Eight Lectures on the Quine Systems for Part IV

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Contents

These notes are messages to myself, *aides mémoires* to remind me in what order i should do things, and to remind me to include things i might otherwise miss out. The only actual *details* that they contain are (ii) those details i am liable to overlook or (ii) those i might get wrong when put on a spot and (iii) things that readers might want to know but which i might not have time to cover. I will gladly make the source code available to anyone who wants to write up their notes of my lectures.

The consistency question for Quine's 1937 [13] system of set theory ("New Foundations") is—by some reckoning—the oldest open problem in set theory.[1] In this course we will explain the origins of this theory, and prove the standard

[1] There will be quibbles over this. Some will say that NF is not Set Theory, and some will say that the continuum hypothesis is still open.

results concerning it (failure of AC, equiconsistency with Type theory + a typical ambiguity scheme). We will also develop the system NFU of Jensen, which differs from NF only in allowing *urelemente*. Remarkably this theory *is* known to be consistent, and we will give several proofs of this fact.

Lecture 1: History, and roots in type theory. Stratification;
Lecture 2: The theory of (positive and) negative types;
Lecture 3: The failure of AC; NF + Counting; cardinals of infinite rank;
Lecture 4: Rieger-Bernays permutation methods. Invariant sentences;
Lecture 5: Formal development of set theory inside NF;
Lecture 6: Interpreting ZF, Z, KP etc in NF;
Lecture 7: Church-Oswald models and the consistency of fragments of NF;
Lecture 8: NFU.

Dependency chart:

stuff to fit in: finite axiomatisability, the Baltimore paper etc etc.

There is an expositional problem with ordered pairs. One wants to use Quine pairs to keep things simple, but one can't do that until one has proved the axiom of infinity. One can't do that until one has defined cardinals and ranks. One can do cardinals without ordered pairs (by Henrard's trick) but not ordinals. So it seems one has to bootstrap the system by initially using W-K pairs. That means that one has to postpone a discussion of Burali-Forti and the T function on ordinals until after the proof of the axiom of infinity. Does one have to have a discussion on the exponent on T in the statement of Hartogs' theorem?

1 Lecture 1: Definitions and NF's roots in Type Theory

The language of set theory. Set theory is a *subject*, a *topic* (like Number theory or Ramsey Theory or Morse theory); it is not a *body of axioms*. The tendency to think that set theory *is* ZF in unfaithful to mathematical terminology and is an attempt to establish by stealth a position which should properly be argued for. Also when people like Quine say that SOL is set theory in wolf's clothing, it's not ZF he means (he couldn't possibly mean ZF!) but something like NF. This takes care of an objection of Boolos' that Second-order logic cannot be set theory in disguise beco's the theorems of ZF do not coincide with the valid sentences of second-order logic.

The di Giorgi picture is a very useful place to start. Cantor's theorem says you always have to leave something out. Limitation of size very well motivated in this context. Restriction to wellfounded sets (or to small sets for that matter) should only have ever been a temporary expedient. *Reculer pour mieux sauter* when you encounter a contradiction. It was a sound temporary expedient that became fossilised.

Start with basic Church-Oswald models to ram home the point that the universal set is not a paradoxical object. It tends to be the *intermediate* sets that are paradoxical. (and lfps rather than gfps) Also "universal" collections like the collection of all cardinals are not always paradoxical. Indeed in NF the collection of all (Frege-style) cardinals is a set, usually written 'NC'. Some of them are not paradoxical even in the presence of full separation. "The collection of cardinals is a set" is consistent with Zermelo set theory for some implementations of cardinal (and gives a conservative extension. Does everybody here know what a conservative extension is?)[2]

Then explain stratification *very carefully*. weakly stratified; homogeneous. *weakly stratified* needed for proof-theoretic reasons.

A formula of set theory is **stratified** iff by assigning type subscripts to its variables we can turn it into a well-formed formula of simple type theory. That is to say, a wff ϕ is stratified iff we can find a **stratification** for it, namely a map f from its variables (after relettering where appropriate) to $\mathbb{N}$ such that if the atomic wff '$x = y$' occurs in ϕ then $f(\text{`}x\text{'}) = f(\text{`}y\text{'})$, and if '$x \in y$' occurs in ϕ then $f(\text{`}y\text{'}) = f(\text{`}x\text{'}) + 1$. Variables receiving the same integer in a stratification are said to be of the same **type**. If n successive naturals are used, the formula is said to be n-**stratified**. A stratified formula with a free variable is said to be an n-**formula** iff there is a stratification giving that variable the label 'n', and gives at least one variable the label '0' and no variable a negative label. A *function* is said to be stratified iff it is represented by a stratified expression ϕ such that $\forall x_1 \ldots x_n \exists! y \phi$. This idea is less natural than one might think,

[2]Is there a strongest natural extension T of Zermelo such that "T + NC is a set" is a conservative extension of T? Here's another thought . . . OK the collection of cardinals can be a set. say $x \sim_n y$ if there is a bijection $\pi : \bigcup^n x \longleftrightarrow \bigcup^n y$ which lifts to a bijection between x and y. What about the collection of Church-style 2-cardinals? 3-cardinals. . . ?

for the class of stratified functions is not closed under composition: singleton and binary union are both stratified, but their composition, $x \mapsto x \cup \{x\}$, is not. The largest class of stratified functions of unbounded arity closed under composition is the class of **homogeneous** functions, and the smallest class of functions closed under composition and containing all stratified functions is the class of **weakly stratified** functions. It is simple to check that a function is homogeneous iff there is a stratified expression ϕ such that $\forall x_1 \ldots x_n \exists! y \phi$ wherein all the $\vec{x}$ and y have the same type, and equally simple to check that a function is weakly stratified iff there is an expression ϕ such that $\forall x_1 \ldots x_n \exists! y \phi$ wherein such failures of stratification as there may be involve the $\vec{x}$ only. Thus we can apply the adjectives 'homogeneous' and 'weakly stratified' to formulæ as well as to functions.

Observe that most of the axioms of ZF are stratified. Make the buggers work it out.

Then say what the axioms of NF are. Extensionality plus the (universal closures of all the) weakly stratified instances of $\exists y \forall x (x \in y \longleftrightarrow \phi)$; y not free in ϕ.

Say something about how you can't relax the stratification constraint. We can tighten it in various ways: restrict the number of distinct integers you are allowed to use; try circle-free comprehension which—astonishingly—turns out to be NF.

Some slang. If T is a name for a system of axiomatic set theory (with extensionality of course), then TU is the name for the result of weakening extensionality to the assertion that *nonempty* sets with the same elements are identical. 'U' is for 'Urelemente'—German for 'atoms'.

The operation j is defined so that $(jf)(x) = f``x$. '$\sigma = j(\tau)$' is stratified with 'σ' one type higher that τ. J_0 is the full symmetric group on V.

For concrete natural numbers n, J_{n+1} is $j``J_n$. When suffixes are not in play we will sometimes write the more usual 'Symm(V)' for 'J_0'.

$\iota(x)$ is $\{x\}$, $RUSC(\pi)$ is the permutation that some folk might notate 'π^ι', namely $\{\langle\{x\},\{y\}\rangle : \langle x, y\rangle \in \pi\}$.

A BFEXT is a digraph with a designated ("top") element which can be reached from any vertex by following directed edges. It also satisfies the obvious extensionality and wellfoundedness conditions needed to make it look like the set picture of the transitive closure of a [wellfounded] singleton.

When $\sim$ is an equivalence relation $[x]_\sim$ is the equivalence class of x under $\sim$.

We use lambda notation: $\lambda x.[\ldots]$

These definitions are standard from the literature. Further (novel) definitions will follow as the story unfolds.

1.1 Specker's Equiconsistency Theorem

We can define a correlation as follows. First on the points: For all i and j, $p_i \mapsto g_i$, $q^i_j \mapsto h^i_j$, and $q_j \mapsto h_j$. Then on the lines: For all i and j, $g_i \mapsto p_{i+3}$, $h^i_j \mapsto q^i_{j+3}$ and $h_j \mapsto q_{j+3}$, with all addition mod 6.

Figure 1: A configuration with a correlation but no polarity

The theory of K (the set of formulæ true in the configuration K of Figure 1) is dual and complete (every formula is provable or refutable), but it has no model with a polarity, for all its models are isomorphic to K and K has no polarity.

We say x and y are *n-equivalent*—written '$x \sim_n y$'—if there is $\sigma \in J_n$ such that $\sigma(x) = y$. A moment's reflection will persuade the reader that $[x]_n$ is the orbit of x under J_n, and we will often refer to $[x]_n$ as the *n-orbit* of x.

A set x is *n-symmetric* iff $\sigma``x = x$ for all $\sigma \in J_n$; it is *symmetric* iff it is n-symmetric for some n.

Thus an n-symmetric set is a union of n-orbits.

2 Beginning to do Set Theory in NF

big sets (cardinals) *vs* **large** sets (cardinals)

2.1 Inductive and Coinductive Definitions in ZF and NF

Synopsis:

> Top-down and bottom-up definitions.
> Bounded character and unbounded character.
> LFPs more likely to be paradoxical than GFPs.
> homogeneous vs inhomogeneous
> justifying induction principles for LFPs

You might be trying to prove the existence of a lfp or of a gfp;

The operation for which you seek a fp might be of bounded or of unbounded character;
The operation for which you seek a fp might be homogeneous or inhomogeneous;

You might try to prove the existence of the fp by top-down or by bottom-up construction

How does the construction of the fp underpin the (co)-induction principle for it?

Explain 'bounded' and 'unbounded' character. lfps for operations of bounded character ("$x \mapsto$ set of all κ-sized subsets of x" is of bounded character) are (as far as i know) never paradoxical. Lfps for operations of bounded character are often paradoxical. The lfps for
$x \mapsto$ set of all subsets of x
$x \mapsto$ set of all transitive subsets of x
$x \mapsto$ set of all wellordered subsets of x
are all paradoxical. These are "unbounded".

Exercise 1
Suppose f is monotone and injective: $(\forall xy)(x \subseteq y \longleftrightarrow f(x) \subseteq f(y))$.
Then $\bigcap\{x : \mathcal{P}(f(x)) \subseteq x\}$, the lfp for $\mathcal{P} \circ f$, is not a set.

...is a kind of omnibus paradox for lfps for operations of unbounded character. From memory the only set theoretic axiom one needs is *subscission*. Curiously the gfps for the three operations above appear not to be paradoxical.

How are we to prove the existence of the lfps? (I'm not going to consider the gfps, tho' i know i should). There are two methods, (i) top down (intersection of all things containing this and closed under that) and (ii) bottom up (as the union of a transfinite sequence of things indexed by ordinals)

Typically with constructions of bounded character one can do it either way. The transitive closure R^* of a binary relation R can be thought-of/obtained-as either

- The intersection $\bigcap\{S \supseteq R : S^2 \subseteq S\}$ or
- The union $\bigcup_{i \in \mathbb{N}} R^n$.
- Bottom-up tends to need replacement to construct the sequence;
- Top-down requires us to have a set big enough to contain the founders and be closed under the relevant operation.

The naïve way to attempt to prove the existence of the rectype containing founders in F and closed under constructors in C is to take the collection $\mathcal{X}$ of all sets containing all of F and closed under all constructors in C, and then take its intersection. This naïve strategy does not work in ZF because (except in trivial cases) X cannot be a set. The trouble is that, for some small n, $\bigcup^n X$ turns out to be the universe.

However, help is at hand, in the form of the following triviality:

> For all X and Y, $X \subseteq Y \to \bigcap Y \subseteq \bigcap X$. Further, if everything in Y is a subset of something in X, then $\bigcap Y = \bigcap X$.

This tells us that, in order to construct the rectype containing founders in F and closed under constructors in C, it is not necessary for the collection X of all sets containing all of F and closed under all constructors in C to be a set. It is sufficient for there to be even one set $Y \supset F$ that is closed under constructors in C, because everything in the desired rectype will be in Y and so the rectype will be a set by separation.

So far so good, but how are we to come by such a set Y, given F and C? It turns out that in ZF we typically need the axiom scheme of replacement. The situation in NF is rather different, because we don't have the difficulty ZF has with the collection $\mathcal{X}$ being a set. In ZF the fact that the sethood of $\mathcal{X}$ results in V being a set means that $\mathcal{X}$ cannot be a set. In NF this isn't a problem, and we can give straightforward top-down definitions of inductively defined sets as the intersection of all set containing this and closed under that.

However there is an important proviso. This will work as long as $\mathcal{X}$ is denoted by a stratified set abstract; if it isn't then all bets are off.

$$\mathcal{X} := \{Y : F``Y \subseteq Y\}$$

is a stratified set abstract as long as F is homogeneous. To summarise:

THEOREM 1 *NF proves the existence of the least-fixed point for F as long as F is homogeneous.*

If we run into an F that is homogeneous (so that NF can—as above—prove the existence of the lfp for F) but where this F is a bad unbounded operation whose lfp is paradoxical, then we would be in trouble. When I last looked, no such F were known, so—for the moment at least—NF appears to be consistent. So: no bad F seem to be homogeneous. However we would like NF not only to be *sound* for inductive definitions (in the sense that the only inductively defined sets whose existence it proves are non-paradoxical) but also *complete*: is it the case that every well-behaved F of bounded character is homogeneous? (So that we can prove that it has an lfp)

Sadly the answer to this is: clearly not! The collection V_ω of hereditarily finite sets is the lfp for $x \mapsto$ the set of finite subsets of x. V_ω is not a paradoxical object, but the operation $x \mapsto$ the set of finite subsets of x is not homogeneous, its value having a type one higher than its argument. This means that—to the best of our knowledge—NF does not prove the existence of V_ω.

There remains the matter of justifying in each case the induction principles that the inductively defined sets are supposed to support.

Of course if we have defined a rectype X as $\bigcap\{Y : F``Y \subseteq Y\}$ then we have $F$-induction for $X$. If $Y$ is a property preserved by $F$ then $F``\{x : Y(x)\} \subseteq \{x : Y(x)\}$ so surely $X \subseteq \{x : Y(x)\}$, which means that everything in X has Y.

This works in NF, but there are difficulties in ZF with lpfs for operations of unbounded character, when the lfp is a proper class. Our conception of the family of wellfounded sets as an inductively defined set gives us a formal definition of *WF*, namely

$$WF = \bigcap\{Y : \mathcal{P}(Y) \subseteq Y\} \tag{1}$$

Now there is something obviously wrong with this definition: "$\mathcal{P}(Y) \subseteq Y$" contradicts Cantor's theorem, which is—like everything else—a theorem of naïve set theory. This would make WF the intersection of the empty set, and therefore $WF = V$. But Cantor's theorem does not imply that every set is wellfounded (there could be Quine atoms for example) so clearly something has gone wrong.

One thing we could try is to define a set to be wellfounded if it belongs to all **classes** that extend their own power class. The trouble with this definition is that it gives us a definition of wellfounded which involves bound class variables and is not first order. In particular it cannot be done in *ZF* but only in the theory usually known as Morse-Kelley.[3]

The treatment that is universally resorted to by those who wish to develop a classical theory of wellfounded sets in *ZF* is the use of **regular** sets.

Definition 2 *Let us abbreviate "every set to which x belongs has a $\in$-minimal element" to "x is regular".*

The way forward is to prove in *ZF* that regular sets behave in the way wellfounded sets should. In particular we will have to justify a principle of

[3]It was actually first discussed by Wang [17] who called it 'BQ'.

$\in$-induction for regular sets.

$$\frac{(\forall y)(y \in x \ \to \psi(y)) \to \psi(x)}{(\forall x \in WF)(\phi(x))}$$

This is standard in modern treatments of set theory, and we do it as follows. Suppose we know $(\forall x)((\forall y \in x)(\phi(y)) \to \phi(x))$, and suppose there is a regular set z such that $\neg\phi(z)$. Let Y be a transitive set containing z. Let $X = \{y \in Y : \neg\phi(y)\}$. $z \in X$ and z is regular, so X must be disjoint from one of its members. But this contradicts the induction hypothesis.

So—at least if we have something like transitive closure—regularity is a tolerable definition of wellfoundedness. However from a constructive point of view there is a very serious drawback to this whole approach.

Remark 3 *(Diaconescu [2])*

If there is even one nonempty regular set then excluded middle holds.

Proof:

Suppose x is a nonempty regular set, and ϕ any proposition. Consider the set $\{x\} \cup \{y \in x : \phi\}$. This set has x as a member, so it is nonempty and must be disjoint from one of its members. Which one? If it is disjoint from x then $\{y \in x : \phi\}$ is disjoint from x, so $\{y \in x : \phi\}$ must be empty, and we infer $\neg\phi$ whence $\phi \vee \neg\phi$. If it is disjoint from something in $\{y \in x : \phi\}$ then $\{y \in x : \phi\}$ must be inhabited and so we infer ϕ, whence—again—$\phi \vee \neg\phi$. ■

This ought not to surprise anyone. For one thing the concept of regular set arises from an attempt to deal with wellfounded sets by talking about complements of (big) sets instead of the sets themselves, so we would expect classical behaviour as always with negative interpretations. For another the concept of regular set plays the same rôle in the theory of wellfounded sets as the least-number principle does in intuitionistic arithmetic and—as we all know—the least-number principle is not constructive.

Can we escape from this by defining $reg(x)$ to be instead $(\forall y)(x \in y \to \neg\forall z\neg(z \in y \wedge z \cap y = \emptyset)$? We then find that we can justify $\in$-induction for regular sets only for stable formulæ. This works as follows.

Suppose $(\forall y \in x)(\phi(y)) \to \phi(x)$, and that x is regular (in the new sense) but is not ϕ. Think of $\{y \in TC\{x\} : \neg\phi(y)\}$ or B for short. $x \in B$ so $\neg\forall z\neg(z \in B \wedge B \cap z = \emptyset)$. Now assume $z \in B \wedge B \cap z = \emptyset$. Since $z \in B$ we have $\neg\phi(z)$. Also $B \cap z = \emptyset$ so $(\forall w \in z)(\neg\neg\phi(w))$. This is no use to us unless ϕ is stable!

So the intuitionistic theory of wellfounded (i.e, regular) sets in *ZF* is the same as the classical theory of wellfounded (i.e, regular) sets in *ZF*. Accordingly if we want a constructive theory of wellfounded sets we cannot do it in *ZF* because *ZF* —lacking big sets as it does—is committed to regular sets as the only possible account of wellfounded sets.

> *If we want a constructive account of wellfounded sets we must develop it inside a theory that has big sets but does not have Cantor's theorem.*

What sort of theory is available? The following consideration narrows the field still further. We certainly wish to be able to prove that the proper class of wellfounded sets is transitive.

Lemma 4 *Every member of a wellfounded set is wellfounded.*

If x is wellfounded, and $(\forall y)(y \subseteq X \to y \in X)$ then $x \in X$. It will suffice to show that $x \subseteq X$ as well.

Suppose $x \not\subseteq X$. We will show that $(\forall y)(y \subseteq (X \setminus \{x\}) \to y \in (X \setminus \{x\}))$ whence $x \in (X \setminus \{x\})$ (since x is wellfounded). This is impossible.

Suppose $y \subseteq (X \setminus \{x\})$. Then $y \subseteq X$ and $y \in X$. To deduce $y \in (X \setminus \{x\})$ it will suffice to show $y \neq x$, which would follow from $x \not\subseteq (X \setminus \{x\})$. But we have assumed that $x \not\subseteq X$ so *a fortiori* $x \not\subseteq (X \setminus \{x\})$. ■

This proof is not constructive, but it is very light on set-existence axioms. The only one it uses is the axiom Allen Hazen calls *subscission*: existence of $x \setminus \{y\}$. The proof that follows is constructive, but it does use the power set axiom.

Suppose x is well-founded, so $(\forall Y)(\mathcal{P}(Y) \subseteq Y \to x \in Y)$. Now $\mathcal{P}$ is monotone, so $\mathcal{P}(Y) \subseteq Y \to \mathcal{P}^2(Y) \subseteq \mathcal{P}(Y)$ and $x \in \mathcal{P}(Y)$, so every member of x is in Y. So every member of x is well-founded too. ■

I do not know of any constructive proof that doesn't use power set.

So, if we want a constructive theory of wellfounded sets, we need a set theory with a universal set and an axiom of power set.

2.1.1 The theory of wellfounded sets in NF

What does NF say about wellfounded sets? We can define "wellfounded" in NF in the above natural way, and it supports $\in$-induction—but of course only for stratified expressions. But then let us recall that in any set theory you have induction only for those formulæ for which you have comprehension principles. There is clearly going to be a theory T such that NF proves that the wellfounded sets [in any model of NF] form a model of T. We would expect T to be a fragment of ZF (we would be very surprised if it turned out to be stronger than ZF!) and the fragment appears to be very weak. It is not known if this fragment contains infinity or transitive containment, tho' it is not hard to show that it contains power set, sumset, pairing, stratified replacement—and of course extensionality. Miniexercise: check for yourself that it really does satisfy stratified replacement. This might be hard!

There is at this stage no obvious reason why we should not adorn NF with some extra axioms to ensure that the theory of wellfounded-sets-in-NF contains all of ZF—or indeed all of Woodin's Ω-logic. We shouldn't think of this as artificial: Z had to be sexed up to get ZFC (which has been for ages the baseline theory of wellfounded sets) and—just as Z didn't get it right first time—we shouldn't be surprised if we find we have to add things to NF. We will consider later (section 5) the possibility that one might be able to prove facts about wellfounded sets by reasoning about illfounded sets.

2.2 Implementing Mathematical Objects in NF

Abstract objects arising from equivalence relations can be implemented as equivalence classes as long as the equivalence relation is homogeneous. Is this a restriction? Typically no, beco's *Mathematics is stratified*. What do we mean by this? Typically anything that is typed in any of the senses that CS people use is likely to be implementable into NF in a stratified way. Pairing, ordinals, cardinals etc etc. Observe that most of the instances of comprehension that are of interest to us are of interest only beco's they are actually talking about mathematical objects familiar from other contexts and are therefore not in primitive notation. So we'd better get fluent at testing-for-stratification formulæ that are not in primitive notation. This is trickier than it looks, and it takes some getting used to.

We implement cardinals as equipollence-classes. Notice that this implementation does not depend on how we implement ordered pairs. All that matters is that in the expression of $\mathcal{L}(\in,=)$ that says "x is the ordered pair of y and z" (which we abbreviate to $x = \langle y, z\rangle$) 'y' and 'z' receive the same type. For the moment we will use W-K ordered pairs, since they satisfy this condition. Observe that as long as the condition is satisfied, $X \times Y$ exists, and the graph of any homogeneous function is a set, co's it's $\{z : (\exists xy)(z = \langle x, y\rangle \wedge \phi(x, y)\}$

Talk through the two versions of Cantor's theorem: the standard version and the NF version.

Proposition 5 *The sets of NF do not form a cartesian closed category.*

Proof: We start from the two uncontroversial assumptions

1. For the category of sets of a theory to be cartesian closed it is necessary for the theory to believe that the graph of the function

$$\texttt{curry}\colon\ (A \times B) \to C. \quad \to . \quad A \to (B \to C)$$

   is a set (at least locally, in the sense that its restriction to any set is a set).

2. In any sensible pairing function for NF, the expression '$x = \langle y, z\rangle$', when written out in primitive notation, must be stratified with 'y' and 'z' having the same type, and 'x' having a type which is not lower then the type of 'y' and 'z'.

In NF the qualification at the end of item (1) makes no difference, since there is a universal set and the graph of `curry` local to it will be the graph of `curry` itself.

If the graph of `curry` is a set then in particular so is the graph (call it f_1) of the function that for each x sends $(\{\emptyset\} \times \{\emptyset\}) \to x$ to $\{\emptyset\} \to (\{\emptyset\} \to x)$. $\{\emptyset\} \to x$ is one type higher than x so—by NF comprehension—the graph (call it f_2) of the function sending $\{x\}$ to $(\{\emptyset\} \times \{\emptyset\}) \to x$ is a set. By the same token $\{\emptyset\} \to (\{\emptyset\} \to x)$ is two types higher than x, and—by NF comprehension again—the graph (call it f_3) of the function sending $\{\emptyset\} \to (\{\emptyset\} \to x)$ to $\{\{x\}\}$ is also a set.

Then the composition $f_3 \cdot f_1 \cdot f_2$ sends $\{x\}$ to $\{\{x\}\}$. This immediately gives us the graph of the singleton function as a set and as we have seen this is impossible in NF. ■

(Another way of characterising cartesian-closed categories is by the presence of an evaluation function $ev : (A \to B) \times A \to B$. Naturally a similar exercise will show that the graph of this function cannot be a set.)
[4]

Burali-Forti and the first edition of ML. That was a blunder. Quine really should have known better. No, do that later

[4] How serious is this breakdown? It may be much less serious than one thinks. NFistes have known for a long time that whenever a desired result P—familiar from ZF—fails in NF then the proof method that gave rise to P can be tweaked to prove a variant P' which (i) is equivalent to P in ZF; and (ii) differs from P in having a T function inserted judiciously, or in having some occurences of a variable 'X' replaced by 'ι"X', or some similar modification. P' discharges many of the functions of P and life goes on roughly as normal. One thinks of the theorem $|\iota``x| < |\mathcal{P}(x)|$ which does duty for Cantor's theorem in NF. Perhaps the failure of cartesian-closedness of NF will turn out to be similarly un-concerning.

There is a phenomenon in NF which we are all sort-of familiar with, but which we (or at least I) do not properly understand. Observe that although we cannot prove Cantor's theorem, we can prove the modified version of it (and it has the "same" proof) which gives most of the consequences of the original and in some sense captures the same mathematics. The same thing happens with Hartogs' theorem. Somehow the failure of NF to prove (unstratified) results that we expect from our experience always evades disaster at the last moment. (Typically T functions ride in and save the day, so perhaps we can tie this in with a long-overdue discussion of T functions.)

I have wondered for years whether the delightful (annoying?) fact first published by Colin McLarty might be an example of the same phenomenon. I allude of course to the fact that it is a theorem of NF that the category of sets is not cartesian-closed.

I think the time may be ripe for an investigation of this *get out of jail free* behaviour with which NF always confounds its detractors at the last minute. There is a common pattern to all these episodes and it needs to be understood.

When i suggested this to Colin McL he said:

In a cartesian-closed category $X \mapsto X \times B$ is left-adjoint to $X \mapsto X^B$. Left-adjoints preserve quotients and disjoint unions. So we might lose the fact that the product of a disjoint union is a disjoint union of the products:

$A \wedge (B \sqcup C) \to A \wedge B \sqcup (A \wedge C)$

But we don't! This holds in NF..

Valeria says this is a *distributive* category

Peter Lumsdaine says that if NF really does recover the effect of cartesian-closedness then there is a cartesian-closed category in there somewhere . . . it just isn't to be found where you think.

3 Cardinal and Ordinal Arithmetic, T functions, Polymorphism and Monomorphism

Cardinals and ordinals—and relational types generally—can be implemented in NF as equivalence classes: painless and natural. In particular cardinals are equivalence classes under equipollence, so the cardinal of x (which we write '$|x|$') really is a set. And the collection of cardinals is a set. $\mathbb{N}$ is the set of finite cardinals, so that n is the set of n-membered sets.

How does ordinary mathematical induction work? We prove that every natural number is F if $\{x : F(x)\}$ contains 0 and is closed under `succ`, for then every natural number is in $\{x : F(x)\}$; $\mathbb{N}$ is the intersection of all sets containing 0 and closed under `succ` after all. For us to prove by this method that every natural number is F we require $\{x : F(x)\}$ to be a set. The effect of this is that NF proves only the stratified instances of mathematical induction.

For the moment we will use W-K ordered pairs. It's worth pointing out that we can define natural numbers without implementing pairing at all, but this won't work for cardinals in general without a lot of extra work. I think we have to bite the bullet and show that it doesn't matter

Explain the T function on relational types. **T commutes with everything!!** This is important, and it looks bizarre, so go over it in excruciating detail!

Discussion of Burali-Forti again. Failure of Hartogs' theorem. Explain how ranks can be assigned to wellfounded structures. (**without** running out of ordinals!)

Ordinals emerge from a theory of wellorderings and isomorphisms between them, and to discuss wellorderings in set theory we have to decide how to think of wellorderings as sets. There are two standard ways of doing this. We can

(A) think of a wellordering of a set X as a special kind of subset of $X \times X$, and if we are to do this we have to make an implementation decision about ordered pairs.

(B) think of a wellordering of a set X as the set of (domains of) its initial segments; this encodes a wellordering of X as a subset of $\mathcal{P}(X)$.

In type theory and the Quine systems the cute implementation of ordinals as wellfounded-hereditarily-transitive-sets-wellordered-by-$\in$ is not available. The point is not just that the formulæ are highly unstratified; the point is that we can exhibit wellorderings which are not isomorphic to any such object. (If x can be wellordered to an order-type that can be implemented as a von Neumann ordinal in this way then x is strongly cantorian.) This is not a problem, since there are other implementations of ordinal available. The best option is to implement ordinals as isomorphism classes of wellorderings. The ordinal of a wellordering is then the unique ordinal to which it belongs.

Say something about how this is a failure of replacement. Goes phut in Zermelo too.

It is routine to establish that if α is an ordinal then the set of ordinals strictly less than α is naturally wellordered by magnitude.

The von Neumann implementation of ordinals causes this elementary fact to be "disappeared" into the notation—according to that implementation an ordinal just *is* the set of earlier ordinals. What we need here is a proof that every initial segment of the *ordinals-as-defined-here* is wellordered by magnitude. It is even possible to give a much more general proof—one that is implementation-insensitive—and here it is.

Say something about here about disappearing things into notation?

THEOREM 6 *The order-by-magnitude relation on ordinals is a wellordering.*

Proof: Assume ordinals implemented somehow, and let α be an ordinal. Let $\mathcal{A}$ be a set all of whose members are ordinals $\leq \alpha$. We will show that $\mathcal{A}$ has a bottom element. Without loss of generality we can suppose $\alpha \in \mathcal{A}$, since $\mathcal{A}$ is not going to suddenly acquire a bottom element by being given a top one. Let $\mathfrak{A} = \langle A, \leq_A \rangle$ be a wellordering belonging to $\mathcal{A}$. For each $\beta \in \mathcal{A}$ let $\mathfrak{A} \restriction \beta$ be the unique initial segment of $\mathfrak{A}$ of length β and let a_β be the sup of that initial segment in the sense of $\leq_A$. Clearly $\beta < \beta'$ iff $a_\beta <_A a_{\beta'}$. The set $\{a_\beta : \beta \in \mathcal{A}\}$ has a $<_A$-least element, so $\mathcal{A}$ must have a least element too. ■

So the set of ordinals below α is wellordered, and therefore has a length which is an ordinal. What is this ordinal? Recent experience with natural numbers will have led the reader to suspect that this ordinal might not be α. The assertion that it is, nevertheless, the same as α, is worth spelling out and naming:

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We have already seen Cantor's theorem for NF: $T|V| < |V|$.

The stratification constraint means that we have to be very careful when implementing these operations. An important example is cardinal exponentiation. Since (as we have seen) A and $\{\{y\} : y \in A\}$ are not reliably the same size, the two definitions of 2^α—(i) as $|\mathcal{P}(A)|$ where $|A| = \alpha$ or (ii) as $|\mathcal{P}(A)|$ where $|\{\{y\} \mid y \in A\}| = \alpha$—are not equivalent as they are in ZF: it makes a difference which one we choose. If we choose the second it turns out (easy to check) that the property of containing a given cardinal α and being closed under exponentiation is stratified. This means that the collection $\{\alpha, 2^\alpha, 2^{2^\alpha} \ldots\}$ is a set for all α. Were we to use the other definition it wouldn't be.

Worth asking why this definition is legitimate. Surely $2^{|x|}$ was *defined* to be $|\mathcal{P}(x)|$??

Homogeneous definition of exponentiation rephrases this as $T|V| < 2^{T|V|} = |V|$

Make the point (tho' possibly not here) that this means $|V| > T|V| > T^2|V| > T^3|V| \ldots$ but that this by itself is not enuff to refute AC because this descending family of cardinals cannot be proved to be a set. In fact we can prove that it isn't a set.

. . . enables us to run with Specker's Φ function; the set SM. Cardinal trees.

PROPOSITION 7
$(\forall n \in \mathbb{N})(\forall \alpha, \beta \in NC)(\beta = \beth_n(\alpha) \longleftrightarrow T\beta = \beth_{Tn}(T\alpha))$.

Proof: . The assertion is stratified and so can be proved by induction on n. This is beco's "$\beta = \beth_n(\alpha)$" is a stratified formula with three free variables in it. For the induction step we need only that T commutes with exponentiation. ■

Proposition 8
$(\forall \alpha \in NC)(|\phi(T(\alpha))| \geq T(|\phi(\alpha)|) + 1)$

Proof:
By proposition 7 the Tnth member of $\Phi(T\alpha)$ is T of the nth member of $\Phi(\alpha)$, so it is certainly $\leq |V|$ and is therfore not the last member of $\Phi(T\alpha)$. ■

3.1 Proof of the Axiom of Infinity

Specker trees. What is the rank of $\mathcal{T}|V|$? As it happens we can prove that all these trees are wellfounded, but in fact we don't need that if we are merely trying to prove infinity. If the rank is undefined (or is defined and infinite) then clearly $\mathcal{T}|V|$ is an infinite set. So suppose it is finite. But then we must have $T\rho(\mathcal{T}|V|) = \rho(\mathcal{T}|TV|)$.

But this means that $\mathcal{T}|V|$ branches at $|V|$. So $|V|$ cannot be a natural number.

Next: if $\rho(|V|)$ is a natural number is must be nonstandard. So Counting implies that it's infinite. Show that cardinals of infinite rank contradict AC. Not known if ZF has models (in which Choice fails and) there are cardinals of infinite rank.

Where do we introduce the axiom of counting?

We can now use Quine pairs!!!!

Definition 9
$\theta_1(x) := \{n+1 : n \in x \cap \mathbb{N}\} \cup (x \setminus \mathbb{N})$;
$\theta_2(x) := \{n+1 : n \in x \cap \mathbb{N}\} \cup (x \setminus \mathbb{N}) \cup \{0\}$;

Astonishingly '0' here really is the natural number 0.[5] Everything is a value of θ_1 or θ_2 but not both.

The Quine pairing function has two quite desirable features. The first is that it makes everything into a pair. The second is that the formula $P(x, y, z)$ (that says that z is the Quine pair of x and y) makes 'x', 'y' and 'z' all the same type. We noticed that the considerations earlier [deleted] did not constrain the type of 'z', but there is no doubt that having 'x', 'y' and 'z' all the same type makes life superficially easier. It ensures that when we procede to triples and quadruples etc as in the previous paragraph we do not have to wrap curly brackets around variables to ensure that all components of tuples are the same type—though this makes no substantial mathematical difference. What it does do is vary the exponent we employ on the T-functions.

(There are other advantages as well. $\langle V, \subseteq, - \ldots \rangle$ is a boolean algebra, and so is $V \times V$. The Quine pairing function is actually an **isomorphism** between

[5] Check your NF skills . . . what is this object?

$V \times V$ 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. If we need a disjoint union function $x \sqcup y$ then $\langle x, y\rangle$ would do.)

If V is infinite, so is $\iota``V$. That is to say that no finite set can contain all singletons, which means that we can prove by induction on the finite sets that every finite set is the same size as a set of singletons. In other words (since our natural numbers are equipollence classes) every natural number contains a set of singletons. So 2^n is defined for every natural number n; since clearly 2^n is a natural number as long as n is, this in turn means that if $n \in \mathbb{N}$ then $\phi(n)$ is an infinite set. It also means that $\lambda n \in \mathbb{N}.\{T^{-1}n\}$ is a 1-1 map $\mathbb{N} \longleftrightarrow \iota``\mathbb{N}$. (Observe that this collection of ordered pairs is a stratified set abstract and is therefore a *set*.)

3.2 Subversion of Stratification

. . . by strongly cantorian sets

The natural notion of good behaviour is not "cantorian" (so that $|x| = T|x|$ and x obeys Cantor's theorem) but *strongly* cantorian.

Supply details here

3.3 Refutation of the Axiom of Choice

Assume the Axiom of Choice. We will deduce a contradiction. Let SM ("Speckermenge") be $\{\alpha \in NC : |\Phi(\alpha)| < \aleph_0\}$. (For all we know at this stage, SM could be precisely NCI the set of cardinals of infinite sets).

We now use AC to tell us that SM has a least member, which we call κ. We now consider the last (top) member of $\Phi(\kappa)$. Might it be $|V|$ by any chance? Well, if $|V|$ is the nth mmber of $\Phi(\kappa)$ then $T|V|$ is the Tnth member of $\Phi(T\kappa)$ so $|V|$ (which is $2^{T|V|}$, remember) is the last member of $\Phi(T\kappa)$, so $T\kappa$ is in SM too. By prop 8 we must have $T^{-1}\kappa \in SM$, whence $\kappa \leq$ bot $T\kappa$ and $T^{-1}\kappa$ (by AC—comparability of cardinals). This gives $\kappa = T\kappa$. Now if $|V|$ is the nth (and last) member of $\Phi(\kappa)$ then $T|V|$ is the Tnth (and penultimate) member of $\Phi(T\kappa)$ so $|\Phi(T\kappa)| + 1 = |\Phi(\kappa)|$ which is clearly impossible.

So the last (nth) member of $\Phi(\kappa)$ must be a cardinal strictly between $|V|$ and $T|V|$. (It must be less than $|V|$ but cannot be $\leq T|V|$.) So T of this last member (which is the Tnth member of $\Phi(T\kappa)$) must be a cardinal strictly between $T|V|$ and $T^2|V|$. This puts either one or two more cardinals on the end of $\Phi(T\kappa)$, depending on whether 2-to-the-power of this cardinal is $T|V|$ or not. But observe that, since κ is the least element of SM, it must be $\leq$ both $T^{-1}\kappa$ and $T\kappa$. So we must have $\kappa = T\kappa$. Now $|\Phi(T\kappa)|$ is either $T|\Phi(\kappa)| + 1$ or $T|\Phi(\kappa)| + 2$, but both of these are impossible, since T must preserve congruence mod 3, since $3 = T3$. ■

3.4 How many alephs?

Consider the proof of Hartogs' theorem for NF:

Suppose X is a wellordered set, of size κ. We want to prove that there is a set of size κ^+. The usual candidate for this post is the collection $\mathcal{X}$ of isomorphism classes of wellorderings of subsets of X. However, it turns out that $\mathcal{X}$ is not reliably of size κ^+ but is rather of size $T^2\kappa^+$. This is less than ideal, since—for all we know—$T^2\kappa^+$ might be strictly less than κ. Will there in fact be a set of size κ^+? There will if $\mathcal{X}$ is the same size as a set of double singletons, but not otherwise.

3.5 The Axiom of Counting

For any natural number n the set $[0, n-1]$ of natural numbers less than n is finite (that is to say, is in FIN) and we prove this by induction on 'n') and its cardinal—$|[0, n-1]|$—is therefore a natural number by lemma **??**. Do we have

$$|[0, n-1]| = n? \qquad (AxCount)$$

The obvious way to prove this would be by induction. However the induction fails because it is not stratified.

Nevertheless we still have the fact that $|[0, n-1]|$ is a member of $\mathbb{N}$.

Observe that $|[0, n-1]|$ is T^2n' not Tn, because $|[0, n-1]|$ is *two* types higher than n not one.

The assertion *AxCount* above (that each natural number counts the set of its predecessors) was dubbed "The Axiom of Counting" by Rosser [14], who was the first to notice that NF withheld the obvious inductive proof of it. more detail here

The axiom of counting is usually taken to be completely fundamental; *so* fundamental, in fact, that it is never brought out into the open and identified as an assumption. What is distinctive about the story told by the stratification tradition is that the axiom of counting appears late in the piece. The stratified take on the axiom of counting is that the axiom is actually a sophisticated and metaphysically significant move...

3.6 Ordinals and the Extended Axiom of Counting

DEFINITION 10 The Axiom of Counting:

Every natural number n has precisely n natural numbers below it;

The Extended Axiom of Counting:

Every ordinal is the order type of the (wellordering of) the ordinals below it

[should make the point that the axiom of counting implies that if x is of size n then there is a bijection between x and the natural numbers below n, so it's a set existence axiom. Quite which set-theoretic assertion it turns out to be will depend on how we have implemented ordinals and natural numbers.]

There are three assertions we have to distinguish:

(i) Every ordinal counts the set of its predecessors;

(ii) R and $RUSC(R)$ (aka R^{ι}) are isomorphic; and (iii) Every wellordering is isomorphic to the wellordering of its initial segments under end-extension.

Where should we define $RUSC(R)$?

Of these (i) is the extended axiom of counting. To show that (ii) and (iii) are equivalent it is enough to show that $\langle \iota``X, RUSC(R)\rangle$ is isomorphic to the wellordering of initial segments of $\langle X, R\rangle$ under end-extension. Now the bijection $\{x\} \mapsto \{y : R(y,x)\}$ is given to us quite cheaply: for suitable implementations of pairing-plus-unpairing its existence is provable even in KF.

KF?

The equivalence of (i) and (ii) is a more complex matter. Chief of these extra complexities is explaining the exponent on the 'ι'. We might try to explain this.

Q: Which of these assertions is the one we need to add to NF to prove Con(NF)?
A: $(\forall n \in Nn)(n = Tn)$ where 'T' denotes the usual type-raising (set-theoretic) operation.

Ward Henson, who was the first person to consider this function applied to ordinals rather than cardinals (see [7]), was properly sensitive to the difference between ordinals and cardinals, and he wrote the operation on ordinals with a 'U' rather than a 'T'.[6] We noted that $|[0, n-1]|$ is two types higher than n. How many types higher than 'α' is the ordinal of the set of ordinals below α ordered by magnitude? Let's calculate it. Ordinals are implemented as isomorphism classes (which turn out to be sets, since their defining condition is stratified) of wellorderings. So we consider the set of ordinals below α, and we wellorder it by magnitude. This gives us a set ('A' for the moment) of ordered pairs of ordinals, and we take its equivalence class (again, *set*) under isomorphism, and this is the ordinal we want. It will of course be one type higher than A. But what is the type of A relative to the type of α? The answer to this will depend on our choice of pairing-unpairing machinery! If we are using Quine pairs it will be one type higher than α, but if we are using Wiener-Kuratowski pairs the difference will be three! Thus if we are using Quine pairs and implementing ordinals as equivalence classes of wellorderings then the order type of the ordinal below α is two levels higher than the level of α.

The fact that under any sensible implementation of ordered pair (or even without it, by using the initial segment coding) the collection of all ordinals is a set has the consequence that there must always be a nontrivial appearance of the T function to enable us to say that

$$T^k\alpha \text{ is the length of the ordinals below } \alpha: \tag{4}$$

If α counted the length of the ordinals below α we would be able to prove the Burali-Forti paradox. Therefore any *true* (4)-like assertion about the length of an initial segment of ordinals *must* involve a T-function, and with the exponent $k \neq 0$. (This is in sharp contrast to the case with natural numbers, where the assertion that each natural number counts the set of its predecessors appears to

[6] Nowadays it usually written with a 'T', using overloading.

be consistent—albeit strong. See section **??**) The appearance of the T function here is therefore *not* an artefact of our choice of implementation for ordered pairs or wellorderings: it is a genuine manifestation of the underlying mathematics. It is true that we might not have had our noses rubbed in it had we not casually assumed there was a set of all wellorderings, but it was there all along anyway. The Burali-Forti paradox may be the *bearer* of the bad news, not it is not its author.

Despite the inevitability of the appearance here of a T-function, there is nothing in the underlying mathematics to tell us what the exponent on it must be in formula (4)!

to be continued

H I A T U S

Just as the T function on $\mathbb{N}$ is an automorphism of $\mathbb{N}$, so the T function on ordinals is a structure-preserving map: an endomorphism (but not an isomorphism: not every ordinal is a value of T). Since we expect, in a less strongly typed world, that T should actually be the identity function, we cannot entertain the possibility (except on pain of inconsistency) of T exhibiting any behaviour that prevents it being a structure-preserving map. On the contrary we look for consistency proofs of assertions that T partakes of more of the properties of equality—for example, $(\forall\alpha)(T\alpha \leq \alpha)$, or $(\forall\alpha)(\alpha = T\alpha \to (\forall\beta < \alpha)(\beta = T\beta))$—both of which are obviously true if we take T to be equality) Pleasingly, assertions like these turn out to have the consistency strength of assertions that H_κ (the collection of sets hereditarily of size $< \kappa$) exist, for suitable κ. See [6].

In a stratified set theory a T-function for a particular implemented abstract data type (ADT) is a type-raising endomorphism for that ADT: it sends objects of that ADT to another object of the same ADT, but one of higher type—in the *set-theoretic* sense of 'type'. Thus the expression '$x = T(y)$' will be stratified with the variable 'x' being allocated an integer one greater than the integer given to 'y' in any stratification.

The first T-function was defined by Specker [15]. It was a function on cardinals: $T|x|$ is $|\iota``x|$. Somebody (Rosser? (Certainly it's in Ward Henson [7] but it may be in Rosser [14] first) considered a T-function on ordinals. Ward Henson (alert to the fact that ordinals and cardinals are different sorts of things) wrote this ordinal T-function with a capital 'U' instead of a 'T' but in the three NF Ph.D. theses written in the 1970's under the guidance of the late Maurice Boffa (Hinnion [8], Forster [3] and Pétry [12]) the same letter 'T' is used for both. Hinnion [8] further considers a T-function for isomophism types of wellfounded extensional relations, and this was the first time (at least in writing) that anyone had considered the possibility of T-functions being applied to anything other than cardinals or ordinals; he, too, writes that function with a 'T'. The time has come to give an explanation of why Boffa, Hinnion, Pétry and Forster were justified in using one single notation for all these objects. And this explanation will comprise an analysis of how they are all instances of the same general idea.

Randall, can U chk this? I haven't got a copy

Definition 11
Consider the set of orbits of V under J_n, and then the set of arbitrary unions of orbits. This is the nth **domain**, *D_n, the set of things that are n-symmetric.*

We shall eventually define generalised T-functions in such a way that they are defined on all symmetric sets. However in the first instance we define them only on n-orbits. (Not every n-symmetric set is an n-orbit but every n-symmetric set is a union of n-orbits. The n-orbit of x is the same as the n-equivalence class of x.) For each concrete n we define T_n—the nth T-function—as that function that takes the n-equivalence class $[x]_n$ of an object x and returns the n-equivalence class $[j^n\iota(x)]_n$ of $j^n\iota(x)$. Thus

Definition 12 $T_n =: \lambda[x]_n.[j^n\iota(x)]_n$

Observe that, for every concrete n, '$x = T_n(y)$' is stratified with 'x' one type higher than 'y'.

Doesn't this amount to saying that $T_k(x) = \{y : (\exists y' \in y)(y \sim_k j^k\iota y')\}$? oops, where's the '$x$' gone?

We will need the following lemma.

Lemma 13 *$j^n\iota$ preserves n-equivalence in the sense that*

$$(\forall xy)(x \sim_n y \to j^n\iota(x) \sim_n j^n\iota(y))$$

Proof:
Suppose x and y belong to the same n-orbit. That is to say there is $\pi \in \mathrm{Symm}(V)$ such that $j^n(\pi)(x) = y$. Now clearly $j^n(RUSC(\pi))((j^n(\iota)(x))) = (j^n(\iota)(y)$. This doesn't establish *literally* that $(j^n(\iota))(y)$ and $(j^n(\iota))(x)$ belong to the same n-orbit because $RUSC(\pi)$ is not a permutation of V. However it can be extended to one by fixing everything that is not in ι"V, and then this extension of $RUSC(\pi)$ to the whole of V is witness to the fact that $(j^n(\iota))(y)$ and $(j^n(\iota))(x)$ belong to the same n-orbit. ■

> *HOLE Can we not strengthen this to $(\forall xy)(x \sim_n y \to j^n\iota(x) \sim_{n+1} j^n\iota(y))$?*
>
> *HOLE Explain the difference between this and "n-equivalence is a congruence relation for ι^n". We might need the concept of a "suite of conguence relations" from [5]. It might help the reader*

If this were not so, definition 12 above would be sensitive to our choice of x from the orbit and would not be legitimate.

Definition 12 gives us the result we want, namely that the familiar T-functions on ordinals, cardinals and BFEXTs are all T_n for some small n (depending on your implementation of ordered pairs, in the case of ordinals and BFEXTs)).

It may be worth commenting that the converse to lemma 13 is not true. Consider two cofinite sets whose complements are of different sizes: $V \setminus \{\emptyset\}$ and $V \setminus \{\emptyset, V\}$. For example. These two sets are not 1-equivalent (i.e., they belong to different 1-orbits) but $\iota``(V \setminus \{\emptyset\})$ and $\iota``(V \setminus \{\emptyset, V\})$ *are* 1-equivalent (i.e., they belong to the same 1-orbit). This means that T_1 is not injective! Observe that this converse doesn't fail if we instead consider cardinals (which are unions of 1-orbits) rather than 1-orbits. Perhaps the moral is that we should coarsen the objects on which we profess to define T-functions so that they really are injective.

Holmes has something to say about this...

Observe that definition 12 defines the function T_n only on sets that are n-orbits. We can extend it to encompass unions of n-orbits in the obvious way, as follows.

DEFINITION 14

$$T_n(\bigcup_{i \in I} A_i) = \bigcup_{i \in I} T_n(A_i)$$

(as long as each A_i is an n-orbit).

Since every cardinal is a union of 1-orbits we see that T_1, defined as in definition 14, is precisely the original T function on cardinals defined by Specker.

The various T_n *cohere* in the following sense: T_n of an n-orbit (or union of n-orbits) O_n is the union

$$\bigcup\{T_{n+1}(O_{n+1}) : O_{n+1} \text{ is an } (n+1)\text{-orbit } \subseteq O_n\}$$

DEFINITION 15 *An n-orbit τ_n is* **cantorian** *if it is fixed by T_n.*

Observe that this usage of 'cantorian' does not imply that τ_n is a cantorian *set*. NFistes have written of *cantorian cardinals* for some time, meaning *cardinal of a cantorian set*. Cardinals in NF—as usually implemented—are not cantorian sets.[7]

Presumably if τ_n is cantorian so are all the $n+1$-orbits into which it fissions ...?

Also we need to make clear that if n is n-symmetric then all the $T_k(x)$ with $k \geq n$ are identical. [ouch!! is this true??]

Perhaps we should drop the 'n' subscript and write 'T' *simpliciter*.

Let's have a few examples, as a brief reality check.

- What is $T(V)$? V is a 0-symmetric set. If you are in V then so is anything that is sent to you by a permutation that is at least j^0 of anything. So $T(V)$ is the 0-orbit of a singleton, which is of course V.
- What is $T(\iota``V)$? Well, $\iota``V$ is a 1-symmetric set. If you are in $\iota``V$ then so is anything that is sent to you by a permutation that is at least $j^1$ of anything. So $T(\iota``V)$ is the 1-orbit of a singleton of a singleton—which is to say it is $\iota^2``V$, the set of all singletons^2.

[7] An exception is the cardinal number 0, which is in fact cantorian, being $\{\emptyset\}$.

- What is $T|V|$? Well, $|V|$ is a 1-symmetric set, being a cardinal (tho' it is not actually a 1-orbit: it's a union of 1-orbits). If you are in $|V|$ then so is anything that is sent to you by a permutation that is at least j^1 of anything. Take any member x of $|V|$, and consider $\iota``x$. $T|V|$ is now the set of things that are 1-equivalent to $\iota``x$, and this set turns out to be $|\iota``V|$.

  Worth noting that $|V|$ is not a single 1-orbit but a union of orbits; in contrast $T(|V|)$ is a single 1-orbit.

Is this related to the failure of injectivity that Randall speaks of?

Must check (is this obvious?) that no harm comes from every n-symmetric set being k-symmetric for all $k \geq n$.

Isn't this a huge problem? We want to calculate $T(V)$. If we do it in the straightforward way, as above, we find that it is V. But V is a union of a hatful of 1-equivalence classes. If we take the union of T of all these 1-equivalence classes we get $\bigcup T``NC$ do we not? And that is not V!!!

Coming at T functions from a more conventional background one expects all these T functions to be the identity. In ZF of course they are. The *abode of peace* is that part of the universe where the T functions *are* the identity. Those types on which the appropriate T function is the identity are *monomorphic*. The rest are polymorphic. Computer Scientists are receptive to the idea that the default is for types to be polymorphic.

The abode of peace (dar-es-salaam, or *comfort zone*) is characterised not by the fact that when you are in it the objects around you are sets rather than proper classes, but instead by the fact that types are monomorphic rather than polymorphic. That is to say, by the fact that you have complete freedom to manipulate things.[8]

This is one of the areas where the protean nature of set theory renders it *un*illuminating rather than illuminating. Questions of set existence and questions of ease-of-manipulation-of-sets (which we are trying to keep separate here) are conflated: they all become questions about the existence of sets. "common currency makes everything look the same"

Thinking that the abode of peace is characterised by everything in sight being a set is not so much a bald error of fact (as it happens the abode of peace is indeed so characterised—but that is a mere coincidence, as i have just said) as a mistake like that of allowing one's attention to be misdirected by a conjuror . . . the conjuror in this case being dispensers of foundationalism. The difference is that in this case there are no amusements or marvels to delight the Deceived.

The only reason why the abode of peace can be characterised as that area where everything is a set is that if everything-is-a-set then you have the desired freedom of manipulation, and it is this freedom of manipulation that makes it the abode of peace. It is much more illuminating to think of freedom of manipulation in terms of type disciplines than in terms of set existence. Indeed it is in precisely the fact that typed programming languages interfere with programmers' freedom

[8]We have to be careful how we use the word 'polymorphic'. Even in NF, where the ordinals are polymorphic in the sense i mean, there is only one **sort** of ordinals. It's not a many-sorted theory.

of manipulation (and thereby prevent them from making silly mistakes) that the appeal of those languages lies.

Probably worth making the point that in the first instance (and in the first instance natural numbers are virtual) natural numbers are polymorphic at least to the extent that naturals of sets of objects are distinct from naturals of sets of naturals of sets of objects. Mostly we do our type-checking lazily because for quite a lot of the time it doesn't matter what types our variables are as long as they as—as it might be—numbers of some kind.)

4 Permutation Methods

Originally developed to prove the independence of foundation from the other axioms of ZF(C). It's a special case of a slightly more general construction ("Extracted models") which can be used to prove the independence of extensionality.

We start with a model $\langle V, \in \rangle$ of ZF. The traditional method is to define a new membership relation by taking everything that wasn't a singleton to be empty, and then set $y\ IN\ z$ iff $z = \{x\}$ for some x such that $y \in x$: it turns out that the structure $\langle V, IN \rangle$ is a model of ZFU. However there is nothing special about the singleton function here. Any injection from the universe into itself will do. So let's explore this. We start with a model $\langle V, \in \rangle$ of ZF, and an injection $f : V \to V$ which is not a surjection (such as ι).

We then say $x \in_f y$ is false unless y is a value of f and $x \in f^{-1}(y)$. (So that everything that is not a value of f has become an empty set (an *urelement*) in the sense of $\in_f$).

This gives us a new structure: its carrier set is the same universe as before, but the membership relation is the new $\in_f$ that we have just defined.

Now we must prove that the structure $\langle V, \in_f \rangle$ is a model of ZF with extensionality weakened to the assertion that *nonempty* sets with the same elements are identical.

What is true in $\langle V, \in_f \rangle$? Try pairing, for example: what is the pair of x and y in the sense of $\in_f$? A moment's reflection shows that it must be $f\{x, y\}$: if you are a member of $f\{x, y\}$ in the sense of $\in_f$ then you are a member of $f^{-1} \cdot f\{x, y\}$, so you are obviously x or y. Think about this until you are happy about it. Then try power set and sumset. (The power set of x in the new sense must be f of the set of those things that are subsets-of-x-in-the-new-sense ...). Only later should you start worrying about proving a theorem about what statements are preserved.

For use later

We define an embedding $e : V \to V$ by recursion on $\in$ by

$$e(x) =: f(e``x).$$

It's easy to show that $x \in y$ iff $e(x) \in_f e(x)$ so e is rather nice. You are probably comfortable with the idea of an **end-extension** in connection with, say, linear orders. "All the new stuff is put *on the end*". There is a corresponding notion of an end-embedding: the thing being embedded-into is an end-extension of the range of the embedding. There is a corresponding notion of end-extension in models of set theory. You have a model of set theory. Add some new sets to it. As long as none of the original sets acquire new members when you do this you say we have an *end-extension*. "No new members of old sets!". This is an important notion.

The injection we have just defined—e—is an end-embedding. It's also what we call a **$\mathcal{P}$-embedding**, namely an embedding that not only adds no new members of old sets but doesn't even add any new subsets of old sets. A $\mathcal{P}$-embedding preserves $\Delta_0^{\mathcal{P}}$ formulæ, where the class of $\Delta_0^{\mathcal{P}}$ formulæ is the smallest class containing atomics and closed under boolean operations and restricted quantification AND $(\forall x \subseteq Y)(\ldots$ and $(\exists x \subseteq Y)(\ldots$. A $\mathcal{P}$-embedding not only adds no new members of old sets (so it's an end-extension) it also adds no new subsets.

There are $\mathcal{P}$-embeddings all over the place: the embedding from a well-founded model of ZF into any Rieger-Bernays permutation model of it is always a $\mathcal{P}$-embedding.

I claim e is a $\mathcal{P}$-embedding. It is a simple matter to check that the range of e is the whole of M. It is easy too to check that anything which $\mathfrak{M}$ believes to be a subset of a thing in the range of e is also in the range of e, and this makes e into a $\mathcal{P}$-embedding.

For T a theory in the language of set theory let T^* be T with an extra unary function letter: *, and two new axioms, (i) $(\forall x)(x^*$ is an *urelement)*; (ii) $(\forall x, y)(x^* = y^* \longleftrightarrow x = y)$. We interpret T^* in T by means of a map σ defined on formulæ in the extended language as follows:

- σ of $\in$ is $\in_f$.
- σ of $y = x^*$ is to be $y = g(x)$, where g is any old injective function whose range is disjoint from the range of f. .

and we extend σ to all other formulæ by recursion.

We then check that σ of an axiom of ZF(C)U is a theorem of ZF(C)*.

Theorem 16

A formula is (equivalent to a) stratified (formula) iff the class of its models is closed under the Rieger-Bernays construction

We probably won't prove it.

5 NFU

Here we do the Boffa-Jensen consistency proof for NFU using extracted models and Ramsey's theorem.

Also ZFJ

6 The Attic

(This is what Andrey Bovykin calls the big sets of NF, the place where all the interesting mad things happen, where the wild things are. I'm not sure that Andrey is sufficiently anglicised to know about Mrs. Rochester, so perhaps this terminology is just a happy coincidence.)

What is the attic? We could take it to be (i) the collection of those sets not the same size as any wellfounded set, or (ii) the collection of those x s.t. $TC(x) = V$, or (iii) the collection of those x s.t. $|\Phi(|x|)|$ is a standard natural. (Worth recalling in this connection that, by a theorem of Bowler-Forster [1], if x is wellfounded then $|x| < T^k|V|$ for all concrete k.) (i) might turn out to be too inclusive because we don't know at this stage how to prove that there are any infinite wellfounded sets. However, in any case—since we are not going to make formal use of this concept—we don't really need to define it, so we will leave it as a piece of slang. At all events it contains big sets (*big* not *large*) that are not wellfounded.

Why should we care about the attic? 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 matters to people beyond set theory because these sets of low rank are the sets that we turn to when we want 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.) This story is usually told as *large sets giving us information about small sets*. I am offering the thought that what is really going on is that

Sets of large rank give us information about sets of small rank. (A)

That is to say, 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, because i think this leads us to profitable generalisations. In any case this can be related to the message brought to us by the incompleteness theorem: more set existence axioms gives more theorems about arithmetic. More set existence axioms gives us sets of higher rank. One thinks also of Rowbottham's refinement of Scott's theorem theorem, to the effect that if there is a measurable cardinal then $L \cap \mathbb{R}$ is countable.

What does Scott's proof tell us about the least rank of a non-constructible set?

A set is illfounded if its internal $\in$-structure is so complicated that its rank is not so much *high* as—in Cantor's sense—*absolutely infinite*. Seeing illfounded sets in this light—and bearing in mind (A), above—one would expect the sets of the attic to have things to tell us about the familiar 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, in fact—in some sense—no new Mathematics at all).[9] If we are to formulate a thesis along the lines that *illfounded sets should tell us things about sets of low set-theoretic rank* (by analogy with (A) above, then we have to get straight what flavour of illfounded sets we mean. With a view to obtaining a steer on this question, consider the illfounded sets added by CO constructions. These are clearly not going to tell us anything about sets of small rank, since they are merely wellfounded sets in wolf's clothing.

If we want novel information (from illfounded sets) 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.

Once we've got straight the concept of useful-seriously-illfounded-set we will be in a position to ask: does the attic tell us anything about arithmetic? Well, yes: the obvious shining example is the proof of the axiom of infinity![10] 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.

One of the striking facts about the attic is that it contains cardinals (whose Specker trees are) of infinite rank. (To this day it is unknown whether there are models of ZF containing sets whose cardinals are of infinite rank.) If we are to extract information about sets of low rank from the attic, perhaps Specker trees are the things to look at. Here are some ways in which we can use Specker 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 cardinals trees (whose Specker trees are) 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 a 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 $\mathrm{AxCount}_{\leq}$) tells us that there are lots of such cardinals inside $\mathcal{T}|V|$ we have sets of reals definable with parameters *from the attic*.

[9] Say something about CO models here. The CO sets all have low rank in some sense. Connect this with Kaye's *aperçu* about CO methods never giving a consistency proof for NF. CO methods won't give you any new theorems about wellfounded sets. Well of course not, duh!

[10] Careful! It doesn't seem to prove that there is an infinite *wellfounded* set.

- Let κ be any cardinal of infinite rank. Let $\mathcal{T}(\kappa)\restriction_{NO}\beta$ be 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 two-player 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. $\alpha \mapsto (\lambda n \in \mathbb{N})(\rho(\beth_n(\alpha)))$.

- $\mathcal{T}\alpha$ is a wellfounded tree and gives rise to a determinate two-player 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 a Win 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?

- Recall that in NF we define exponentiation of cardinals so that 2^α is $|\mathcal{P}(A)|$ where α is $|\{\{x\} : x \in A\}|$. This has the effect that for some cardinals α the cardinal 2^α cannot be defined. A brief look at the definition will reassure us that if $\alpha \leq T|V|$ then 2^α is defined, but that 2^α is not defined if $\alpha \not\leq T(|V|)$. So, for a cardinal α, it may well happen that the attempt to keep extending the sequence

  $$\alpha, 2^\alpha, 2^{2^\alpha} \ldots$$

  will crash at some finite stage. We now know that this object (otherwise known as $\Phi(\alpha)$) will always be a set in NF, since the property of containing α and being closed under exponentiation is stratified. Clearly $|\Phi(\alpha)| \leq \aleph_0$ always. Now let A be a suitable big set (this is only ever going to give us interesting results if A is big) and α its cardinal and consider the sequence

  $$|\phi(\alpha)|,\ |\phi(T(\alpha))|,\ |\phi(T^2(\alpha))| \ldots.$$

  We can prove that $|\phi(T(\alpha))| \geq T(|\phi(\alpha)|)+1$ always (this was proposition 8); we know that for some α it can happen that for n sufficiently large $|\phi(T^n(\alpha))|$ is $\aleph_0$ but it is known that, for at least some big α (such as $|V|$) $|\phi(T^n(\alpha))|$ is finite for at least all concrete n. If $|\phi(T^n(\alpha))|$ is finite for all natural numbers n of the model then the model has a class of natural numbers definable with a big cardinal α as a parameter. *Prima facie* it is a proper class rather than a set because its definition is highly unstratified, but—again *prima facie*—there is no obvious reason why the axiom saying that all such classes are sets should be inconsistent.

- The extensional quotient of $\mathcal{T}(\kappa)$ is a $BFEXT$, a wellfounded set picture. If κ is a cardinal of infinite rank then this $BFEXT$, too, 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.

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.

There is a temptation to think that wellfounded sets and illfounded sets are such different kinds of chap that there should be 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.

But when these results start coming in, should we believe them? That clearly depends on whether or not we 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 [9], that the authors were expecting 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 of study of the cumulative hierarchy spurs us to find explanations for our inability to find one, and stories about cumulative hierarchies are co-opted to provide them. Altho' it is clear how a belief that the cumulative hierarchy can and should be extended as far as possible can explain the Mahlo cardinals, 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—since nobody has yet refuted them—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". *non sequitur*

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 has not 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.*

Assumptions about natural numbers tell us things about the attic: $\text{AxCount}_{\leq}$ implies that $\rho(\mathcal{T}(|V|)) > \omega$, for example. But i don't think that's what people mean.

****

NF knows about certain structures (like the Specker tree $\mathcal{T}|V|)$) 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?)

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.

H I A T U S

Now let's apply this to the general question of closure ordinals of homogeneous monotone inductive definitions. Boffa asked many years ago whether or not the least set containing all singletons and closed under wellordered unions was equal to V. Boffa's question remains unanswered; in fact we do not even know how to show that the smallest σ-ring in $\langle V, \subseteq\rangle$ containing all singletons (the smallest set containing all singletons and closed under countable unions) is not

V! This will serve as an suitable subject for our study. Let $\mathcal{C}(X) =: \bigcup``(\mathcal{P}_{\aleph_1}(X))$ and consider the $\subseteq$-least set containing C_0 (the set of all countable sets) and closed under $\mathcal{C}$ and unions of chains. This set (the set of *stages*) exists, and is not trivial (*i.e.*, not the intersection of the empty set). This is because there is at least one set (V is an example) that contains C_0 and is closed under $\mathcal{C}$ and unions of chains. It is of critical importance to this construction that $\mathcal{C}$ be homogeneous, because this ensures that the property of containing-C_0-and-being-closed-under-$\mathcal{C}$-and-unions-of-chains is stratified, which in turn ensures that the intersection of all sets with this property is a set. Further, the intersection is wellordered (for the usual reasons) by $\subseteq$, and will accordingly have a length $\alpha_{\mathcal{C}}$, the **closure ordinal**. The fact that the set of stages is wellordered means that we can associate to each stage the ordinal which is the length of the wellordering of the set of those stages that are $\subseteq$ it. Thus all the members of $\mathcal{C}$ come to be decorated by ordinals, as in the following definition, which looks a bit more familiar.

DEFINITION 17
$C_0 =:$ *set of countable sets;*
$C_\alpha =:$ *set of countable unions of sets in* $\bigcup_{\beta<\alpha} C_\beta$*;*

$$C_\infty = \bigcup_{\beta \in NO} C_\beta;$$

$\alpha_{\mathcal{C}}$*, the* **closure ordinal***, is the least* α *such that* $C_\infty = C_\alpha$*.*

The function sending an ordinal α to C_α is stratified. It may not be homogeneous but that won't matter. We are interested in the closure ordinal $\alpha_{\mathcal{C}}$: we would like it to be quite small.

LEMMA 18 $(\forall x)(\forall \alpha)(x \in C_\alpha \longleftrightarrow \iota``x \in C_{T\alpha})$.

Proof: By induction on α. It is certainly true for $\alpha = 0$. Suppose $X \in C_{\alpha+1}$. Then $X = \bigcup_{i\in\mathbb{N}} X_i$ where each $X_i \in C_\alpha$. By induction hypothesis this is equivalent to $(\forall i \in \mathbb{N})(\iota``X_i \in C_{T\alpha})$. But then $\iota``X \in C_{T\alpha+1}$. ∎

limit case is easy but should probably put it in

LEMMA 19 $T\alpha_{\mathcal{C}} \leq \alpha_{\mathcal{C}}$.

Proof: We want to show that if the process has closed by stage $T\alpha$ then it has also closed by stage α. Suppose the process has closed by stage $T\alpha$, and suppose further that $X = \bigcup_{i\in\mathbb{N}} X_i$ is in $C_{T\alpha+1}$ with all the $X_i \in C_\alpha$. Then all the $\iota``X_i$ are in $C_{T\alpha}$, and $\iota``X$—which is $\bigcup_{i\in\mathbb{N}} \iota``X_i$—will be in $C_{T\alpha+1}$, which is to say in $C_{T\alpha}$ since, by hypothesis, the process has closed by stage $T\alpha$. But, if $\iota``X \in C_{T\alpha}$, we must have $X \in C_\alpha$ by lemma 19. ∎

Thus $T\alpha_{\mathcal{C}} \leq \alpha_{\mathcal{C}}$ as claimed. We would rather have the inequality going the other way, but that's life.

Now we want to examine the proof and identify the rôle played by the T function and see what scope there is for generalising the result.

In the general setting we have a homogenous $\subseteq$-monotone function ϕ which is the graph of a formula ϕ that is n-stratifiable (as it might be $x \mapsto \bigcup$ "$(\mathcal{P}_{\aleph_1}(x))$). We start with a set F_0 which is closed under T_n in the sense that if O is an n-orbit included in F_0 then $T_n(O)$ is also a subset of F_0 (F_0 could be the set of countable sets—as in the case we have just seen—in which case $n = 1$). We also want $F_0 \subseteq \phi(F_0)$. (This is true in the case we have just considered).

As in the case we have just seen we consider the $\subseteq$-least set $\mathcal{F}$ containing F_0 and closed under ϕ and unions of chains. This set (the set of "stages") is wellordered by $\subseteq$ for the usual reasons, and has a length α_ϕ, the closure ordinal, as before. We want to show $T\alpha_\phi \leq \alpha_\phi$ as before.

We stipulated that F_0 is closed under T_n in the sense that if O is an n-orbit included in F_0 then $T_n(O)$ is also a subset of F_0. This is to take care of the $\alpha = 0$ case in the inductive proof of the analogue of lemma 19. Let F_α be that element of $\mathcal{F}$ that bounds a $\subseteq$-initial segment of length α.

LEMMA 20 $(\forall x)(\forall \alpha)(x \in F_\alpha \longleftrightarrow (j^n\iota)(x) \in F_{T\alpha})$

Proof: By induction on α. It is certainly true for $\alpha = 0$. Suppose $X \in F_{\alpha+1}$. Then $X = \phi(X')$ where $X' \in F_\alpha$. By induction hypothesis this is equivalent to $(j^n\iota)(X') \in F_{T\alpha}$.

ANYWAY! the idea is that we prove the following

To be concluded

THEOREM 21 *If F_0 is closed under T in the sense that $X \subseteq F_0 \to T(X) \subseteq F_0$, and ϕ is a $\subseteq$-monotone function whose graph is a set such that $F_0 \subseteq \phi(F_0)$ then there is a least fixed point for ϕ above F_0 and the closure ordinal is cantorian.*

References

[1] Nathan Bowler and Thomas Forster. Normal Subgroups of Infinite Symmetric Groups, with an Application to Stratified Set Theory. *Journal of Symbolic Logic* **74** (2009) pp 17–26.

[2] Radu Diaconescu. Axiom of Choice and Complementation. Proc. AMS **51** (1975) 176–178.

[3] Thomas Forster , "N.F.", Ph.D. thesis Cambridge 1976

[4] Thomas Forster. Set Theory with a Universal set: Exploring an untyped Universe. Oxford Logic Guides **20** 1992

[5] Forster, T. E. Reasoning about Theoretical Entities. Advances in Logic vol. 3 World Scientific (UK)/Imperial College press 2003.

[6] Thomas Forster. Permutations and Wellfoundedness: the True Meaning of the Bizarre Arithmetic of Quine's NF. *Journal of Symbolic Logic* **71** (2006) pp 227-240.

[7] Henson, C.W. Type-raising operations in NF. *Journal of Symbolic Logic* **38** (1973) pp. 59−68.

[8] Hinnion, R. [1975] Sur la théorie des ensembles de Quine. Ph.D. thesis, ULB Brussels.

[9] H.Jerome. Keisler and Alfred Tarski. From accessible to inaccessible cardinals, Fund. Math. **53** (1964), pp. 225-308. 13.

[10] McLarty, C. [1992] Failure of cartesian closedness in *NF*. *Journal of Symbolic Logic* **57** pp. 555−6.

[11] Mitchell, E. A model of set theory with a universal set. Ph.D. thesis, University of Wisconsin, Madison, Wisconsin, 1976 (The pdf is linked from Holmes' bibliography)

[12] Pétry, A. [1976] Sur les cardinaux dans le "New Foundations" de Quine. Ph.D. thesis, University of Liège 66pp.

[13] Quine, W.v.O. [1937a] New foundations for mathematical logic. *American Mathematical Monthly* **44** pp. 70−80 (reprinted in Quine [1953a]).

[14] Rosser, J. B. *Logic for Mathematicians*. McGraw-Hill, 1953 reprinted (with appendices) by Chelsea, New York, 1978.

[15] Specker, E. P. The Axiom of Choice in Quine's New Foundations for Mathematical Logic. *Proceedings of the National Academy of Sciences of the USA* **39** (1953) pp. 972−5.

[16] Wang, H. [1952a] Negative types. *MIND* **61** pp. 366−8.

[17] Wang, H. [1949] On Zermelo's and von Neumann's axioms for set theory. *Proceedings of the National Academy of Sciences of the USA* 35 150−5.