets is $\{\emptyset\}$]

Do we ever need extensionality to obtain a contradiction? Zachiri suggested the paradoxical collection $\{x : (\exists y)((\forall z)(z \in y \longleftrightarrow z \in x) \wedge y \notin x)\}$ but you don't need extensionality to obtain a contradiction. But something like that might work.

However we do sometimes need trivial axioms like subcision. See Forster-Libert ...

Does every finite fragment of NFpf have a model?

– Probably

Does every finite fragment of NFpf have a term model?

We have to be careful here. We can't expect that every finite fragment has a term model in which every term answers to a single set existence axiom *of that fragment*. Consider the theory that is extensionality + existence of the von Neumann ordinal 2. This has a term model, but the model contains $\emptyset$, $\{\emptyset\}$ and $\{\emptyset, \{\emptyset\}\}$. So we mean that the fragment should have a model consisting of the denotation of closed terms possibly additional to those mentioned in the axioms.

Once that is cleared up the answer is: quite possibly, but it isn't much use, beco's there is no obvious way to stitch them together. Here's why. Let $\mathcal{X}$ be the set of closed set abstracts. The finite fragments of NFpf are indexed by $\mathcal{P}_{\aleph_0}(\mathcal{X})$. Consider the collection of $\subseteq$-closed subsets of $\mathcal{P}_{\aleph_0}(\mathcal{X})$. This has the finite intersection property so we can find a nonprincipal ultrafilter $\mathcal{U}$ on $\mathcal{X}$ containing all these $\supseteq$-closed subsets. This gives us a model of NFpf without extensionality as follows. We rule that $s \in t$ holds iff the set of finite fragments that believe $s \in t$ belongs to $\mathcal{U}$. But now consider the empty set and the set of all total orders of V. Our model might believe these terms are distinct, because a large set of factors believe it. Each factor will have a witness to their distinctness. But there may be infinitely many witnesses, so that no one of them is believed by a large set to be a witness to their distinctness.

Is it plausible that there should be a finite fragment of NFpf that is consistent but has no finite models?

Observe that finite fragments with apparently quite sophisticated axioms can have trivial models. Consider the fragment that says that the Frege $\mathbb{N}$ exists. There might not be any set other than V that contains 0 and is closed under S – in which case $\mathbb{N}$ is the intersection over the empty set and is V! Thus a model consisting of a solitary Quine atom is a model of extensionality + the existence of the Frege $\mathbb{N}$. In fact such a trivial model is a model of that fragment of NFpf that asserts the existence of any set defined as the $\subseteq$-minimal set containing this and closed under that – as long as the set abstract is stratified of course.

Is the set of axioms of NFpf closed under conjunction? Is the fragment that asserts the existence of terms $t_1 \ldots t_n$ is implied by the single axiom asserting the existence of $\{t_1 \dots t_n\}$? (Tho' the converse implication clearly does not hold). Probably not: as Randall says, the axiom asserting the existence of the unordered pair of the empty set and the Russell Class succeeds in asserting the existence only of the empty set.

In 'The Quantifier Complexity of NF', Bulletin of the Belgian Mathematical Society Simon Stevin, ISSN 1370-1444, 3 (1996), pp 301-312. Kaye shows that

NF = NFpf + NF0 + existence of sumset (*)

(This is theorem 2.3.) ("which seems to me to be saying that if parameterless NF is consistent it is a very strange theory and actually not very nice (there is something bad in the model!" says Kaye; he continues "Talking of bad, I don't appear to need the B axiom of NF0 to prove the result (*), so something very natural and ZF-like goes wrong in any possible model of parameterless NF.")

If Kaye is correct (and he usually is) so that NF = NFpf + NF0 + existence of sumset, then the RHS must be finitely axiomatisable. So

$$\text{NFpf} \vdash (\text{NF0} + \text{existence of sumset }) \to \text{NF}.$$

But then this must follow from finitely many axioms of NFpf. But that doesn't make NFpf finitely axiomatisable. Actually that's not scary at all.

Consider the set of those theorems of ZF(C) that are of the form $(\exists x)(\forall y)(y \in x \longleftrightarrow \phi(y))$ where $FV(\phi) = \{\text{'}y\text{'}\}$.

This is a recursively axiomatisable theory. Let's call it T. My guess is that Con (NF) $\to$ Con(NF $\cup$ T). Does the axiom of choice make any difference?

16.1.1 A cunning plan to prove Con(*i*NF)

Just had a bright idea. Extensionality is what makes life difficult, buggers up the proof theory. SF is NF without extensionality, and it's obviously consistent. Does it have a term model? *The point is that a term model for SF is perforce a model of NF: you get extensionality free!* Indeed a term model for the *parameter-free* version of SF is a model for NF! Another thing that makes proof theory more tractable is restricting ourselves to a constructive logic. Restricting ourselves thus means that we are looking for a consistency proof for *i*NF rather than NF, and that may well be a weaker result so we might be able to get something without strong assumptions. So let us consider the constructive parameter-free fragment of SF, which we will call *i*SF^{pf}. It will have very nice proof theory. In fact i'm banking on it having cut-elimination, by work of Crabbé. Can we use this nice proof theory to show that it has a term model?

So *i*SF^{pf} is a theory in the language of set theory enriched with term-forming apparatus: '$\{x : \phi\}$' is a term whenever $\phi(x)$ is stratified and has no free variables other than 'x'. It has the usual rules for constructive first-order predicate logic, plus $\in$-int and $\in$-elim for stratified parameter-free expressions:

$$\frac{\phi(x)}{x \in \{y : \phi(y)\}} \qquad \frac{x \in \{y : \phi(y)\}}{\phi(x)}$$

where ϕ is stratified and has no parameters. Actually i have rather got into the habit of using a sequent presentation, so i will use instead the two rules of $\in$-L and $\in$-R (wot i got from Marcel's slides):

$$\frac{\Gamma\phi(x) \vdash \Delta}{\Gamma, x \in \{y : \phi(y)\} \vdash \Delta} \quad \frac{\Gamma \vdash \phi(x)}{\Gamma \vdash x \in \{y : \phi(y)\}}$$

We will need to establish that *i*SF^{pf}has the existence property. If we haven't got that we are completely stymied. Cut-elimination, and so on.

The plan is to show that *i*SF^{pf}has a term model, and that that term model is a model of *i*NF. If we then think of that term model as a Kripke model it appears that the carrier set of each component world in the Kripke model will simply be the set of closed stratified set abstracts, so we will presumably get not just a model of *i*SF^{pf}but of *i*SF^{pf}+ CD, the constant domain axiom. Each world will correspond to an extension of *i*SF^{pf}, and it will believe an atomic $s = t$ or $s \in t$ iff that is a theorem of the corresponding theory. We hope that the theories corresponding to worlds in this way all have the disjunction property!

The way to do this is to show that *i*SF^{pf}can be extended to a theory that locally omits the 1$-$type that says that x is not the denotation of a closed set abstract. This will involve getting straight quite what form omitting types takes in a constructive context.

Let ϕ be some property such that *i*SF^{pf}proves that $\phi(x) \rightarrow$ "x is not the denotation of a closed stratified set abstract". That is to say, for each stratified set abstract t, *i*SF$^{pf}\vdash (\forall x)(\phi(x) \rightarrow x \neq t)$. (Let us say such a ϕ is *bad*).

It will be quite useful in what follows that every bad ϕ is equivalent to something unstratified that is bad: $\phi(x) \longleftrightarrow (\phi(x) \wedge \neg\neg(x \in x \vee \neg(x \in x)))$

Notice that, for each theory T, the set of one-place predicates that T believes to be bad is closed under disjunction. We want to be able to consistently add $\neg\exists x\phi(x)$ for all bad ϕ. These are the *badness avoidance axioms*. The fact that bad predicates are closed under disjunction means that to prove the simultaneous consistency of all the badness avoidance axioms for T it is enuff to prove the consistency of each one.

That means that we have to establish that *i*SF$^{pf}\not\vdash \neg\neg\exists x\phi(x)$.

The way to do this is by reasoning about proofs in *i*SF^{pf}.

Let's start with the – presumably easier – claim that *i*SF$^{pf}\not\vdash \exists x\phi(x)$. It's a rehearsal, and if it doesn't work then we know not to try to establish *i*SF$^{pf}\not\vdash$ $\neg\neg\exists x\phi(x)$.

This would be straightforward were it not for the availability of the $\in$-rules, and it is indeed straightforward for any unstratified ϕ. For stratified ϕ the problem is that any stratified expression with a free variable in it will match the output of $\in$-elim. But we can at least bank the fact that if ϕ is unstratified or has no embedded terms then $\exists x\phi(x)$ doesn't match the output of $\in$-elim, and can only have come from $\exists$-R – which is of course impossible. Fortunately every bad predicate is equivalent to an unstratified bad predicate.

So we need to consider the cases where ϕ is stratified and has embedded terms.

We will need the concept of a *reduced* expression. This will be one wherein all occurences of '$x \in \{y : \theta(y)\}$' have been replaced by '$\theta(x)$'. This will ensure that no closed set abstract appears to the right of an '$\in$'. We must check that the process of rewriting that this appeals to is terminating and confluent. (Termination matters, confluence perhaps not so much) Without loss of generality we may assume all expressions are reduced.

Now consider the equivalence relation on (denotations of) stratified closed parameter-free set abstracts of belonging to the same things. This supports a rule of substitution. Suppose s and t are (denotations of) stratified closed parameter-free set abstracts that belong to the same things. Consider $\phi(s)$. Put it into reduced form. Now all occurrences of s appear only to the left of $\in$. But by hypothesis, s and t belong to the some things, so we can replace 's' by 't' *passim*. In particular two (denotations of) stratified closed parameter-free set abstracts that belong to the same things have the same members. One can also prove this by considering the B function.

Notice that in term models this relation of belonging-to-the-same-things is the same as the relation of having-the-same-stratified-parameter-free-properties.

But we want extensionality. The idea is to use Boffa's (Coret's ...?) lemma on permutations to show that if s and t are n-equivalent then they satisfy the same n-formulæ. So suppose s and t have the same members but do not belong to the same things. Then there is a term u that contains one but not the other. So there is some stratified property U that holds of one but not the other. But this contradicts the fact that they are n-equivalent. Unfortunately Boffa/Coret's lemma relies on extensionality!

Turn this into a proof!

We want $\exists x \phi(x)$ to NOT be the result of a $\in$-elim. But suppose it is. Then $\exists x \phi(x)$ can be expanded to $\exists x \psi(x, t)$ for some term t (where ψ is reduced – and stratified!) and this was obtained by $\in$-elim from $t \in \{y : \exists x \psi(x, y)\}$ where ψ is stratified and the eigenvariable 'y' of the abstraction never appears to the right of an '$\in$'. Now surely we can do something with *that!*

One can ask some awkward questions about how we managed to obtain a sequent proof of $\vdash t \in \{y : \exists x \psi(x, y)\}$ whose last line wasn't a $\in$-int. Yes, we could perhaps have obtained it from a $\in$-elim but that uses up another term so we can only do it so often.

'$s \in t$' can always be the output of a occurrence of $\in$-elim: it can come from $t \in B(s)$. Come to think of it it can also be obtained by $\in$-elim from $s \in \{x : x \in t\}$. But of course we want to formulate $\in$-int in such a way that we don't add terms like '$\{x : x \in t\}$'. Extensionality raises its ugly head.

For any expression in this sexed-up language – stratified or not – we consider the "occurs within" relation on (occurrences of) terms inside ϕ. This relation is obviously wellfounded. We expand all terms to the R of an $\in$ successively by recursion on this wellfounded relation. This kills off all occurrences to the R of an $\in$. Occurrences to the L of an $\in$ we can do nothing about.

edit below here

We can certainly exclude it if our bad formula $\phi(x)$ is of the form $\neg\psi(x)$. Suppose $\neg\psi(x)$ is bad, but $i\mathrm{SF}^{pf} \vdash \neg\neg\exists x \neg\psi(x)$. Then $i\mathrm{SF}^{pf} \vdash \neg\forall x \psi(x)$. So consider the sequent

$$\vdash \neg\forall x\psi(x)$$

– which is provable. It must've come by means of a $\neg$-L from

$$\forall x\psi(x) \vdash$$

which must've come by means of a $\forall$-L from

$$\psi(t_1), \cdots \psi(t_n) \vdash$$

But each of $\neg\neg\psi(t_i)$ is provable beco's $\neg\psi$ is bad, so a sequence of cuts will give us a contradiction. But *i*SF^{pf} is consistent.

But there are plenty of other forms that a bad $\phi(x)$ might take, so there is still work for us to do.

A proof of the sequent

$$\vdash \neg\neg\exists x\phi(x)$$

can only have come from the sequent

$$\neg\exists x\phi(x) \vdash \neg\neg\exists x\phi(x)$$

and that must have come from the sequent

$$\neg\exists x\phi(x) \vdash \exists x\phi(x).$$

That must have come from an $\exists$-R. So there is some term t such that

$$\neg\exists x\phi(x) \vdash \phi(t).$$

But there is a proof of the sequent $\phi(t) \vdash$, so how might there be a proof of the sequent $\neg\exists x\phi(x) \vdash \phi(t)$? We *could* obtain it by weakening-R from $\neg\exists x\phi(x) \vdash$, but that takes us back where we started, so there must be another way. We can only have got it from the rule on the R for the principal connective of $\phi(t)$. OK, suppose we have such a sequent proof, call it $\mathcal{D}$. Consider the first line of $\mathcal{D}$ at which we find the occurrence of $\neg\exists x\phi(x)$. If it was put there by weakening-L we can simply delete it, and obtain thereby a proof of $\vdash \phi(t)$. There is no such proof, so it wasn't put there by weakening-L. So it was put there by $\neg$-L. It means we have have worked backwards to a sequent $\Gamma \vdash \exists x\phi(x)$, (which we whack with $\neg$-L to get $\Gamma, \neg\exists x\phi(x) \vdash$). Now: whence came $\Gamma \vdash \exists x\phi(x)$? It came eventually from a sequent $\Gamma' \vdash \phi(s)$ by $\exists$-R, for some set abstract s. But we know $\phi(s) \vdash$ beco's ϕ is bad, so Γ' is inconsistent. But to get this far, we must have somehow moved all of $\phi(t)$ over to the LHS, so it's sort-of negative.

So that ought to mean that we can consistently add an axiom $\neg\exists x\phi(x)$.

probably ignore from here down to ■

But when we add axioms that arise in this way ("badness-avoidance axioms") might we not find that new predicates become bad? We might indeed, but in a sense nothing happens. Let A be any closed formula, and suppose

$i\mathrm{SF}^{pf}\cup\{A\} \vdash \phi(x) \to x$ is not the denotation of a closed stratified set abstract

So, for each closed term t, $i\mathrm{SF}^{pf} \cup \{A\} \vdash \neg\phi(t)$ and this is (even constructively) the same as

$i\mathrm{SF}^{pf} \vdash A \to \neg\phi(t)$

and this is constructively equivalent to

$i\mathrm{SF}^{pf} \vdash \neg(A \wedge \phi(t))$

which is as much as to say that $A \wedge \phi$ is bad.

Now consider the badness-avoidance axiom in a setting where A is consistent (secretly a badness avoidance axiom). We want $\neg\exists x(A \wedge \phi(x))$ to be consistent. But this is just another badness avoidance axiom and so is consistent as before.

Evidently if ϕ and ψ are both bad, so is $\phi \vee \psi$. So the set of badness avoidance axioms is closed under conjunction. Any individual badness avoidance axiom is consistent so by compactness the set of all of them is consistent. So: add them all, getting a theory which we can call $i\mathrm{SF}^{pf\infty}$.

What we now have to check is that $i\mathrm{SF}^{pf\infty}$ "locally omits" (I am using scare quotes beco's i am not sure about omitting types in a constructive context) the 1-type that says there is a thing that is not the denotation of a closed set abstract. This might not happen, beco's the process is not ω-continuous: we might find that a predicate becomes bad at this ωth stage that didn't become bad at any finite stage.

■

But if we can extend $i\mathrm{SF}^{pf}$ to a theory $i\mathrm{SF}^{pf\infty}$ that sort-of locally omits the type that sez that x is not the denotation of a closed stratified set abstract, it suggests that there should be an actual model in which every element really is the denotation of a closed stratified set abstract, and we should enquire about what this model looks like. The obvious thought is that it is a Kripke structure all of whose component worlds have as members precisely the closed stratified set abstracts, and the worlds are indexed by the theories we obtain by progressively adding the badness avoidance axioms. Any model obtained in this way would satisfy the *constant domain axiom* CD.

Let's think a bit about this model. *Prima facie* there is a problem at each world with atomic sentences. The atomic sentences believed by W are the expressions $s = t$ and $s \in t$ proved by W's tutelary theory T_W. What happens if T_W proves a disjunction $s \in t \vee s' \in t'$ but doesn't prove either disjunct? This doesn't happen in the root world beco's $i\mathrm{SF}^{pf}$ has the disjunction property. Later worlds are safe too, beco's each one is a finite extension of $i\mathrm{SF}^{pf}$ obtained by adding things of the form $\neg\exists x\phi(x)$. Granted, $A \to (B \vee C) \vdash (A \to B) \vee (A \to C)$ is not constructively correct. (A could be $B \vee C$ after all!) However if we have a proof of $\neg A \to (B \vee C)$ then we must have either a proof of $A \to B$ or of $A \to C$. This is beco's the last rule in the proof of $\neg A \vdash B \vee C$ can only be a $\vee$-R.

So far so good!

So we characterise a recursively defined family of theories. We start with $i\mathrm{SF}^{pf}$. If we have a theory T, and ϕ is such that $T \vdash \neg\phi(t)$ for every term t, then we add to our collection the theory $T \cup \{\neg\exists x\phi(x)\}$. I think we want the family of theories obtained in this way to be a directed poset under $\subseteq$. We want all these theories to be consistent and have the disjunction property. To each of these theories T there corresponds a possible world T_W such that $T_W \models s = t$ iff $T \vdash s = t$ and $T_W \models s \in t$ iff $T \vdash s \in t$.

Must check that this model believes extensionality!!

I've made some progress while up at the farm.

First thing to note is that there are infinitely many contradictory stratified formulæ with one free variable so infinitely many closed set abstracts that denote empty sets. So a term model of NFU makes perfect sense.

OK, suppose we have a term model of $i\mathrm{SF}^{pf}$. Consider the binary relation of satisfying the same one-place stratified formulæ. This implies the relation of belonging to the same things, and that implies the relation of belonging to the same things and having the same members. That relation in turn supports being swapped by an automorphism and that supports a rule of substitution! And that of course implies satisfying the same stratified one-place relations. So they are all equivalent. Now we have to establish that it is implied by having-the-same-members. Extensionality is a problem after all!

No, scrub all that, or most of it.

Proving inequations in *i*SF

Randall sez: prove $x \neq y$ by exhibiting a set that contains one but not the other. I say: things might be unequal while having the same stratified properties. He sez, this is not a problem beco's consider. Suppose we have concluded that $x \neq y$ beco's $x \in x$ and $y \notin y$. Then we do a case split: either

1. $x \in y$ in which case we conclude $x \neq y$ beco's there is a thing – namely x – which belongs to one but not the other, so $B(x)$ contains one but not the other) or

2. $x \notin y$ in which case we conclude $x \neq y$ beco's there is a thing – namely y – which contains one but not the other.

Can we do this in general? Suppose $\phi(x) \wedge \neg\phi(y)$ where ϕ is unstratified. What can we do?

But this only works for weakly stratified formulæ– '$x \notin x$' is weakly stratified. Can we find a trick that works in general.

16.1.2 Specker's refutation of AC: does it need parameters?

`https://us.metamath.org/nfeuni/nchoice.html` provides a proof, and cites the following axioms:

`ax-1`	5,
`ax-2`	7,
`ax-mp`	8,
`ax-meredith`	1406
`ax-gen`	1546,
`ax-5`	1557,
`ax-17`	1616,
`ax-9`	1654,
`ax-8`	1675,
`ax-13`	1712,
`ax-14`	1714,
`ax-6`	1729,
`ax-7`	1734,
`ax-11`	1746,
`ax-12`	1925,
`ax-ext`	2334,
`ax-nin`	4078,
`ax-xp`	4079,
`ax-cnv`	4080,
`ax-1c`	4081,
`ax-sset`	4082,
`ax-si`	4083,
`ax-ins2`	4084,
`ax-ins3`	4085,
`ax-typlower`	4086,
`ax-sn`	4087.

That's 27 axioms. I assume that the numbers in the second column are line numbers.

Everything in the list before `ax-ext` is a logical axiom.

`ax-ext` is the axiom of extensionality.

`ax-nin` is the axiom that give $\overline{x \cap y}$. It is of course not an axiom of NFpf

`ax-xp` gives cartesian products; it is not an axiom of NFpf;

`ax-cnv` gives converses of relation; it is not an axiom of NFpf;

`ax-1c` gives the set of all singletons; it is an axiom of NFpf;

`ax-sset` gives the graph of $\subseteq$; it is an axiom of NFpf;

`ax-si` is Hailperin's axiom P2; it is not an axiom of NFpf;

`ax-ins2` is Hailperin's axiom P3; it is not an axiom of NFpf;

`ax-ins3` is Hailperin's axiom P4; it is not an axiom of NFpf;

`ax-typlower` is Hailperin's axiom P6; it is not an axiom of NFpf;

`ax-sn` asserts the existence of a singleton; it is an axiom of NFpf.

The axioms we need to worry about are flagged by red

16.2 Stratification and Proof Theory

Le 3 avr. 2020 à 00:41, Thomas Forster <tf@dpmms.cam.ac.uk> a écrit :

Marcel (cc Randall and Beeson)

I am thinking about the strongly typed fragment of the first-order language of Set theory, the language i call $\mathcal{L}(\mathrm{T}\mathbb{Z}\mathrm{T})$, types for every (positive and negative) integer. Consider the first-order logic that lives inside this language: no nonlogical axioms. It is known that this logic admits cut-elimination. (Is that Takeuti. . . ?) I am now asking about the result of adding a rule of inference of typical ambiguity to this logic. Two questions:

(i) Do we still have cut elimination for this logic?
(ii) What subformula property do we have for cut-free proofs? ϕ^+ is a subformula of ϕ?

I'm trying to reconstruct what Marcel was thinking in the 1990s!

16.2.1 Marcel writes

Dear Thomas,

Does this answer your question?

The sequent

$$\vdash (\forall x)((\forall y)(y \in x) \to (\exists z)(x \in z))$$

is provable in predicate calculus, but no typed version of it is provable in typed predicate calculus.

However its typed versions are provable in typed predicate calculus with an ambiguity rule (as the one of page 14 in `http://logoi.be/crabbe/textes/ambstrat.pdf`), but none of it is provable without cut.

Best wishes,

Marcel

However our notion of substitution is going to ensure that the class of weakly stratifiable formulæ is closed under substitution. Thus:

any stratifiable theorem has a cut-free proof in which every formula is weakly stratifiable.

Can we do better? No: if you want to drop the 'weakly' you have to drop the 'cut-free' too.

To prepare the ground for this, start with the rather nice formula i stumbled into.

$$(\forall x)[(\forall y)(y \in x \to ((\forall z)(z \in y) \to \bot)) \to ((\forall z)(z \in x) \to \bot)] \qquad (**)$$

(If none of your members is V then neither are you.) This formula has (of course) a cut-free proof wherein every formula is weakly stratifiable:

$$\dfrac{\dfrac{\dfrac{\dfrac{\dfrac{[(\forall z)(z \in x)]^1}{x \in x}\,\forall\text{ elim} \qquad \dfrac{[(\forall w)(w \in x \to ((\forall z)(z \in w) \to \bot)]^2}{x \in x \to ((\forall z)(z \in x) \to \bot)}\,\forall\text{ elim}}{(\forall z)(z \in x) \to \bot}\,\to\text{-elim} \qquad [(\forall z)(z \in x)]^1}{\bot}\,\to\text{-elim}}{(\forall z)(z \in x) \to \bot}\,\to\text{-int (1)}}{(\forall w)(w \in x \to ((\forall z)(z \in w) \to \bot)) \to ((\forall z)(z \in x) \to \bot)}\,\to\text{-int (2)}$$

Observe that this proof is constructive. And, altho' every formula within it is weakly stratifiable not all of them are stratifiable. Now we doctor it by introducing a maximal formula, so that all formulæ in it are stratifiable:

$$\dfrac{\dfrac{\dfrac{\dfrac{\dfrac{\dfrac{[(\forall y)(y \in x)]^3}{a \in x}\,\forall\text{ elim} \qquad \dfrac{[(\forall w)(w \in x \to ((\forall y)(y \in w) \to \bot)]^2}{a \in x \to ((\forall y)(y \in a) \to \bot)}\,\forall\text{ elim}}{(\forall y)(y \in a) \to \bot}\,\to\text{-elim} \qquad [(\forall y)(y \in a)]^1}{\bot}\,\to\text{-elim} \qquad \dfrac{[(\forall y)(y \in x)]^3}{(\exists w)(\forall y)(y \in w)}\,\exists\text{-int}}{\bot}\,\exists\text{-elim(1)}}{(\forall y)(y \in x) \to \bot}\,\to\text{-int (3)}}{(\forall w)(w \in x \to ((\forall z)(z \in w) \to \bot)) \to ((\forall z)(z \in x) \to \bot)}\,\to\text{-int (2)}$$

The maximal formula is $(\forall y)(y \in a)$, which is the premiss (flagged with a '1') of an $\exists$-elim, and simultaneously the conclusion of a $\exists$-int.

Actually it has got garbled

This second proof above is the result of some *ad hoc* manipulation by your humble correspondent, but he thinks he can see a general technique.... One needs to ask how the unstratifiable (but weakly stratifiable) formulæ got in there. They might have just been put in by brute force and one can't do anything about that. However the unstratifiable-but-weakly stratifiable formula we are trying to get rid of could have arisen from a $\forall$-elim (as it did in this case). $\forall$-elim can give us unstratifiable conclusions from stratifiable premisses, and none of the other rules can do this. So we simply specialise to a different variable, do a $\exists$-int and start a new branch ... which is exactly what we did above. I believe that this technique was known to Crabbé 30-odd years ago.

Observe that altho' this doctored proof (which, like its undoctored progenitor, is constructive) contains only stratifiable formulæ, it lacks a global stratification. Not only does it lack a global stratification but the one place where our attempted stratification fails is at the variable 'a'. We stratify the variables as follows:
'x' $\mapsto 2$;
'y' $\mapsto 1$;
'w' $\mapsto 2$;
'z' $\mapsto 1$;

but we want to send 'a' to both 1 and 2. And 'a' is of course the eigenvariable of the $\exists$-elim we inserted to make the proof stratified. It is the poxy proxy for the variable 'x' that caused the failure of stratification in the first place – in the original cut-free proof.

My guess is that stratifiable proofs obtained in this manner from weakly stratifiable (but not stratifiable) proofs of expressions like ** never have global stratifications. I believe that if one eliminates these maximal formulæ in the obvious way one gets back the original proofs. (Marcel says as much). So perhaps the conclusion is that they are not significantly different from the original cut-free (normal) proofs and the exercise largely lacks point. Certainly Marcel never got very excited about them.

Globally stratifiable proofs are important beco's the global stratification can be brutally tattooed onto the variables so that the proof becomes a proof in T$\mathbb{Z}$T.

We need to think about formula **, and the idea that there is no universal set, in a strongly typed context. If our variables have to have type subscripts (so we are in $\mathcal{L}$(T$\mathbb{Z}$T)) then of course ** cannot be proved – it's false in any model of T$\mathbb{Z}$T. This ties in with the fact that ** has no globally stratified proof.

I think:

- If ϕ has a globally stratifiable proof then it is a theorem of T$\mathbb{Z}$T and will have a cut-free globally stratifiable proof. (Because of a theorem of Takeuti about cut-free proofs in type theory)

- If ϕ has a cut-free proof in which every formula is stratifiable then that proof is globally stratifiable.

Beeson thinks he's proved the second bullet and i'm inclined to believe him.

Where do the axioms of typical ambiguity fit in?

I think that with the axioms of typical ambiguity we can give a globally stratifiable proof of (**) – which is to say, a proof in the first-order logic of $\mathcal{L}$(T$\mathbb{Z}$T), as follows.

Suppose no member of the level-$n+1$-set x is a universal set but that x itself is a universal set. So there is universal set of level $n+1$, and therefore by (downwards) ambiguity there is a universal set of level n. This set is a member of x (since x is a universal set) contradicting the assertion that no member of x is a universal set. The official proof object is displayed below.

I'm guessing that, generally, the ambiguity axiom works by changing the level of the conclusion of an $\exists$-int that gives the cut-formula, and thereby absorbs the cut. Is that what Marcel meant all those years ago by '*cut-absorbing operations*'? Is this the shape of things to come? Will it turn out that all stratifiable formulæ like (**) that have proofs wherein every formula is stratifiable but no globally stratifiable proofs will have globally stratified proofs using ambiguity axioms?

$$
\dfrac{
\dfrac{
\dfrac{
\dfrac{(\forall y_1)(y_1 \in x_2 \to ((\forall w_0)(w_0 \in y_1) \to \bot))}{x_1 \in x_2 \to ((\forall w_0)(w_0 \in x_1) \to \bot)}\ \forall\text{ elim}
\qquad
\dfrac{[(\forall w_1)(w_1 \in x_2]^1}{x_1 \in x_2}\ \forall\text{ elim}
}{[(\forall w_0)(w_0 \in x_1) \to \bot]^1}\ \to\text{-elim}
\qquad
[(\forall w_0)(w_0 \in x_1)]^2
}{\bot}\ \to\text{-elim}
\qquad
\dfrac{\dfrac{[(\forall w_1)(w_1 \in x_2)]^1}{(\exists x_2)(\forall w_1)(w_1 \in x_2)}\ \exists\text{-int}}{(\exists x_1)(\forall w_0)(w_0 \in x_1)}\ \text{Typical Ambiguity}
}{
\dfrac{\bot}{(\forall w_1)(w_1 \in x_2) \to \bot}\ \to\text{-int (1)}
}\ \exists\text{-elim(2)}
$$

And these 'cut-absorbing' things are bad beco's they don't respect the subformula property? Or rather, the notion of subformula that they impose is no use.

Thinking ahead ....

Consider the formula

$$(\forall x)[(\forall y)(y \in^2 x \to ((\forall z)(z \in y) \to \bot)) \to ((\forall z)(z \in x) \to \bot)] \qquad (***)$$

This presumably has exactly the same behaviour as (**). It's a stratifiable theorem of first order logic and has both a cut-free proof and also a cut-proof wherein every formula is stratifiable but which is not globally stratifiable. It also has a proof in $\mathcal{L}(\mathrm{T}\mathbb{Z}\mathrm{T})$ using the ambiguity rule.

Now what about

$$(\forall x)((\forall y \in x)\neg(\forall z)(z \in^2 y) \to \neg(\forall w)(w \in^2 x)) \to (\forall x)(\neg(\forall w)(w \in^2 x))$$

This is classically equivalent to:

$$(\forall x)((\forall y \in x)((\forall z)(z \in^2 x) \to (\forall w)(w \in^2 y)) \to (\forall x)(\neg(\forall w)(w \in^2 x))$$

Consider $V = \{a, b, c, d\}$ with $b \in a \in b \in b$; $d \in c \in y$. Then $(\forall y \in a)(\forall z)(z \in^2 y)$ but $\neg(\forall z)(z \in^2 a)$ Better check this!

$d \in c \in b \in b \in a \in c$ works too, i think.

Another formula of Marcel's:

$$(\forall x)((\forall w)(w \in x) \to (\exists y)(x \in y))$$

Here is a natural deduction proof

$$
\begin{prooftree}
\AxiomC{$[(\forall w_0)(w_0 \in x_1)]^{(2)}$}
\RightLabel{$\exists$-int}
\UnaryInfC{$(\exists x_1)(\forall w_0)(w_0 \in x_1)$}
\RightLabel{Typical Ambiguity}
\UnaryInfC{$(\exists x_2)(\forall w_1)(w_1 \in x_2)$}
\AxiomC{$[(\forall w_1)(w_1 \in x_2]^{(1)}$}
\RightLabel{$\forall$ elim}
\UnaryInfC{$x_1 \in x_2$}
\RightLabel{$\exists$-int}
\UnaryInfC{$(\exists x_2)(x_1 \in x_2)$}
\RightLabel{$\exists$-elim(1)}
\BinaryInfC{$(\exists x_2)(x_1 \in x_2)$}
\RightLabel{$\to$-int (2)}
\UnaryInfC{$(\forall w_0)(w_0 \in x_1) \to (\exists x_2)(x_1 \in x_2)$}
\RightLabel{$\forall$-int}
\UnaryInfC{$(\forall x_1)((\forall w_0)(w_0 \in x_1) \to (\exists x_2)(x_1 \in x_2))$}
\end{prooftree}
$$

This does give one to think about the models one obtains for falsifiable stratifiable formulæ by applying the 'build proofs backwards' strategy.

What is the notion of subformula needed so that the fragment of FOL in $\mathcal{L}(\mathrm{T}\mathbb{Z}\mathrm{T})$ plus ambiguity axioms enjoys the subformula property?

Does the $\mathcal{L}(\mathrm{T}\mathbb{Z}\mathrm{T})$ fragment of FLO plus upward ambiguity admit cut-elimination? Even without proving cut-elimination for such a system one can at least piggy-back on cut elimination for the $\mathcal{L}(\mathrm{T}\mathbb{Z}\mathrm{T})$ fragment of FLO. Suppose we have a proof of ϕ that uses some ambiguity axioms $\psi \to \psi^+$. Then there is a cut-free proof of the sequent $\psi \to \psi^* \vdash \phi$. The ambiguity axioms in such a proof all appear as major premisses of $\to$-eliminations (don't they? Can they get exploited in any other way…?) and it is simple enough to replace a derivation

$$\dfrac{\begin{matrix}\vdots \\ \psi\end{matrix} \quad \psi \to \psi^+}{\psi^+}$$

with a derivation using the ambiguity rule.

Note, too, that in that proof we used ambiguity propagating as-it-were *downwards*. We could have done a proof using ambiguity that propagated upwards but we would have proved the contrapositive of the conditional and constructively that's different.

If we have classical logic then there is no difference between upward propagation and downward propagation. The same holds for TCT of course. If we don't have classical logic then the upward and downward schemes seem to be inequivalent. Each implies the other for negative formulæ.

One has the feeling that downward propagation should be stronger than upward.

Presumbly the two restrictions, of downward-propagating ambiguity and upward-propagating ambiguity to negative formulæ in the range of the negative interpretation are equivalent!

Presumably the $\mathcal{L}(\mathrm{T}\mathbb{Z}\mathrm{T})$ version of SF has cut-elimination. Let's think about this theory with upward and downward ambiguity. Is there a difference in strength?

16.3 Analogues for other syntactic disciplines

There are other syntactic disciplines we need to consider:

Stratification-mod-n and acyclicity. Try, for example,

$$(\forall x)((\forall y \in x)(\neg(y \in^2 y)) \to \neg(x \in^2 x)) \to (\forall x)(\neg(x \in^2 x))$$

This is stratifiable-mod-2 and has a cut-free globally-stratifiable-mod-2 proof. It is also a logical truth of the stratifiable-mod-2 version of $\mathcal{L}(\in, =)$.

So far soo good.

16.3.1 Acyclic Analogues

When acyclicity turned up as a genuinely useful idea many of us thought that the extra discipline imposed by acyclicity might make the proof theory easier. The above discussion is probably a good context for an airing of these possibilities. Acyclic comprehension. Presumably we have an exact analogue of weakly stratified called something like 'weakly acyclic'. We should get straight what the closure is of the class of acyclic formulæ under subformula, or rather: what is the closure of the set of acyclic formulæ under subformula. And we can rerun the above analysis with 'stratifiable' replaced *passim* by 'acyclic'.

A message to Randall, Nathan and Zuhair:

I'm thinking again about acyclicity. This is because i have dusted off my notes on stratification and cut-elimination; this is stuff Marcel Crabbé wrote about years ago, and i may now finally be approaching the level of understanding he had then.

It's the proof theory of NF that gives rise to the notion of weakly stratifiable formula, the point being that the subformula property for cut-free proofs alerts us to the fact that subformulæ of stratifiable formulæ are not always stratifiable. The class of weakly stratifiable formulæ is the smallest class containing all stratifiable formulæ that is closed under subformula. That's why, in a natural deduction formulation of NF, one has $\in$-introduction and elimination for *weakly* stratifiable formulæ not just stratifiable formulæ.

When acyclicity came up, some of us thought that the extra discipline imposed by the stronger condition (than stratifiability) might make the proof theory easier. I now think that those hopes were exaggerated, but it's still a good idea to sort that out. The first step is to think about the closure of the set of acyclic formulæ under the subformula relation. I'm guessing that this is precisely the set of weakly stratifiable formulæ. Does that sound correct?

Then one might expect to be able to prove – using Marcel's methods – an acyclic analogue of what Marcel proved (and i re-proved, as an exercise) namely that every stratifiable theorem has a cut-free proof wherein every formula is weakly stratifiable, and that from that proof one can obtain a proof-with-cut wherein every formula is stratifiable, and which gives us back our original proof when one eliminates the cuts. Thus i predict that:

"every acyclic theorem has a cut-free proof wherein every formula is weakly stratifiable, and that from that proof one can obtain a proof-with-cut wherein every formula is acyclic, and which gives us back our original proof when one eliminates the cuts."

Does that sound correct. . . ?

Actually i think Marcel did this years ago. . .

A Message from Marcel in 1993

"Thomas,

Let's be precise. Consider the sequent calculus for classical first order logic. Then, a cut free derivation of a weakly stratified sequent contains only weakly stratified sequents. This follows trivially from the subformula principle (every formula in a cut free derivation is a subformula of a formula in the final sequent) and the observation that a subformula of a weakly stratified formula is always weakly stratified: this is not true for stratified formulas, $x \in x \to (\forall y)(y \in x) \to \bot)$ is a weakly stratified and unstratified [sub]formula of $(\forall w)(w \in x \to (\forall y)(y \in w) \to \bot)$.

When I proved "jadis" that every stratified theorem of the predicate calculus has a stratified (but not necessarily normal!) proof, I proceeded as follows: first I took a cut free proof of the stratified sequent, this proof is weakly stratified but could be unstratified as in your example, then I gave a method to introduce suitable cuts in order to obtain a stratified proof. The resulting derivation has moreover the property that if you remove the cuts of it in Gentzen's way you get (almost) the original cut free derivation.

Now if you take natural deduction and/or intuitionistic logic you have the same results with normal instead of cut free (you can even drop the "(almost)").

The situation is similar in the logic with terms $\{x : A\}$. But here you have to be a little more careful to avoid triviality."

Marcel

Jan Ekman says there is no normal proof of the nonexistence of V

Let's try to get a normal derivation of $(\forall y)(y \in x \to \bot)$ from $(\forall w)(w \in x \to (\forall y)(y \in w \to \bot))$. The last line can only be the result of an introduction rule, and this is presumably the $\forall y$. So we have

$$\dfrac{\dfrac{\vdots}{y \in x \to \bot}}{(\forall y)(y \in x \to \bot)}$$

and the $y \in x \to \bot$ can only be an $\to$-introduction (How can i be sure?) so it must be

$$\dfrac{\dfrac{\dfrac{\vdots}{\bot}}{y \in x \to \bot}}{(\forall y)(y \in x \to \bot)}$$

and then, since there is no rule to introduce $\bot$, the preceding step must have been an elimination ...

16.4 leftovers

I asked: Is there any relation between lurking non-normalisability and the presence of contraction?

Thanks to Torkel, for the proofs.

I'd like to elaborate a bit on the role of contraction. I'll leave comments about the relation between natural deduction and sequent calculus for another time.

Let's assume a na¨ive comprehension scheme.

Let $\{x : \phi\}$ be a name such that $\forall y(y \in \{x : \phi\} \longleftrightarrow \phi(y))$

Let $a = \{x : x \in x \to A\}$ for any sentence A

1. $a \in a \longleftrightarrow (a \in a \to A)$ by comprehension

2. $a \in a \to (a \in a \to A)$ from 1
3. $a \in a \to A$ contraction on 2
4. $(a \in a \to A) \to a \in a$ from 1
5. $a \in a$ 3,4 modus ponens
6. A 3,5 modus ponens

The sentence A can be anything. We could, like Fitch is supposed to have urged, give up modus ponens. But if we want na¨ive comprehension, I think it better to give up contraction rather than modus ponens. In terms of na¨ive plausibility, modus ponens is surely more na¨ively plausible than is contraction.

Note: Löb's "paradoxical" tautology is $(B \longleftrightarrow (B \to A)) \longleftrightarrow (B \wedge A)$

Now consider the usual formulation of the Russell paradox, which involves negation. We have $A \longleftrightarrow \neg A$ and derive A and $\neg A$.

1. $A \longleftrightarrow \neg A$
2. $A \to \neg A$ from 1
3. $(A \to \neg) \to \neg A$ minimal negation
4. $\neg A$ 2,3 modus ponens
5. $\neg A \to A$ from 1
6. A 4,5 modus ponens

Minimal negation looks to be the weakest assumption available to derive the contradiction.

Is contraction at work here?

I noticed as a result of this thread that $(B \longleftrightarrow (B \to A)) \longleftrightarrow (B \wedge A)$ is odd in some way, and now Graham says this is Löb's 'paradoxical tautology'. What did he say about it?

Thomas

```
From phil-logic@bucknell.edu Fri Apr  4 00:30:12 1997
From: g.solomon@phil.canterbury.ac.nz (Graham Solomon)
```

Let me re-write the tautology as $(A \longleftrightarrow (A \to B)) \longleftrightarrow (A \wedge B)$ so it's more easily comparable with $(A \longleftrightarrow \neg A) \longleftrightarrow (A \wedge \neg A)$ which this thread started with. The former is connected to Curry's paradox and the (better known?) related Lob's theorem. Lob didn't say anything specifically about the tautology (at least not that I recall), but keeping the tautology in mind can help one see that there's no real trickery going on. At this level of analysis, the former is a negation-free variant of the latter. I think it's interesting (but not surprising) that contraction shows up explicitly in the negation-free "paradoxes".

Re: Neil's comments, which I won't quote

1. You can write contraction as a tautology, though I like to use it as the rule “from $A \to (A \to B)$ infer $A \to B$”. In so far as we are concerned with the consistency of na¨ive comprehension and contraction, we’d probably like to look at a generalization which reduces $n+1$ As to n As, as well as which applies to any arrow-like connective. Greg Restall discusses this in print somewhere.

2. I suspect if you think about it carefully you’ll realize that your suggested “$a \in a \to (a \in (a \to A))$” is not well-formed.

Torkel replied:

¿ Well, as I usually understand minimal logic, $\neg A$ is short for $A \to \bot$, which ¿makes the validity of $(A \to \neg A) \to \neg A$ a special case of the validity ¿of contraction (in your sense). How would you explain minimal negation?

I hope we aren’t talking at cross-purposes. I had in mind an axiomatization of minimal logic using negation rather than F.

At any rate, it’s helpful for me to think of $(A \to \neg A) \to \neg A$ as a special case of contraction. Then, is it alright with you to claim that contraction does indeed play a significant role in both the derivation of B from $A \longleftrightarrow (A \to B)$ and of $A \wedge \neg A$ from $A \longleftrightarrow \neg A$, in the usual axiomatics? The use of $\neg$ in the latter just buries contraction a bit.

My speculation about the normalisation stuff is that the puzzle shows up because of contraction (which shows up whenever there’s multiple use of the same assumption)*, and that sequent calculus handles contraction better than does natural deduction. But “handles” has to be given some content.

* Like Thomas I’ve been wondering if it isn’t “always the case that where something doesn’t normalise there must be a premiss that is introduced twice? And doesn’t this mean that contraction is used somehow?”

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From phil-logic@bucknell.edu Wed Apr  9 09:03:21 1997
From: Torkel Franzen <torkel@sm.luth.se>
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Graham says:

¿I suppose this should really be under the subject heading: Curry sequents ¿and contraction. But here goes. Following is a sequent calculus proof of ¿ $p \to (p \to q), (p \to q) \to p \vdash q$ ¿written up using Gentzen’s original rules (for sequents regarded as lists ¿rather than sets). I hope it doesn’t break up in transmission (and survives ¿close scrutiny!).

The proof as written can’t be quite what you are after. Look at the first 9 lines:

1. $p \vdash p$ Axiom
2. $q \vdash q$ Axiom
3. $p \to q, p \vdash q$ 1, thinning left
4. $p, q \vdash q$ 2, thinning left
5. $q, p \vdash q$ 4, interchange left

6. $p \to q, p \to q, p, p \vdash q$ 3,5, $\to$-left
7. $p \to q, p, p \vdash q$ 6, contraction left
8. $p, p, p \to q \vdash q$ 7, interchange left (twice)
9. $p, p \to q \vdash q$ 8, contraction left

Line 3 is not obtainable from line 1 by thinning.
A correct proof of $p, p \to q \vdash q$ would be

1. $p \vdash p$ Axiom
2. $q \vdash q$ Axiom
3. $p \to q, p \vdash q$ 1,2 $\to$ left
4. $p, p \to q \vdash q$ 3, interchange left

`From phil-logic@bucknell.edu Fri Apr 11 00:54:43 1997`
`From: g.solomon@phil.canterbury.ac.nz (Graham Solomon)`

Charlie:
> I am completely lost about what is going on here. Is it at all relevant
>to the discussion for me to observe that on the lines below, all sentences
>to the left of the turnstiles can be true and the one to the right false?

Here's a quick sketch.

Let W, X, Y, Z, be finite, possibly empty, sequences of formulas. Let A, B, be arbitrary formulas.

The sequent $X \vdash Y$ informally reads: if all formulas in X are true then at least one formula in Y is true; or, for $X \vdash A$: there's a natural deduction proof of A from X.

Tree proofs are basically inverted sequent proofs. So formulas on the left of $\vdash$ map to formulas signed with T and formulas on the right map to formulas signed with F, when moving from sequent-style to tree-style. In classical logic by trees the Ts and Fs are eliminable, but seem to be essential for nonclassical logics.

For many logics you can regard X, Y, etc as sets. But doing so will automatically give you various structural rules you might want to reject. So I think it's better to make them explicit. But, like Torkel notes in a recent message, for some kinds of investigations you don't need this degree of explicitness.

Algebraists will recognize the groupoid aspects of sequent systems.

We start derivations wth axioms of the form $A \vdash A$

Structural rules:
from $X \vdash Y$ infer $A, X \vdash Y$ thinning left
from $X \vdash Y$ infer $X \vdash Y, A$ thinning right
from $A, A, X \vdash Y$ infer $A, X \vdash Y$ contraction left
from $X \vdash Y, A, A$ infer $X \vdash Y, A$ contraction right

from $W, A, B, X \vdash Y$ infer $W, B, A, X \vdash Y$ interchange left
from $X \vdash Y, A, B, Z$ infer $X \vdash Y, B, A, Z$ interchange right
from $X \vdash W, A$ and $A, Z \vdash Y$ infer $X, Z \vdash W, Y$ cut
Operational rules:
from $X \vdash W, A$ and $B, Z \vdash Y$ infer $A \to B, X, Z \vdash W, Y$ $\to$ left
from $A, X \vdash Y, B$ infer $X \vdash Y, A \to B$ $\to$ right

I'll skip the other rules. You can distinguish intuitionistic logic from classical by the number of formulas allowed on the right of $\vdash$ (I'll let you figure it out yourself).

Here's a proof for $p \to (p \to q) \vdash p \to q$

1. $p \vdash p$ Axiom
2. $q \vdash q$ Axiom
3. $p \to q, p \vdash q$ 1,2, $\to$ left
4. $p \to (p \to q), p, p \vdash q$ 1,3, $\to$ left
5. $p, p, p \to (p \to q) \vdash q$ 4, interchange left (twice)
6. $p, p \to (p \to q) \vdash q$ 5, contraction left
7. $p \to (p \to q) \vdash p \to q$ 6, $\to$ right

which shows how contraction as a structural rule underlies the natural deduction rule. One more step gives us

8. $\vdash [p \to (p \to q)] \to (p \to q)$ 7, $\to$ right

Okay, (given that I've typed everything in properly!), what does this tell us about the normalization business? I'm not at all sure. I've been doing this exercise in order to figure out where contraction shows up in the sequent system proofs of the paradoxical sentences. It seems to me that sometimes natural deduction doesn't handle multiple uses of one premise well. But I want to think about Peter Milne's remark about choice of rules and also chew over Tennant's article.

"To seek knowledge one must prefer uncertainty" – the first Bayesian koan.

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From phil-logic@bucknell.edu Fri Apr 11 09:39:17 1997
From: Torkel Franzen <torkel@sm.luth.se>
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Graham says:

For many logics you can regard X, Y, etc as sets. But doing so will automatically give you various structural rules you might want to reject. So I think it's better to make them explicit. But, like Torkel notes in a recent message, for some kinds of investigations you don't need this degree of explicitness.

Although it isn't at all relevant to the question about the proof of $\neg(A \longleftrightarrow \neg A)$, I would like to add that the degree of explicitness embodied in the rule I mentioned, i.e.

$[A \to B], \Gamma \vdash AB, \Gamma \vdash C$

– – – – – – – – – – – –
$A \to B, \Gamma \vdash C$
lies in between treating Γ etc as sets and the full use of structural rules. B, Γ is not a set in the rule above, but a multi-set. We can only use a formula on the left of a sequent as many times, *in any one branch of the proof*, as it has occurrences. In classical propositional logic, we we need never use any formula more than once in any one branch. In intuitionistic logic, reuse of $A \to B$ n times is sometimes necessary.

`From phil-logic@bucknell.edu Sat Apr 12 01:56:54 1997`
`From: g.solomon@phil.canterbury.ac.nz (Graham Solomon)`

A small remark about Lemmon's natural deduction system. Peter Milne gave it as an example of a system with a case where an assumption is used only once but the proof can't be normalized.

Lemmon's system doesn't allow us to infer directly from B to $A \to B$. We need instead to do something along the following lines: assume A and B and do $\wedge$-introduction, then eliminate for B, and on that basis infer $A \to B$. The assumption A is used only once but the proof isn't normalizable. Let's look at the sequent calculus proof (with the original Gentzen rules).

1. $B \vdash B$ Axiom
2. $A, B \vdash B$ 1, Thinning left
3. $B \vdash A \to B$ 2, $\to$ left

Not much to it. Contraction isn't needed, so it isn't the case that non-normalisability must have something to do with contraction. So what's Lemmon doing? He must be admitting non-normalisable proofs instead of using thinning as a structural rule. Indeed, John Slaney, in his reconstruction of Lemmon's system as a sequent system, explicitly draws the connection between non-normalisability and the absence of thinning as a primitive rule ("A General Logic" AJP 68 (1990):74-88).

`From phil-logic@bucknell.edu Mon Apr 21 14:21:43 1997`
`From: IrvAnellis@aol.com`

In 1985, Alexander Abian proposed the following expression:
(1) for all x, A is an element of x iff x is not an element of x
and the equivalent expression:
(2) for all x, A is not an element of x iff x is an element of x
By unrestricted universal instantiation, we get
(1') A is an element of A iff A is not an element of A
and
(2') A is not an element of A iff A is an element of A.
Looking at (1), we see that A can be neither a set nor a class because replacing x by the empty set in (1), we get

(1”) A is an element of the empty set iff the empty set is not an element of the empty set.

and of course “A is an element of the empty set” is always false – whether A is a class or a set – and “the empty set is not an element of the empty set” is of course always true, so that we have

(1”’) False iff True

which Abian regards as a paradox.

Whether (1) – or for that matter (1”’) – is a paradox or a simple contradiction will probably depend upon one’s outlook. G. E. Mints pointed out, however, that the so-called Abian paradox has the same structure as Curry’s paradox. For his part, Abian sees the expression as indicating that neither sets nor classes should be formulated in terms of arbitrary unrestricted properties, and that set theory requires some axioms for prescribing some rules for formation of sets and classes.

Irving H. Anellis

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