More on Church-Oswald Models for Set Theory

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Preface

This article is the third in a series of reports from a continuing project of mine to develop and explore the Church-Oswald construction of models of set theory with a universal set. This project started with my attempt to master the first version of Sheridan's Ph.D. thesis (which has now appeared as [20]). Its first fruits were [9] and [10]) and in this third article I consider various developments not hitherto treated, such as inner-models CO-style, and iterated CO constructions. I also attempt to explain how the CO-construction is really the same as two other known constructions. Other things I shall look at are enhancements of CO that add other gadgets.

Other topics which need to be looked at include the question of which CUS-like theories are synonymous with which ZF-like theories (and we make a start on this in section **??**) and the possibility of making CO methods constructive.

I would like, once again, to thank Flash Sheridan for introducing me to this fascinating material, and for several profitable discussions of it over the years. His grasp of the history—being first-hand—is sounder than mine, and any historical errors that remain in what follows are the result of my inattention to his remonstrances. One of those remonstrances is that is that I should probably call these constructions *Church* models rather than Church-Oswald models, since Church's first publication definitely antedates Oswald's. I'm assuming that Oswald's discovery of these ideas was independent. I really should ask him! However the expression 'CO models' seems to be catching on.

I would also like to tank Tim Button for asking me lots of helpful questions . . . helpful in the sense that in attempting to answer them i have been compelled to think through things i would surely otherwise have left vague . . . "*Thomas, il faut l'écrire!*" Boffa used to say to me.

This document is intended as a kind of tutorial. I am not trumpeting the fact that much of the contents are original (even tho' they are) beco's practically all the original material is fairly elementary, and it is merely the result of my trying to work out—and *write out*—the details implicit in the work of Church, Mitchell and Sheridan. This is partly for the sake of my own understanding (one never really understands anything properly until one has written it out and attempted to explain it to someone else!)[1] but also in the hope that my efforts might be useful to others who share my desire to get on top of this stuff.

One thing I am going to try to do in this file is keep track of recent literature on CO models and ensure that all such contributions can be found in my bibliography, even if they are not discussed in the body of the text.)

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[1] "How can I know what I think until I hear what i say?"

1 Stuff to Fit in

I've just noticed that in CUS there is a definable choice function on the co-low sets.

$\{y : y \notin (x \cup y)\}$ might not exist, but – if it does – it is constructively *not* a member of x. So, if our logic is classical, we have $\{y : y \notin (x \cup y)\} \in x$ – if it exists, that is. (We get this by considering the old *aperçu* of Zermelo that $\{y \in x : y \notin y\} \notin x$ and applying this to $V \setminus x$.

But observe that, according to CUS, $\{y : y \notin (x \cup y)\}$ exists when x is cosmall – by separation for small sets, since it is a subset of the low set $V \setminus x$.

So $x \mapsto \{y : y \notin (x \cup y)\}$ is a definable choice function on the cosmall sets!! Of course there is no reason to suppose that its graph is a set of the model, tho' presumably this could be arranged.

One thing common to all CO-constructions, even the sexed-up constructions due to Church and Sheridan and me is that the old wellfounded universe consists of precisely those sets that do not have V in their transitive closure. 9/x/21

Observe that in the basic Oswald construction the Russell class is precisely the collection of co-low sets, or odd naturals.

Does the Thue-Morse sequence do anything cute in relation to the Ackermann or Oswald bijections? Let the function $\mathbb{N} \to \{0, 1\}$ that emits the appropriate bits be t. (That's what wikipæia calls it). It turns out that t is the characteristic function for the set of those naturals that encode *via* Ackermann a set with an odd number of elements.

Suppose $t(n) = 1$; look for the next power of 2 above n, that will be 2^k for some k. Now $n + 2^k$ has one more bit set than n has, which is to say that it has one more member; so, in particular, if we think of n and $n + 2^k$ as sets *via* Ackermann, their sizes are of different parities: one is odd and the other is even. Note that one is in the sequence iff the other one isn't. Check it! 0 is even co's it's $\emptyset$, 1 is $\{\emptyset\}$ so is odd; 2 is odd (it's $\{\{\emptyset\}\}$) and 3 is even (it's $\{\emptyset, \{\emptyset\}\}$). And the t sequence begins 0110. . .

Actually, Wikipædia knew this fact—even if not under this description . . . "When counting in binary, the digit sum modulo 2 is the Thue–Morse sequence" and there is a wee box to illustrate the fact. However W does not supply a proof.

Wikipædia also supplies the recursion

$$t(0) = 0; \ t(2n) = t(n); \ t(2n + 1) = 1 - t(n).$$

This strikes a chord with the Oswald bijection beco's it means that the NF_2-set it encodes is a transversal for the class of all pairs $\{x, V \setminus x\}$. I can't see any pattern. . . the first five members are

$$V, \ \{\emptyset\}, \ \{V\}, \ V \setminus \{V, \emptyset\}, \ \{\{\emptyset\}\}, \ V \setminus \{\emptyset, \{\emptyset\}\}\}$$

The trouble is that the nice easy arithmetical operations do not interact nicely with the membership relation of the Oswald model. Successor, addition

and multiplication—none of them do anything. So Morse-Thue isn't going to do anything. The fact that it gives a transversal on complementary pairs is presumably pure accident.

I've just noticed that any antimorphism is an automorphism of $\mathcal{E}$!

We want a theorem along the lines that $\mathcal{E}$ is wellfounded iff $\in$ is pseudowellfounded in the sense of the monograph. There might also be something along the lines of $\mathcal{E}$ is wellfounded iff $\in\restriction$small sets is wellfounded.

Merits attention!

The sets of a basic CO model never form a category. It should be possible (tho' it might be fiddly) to show that the identity relation on a co-low set is not a set. It cannot be low and it cannot be co-low. Perhaps there is a theorem along the lines of: *no CO model is a category*.

So here is a conjecture. Let T be a set theory that is synonymous with the theory $\{\phi : T \vdash WF \models \phi\}$. Then a model of T is not a cartesian closed category. In fact quite possibly not a category at all!

Do we know any CO model whose sets form a category?

This property, of being a set theory that is synonymous with the theory $\{\phi : T \vdash WF \models \phi\}$, seems to be a proxy for being a CO theory. It's worth asking if this has anything to do with the theory $\{\phi : T \vdash WF \models \phi\}$ being invariant. This is something to do with return permutations. If the wellfounded part of V always transitively embeds into the wellfounded part of V^σ then if every permutation has a return permutation then all models have the same wellfounded part. Now *that* would mean that NF + AxCount$_\leq$ proves the existence of V_ω, and that can't be true. In fact Holmes' clever permutation excludes that possibility. So we look askance at the idea that the wellfounded part of V always transitively embeds into the wellfounded part of V^σ. How would we prove it? By $\in$-recursion. $i : x \mapsto \sigma^{-1}(i``x)$. So presumably $i$ is suff unstratified for the replacement wot we need to get $i``x$ to fail.

If we equip a model of ZF with a primitive surjective pairing function and perform the basic CO construction, does the resulting model have a primitive pairing function? We have $k : V \longleftrightarrow V \sqcup V$ and $p : V \longleftrightarrow V \times V$ with inverses fst and snd. We want a bijection $h : V \times \{0,1\} \longleftrightarrow (V \times \{0,1\}) \times (V \times \{0,1\})$. That sounds do-able. Let's devise a function that takes a member x of $V \sqcup V$ and gives back a pair of such things.

$$x \mapsto k^{-1}(x) \mapsto p(k^{-1}(x)) \mapsto \langle k(fst(p(k^{-1}(x)))), k(snd(p(k^{-1}(x)))) \rangle$$

but what does that mean? A pair of two low sets is low and a pair of two co-low sets is co-low, but what of a pair of a low and a co-low set?

No, here's how to do it. Make *four* disjoint copies of V: V_0, V_1, V_2 and V_3—to contain pairs of the four flavours `low, low, low, co-low, co-low, low`

and `co-low, co-low`. We will require $V_0 \cup V_1 \cup V_2 \cup V_3 = V$. We also need a dyadic function k s.t. k_i (curried) is a bijection between V and V_i.   Write this out

Tim Button's endeavour to prove an omnibus synonymy result for CO-like theories has caused me to look again at Church's 1974 paper. It's horrible. There are two points that came up in this re-reading.

(i) Get straight the definition of j-cardinal;

(ii) Properly understand why the j-cardinals have complements.

(i) I now think that Church's definition of j-cardinal is just the wrong definition. He should be using n-cardnals. I don't think replacing Church j-cardinals with NF-style n-equivalence classes will make any difference to the construction.

(ii) How do we add a j-cardinal for a low set x? We don't want to add one for every low set, co's that gives us duplicates. What Church does is use AC to pick one low set from each family of equivalent sets, and then give *that* a j-cardinal. So all we need is a classifier for n-equivalence, and this is a well-understood situation. If we have foundation we can use Scott's trick. It may be worth recording that Savelieff's axiom is enough.

It would be nice to cook up a CO model in which there is a GFP for $x \mapsto$ set of transitive subsets of x. I think we want a set x_0 s.t if we define $x_{n+1} = x_n \setminus \{x_n\}$ taking intersections at limits then $\bigcap \{x_\alpha : \alpha \in On\}$ (which is not a set of the model!) is precisely the collection of von Neumann ordinals. I think such an x_0 will do the trick. We have to arrange for all the x_i to be hereditarily transitive. N.B., you can't just "cap off' the LFP by adding a "point at infinity" the way you get co-$\mathbb{N}$ from $\mathbb{N}$, because we have subcision. If $X = On \cup \{X\}$ then $On = X \setminus \{X\}$ is a set by subcision. The sequence of xs has to be indexed by the von neumann ordinals (which is a proper class) lest it be a set by replacement.

Second-order logic is the Work Of The Devil as we all know, but there are still some points worth making. It is a commonplace that ZF is second-order categorical in the sense that, for each ordinal, it has at most one model (up to isomorphism) of that height. There is a standard story about how this relies on replacement. The same presumably goes for Church's Universal Set Theory, or at least the simplified two-wand version according to which every set is low or co-low, the version that Tim Button calls BLT[2]. The categoricity will be proved not by an induction over $\in$ (as it is done in ZF) but over $\mathcal{E}$. ($x \mathcal{E} y$ iff $(x \in y) \longleftrightarrow \mathtt{low}(y)$). It's another nice illustration of the importance of $\mathcal{E}$. Someone should write out the details. Probably Muggins.

Here goes. Suppose i have two models $\mathfrak{M}$ and $\mathfrak{N}$ of second-order ZF. The idea is to use replacement to define an isomorphism between them by recursion on the membership relation of one of them. Either will do. (They're isomorphic, after all). Let i be the isomorphism we are building: $i : \mathfrak{M} \to \mathfrak{N}$. Suppose we have defined $i(y)$ for every $y \in x$. What is $i(x)$ going to be? The theory in which

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[2]The initials (of course) mean *Button, Lettuce and Tomato.*

we are discussing this construction enjoys replacement—by assumption. So it knows that $i``x$ exists. This $i``x$ is a subset of (the carrier set of $\mathfrak{N}$... indeed a *bounded* subset. But $\mathfrak{N}$ has second-order separation, so every bounded subset of a member is a member. So $i``x \in \mathfrak{N}$ and is the desired destination for i applied to x.

Now we have to do the same for second-order models of BLT. This time we do the induction on $\mathcal{E}$ not $\in$. Let $\mathfrak{M}$ and $\mathfrak{N}$ be two models of the second order version of BLT. Let x be a $\mathcal{E}$-minimal element of $\mathfrak{M}$ on which we have not yet defined i. x is either low or co-low. (It is an axiom of BLT that every set is low or co-low). If x is low we proceed exactly as in the ZF case. If x is co-low, things are more complicated but only slightly. We want to say something like $i(x) = V \setminus i``(V \setminus x)$, with the Vs decorated with '$\mathfrak{M}$' or '$\mathfrak{N}$'.

Since x is co-low we have $y \in (V_{\mathfrak{M}} \setminus x) \to i(y)$ is defined. Since we have replacement in the metalanguage this means that $i``(V_{\mathfrak{M}} \setminus x)$ exists. Further it must be an element of $\mathfrak{N}$, since this "low" case is analogous to the ZF case we saw earlier. But then $V_{\mathfrak{N}} \setminus i``(V_{\mathfrak{M}} \setminus x)$ is an element of $\mathfrak{N}$ since $\mathfrak{N}$ is closed under complementation, and it is the $i(x)$ that we desired.

I suppose i should have been explicit about what i mean by second-order CUS (BLT). First-order CUS/BLT is: extensionality; complementation; the wellfounded sets are a model of (second-order) ZF; every surjective image of a wellfounded set is a set.

BLT is also going to be *tight* in the sense of recent work of Visser, Friedman, Hamkins and Enayat:

Bi-interpretation in weak set theories Hamkins and Roque Freire JSl 2020

Variations on a Visserian theme, Enayat

When Bi-Interpretability Implies Synonymy

Visser, Albert; Friedman, Harvey M. (2014) Logic Group preprint series, volume 320

Is a CO theory one that doesn't try to construct large classes for which there is a recurrence problem? (see [11] for definition of recurrence problem.

Can't why i didn't think earlier of banging this drum. The point about transitive models in the ZFiste context is that they are honest about $=$ and $\in$: There is always a witness to the distinctness of two distinct sets. We need the same concept of transitive (sub)model for CO structures. Perhaps it means no more than that all its small elements are subsets and that complementation is honest. But then we do need to know what a small set is.

Might be a useful illustration of the method to construct a CO model with a global choice function that is a set.

Flash suggests that perhaps all 'natural' CO models satisfy the $\forall\exists$ formula $x \in x \to x \setminus \{x\} \in x$. He's happy to conjecture it but isn't going to try to prove it, bless him. I think they do in fact satisfy it, and it's probably for the same reason that the term model for NF_2 satisfies a version of the $\forall\exists$ conjecture.

1.1 Might there be an abstract account of what a CO theory is?

Think of a collection (it might not even need to be a set) $\mathcal{W}$ of words, or wands. $\mathcal{W}$ has a designated member 0. We work in a theory T of wellfounded sets. There is a bijection k (for $k$ode) $V \times \mathcal{W} \longleftrightarrow V$, and a membership relation defined using $\mathcal{W}$. Starting witha model $\mathfrak{M} \models T$ this construction gives us a model $\mathfrak{N}$ of a new theory which we will call S. One clause of the definition will be $\mathtt{snd}(k^{-1}(y)) = 0 \to (x \in y \longleftrightarrow (x \in \mathtt{fst}(k^{-1}(y)) = 0)$ If $\mathtt{snd}(k^{-1}(y)) = 0$ we say that y is a **low** set of the new model $\mathfrak{N}$. The idea is that the hereditarily low sets of $\mathfrak{N}$ are an isomorphic copy of the model $\mathfrak{M}$ we started with, and further are precisely the sets that are wellfounded in the sense of the new model $\mathfrak{N}$.

We now claim that, as long as we have set this up correctly, S and T are synonymous, and T is the theory of the wellfounded-sets-according-to-S.

1.2 Forster [10] 10 years on..?

My reason for interest in CO models is different from Church's (I can't speak for Urs Oswald). Church was interested in Set Theory with a universal set because he was interested in implementing in set theory various mathematical objects that arise from equivalence classes and—indeed—in implementing them literally *as those equivalence classes*. My reason for interest is quite different. For me the appeal of CO sets is that they can be made to seem fully as natural and as obvious as wellfounded sets, while not being wellfounded. For me this is a way of pointing out a confusion that i detect in the mainstream *ZFiste* literature. We are confusing two thoughts, one of them good (that wellfounded sets are interesting natural things worthy of study) and the other bad (every set is wellfounded). Unlike Church I do not believe that CO models are in any sense an improvement on cumulative hierarchy sets. In fact I have hoped for years that someone would prove a synonymy result connecting ZF-like theories with CUS-like theories (and i tried to bully one of my Ph.D. students into proving it but he was impervious to all my threats and blandishments) since that would be the perfect way to make the point that set theories with a universal set can be precisely as natural as theories of wellfounded sets. That is why I am so pleased that Tim Button seems to have proved such a result.

Could do something with an ordered pair function in the Oswald model for AST. Decode a set into a pair by taking the odd bits and the even bits. Of course this gives us streams too! I think all streams are eventually constant. Trouble is, this doesn't work in the basic Oswald model, since we don't get pairing: if you interleave a finite set with a cofinite set you get nothing. But you can form pairs of similar things. If you brutally add pairs in this style you get something like the model with the odds and the evens, discussed below

In this connection (thinking about synonymy) there may be virtue to be derived by thinking of CO models as a complication/generalisation of Rieger-

Bernays ("RB") models. In both cases you are obtaining a new model from the old by *keeping the same carrier set* and equipping it with a new membership relation that is definable in the model you started with. In both settings you have—in principle—the possibility of reversing the move from the first model into the second. Nathan Bowler writes in the RB context of the *return* permutation, and I think that is a better word than 'inverse' which is the one might o/w be tempted to use.

Actually (and this is what has *only just*—to my shame—occurred to me) one can think of the $\in$-membership of the CO model in *literally* the same way as one thinks of the $\in$ in the RB permutation model, as $x \in \sigma(y)$ where $f : V \longleftrightarrow V \times \{0,1\}$ and σ is

$$\lambda y.(\texttt{let } f(y) = \langle y', \texttt{flag} \rangle \texttt{ in: if flag} = 0 \texttt{ then } y' \texttt{ else } (V \setminus y'))$$

... which is a function all right. Is it injective? It's injective iff no set has a complement, so this is not a trick to play in NF. So how about in ZF? It's injective; is it a permutation? Well, no, since some of its values are complements of sets. And (if we are thinking of iterated CO constructions or return permutations) how definable is σ in the CO model? It'as not looking good, co's the coding function f that we need is certainly not going to be a set of the model, tho' that probably doesn't matter. If we can reliably find a return permutation then we can perhaps get an omnibus synonymy result like the one we are hoping to get for RB methods. The parallel looks promising. Looking ahead, the omnibus synonymy result for RB models will be part of a story that mathematics is strongly typed; what will the synonymy story for CO constructions mean..?

In fact the possibility of thinking of CO models as permutation models in this way makes them look very like extracted models (of the kind one uses to show the independence of extensionality or the consistency of NFU). It sounds as if there should be a unified treatment of all three kinds of models. And it could be that that unified treatment could be narrated as arising from the di Giorgi picture of Set Theory as recounted to me by Furio Honsell and Marco Forti. It is a commonplace that any extensional binary relation on a set A can be thought of as an injection from A into $\mathcal{P}(A)$. di Giorgi's idea is to think of all extensional structures for $\mathcal{L}(\in, =)$ as injections in this way. Then a family of R-B models corresponds to a choice of a set A of atoms and a collection of injections $A \hookrightarrow \mathcal{P}(A)$ all with the same range.

1.3 T-suitability

Quantifiers as subsets of $\mathcal{P}(D)$ is due to Montague and Mostowski.

The Mostowski analysis of monotone quantifiers seems to make all expressions Δ_0. $(\forall x)(\ldots)$ becomes $(\exists X \in \forall)(\forall x \in X)(\ldots)$; $(\exists x)(\ldots)$ becomes $(\exists X \in \exists)(\forall x \in X)(\ldots)$. We knew this from NF of course. In terms of my current preoccupation this says merely that V and $V \setminus \{\emptyset\}$ are not T-suitable for any sensible T.

But of course the notion of T-suitability has to be rethought now we have a different notion of restricted quantifier, using $\mathcal{E}$. V is CUS-suitable in this new

sense.

1.4 CO Models give us the Correct Way to Understand the Limitation-of-Size Principle

We are all agreed that rational numbers are finite *objects*; however if we think of them as sets (in the morally correct field-of-fractions way) then they are (literally) infinite (sets). Nobody thinks that this is a reason for not seeing rationals as finite objects. We should also accept that V, too, is an infinite set but a finite object. Church's *intermediate sets* from [5] (e.g., NO) are not finite objects. The key insight behind CO-models is that the sensible idea is not limitation of size, but limitation of *metaphorical* size: V is a finite object and is therefore acceptable according to the limitation-of-size principle *properly understood*: if we correctly understand the limitation-of-size principle then we see that CO theories respect it.

Notice that, for any $X \subseteq \mathbb{N}$, X and $\mathbb{N} \setminus X$ have the same Turing degree. $\mathbb{N}$ is a very simple object!

2   Definitions and Background

ZF and its congeners are theories of sets that all wellfounded, or at any rate *small*. Models of such theories look like this:

$$V_\omega$$

$$\emptyset$$
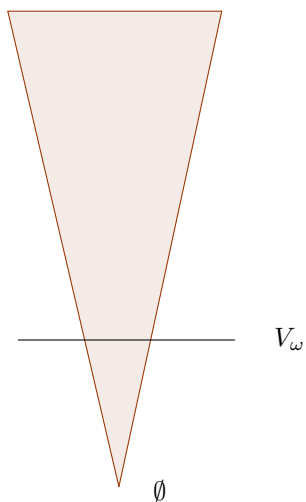
CO models are structures for $\mathcal{L}(\in)$ (the language of set theory) in which (among other things) every set has a complement. These structures look instead rather like this:

V

Co-low

$$H_{LOW}$$

$$\emptyset$$
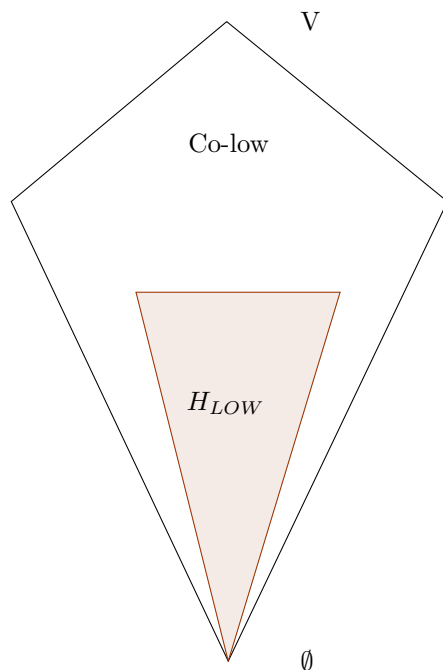
The shaded areas are the same in the two pictures (sort-of!); the difference is

that the CO picture has co-low stuff as well. But read on—all will be revealed.

2.1 Background

The background needed here can be found in the briefing papers [9] and [10]. They explain what is going on in [5] and [17], and (I think and hope) make it possible to skip those papers—at least initially! I am not suggesting that readers should avoid [5] and [17] altogether: there will surely be subtleties in those papers that I have overlooked or forgotten and have in consequence neglected to cover. If you want to know about CO models it should be possible—at least initially—to stick to [9], [10] and the present paper, so readers of the present paper are advised to have [9] and [10] to hand.

The two approaches in those two papers appear at first blush to be quite different: [9] explains the coding construction used by Church and Oswald in [5] and [17] to create big sets from little ones; [10] shows how one might sensibly believe that the big sets were there all along, by postulating a recursive construction of the set-theoretic universe that endows every set with a complement at birth. One theme of the present paper will be a desire to unify these two approaches, and show in particular that the big sets appearing in the two developments are the same big sets. This will be the subject of section 3. Once we have established that the two approaches are essentially the same we are free to develop the underlying mathematics, equivocating cheerfully between the various approaches as seems most convenient.

We will show how to spice up the original constructions of Church and Oswald by adding lots of things: power sets, principal ultrafilters, Church-style j-cardinals, antimorphisms—even if not all at once!

In section 4 we will consider the possibilities of iterating the constructions. We will also consider the possible analogues in this setting of the inner-model constructions (HOD, L etc) and Zermelo cones (the V_αs) familiar from ZF(C).

Finally we consider the nature of the ultimate limitations on this process: Church mused about the possibility of connections with Quine's NF, and this raises some interesting problems.

Later versions of these notes might contain an investigation into the possibility of Church-Oswald constructions giving rise to results saying that this-or-that extension of Amorphous Set Theory (see definition 1 below) is synonymous with that-or-this extension of Kripke-Platek. (The quip in [10] about the tale of the two dreamers shows that this question was already on my mind at that time, even tho' i didn't consciously realise it).

2.2 Definitions

We use upper-case 𝔉𝔯𝔞𝔨𝔱𝔲𝔯 characters to denote structures, and the corresponding upper-case Roman letter to denote the carrier set of the structure.

The word **moiety** will be overloaded. It might denote an infinite, co-infinite set, or it might denote a proper class whose complement is also a proper class. The reader should embrace the associations of the French word *moitié*.

We start with definitions of some set theories.

DEFINITION 1

Amorphous Set Theory *is the set theory whose axioms are extensionality, complementation, existence of V and an axiom scheme of existence of unordered n-tuples for concrete n. Hereafter it is AST*[3].

NF_2 is AST plus axioms saying that the universe is a boolean algebra with $\subseteq$, $\setminus$, $\cup$ and $\cap$. (The subscript '2' is an allusion to the fact that NF_2 can be axiomatised by the set of those axioms of NF that are 2-stratifiable.) NF_2 (unlike AST) is finitely axiomatisable and NF_2 (unlike AST) proves the two principles:

> **Subcision***: Existence of $x \setminus \{y\}$;*

and

> **Adjunction***: Existence of $x \cup \{y\}$.*

These two are equivalent as long as our logic is classical and we have an axiom of complementation[4] *They are worth identifying because they can appear as axioms in other contexts as well. The terminology is due to Allen Hazen.*

$B(x)$ is $\{y : x \in y\}$: the principal ultrafilter generated by $\{x\}$ in the boolean algebra $\langle V, \subseteq \rangle$; $\overline{Bx}$ is $V \setminus B(x)$.

NF0 is NF_2 plus an axiom saying $B(x)$ exists for all x.

The '0' in 'NF0' is an allusion to the fact that NF0 can be axiomatised by extensionality plus the existence of $\{x : \phi(x, \vec{y})\}$ where ϕ is stratifiable and contains no quantifiers. The '0' is not a subscript, and the notation is not part of Oswald's system of notation of the NF_n and N_nF. (I'm not going to explain that system of notation because I am not planning to use it)

The name 'Amorphous Set Theory' is Holmes' idea. His thinking is that the universal set of a model of *AST* can be—at least *internally*—amorphous: an infinite set that cannot be split into two infinite pieces.

AST is clearly a fragment of NF_2, and a proper fragment at that: we will find structures that are models of *AST* but not of NF_2.

> Does NF_2 go back any earlier than Grishin [13]? The earliest references in the philosophical literature seem to be much later ... Allen Hazen writes: "For the record: "Parsons Set Theory" (Second Order Logic with the restricted version of Frege's V that postulates only that "small" and "co-small" classes have objects as their extensions) is mentioned on p. 186 of [4]. Boolos's discussion of it is in [1]: the consistency proof, using a model equivalent to what I describe as the term model of terms built up with brackets and anti-brackets is in the footnote on pp. 234–235 in [2]".

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[3] ... actually i am not sure whether or not to include subcision and insertion in AST.
Albert sez: Adjunctive set theory (without extensionality) is in [18].
He also says that a *sequential* theory is one into which we can interpret AST.

[4] We do not here engage with these ideas in a constructive setting, but there are issues one might wish to consider. A set is finite iff it belongs to every set that contains $\emptyset$ and is closed under insertion. Dually one could say that a set is finite iff $\emptyset$ belongs to every set containing it and closed under subcision. Constructively these definitions are not equivalent.

We appeal (as in [10]) to Kripke's happy image of sets being created by *lassoing*. When we throw out a lasso the plural object that it captures is a **preset**. A preset is turned into a set by the use of a **wand**. In the original conception of sets-constructed-iteratively there was only one wand. This (rather special) wand is the **vanilla** wand, the wand that turns a *preset* into a *set* with the same members. W is the collection of wands. It's not expected to be a set of the model—indeed even the individual wands themselves are not objects of the model—but is going to be a set from the point of view of the metatheory. S is the collection of presets.

The Church-Oswald construction with just two wands (the complement wand and the vanilla wand) starts with a structure $\langle V, \in \rangle$ for $\mathcal{L}(\in, =)$ and construct a new one by means of a bijection $k : V \longleftrightarrow V \times \{0, 1\}$. In this setting we speak of $\langle V, \in \rangle$ as the **ground model**.

Any set theory that has CO models or that can have models built for it by a multiple wand construction will say there is a map $i : V \hookrightarrow \mathcal{P}(S \times W)$. i is a kind of omnibus uniform global destructor: every member of $i(x)$ is an ordered pair of a lasso-contents (preset) and constructor ('W' connotes the *wand* that gave rise to x)[5]. If we can somehow ensure that each set can be constructed in only one way then (the second component of) each value of i will be a singleton, so we will be able to think of i as an injection $V \hookrightarrow (S \times W)$. Until further notice we will restrict ourselves to constructions where this uniqueness condition is satisfied, so that i can be thought of as an injection $V \hookrightarrow (S \times W)$. Unless we do this, the following definition will not make literal sense.

DEFINITION 2 *The relation $y \in \mathtt{fst}(i(x))$—that is borne to a set x by the members y of the preset from which x was constructed—is the* **engendering relation** *and we write it $x \mathcal{E} y$. Because our constructions are iterative (so that every object has a particular [ordinal] stage at which it is first seen) the engendering relation is wellfounded. The* **birthday** *of a set is of course the stage at which it is created—which of course is the same as its rank under the engendering relation.*

I use the Conway-ism [6] 'birthday' here instead of 'rank' because I want to reserve this latter word for its usual—more general—function of describing the ordinal complexity of wellfounded relations (and of course also the set-theoretic rank—of wellfounded sets).

We add to the language of set theory a new symbol '$\mathcal{E}$' whose intended denotation is the engendering relation. Be warned that $\mathcal{E}$ is not extensional! However it does have a property that is a kind of analogue of extensionality. The membership relation $\in$ is extensional in that it corresponds to an injection $A \hookrightarrow \mathcal{P}(A)$; $\mathcal{E}$ corresponds to an injection $A \hookrightarrow (\mathcal{P}(A) \times W)$.

As things stand we are thinking of a CO model as a structure for the language of set theory (which is certainly how Church and Oswald were thinking) but we

Have i stated this correctly??

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[5]I find myself increasingly inclined to think of sets as not being *deconstructed* (by destructors) but *dismembered*.

will often expand it to a structure for the expanded language with the new binary relation symbol.

2.2.1   Low = Small

Let us consider the two-wand construction of [10] and [9].

DEFINITION 3
A set is **small** *if the set of birthdays of its members is bounded.*
A set is **low** *if it is created from a preset by use of the vanilla wand.*

That is to say, a low set is a set with the same contents as the preset from which it is created.

REMARK 1 *For all x, x is low iff x is small.*

Proof:
   Every Low set is small
   This direction is obvious.

   Every small set is low.
   If x is small then one might expect that x could have been created by the vanilla wand, and its birthday will of course be the sup of birthdays–of–its–members–plus-one as usual. Given that we lasso everything we can (at each stage every preset that *can* get lassoed at that stage *does* get lassoed at that stage), then any set whose members are of bounded birthday (and therefore in principle *can* be created by the vanilla wand) *will in fact* be created by the vanilla wand. This being so, then—for any small set—all its members are of earlier birthday, so $\in$ restricted to small sets is wellfounded.
   ∎

Readers should bear in mind that "Low" does not imply "wellfounded": $\{V\}$ is low but not wellfounded. However it is easy to arrange that the *hereditarily* low sets of the CO model are precisely (an isomorphic copy of) the original wellfounded structure with which we started. This is explained in detail in [9].

Reflect that in the simplest CO models **low** is definable: x is low iff $x \notin x$!

DEFINITION 4
An **antimorphism** *is a permutation π of the universe such that*

$$(\forall xy)(x \in y \longleftrightarrow \pi(x) \notin \pi(y)).$$

Antimorphisms that are also involutions are **polarities***.*

We shall see (p. 30) that every two-wand model has a canonical antimorphism which can be defined by recursion on $\mathcal{E}$. (This is in [9].) We prove by induction on $\mathcal{E}$ that the canonical antimorphism is unique: if there were two

[margin notes:]
Is there a third equivalent? "same size as a wellfounded set"?
How much replacement does this use? This needs work.

Find the right place to put this remark
Say something about dfn of $\mathcal{E}$ when freeness fails

distinct antimorphisms their product would be a nontrivial $\in$-automorphism. And the nonexistence of nontrivial $\in$-automorphisms is one of the many things one can prove by a wellfounded induction on $\mathcal{E}$. Since the square of an antimorphism is an $\in$-automorphism we can infer that the canonical antimorphism will be an involution—a polarity.

The canonical antimorphism is of course not a set of the model: it is an *external* rather than an *internal* antimorphism. We shall see later (p. 37) that with some ingenuity we can obtain models of NF_2 with an internal antimorphism.

We can now define the two $\mathcal{E}$-restricted quantifiers: $(\forall x \, \mathcal{E} \, y)(\ldots$ and $(\exists x \, \mathcal{E} \, y)(\ldots$.

DEFINITION 5
The class of $\Delta_0^{\mathcal{E}}$ formulæ is the closure of the set of atomics under propositional connectives and the $\mathcal{E}$-restricted quantifiers.

Then we define the classes of $\Sigma_n^{\mathcal{E}}$ and $\Pi_n^{\mathcal{E}}$ formulæ in the obvious usual way. It may be worth noting that

REMARK 2 *If collection holds in the ground model then we can prove that for each n the classes of $\Sigma_n^{\mathcal{E}}$ and $\Pi_n^{\mathcal{E}}$ formulæ are closed under $\mathcal{E}$-restricted quantification.*

Proof: Suppose $(\forall x \, \mathcal{E} \, y)(\exists w)(\ldots)$. Then the collection of x s.t. $x \, \mathcal{E} \, y \wedge (\exists w)(\ldots)$ is a definable class of the ground model and the ranks of its members are bounded. Therefore it is a set of the ground model. By collection (in the ground model) there will be a set that collects *relata* for such x and it will be low.

I'm assuming that the proof for the bounded existential quantifier is analogous. ∎

This version of restricted quantification genuinely is the correct notion for CO structures. The reason why restricted quantification is logically simpler than unrestricted quantification is that the search we are committed to in ascertaining the truth value of $(\forall y \, \mathcal{E} \, x)\phi(x, y)$ is a search not over all y but a search over all those y that we are somehow "given" when we are "given" x. In the wellfounded case if $y \in x$ then we genuinely are given y when we are given x. In the Church-Oswald setting the ys that we are given when we are given x are the y such that $y \, \mathcal{E} \, x$. To return briefly to the discussion earlier of limitation of size, where the point is made that CO models capture limitation of *metaphorical* size, perhaps the point could be better made by using the word 'information'. When $x \, \mathcal{E}, y$ one certainly has the feeling that y contains more information than x.

This introduces us to the phenomenon of *the conflation of $\in$ and $\mathcal{E}$ in the wellfounded case* which is a topic which we must now address.

2.2.2 The Conflation of $\in$ and $\mathcal{E}$ in the Cumulative Hierarchy

6

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[6] Do we want to think of the engendering relation as the relation borne to x by the-members-of-the-preset-from-which-x-was-created? Or do we want it to be the ancestral (transitive

Set theory on the iterative plan is animated by two spirits: there is the membership relation $\in$, whose combinatorics constitute the subject matter of set theory (set theory however conceived, iterative or not), and there is the engendering relation $\mathcal{E}$. In the cumulative hierarchy these two spirits inhabit the same body (they have the same extension, the same graph). But they are different intensions for all that, and the distinction between them becomes clear in the setting in which we now find ourselves.

> There is a possibly useful parallel here with the contrast between $<_{\mathbb{N}}$ and $<_Z$. The first is clearly (the ancestral of) an engendering relation and the second is not: you can't do induction on $<_Z$.

When approaching the study of CO models from the ZF-iste point of departure one has to take take on board the novel thought that there are two spirits not one, and one has to take thought to consider which—if any—of the '$\in$'s one had hitherto been dealing with (in the cumulative hierarchy context) might turn out to really be '$\mathcal{E}$'s. For example, in the axiom of restriction (notoriously hard for beginners to get their heads round) one of the '$\in$'s is really an '$\mathcal{E}$'. What the axiom of restriction is really saying is "Every nonempty set has an $\mathcal{E}$-minimal ($\in$)-member" which (in plain English) is "$\mathcal{E}$ is wellfounded". If $\in$ and $\mathcal{E}$ are both wellfounded it is only because $\mathcal{E}$ is wellfounded: we find that typically any induction on $\in$ that we see in ZF is really an induction on $\mathcal{E}$. Here are some examples:

1. The forcing relation is defined by recursion on the engendering relation $\mathcal{E}$ rather than on $\in$. See [9]

2. The proof that the cumulative hierarchy is rigid is really a proof by induction on the engendering relation $\mathcal{E}$. This becomes clear when we consider a proof of the analogous result for the model produced by the two-wand construction.

2.3 Gettings things free

What do we mean by *getting things free*? If we get closure under a particular operation beco's we have inserted a clause for that operation into the CO construction then clearly that closure was explicitly purchased. Sometimes we get closure under an operation without having explicitly budgeted for it—for example the Oswald model for *AST* has closure under the finitary boolean operations $\cup$ and $\cap$—and indeed even the *in*finitary (unary) boolean operations $\bigcap$ and $\bigcup$.

---

closure) of this relation? Think of the recursive datatype $\mathbb{N}$ of the natural numbers: do we want to think of the engendering relation of $\mathbb{N}$ as the successor relation S... or as the order relation $<_{\mathbb{N}}$? It doesn't much matter: in both these cases both candidates are wellfounded and will support induction, and typically inductions over a wellfounded relation can be contorted into inductions over its ancestral (transitive closure) and *vice versa*. Readers will be familiar with strategies for transforming a proof by means of one of "mathematical" or "strong" induction into a proof by means of the other, and what we are considering here is merely more of the same.

We lose this closure once we add a clause for power sets, à la Mitchell [16]. (See section 8.1.) There is a point to be made here about how closure under $\bigcup$ and $\bigcap$ is a consequence of the *beschränkheitsaxiom* that says that very set is finite or cofinite.

This phenomenon of getting things free will matter when we start thinking about which theories with a universal set are synonymous with which theories of wellfounded sets.

Both the two wand construction of [9] and the original constructions of Church and Oswald give us models in which explicitly every set has a complement. The constructors of those models correspond exactly to the axioms of *AST*. However the model actually satisfies a great deal more than that: it is a boolean algebra (and so a model of NF_2) and even satisfies the union axiom. We don't get cardinals-(as-equipollence-classes) free, beco's the cardinals of low sets would be neither low nor co-low. (However we do get cardinals of co-low sets because the relation of equipollence between co-low sets is empty—none of the bijections needed are available beco's they would be neither low nor co-low: no co-low set is the same size as anything, not even itself!)

Another thing we get free is replacement for low sets: every surjective image of a low set is a (low) set. We also get all instances of replacement (which says that $f``x$ is a set for all $x$) for all sets, where the function $f$ is a permutation of the universe. Let $\sigma$ be an (external) permutation of the universe, and $x$ any set. Then $\sigma``x$ is a set of the model. Why? Since every x is finite or cofinite, likewise $\sigma``x$ is either finite or cofinite, and all finite and cofinite classes are sets.

2.3.1 The Potemkin Village Problem

Does this offer hope that CO constructions will give us models for richer theories? No, beco's we never get stuff free when it is likely to be of any use. This is illustrated by the fact that if we spice up the original construction to ensure that everything has a power set—as Mitchell very skilfully does in [16]—then we find that the universe is no longer closed under the operations that the original construction gave us "free"—the binary unions and intersections. In the long run you can get out of CO constructions only what you put into them in the first place. This is what Sheridan calls the *Potemkin village problem*.

2.4 Definability of the new notions

Are the predicates *low* and '$\mathcal{E}$' definable in the CO model? Is the original $\in$ relation definable in terms of the new one? Mostly it makes sense to think of the CO structure as an expanded structure for a larger language with these added predicates but it's worth thinking about whether this is really necessary. Is low replacement axiomatisable (in the language of set theory)? Can we axiomatise in the language of set theory the fact that the hereditarily low sets are precisely the wellfounded sets?

If we can define one of *low* and $\mathcal{E}$ then we can define the other

DEFINITION 6

$$x \, \mathcal{E} \, y \longleftrightarrow (x \in y \longleftrightarrow low(y))$$

and

$$low(y) \longleftrightarrow (\forall x)(x \in y \longleftrightarrow x \, \mathcal{E} \, y)$$

Certainly for some particularly simple CO constructions *low* can be defined: for example in the simplest two-wand construction a set is low iff the restriction to it of the identity relation is a set. But that depends on special features of that model.

3 Explaining the relation between CO models, constructions with atoms, and multiple wand constructions

Here's the patter:

> *The two-wand construction provides the motivation for the axioms;*
> *The CO-construction provides the relative consistency proof.*
> *The construction using atoms constructs the di Giorgi map recursively.*

The three constructions of the title are

(i) CO constructions as in [5] and [17],
(ii) Multiple-wand constructions as in [9] and
(iii) Constructions that start from an initially structureless family
      of atoms into which life is breathed by a transfinite iteration.

I supply no reference for (iii) since—although it is a very good and very natural idea that has been discovered independently by everyone[7] who has investigated this matter, none of them (so far as i am aware) has ever published their treatment.

We need to say something reassuring about how every CO construction can be thought of as an iterative construction. If we want to think of a model arising from a CO construction as arising from a two-wand construction with engendering relation $\mathcal{E}$, then this relation is going to have to (i) satisfy $x \, \mathcal{E} \, y$ iff $x \in \mathtt{fst}(k(y))$ ("x can be found in the preset that y was made from") and (ii) $\mathcal{E}$ is going to have to be wellfounded.

To ensure that $\mathcal{E}$ is wellfounded it is clearly sufficient that $\rho(y) > \rho(x)$ for any $x \in \mathtt{fst}(k(y)$. This will follow if $\rho(y) \geq \rho(\mathtt{fst}(k(y)))$, and this last is easy to arrange. If we do not ensure that the function k satisfies this constraint we will

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[7] "Everyone" may be an overstatement, but this idea certainly occurred to both Sheridan and Mitchell.

find that the resulting model contains "gratuitously" illfounded objects such as Quine atoms.

(The other direction is fairly easy.)

3.0.1 Doing it with atoms: the third way

We start in a set theory with a suitably large supply of atoms[8] A, and we define a recursive construction over it. This construction takes the form of gradually building up an injective map $A \hookrightarrow \mathcal{P}(A)$. Marco Forti tells me that this way of thinking of models of set theory is due to his mentor Ennio di Giorgi, so i shall call the map a **Di Giorgi** map.

The construction procedes as follows. Initially no atoms have been used as labels. We will arrange that no act of labelling a set with an atom will ever be countermanded, so the partial di Giorgi map we construct will $\subseteq$-increase monotonically.

At each stage α we have a set A_α of atoms that have been used as labels so far, and a map $f_\alpha : A_\alpha \hookrightarrow \mathcal{P}(A)$. A_0 and f_0 are empty.

At stage $\alpha + 1$ we label with unused atoms all subsets of A_α that have not already been labelled. We also label with with unused atoms all complements (with respect to A) of subsets of A_α that have not already been labelled. This creates new pairs that we add to f_α to get $f_{\alpha+1}$. Thus—for example—A_1 has two atoms in it: the label for the universe (A) and the label for the empty set.

At limit ordinals λ we take unions of earlier A_α's and take unions of earlier f_α.

Naturally we organise matters in such a way that we run out of unused atoms only at a limit stage!

One way of thinking about this development[9] is as a recursive datatype of terms built up from a symbol Λ (for the empty set, an upside down 'V', possibly connoting German *leer*[10]) by means of the unary constructor '$-$' (for complement) and an (infinitary) constructor which puts arbitrarily many terms between a '{' on the left and a '}' on the right, separated severally by commas.

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[8]They don't really have to be atoms in the set-theoretic sense, but it does make the construction a bit more transparent if they are.

[9]Silly word, but i'm trying not to say 'construction' or 'narrative'.

[10]I think that the history would have it the other way round: 'V' [for the universe] was an upside-down 'Λ' which denoted the empty set before Bourbaki decided to use '∅'. Worth checking the history!

4 Iterated CO Constructions

Since a CO model constructed from a model $\mathfrak{M}$ of (as it might be) ZF has a well-founded part that is isomorphic to $\mathfrak{M}$ there is clearly the possibility of repeating the construction—on that wellfounded part. In this section we investigate that possibility.
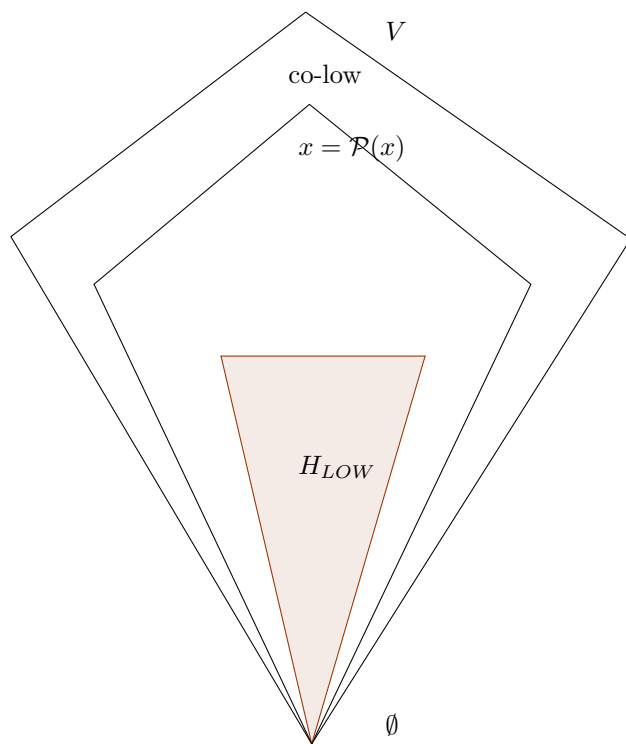
Or perhaps they look more like:
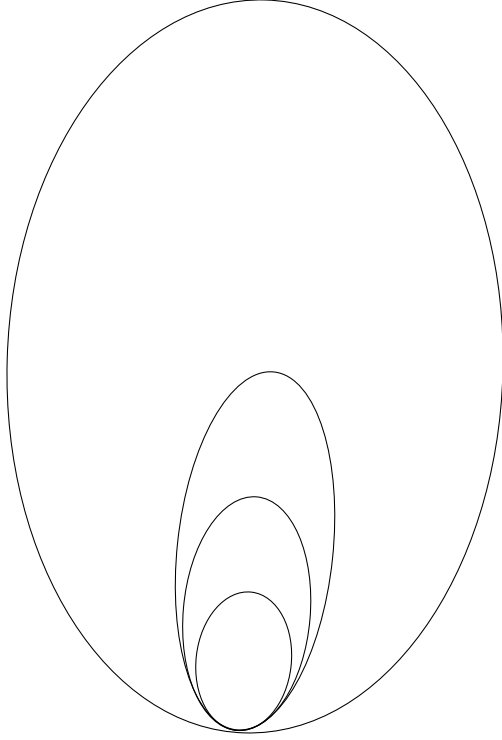
particularly if we iterate lots of times.

Let us start by considering the simplest case, the original Oswald model, $\mathfrak{M}_0 = \langle \mathbb{N}, \in \rangle$.

DEFINITION 7

Let v_1 be the (external) set of those natural numbers that are wellfounded sets of $\mathfrak{M}_0$; $\mathfrak{v}$ is the function that enumerates v_1 in increasing order.

Evidently $0 \in v_1$; and if $X \subset v_1$ and X is finite then $\displaystyle\sum_{i \in X} 2^{i+1} \in v_1$.

(Brief reality check: every number in v_1 is even and therefore corresponds to a low set ... hereditarily low $\to$ wellfounded). We are now going to perform in $\mathfrak{M}_0$ an internal CO construction on v_1 (the wellfounded part of $\mathfrak{M}_0$) while leaving the illfounded part alone, thereby obtaining a new model $\mathfrak{M}_1 = \langle \mathbb{N}, \in' \rangle$.

To this end we rule that

- When y is in v_1 we have $x \in' y$ iff $x = \mathfrak{v}(n)$ and $y = \mathfrak{v}(m)$ for some n and m and $\mathfrak{v}(n) \in' \mathfrak{v}(m)$.

- For numbers $2n + 1$ not in the range of $\mathfrak{v}$ we rule that $2n + 1$ is the $\in'$-complement of $2n$.

- There remain even numbers not in the range of $\mathfrak{v}$. These numbers are to have as members the same *numbers* as they have in $\mathfrak{M}_0$. (These numbers now correspond to different *sets* of course ...).

Observe that $\mathfrak{v}(n)$ is always an even number and encodes a member of v_1. $\mathfrak{v}(n) + 1$ is the complement in V of a member of v_1.

In the Church model—that starts with an arbitrary model $\mathfrak{M}$ of ZF(C)—we will use instead of $\mathfrak{v}$ the recursively defined injection i from [9]. The idea is of course the same as above, but the details might be worth spelling out. Let $\mathfrak{M}_0 = \langle M, \in \rangle$ be the model of NF_2 obtained from a model $\mathfrak{M}$ of ZF by means of a coding function $k_0 : V \longleftrightarrow V \times \{0,1\}$. Let v_1 be the collection of wellfounded sets of $\mathfrak{M}_0$. We will need a bijection $k_1 : v_1 \longleftrightarrow (v_1 \times \{0,1\})$.

We can define a new membership relation on M (giving us $\mathfrak{M}_1 = \langle M, \in \rangle$) by saying

DEFINITION 8

$$\mathfrak{M}_1 \models x \in y \text{ iff } \begin{cases} \text{either } y \text{ is in } v_1 \text{ and } x \in \mathtt{fst}(k_1(y)) \longleftrightarrow \mathtt{snd}(k_1(y)) = 0; \text{ or} \\ y \text{ is not in } v_1 \text{ and } \begin{cases} \text{(i) } \mathtt{snd}(k_0(y)) = 1 \text{ and } \mathfrak{M}_0 \not\models x \in \mathtt{fst}(k_0(y)) \text{ or} \\ \text{(ii) } \mathtt{snd}(k_0(y)) = 0 \text{ and } \mathfrak{M}_0 \models x \in \mathtt{fst}(k_0(y)) \end{cases} \end{cases}$$

In this definition the occurrences of '$\in$' to the left of '$\mathtt{fst}$' all denote membership of the ground model.

$\mathfrak{M}_1$ is a model of the theory $\mathrm{NF}_2 + (\exists x, y)(y \notin x = \mathcal{P}(x))$ saying that there is a set distinct from V that is equal to its own power set. In fact it is (isomorphic to) the term model for this theory.

Binary code		Set in $\mathfrak{M}_0$	Set in $\mathfrak{M}_1$
0	$\mathfrak{v}(0)$	$\emptyset$	$\emptyset$
1		V	V
10	$\mathfrak{v}(1)$	$\{\emptyset\}$	v_1
11		$V \setminus \{\emptyset\}$	$V \setminus v_1$
100		$\{V\}$	$\{V\}$
101		$V \setminus \{V\}$	$V \setminus \{V\}$
110		$\{V, \emptyset\}$	$\{V, \emptyset\}$
111		$V \setminus \{V, \emptyset\}$	$V \setminus \{V, \emptyset\}$
1000	$\mathfrak{v}(2)$	$\{\{\emptyset\}\}$	$\{\emptyset\}$
1001		$V \setminus \{\{\emptyset\}\}$	$V \setminus \{\emptyset\}$
1010	$\mathfrak{v}(3)$	$\{\emptyset, \{\emptyset\}\}$	$v_1 \setminus \{\emptyset\}$
1011		$V \setminus \{\emptyset, \{\emptyset\}\}$	$V \setminus (v_1 \setminus \{\emptyset\})$
1100		$\{V, \{\emptyset\}\}$	$\{V, v_1\}$
1101		$V \setminus \{V, \{\emptyset\}\}$	$V \setminus \{V, v_1\}$
1110		$\{\emptyset, V, \{\emptyset\}\}$	$\{\emptyset, V, v_1\}$
1111		$V \setminus \{\emptyset, V, \{\emptyset\}\}$	$V \setminus \{\emptyset, V, v_1\}$
10000		$\{V \setminus \{\emptyset\}\}$	$\{V \setminus v_1\}$
10001		$V \setminus \{V \setminus \{\emptyset\}\}$	$V \setminus \{V \setminus v_1\}$
10010		$\{\emptyset, V \setminus \{\emptyset\}\}$	$\{\emptyset, V \setminus v_1\}$
$\vdots$			
1000000000	$\mathfrak{v}(4)$	$\{\{\{\emptyset\}\}\}$	$\{v_1\}$
1000000010	$\mathfrak{v}(5)$	$\{\emptyset, \{\{\emptyset\}\}\}$	$v_1 \setminus \{v_1\}$
$\vdots$			
1000001000	$\mathfrak{v}(6)$	$\{\{\emptyset\}, \{\{\emptyset\}\}\}$	$\{v_1, \emptyset\}$
1000001010	$\mathfrak{v}(7)$	$\{\emptyset, \{\emptyset\}, \{\{\emptyset\}\}\}$	$v_1 \setminus \{v_1, \emptyset\}$
$\vdots$			
100000000000	$\mathfrak{v}(8)$	$\{\{\emptyset, \{\emptyset\}\}\}$	$\{\{\emptyset\}\}$
100000000010	$\mathfrak{v}(9)$	$\{\emptyset, \{\emptyset, \{\emptyset\}\}\}$	$v_1 \setminus \{\{\emptyset\}\}$
100000001000	$\mathfrak{v}(10)$	$\{\{\{\emptyset\}, \emptyset\}, \{\emptyset\}\}$	$\{\emptyset, \{\emptyset\}\}$
100000001010	$\mathfrak{v}(11)$	$\{\{\{\emptyset\}, \emptyset\}, \{\emptyset\}, \emptyset\}$	$v_1 \setminus \{\emptyset, \{\emptyset\}\}$
$\vdots$			
101000000000	$\mathfrak{v}(12)$	$\{\{\{\emptyset\}, \emptyset\}, \{\{\emptyset\}\}\}$	$\{v_1, \{\emptyset\}\}$
101000000010	$\mathfrak{v}(13)$	$\{\{\{\emptyset\}, \emptyset\}, \{\{\emptyset\}\}, \emptyset\}$	$v_1 \setminus \{v_1, \{\emptyset\}\}$
101000001000	$\mathfrak{v}(14)$	$\{\{\{\emptyset\}, \emptyset\}, \{\{\emptyset\}\}, \{\emptyset\}\}$	$\{v_1, \emptyset, \{\emptyset\}\}$
101000001010	$\mathfrak{v}(15)$	$\{\{\{\emptyset\}, \emptyset\}, \{\{\emptyset\}\}, \{\emptyset\}, \emptyset\}$	$v_1 \setminus \{v_1, \emptyset, \{\emptyset\}\}$

4.1 Injections into the Iterated Model

We want to understand injections $\mathfrak{M}_0 \hookrightarrow \mathfrak{M}_1$. There are two, as in the two illustrations which follow. [Presumably there are more, but only two that are of interest at this stage.]
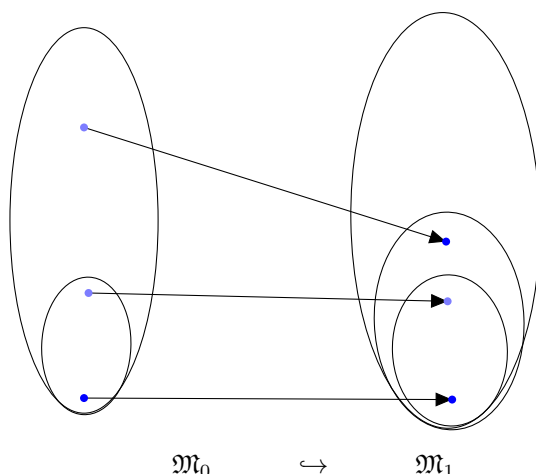
$$\mathfrak{M}_0 \qquad \hookrightarrow \qquad \mathfrak{M}_1$$

The first one arises from the fact that every object in $\mathfrak{M}_0$ is denoted by a word in the alphabet with '$\{\ldots\}$', comma, 'V' and '$\backslash$'. Every such word also denotes a word in the new model $\mathfrak{M}_1$, so we can inject $\mathfrak{M}_0 \hookrightarrow \mathfrak{M}_1$ merely by "sending words to themselves"—send the denotation of word w in $\mathfrak{M}_0$ to the denotation of the word w in $\mathfrak{M}_1$. The things in $\mathfrak{M}_1$ not in the range of this embedding are precisely those denoted by words that mention v_1.

This will work, as literally stated, only for the "pythagorean" (everything is a number) model $\mathfrak{M}_1$ of Oswald, but it is a pointer to how to do it in the more general setting considered by Church.

$$\mathfrak{M}_0 \qquad \hookrightarrow \qquad \mathfrak{M}_1$$
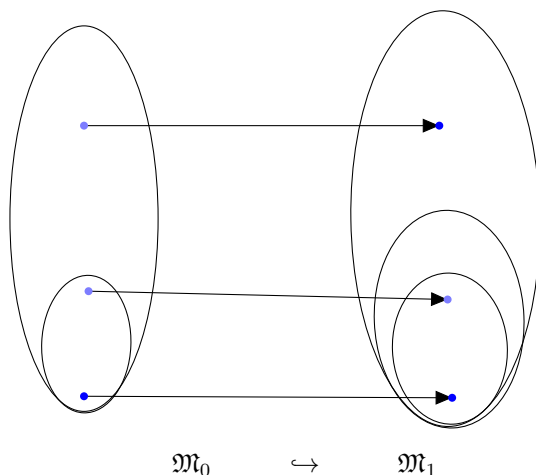
The other embedding sends the universal set of $\mathfrak{M}_0$ to v_1 in $\mathfrak{M}_1$. In general it sends wellfounded sets of $\mathfrak{M}_0$ to the same wellfounded sets in $\mathfrak{M}_1$, and sends complements of wellfounded sets of $\mathfrak{M}_0$ to their complements-with-respect-to-v_1. This second embedding is simply the function $\mathfrak{v}$ that enumerates the members of v_1 in increasing order, as is illustrated in the table on page 24.

Observe that the rows that have an entry '$\mathfrak{v}(n)$' in the second column are precisely the rows where the entry in the third column is a wellfounded set. Observe further that, in every row where there is an entry $\mathfrak{v}(n)$ in the second column, the entry in the fourth column is like the second-column entry in the nth row, with 'V' replaced throughout by 'v_1'.

Find a set x in the third column; look at the row number which is n, say; go to the row with '$\mathfrak{v}(n)$' in the second column. The set in the fourth column is what x is sent to.

The fact that we have *two* embeddings $\mathfrak{M}_0 \hookrightarrow \mathfrak{M}_1$ opens up possibilities.

The iteration has given us a second model $\mathfrak{M}_1$ of NF$_2$ containing a set $v_1 \neq V$ equal to its own power set, such that the restriction of $\mathfrak{M}_1$ to v_1 is a structure isomorphic to $\mathfrak{M}_0$. Clearly we can repeat the process to obtain a model $\mathfrak{M}_2$ containing two sets $v_1 \subset v_2 \subset V$ both equal to their own power set, such that $\mathfrak{M}_2$ restricted to v_2 is isomorphic to $\mathfrak{M}_1$ (identifying v_1 with the unique set smaller than the universe which is equal to its own power set in $\mathfrak{M}_1$). Indeed we can iterate this finitely to obtain models $\mathfrak{M}_n$ containing a $\subset$-chain of precisely $n - 1$ sets each equal to its own power set and distinct from V. Let us think of the $\mathfrak{M}_n$ as expanded by the addition of names $v_1 \ldots v_n$ for these sets-equal-to-their-own-power-set ("$\mathfrak{V}$ objects"). Our next task is to show that, for every $n < m$ and every strictly increasing injective function $f : [1, n] \to [1, m]$, there is an injection $i_f : \mathfrak{M}_n \hookrightarrow \mathfrak{M}_m$ such that, for each $k \leq m$, i_f sends the v_k of $\mathfrak{M}_n$ to the $v_{f(k)}$ of $\mathfrak{M}_m$. This we do by induction on m. The induction hypothesis is that

$$(\forall n < m)(\forall f : [1, n] \to [1, m])(\exists i_f : \mathfrak{M}_n \hookrightarrow \mathfrak{M}_m)(\forall k \leq n)(i_f(v_k) = v_{f(k)})$$

where the f are assumed to be injective and order-preserving.

Proof: We are given $f : [1, n] \to [1, m + 1]$ with $n \leq m$ and we are required to produce $i_f : \mathfrak{M}_n \hookrightarrow \mathfrak{M}_{m+1}$ satisfying $(\forall k \leq n)(i_f(v_k) = v_{f(k)})$. There are two cases to consider, depending on whether or not $f(n) = m + 1$.

We will need to be able to inject $\mathfrak{M}_n$ into $\mathfrak{M}_{n+1}$ in a way that sends each v_i in $\mathfrak{M}_n$ to the v_i for $\mathfrak{M}_{n+1}$. That is to say, we want an injection that makes $\mathfrak{M}_{n+1}$ into an end-extension of $\mathfrak{M}_n$. This is precisely what the original CO construction does: make a new model that is an end-extension of the model you start with. And of course we do the same thing here. Nothing in the original CO construction relied on the membership relation of the ground model being wellfounded. It says merely: "make two copies, one labelled 0 and the other labelled 1 ...". The carrier sets of $\mathfrak{M}_n$ and $\mathfrak{M}_{n+1}$ are both $\mathbb{N}$, after all, and

the function $\mathfrak{v} : \mathbb{N} \to \mathbb{N}$ is accordingly an injection $\mathfrak{M}_n \to \mathfrak{M}_{n+1}$—and it is the function we want.

The other thing we need to be able to do is to lift an injection $f : \mathfrak{M}_n \hookrightarrow \mathfrak{M}_m$ to an injection $f' : \mathfrak{M}_{n+1} \hookrightarrow \mathfrak{M}_{m+1}$ that sends v_i to $f(v_i)$ for $i \leq n$ and sends $v_{n+1}^{(\mathfrak{M}_{n+1})}$ to $v_{m+1}^{(\mathfrak{M}_{m+1})}$. But the embedding required for this is just the same as the first of the two embeddings on page 25.

Now we have all we need. Every desired embedding $i_f : \mathfrak{M}_n \hookrightarrow \mathfrak{M}_{m+1}$ satisfying $(\forall k \leq n)(i_f(v_k) = v_{f(k)})$ can be obtained from an earlier embedding by one of these two ruses.

We are now in a position to consider a possible project of taking a direct limit of finite iterates to obtain a model of NF_2 containing a family of $\mathfrak{V}$ objects totally ordered by inclusion to any desired linear order type.

E D I T   B E L O W   H E R E

Once we've done that we can get a model of NF_2 where the $\mathfrak{V}$ objects are ordered by $\subseteq$ into any total order type we choose, in particular it can be infinite. Presumably we can then work all the usual trickery facilitated by the Ehrenfeucht-Mostowski theorem. This should broaden the palette available to workers in the CO mode, and just might give us models containing large or intermediate sets with special properties perhaps not possible for large-or-intermediate sets created directly by wands. Sets constructed by wands are always closely tied to individual low sets . . .

Presumably for every recursive total order $\langle \mathbb{N}, \leq \rangle$ we can find a recursive model in which the $\mathfrak{V}$ objects are ordered by inclusion isomorphically to $\langle \mathbb{N}, \leq \rangle$.

5 Model Constructions inside CO models: Inner Models and Zermelo Cones

In the cumulative hierarchy setting there are two standard lines of supply of models for fragments of the theory. There are the V_αs, ("Zermelo cones") the initial segments of the universe consisting of things of suitably bounded rank. There are also the things of the form H_ϕ, the collection of things that are hereditarily ϕ for some ϕ. H_ϕ that are of unbounded rank are of special interest, since traditional inner models (HOD, $HROD$, L, $L(\mu)$...) tend to be of this form. Analogues of both these flavour of models appear in the CO context, and the parallel needs explanation.

5.1 Analogues of the V_α

DEFINITION 9
When we are working with a CO model $\mathfrak{M}$ let us write '$\mathfrak{M}_\alpha$' for the substructure of $\mathfrak{M}$ consisting of those elements whose birthdays are less than α.

Clearly:

REMARK 3
The inclusion embedding from $\mathfrak{M}_\alpha \hookrightarrow \mathfrak{M}$ is elementary for $\Delta_0^{\mathcal{E}}$ formulæ.

We observe without proof that the CO construction commutes with truncation in the sense that if we start with any model of ZF(C), and perform the CO construction on an initial segment V_α of it, the result is the same as performing a CO construction on the whole model—thereby obtaining a CO model $\mathfrak{M}$—and then cutting down to $\mathfrak{M}_\alpha$.

5.2   Inner models

Set theories that allow a universal set do not *prima facie* have a good notion of *inner model*. If a theory T believes there is a universal set, then any inner model for T must have a universal set. Is this universal-set-of-the-inner-model to be the universal set of the original model? If so then the inner model is not transitive. But if it is not to be the universe of the original model then it has to be rather special: since it has to be a member of itself (it's supposed to be the universe of the inner model after all) then it cannot be a proper class (which inner models always are in the ZF setting) so it has to be a *set*—of the original model. And a rather big one at that. This is a huge extra hurdle—since typically theories that prove that there is a universal set tend not to prove the existence of lots of big sets—and even in Quine's NF (which is troublingly good at proving the existence of suspect big sets) we know no good notion of inner model.

We must (or at least *I* must) get straight the notion of *inner model* in the classic setting it is used to, the better to know how to port it to the CO setting.

On p. 182 of [14], Jech defines inner model in the usual way: "An inner model of ZF is a transitive class that contains all ordinals and satisfies the axioms of ZF."

He then formulates a very compact criterion:

Theorem 13.9. A transitive class M is an inner model of ZF if and only if it is closed under Gödel operations and is almost universal, i.e., every subset $X \subseteq M$ is included in some Y element of M.

Koepke remarks that a subclass of a model of ZF that satisfies these conditions is itself a model of ZF.

Given the NF-ish context of the current discussion one wonders if one can relativise this remark of Koepke's to stratifiable expressions thus:

CONJECTURE 1 *Suppose $V \models str(ZF)$, and $M \subseteq V$ is transitive, almost universal, and closed under the stratifiable Gödel F functions.*

Then $M \models str(ZF)$. (Here str(ZF) is the theory axiomatised by the stratifiable axiom of ZF, taking the axiom of infinity in the (stratifiable) form: there is a Dedekind-infinite set.)

There is a definition of inner model implicit in [19] as a class $M \subseteq \mathcal{P}(M) \subseteq \bigcup_{x \in M} \mathcal{P}(x)$ that is closed under the Gödel functions.

Dana Scott's formulation of $V = L$ in [19]... Suppose M is a proper class such that

$$M \subseteq \mathcal{P}(M) \subseteq \bigcup_{x \in M} \mathcal{P}(x)$$

that is rud-closed. Then $V = M$. Compare Shepherdson.

The first inclusion says that M is transitive, so obviously in the new CO context we want to say that M is $\mathcal{E}$-transitive (" $\mathcal{E}$"$M \subseteq M$ " would perhaps be better.[11]). The second inclusion says that every subset of M is a subset of a member of M. I think what this must mean in the CO context is that every low subset is a subset of a low member. And every co-low subset is a superset of a co-low member.

(This question is related to the question of what analogue of "every set has a transitive closure" we need in the endeavour to find an extension of NF_2 which is synonymous with PA via the Oswald interpretation. See section **??**.)

So our definition of CO-style Inner Model will be a class that is $\mathcal{E}$-transitive, every low subset is a subset of a low member (and every co-low subset is a superset of a co-low member) and something like closure under the rud-functions. What might that last clause be...? Stratifiable rud functions plus complementation? That's too strong of course, since we are not asking for stratifiable separation. But it'll be something along those lines. Perhaps just closed under complementation—for inner model of a model of AST.

There are two obvious tests to which one might subject any definition of inner model:

Have a look at [15]

    (i) One might expect that a class of the CO model is an inner model iff it's an inner model in the sense of the ground model;

    (ii) Is the class of things that are x hereditarily-$\phi^{\mathcal{E}}$ ever an inner model in our sense?

To answer the second we need a definition of hereditarily-$\phi^{\mathcal{E}}$.

DEFINITION 10
x is hereditarily-$\phi^{\mathcal{E}}$ (in the CO model) iff
x is ϕ and every $y \mathrel{\mathcal{E}} x$ is hereditarily-$\phi^{\mathcal{E}}$.

This makes the substructure of things that are hereditarily-$\phi^{\mathcal{E}}$ into a well-behaved substructure. Since $(\forall x)(\neg(x \mathrel{\mathcal{E}} V))$, V is vacuously hereditarily-$\phi^{\mathcal{E}}$ (as long as $\phi(V)$). Thus V is *always* an element of the structure of things-that-are-hereditarily-$\phi^{\mathcal{E}}$ (again, as long as $\phi(V)$) so we don't have the usual problem of the universe of the inner model being a proper class, and therefore not being a model of $(\exists x)(\forall y)(y \in x)$.

The following is easy.

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[11] Why do we never write " $\in$ "$x \subseteq x$" but always "$\bigcup x \subseteq x$"?

Remark 4

- *The collection of things that are hereditarily-$\phi^{\mathcal{E}}$ is transitive-in-the-sense-of-$\mathcal{E}$;* so
- *The inclusion embedding from the class of things that are hereditarily-$\phi^{\mathcal{E}}$ into the (CO) universe is elementary for $\Delta_0^{\mathcal{E}}$ formulæ.*

These are the obvious $\mathcal{E}$-analogues of the two important trivialities (in the cumulative hierarchy context) that (i) the collection H_ϕ (of things that are hereditarily ϕ) is transitive, and that (ii) the inclusion embedding $H_\phi \hookrightarrow V$ is elementary for Δ_0 formulæ.

We observed earlier (p. 30) that the simplest CO model (of *AST*, where the wellfounded sets are a copy of the original model) has an (external) antimorphism, σ, defined by $\sigma(x) := V \setminus \sigma\text{``}x$. We prove by induction on $\mathcal{E}$ that this definition is legitimate. This phenomenon illustrates the naturalness and utility of the notion of being hereditarily $\phi^{\mathcal{E}}$. The wellfounded sets of the CO will be[12] precisely the hereditarily low$^{\mathcal{E}}$ sets. The image in σ of the class of wellfounded sets is precisely the collection of sets that are hereditarily co-low$^{\mathcal{E}}$: $\sigma(\emptyset) := V \setminus \sigma\text{``}\emptyset = V$, and $V$ is hereditarily co-low$^{\mathcal{E}}$. (Recall that we remarked above that $V$ is hereditarily$\phi^{\mathcal{E}}$ for all $\phi$, just as the empty set is). Then $\sigma(\{\emptyset\}) := V \setminus \sigma\text{``}\{\emptyset\}$ which is $V \setminus \{V\}$. And $V \setminus \{V\}$, too, is hereditarily co-low$^{\mathcal{E}}$ because the only x such that $x\,\mathcal{E}\,(V \setminus \{V\})$ is V—which we have just shown to be hereditarily co-low$^{\mathcal{E}}$!

The following elementary fact might sow confusion among the nervous: *hereditarily low* and *hereditarily low$^{\mathcal{E}}$* are the same notion! If x is low then $(\forall y)(y \in x \longleftrightarrow y\,\mathcal{E}\,x)$—recall that this is actually the *definition* of '*low*' (see definition 6). But then "x hereditarily low iff x hereditarily low$^{\mathcal{E}}$" follows by wellfounded induction on $\mathcal{E}$.

Worth writing this out?

5.2.1 Are the inner models extensional?

This is where the divergence between $\in$ and $\mathcal{E}$ starts to cause trouble. In the cumulative hierarchy case H_ϕ is extensional (*i.e.*, it is a model of the axiom of extensionality). Can we show that the class of things that are hereditarily-$\phi^{\mathcal{E}}$ is extensional^{CO}? Observe that this will not follow straightforwardly from the fact that the inclusion embedding is elementary for $\Delta_0^{\mathcal{E}}$ formulæ because '$(\forall z)(z \in x \longleftrightarrow z \in y)$' is not a $\Delta_0^{\mathcal{E}}$ formula. Suppose ϕ is such that the empty set has ϕ in the CO model; there doesn't seem to be any obstacle to the existence of a nonempty hereditarily-$\phi^{\mathcal{E}}$ set x that has no members^{CO} that are hereditarily-$\phi^{\mathcal{E}}$. So we would need special assumptions. One instance where it works well is in the CO model for NF0 (see section 7.3.2), in which the class of sets that are hereditarily low–or–co-low turns out to be just a CO model for NF_2.

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[12] as long as some minor technical details are satisfied: see the discussion on p. 19.

5.2.2 Inner Models of sets that are hereditarily-ϕ

Start with a model of ZF. Recall the construction of symmetric sets from [12]. Take the inner model HS of hereditarily symmetric sets (as in [12]) and do a CO construction inside that inner model. Alternatively do a CO construction and then take the inner model of hereditarily symmetric sets in the way just described above. Do we get the same result both times?

Hereditarily-low-or-co-low$^{\mathcal{E}}$. This gives a simple illustration.

We are reasoning in some set theory or other about a model $\mathfrak{M}$ of NF_2, with M its carrier set.

We define a sequence $\langle M_\alpha : \alpha \in On \rangle$ of subsets of M by

$$M_\alpha := \mathcal{P}(\bigcup_{\beta < \alpha} M_\beta) \cap M \ \cup \ \{\mathcal{M} \setminus X : X \subseteq (\bigcup_{\beta < \alpha} M_\beta)\} \qquad (1)$$

The collection M_α is precisely the collection of sets of birthday $\leq \alpha$ that are hereditarily-low-or-co-low$^{\mathcal{E}}$. The sequence will close off at some point, and the union will be called 'M_∞'. $\langle M_\infty, \in^{\mathfrak{M}} \restriction M_\infty \rangle$ is a substructure of $\mathfrak{M}$ and is clearly a model of NF_2.

But we can do better. Observe that any model of NF_2 + Infinity has an inner model that violates choice, as follows. Let $\mathfrak{M}$ be a model of NF_2.

$$M_\alpha := \{X \subseteq \bigcup_{\beta < \alpha} M_\beta : \mathfrak{M} \models symm(X) \vee \mathfrak{M} \models symm(V \setminus X)\}$$

where '$\mathfrak{M} \models symm(y)$' of course means that y is a set of $\mathfrak{M}$ believed by $\mathfrak{M}$ to be symmetric.

As before, the sequence will close off at some point, and the union will be called 'M_∞'. $\langle M_\infty, \in^{\mathfrak{M}} \restriction M_\infty \rangle$ is a substructure of $\mathfrak{M}$ and is clearly a model of NF_2. Equally clearly it does not satisfy AC.

More generally, if $\mathfrak{M}$ is a model constructed using a particular collection W of wands, then, if $W' \subseteq W$, we can construct an inner model containing precisely those elements constructed using only the wands in W'. Here is an example. . .

S T U F F   M I S S I N G

6 Forcing

This is copied *verbatim* from iterativesl.pdf:

> Forcing is possible in the two-wands construction. The recursions on $\in$ that feature in the truth lemma and the definition of forcing for atomic formulæ are replaced by recursions on the relation (3). Also $\check{a} =: \{\langle \check{b}, 1 \rangle : b \in a\}$ if a is low. If a is co-low then we must have $\check{a} =: V \setminus \{\langle \check{b}, 1 \rangle : b \notin a\}$.

$$\check{a} =: \begin{cases} \{\langle \check{b}, 1 \rangle : b \in a\} \text{ if } a \text{ is low} \\ V \setminus \{\langle \check{b}, 1 \rangle : b \notin a\} \text{ if } a \text{ is co-low;} \end{cases}$$

Similarly

$$a^F =: \begin{cases} \{b^F : (\exists p \in F)(\langle b, p \rangle \in a)\} \text{ if } a \text{ is low} \\ V \setminus (V \setminus a)^F \text{ if } a \text{ is co-low.} \end{cases}$$

Notice that there can be no co-low poset (the graph of a partial order on a co-low set would not be co-low) so all sets of conditions are low sets as before.

As Wikipædia would say, *this article is a stub*.

Probably the thing to do is to show that forcing commutes with the simplest CO construction, (the one where one just adds complements) in the obvious sense that a forcing extension of a CO model can be obtained as the CO model arising from a forcing extension.

So we need to define $\mathbb{P}$-names. A $\mathbb{P}$-name is a pair $\langle p, X \rangle$ where $p \in \mathbb{P}$ and X is either a set of $\mathbb{P}$-names or a complement of a set of $\mathbb{P}$-names.

7 Spicing up Oswald's Construction

In this section we briefly go over Church's j-cardinals (equivalence classes for any equivalence relation over **low** sets) from [5], mention Flash's singleton function' Mitchell's theory, NF0—and a few enhancements due to your humble correspondent, such as adding an internal antimorphism.

Some of this section goes over material covered in [9].

7.1 Equivalence classes for equivalence relations on low sets

Place yourself in a model $\mathfrak{M}$ of ZF, and suppose $\sim$ is an equivalence relation in $\mathfrak{M}$. We don't mind how big the quotient over $\sim$ is, but we need to know. Let's suppose it is a proper class, since that makes the trick more impressive. Reserve a proper class S of things to serve as $\sim$-equivalence classes. How do we do this? Well, we could take S to be the class of Scott-style equivalence classes $[x]_\sim$, where $[x]_\sim$ is the set of those $y \sim x$ thare are of minimal rank. Next we need a bijection k between $M \times \{0, 1\}$ and $M \setminus S$. We can now define a membership relation for a new model by $x \in_{new} y$ iff either

(i) $y \in S$ and $x \sim x'$ for some $x \in y$; or
(ii) $y \notin S$ and $x \in \mathtt{fst}(k(y)) \longleftrightarrow \mathtt{snd}(k(y)) = 1$

It's a bit fiddly to see what happens in the new structure. Of course what one *wants* is that clause (i) should ensure that in the new model there exists,

for every low set x, the set $\{y : x \sim y\}$—where of course $\sim$ is interpreted in the sense of the new model. Of course one cannot simply stipulate this, for that would be circular. One has to hope that some straightforward trick like the above will work. In fact it will turn out that clause (i) will achieve it, and this is beco's of a kind of analogue of Coret's lemma (Or do i mean Henson's lemma?) for the CO construction. The details are in [9] section???

Clause (ii) will ensure that every low set has a complement.

Does this construction ensure that the equivalence classes have complements?

7.2   Larger Boolean Algebras

In this section we generalise Oswald's construction so as to add moieties: Oswald's model has only finite and cofinite sets, and no moieties.

Go back to the Oswald construction (so the carrier set of our model is going to be $\mathbb{N}$), and think of the two values of the flag (the 0th bit) as pointing to two sets (V and $\emptyset$ as it happens) so that then the bits to the left of the flag tell you which elements to add or delete from whichever of the two sets you are pointing to. In Oswald's construction 1 means V and 0 means $\emptyset$, and you leave the nth bit clear if you do *not* want to change the truth-value of $n \in$ whatever-it-is, and set it to 1 if you do. If we think of Oswald's construction in this way, it becomes easy to see how to generalise it to add particular sets.

Let B be a finite Boolean subalgebra of $\mathcal{P}(\mathbb{N})$, with the property that every atom of B is an infinite subset of $\mathbb{N}$ (the atoms partition $\mathbb{N}$)—which will have the effect that if $b, b' \in B$, then b XOR b' is infinite—and we enumerate the elements of B as $b_1 \ldots b_{|B|}$. We define a new relation $x \in_{\texttt{new}} y$ on $\mathbb{N}$ as follows. The rightmost $log_2(|B|)$ bits of y identify a b_i. (The cardinality of a finite boolean algebra is always a power of 2). We truncate them, and renumber the bits in the truncation starting at 0. We then define:

$$x \in_{\texttt{new}} y \text{ iff } (x \in b_i) \longleftrightarrow (\text{the } x\text{th bit of the truncation is 0}).$$

Naturally we have to furnish the truth value of $n \in m$ when m is an atom.

Now every set y in the model will have finite symmetric difference with precisely one of the sets in B (which we can call $\texttt{moiety}(y)$). This will ensure that the new model will satisfy the boolean axioms. Further, every element has a singleton, so the model satisfies NF_2. Indeed it satisfies NF_2 (and not merely AST) for the same reasons that Oswald's original model does. To focus our minds consider the simple modification of the original model where the two moieties are the odds and the evens, and the flag 00 means the empty set, 01 means V, 10 means the odds and 11 means the evens. Thus $\texttt{moiety}(n)$ is $\emptyset$, V, odds or evens depending on whether n mod 4 is 0, 1, 2 or 3. Then we say

$m \in_{\texttt{new}} n$ if

n mod $4 = 0$ and ((the nth bit of $m/4$ is 1) iff $\texttt{true}$);   (or)
n mod $4 = 1$ and ((the nth bit of $(m-1)/4$ is 1) iff $\texttt{false}$);   (or)
n mod $4 = 2$ and ((the nth bit of $(m-2)/4$ is 1) iff (n is even)); (or)
n mod $4 = 3$ and ((the nth bit of $(m-3)/4$ is 1) iff (n is odd)).

...where all four clauses now have the same form. The takeaway message is that the definition of $\in_{\texttt{new}}$ in the simplest case was artificially simple, in that one can massage the correct definition (that uses $\texttt{XOR}$ or $\longleftrightarrow$) into something that doesn't—and is shorter and thereby seems simple and correct. Thus the "correct" version of the first clause is the one displayed, rather than the more usual-looking "$n \bmod 4 = 0$ and (the nth bit of $m/4$ is 1)".

What is the wellfounded relation informing this model?

$$n \, \mathcal{E} \, m \text{ iff the } (n+2)\text{th bit of } m \text{ is } 1.$$

And that is clearly wellfounded, being a subset of $<_{\mathbb{N}}$. Notice that $\mathcal{E}$ has four minimal elements: $\emptyset$, V, the evens and the odds.

\begin{digression}

We can do the same in ZF of course. And we can take our two moieties to be the two pieces of any partition of V we like. Can we do the same in NF? Nothing to stop us. It may be worth thinking about the logistics. We don't need a flag for V co's we've already got V, so we have a three-valued flag. And what do we get? The two moieties have become sets. But beware! We want $\in_{\texttt{new}}$ to be a stratified (inhomogeneous) relation. But that will work only if the two moieties were sets already! So NF is sort-of closed under all these constructions. Let's illustrate:

Let L and R be two pieces of a partition on V. Fix k a bijection between V and $V \times \{0, l, r\}$. We then say $x \in_{\texttt{new}} y$ iff

$\texttt{snd}(y) = 0$ and $x \in \texttt{fst}(y)$ iff $\texttt{true}$;
$\texttt{snd}(y) = l$ and $x \in \texttt{fst}(y)$ iff $x \in L$;
$\texttt{snd}(y) = r$ and $x \in \texttt{fst}(y)$ iff $x \in R$.

(check this)

so we need $x \in L$ and $x \in R$ to be stratified.

Or perhaps write it in the style

if $\texttt{snd}(y) = 0$ then $x \in_{\texttt{new}} y$ iff ($x \in \texttt{fst}(y)$ iff $\texttt{true}$)
if $\texttt{snd}(y) = l$ then $x \in_{\texttt{new}} y$ iff ($x \in \texttt{fst}(y)$ iff $x \in L$)
if $\texttt{snd}(y) = r$ then $x \in_{\texttt{new}} y$ iff ($x \in \texttt{fst}(y)$ iff $x \in R$)

Either way it is clear that for $\in_{\texttt{new}}$ to be well-behaved we need R and L to be sets. So this manœuvre avails us nothing.

\end{digression}

We should think a bit about the possibility of having infinitely many moieties. Instead of using the least significant n bits to encode a partition of V into n moieties, reserve the even bits to encode a partition of V into countably many moieties. We need to think a bit about what this achieves. *Prima facie* there is a problem because to achieve a universal set every even bit would have

to be set, but no bit string with infinitely many set bits encodes a member of $\mathbb{N}$.

Actually i'm not sure that this works. Needs thinking about. There may be problems with wellfoundedness of $\mathcal{E}$.

7.2.1 Antimorphisms

It should be a simple matter to show that the models of this section all have a canonical antimorphism—in fact a polarity—in the same way that the canonical two-wand model does. In fact—as we shall now show—it is possible to contrive such a model in which one of the moieties is the graph of an antimorphism.

To keep things simple the antimorphism τ that we are going to add will be an involution, a *polarity*. This will enable us to think of the antimorphism as a set of *un*ordered pairs, and this simplifies things mightily.

As remarked, τ will be a moiety. The advent of τ brings other objects along, and some of these will be moieties and will have to be accommodated by the methods of this section. These other objects are things like $\tau(\tau)$ and its complement, τ of that, and so on. Let's hope there aren't too many of them! What is $\tau(\tau)$? Clearly it must be $V \setminus \{\tau(\{x,y\}) : \{x,y\} \in \tau\}$, and this is

$$V \setminus \{V \setminus \{\tau(x), \tau(y)\} : \{x,y\} \in \tau\},$$

which is of course

$$V \setminus \{V \setminus \{x,y\} : \{x,y\} \in \tau\}\},$$

since $\{\tau(x), \tau(y)\}$ is just $\{x,y\}$ when $\{x,y\} \in \tau$.

If X is a moiety so are $\tau``X$ and $V \setminus \tau``X$, but this last is just $\tau(x)$. This is good news for us in our search for a finite set containing τ and closed under all operations that create moieties.

To cut a long story short, it seems that the following eight objects are a finite set of the kind we are looking for.

$$\tau, \quad \tau(\tau) \setminus \tau, \quad V \setminus \tau(\tau), \quad V, \quad \emptyset, \quad \tau(\tau), \quad V \setminus \tau, \quad \tau \cup (V \setminus \tau(\tau)) \qquad \text{(A)}$$

and it's mechanical to check (i) that this collection forms a boolean algebra and (ii) that it is closed under τ. (ii) is worth spelling out:

These sets are moved by τ... to these

These sets are moved by τ...	to these
τ	$\tau(\tau)$
$\tau(\tau) \setminus \tau$	$\tau \cup (V \setminus \tau(\tau))$
$V \setminus \tau(\tau)$	$V \setminus \tau$
V	$\emptyset$
$\emptyset$	V
$\tau(\tau)$	τ
$V \setminus \tau$	$V \setminus \tau(\tau)$
$\tau \cup (V \setminus \tau(\tau))$	$\tau(\tau) \setminus \tau$

What are the atoms of this Boolean algebra? $\tau(\tau)$ contains all unordered pairs, so it is a superset of τ, so $\tau(\tau)$ is presumably not an atom while τ is. $\tau(\tau)$ is not an atom and it is not the top element so presumably $V \setminus \tau(\tau)$ is an atom. My guess is that the last atom is $\tau(\tau) \setminus \tau$.

It is easy to check that the atoms form a partition of the universe into three infinite pieces, so we can use the construction of the previous section.
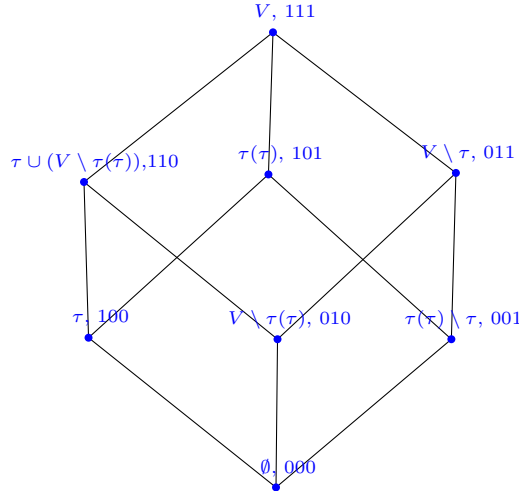
Next to each element of the boolean algebra in the above picture is its three-bit code.

It remains to define the membership relation of the new model. We start by nailing down the truth-values of $n \in m$ for $n, m < 8$, which is to say, the membership relation between the eight objects in the boolean algebra. It will be sufficient to discover, for each of the eight things in the boolean algebra, to which of the three atoms τ, $V \setminus \tau(\tau)$ and $\tau(\tau) \setminus \tau$ they belong: each of the eight things can belong to precisely one of τ, $V \setminus \tau(\tau)$ and $\tau(\tau) \setminus \tau$.

None of the eight belong to τ beco's τ is a set of pairs and none of the eight are pairs. Since $\tau(x) = V \setminus \tau``x$ we can see that $V \setminus \tau(\tau) = V \setminus (V \setminus \tau``\tau) = \tau``\tau$. Now $\tau$ is a set of doubletons, and $\tau$ of a doubleton is the complement of a doubleton, so $\tau``\tau$ (which is $V \setminus \tau(\tau)$) is an infinite set of cofinite sets. No set of cofinite sets can be cofinite, so it is not a member of itself. So it must be a member of $\tau(\tau) \setminus \tau$. That sounds all right, but it might be an idea to check this independently(!) Since $\tau``\tau$ is a cofinite set of cofinite sets, it cannot be cofinite, so $\tau``\tau \notin \tau``\tau$. This gives $\tau``\tau \in (V \setminus \tau``\tau)$. But $V \setminus \tau``\tau = \tau(\tau)$ so we get $V \setminus \tau(\tau) \in \tau(\tau)$. But we have $V \setminus \tau(\tau) \notin \tau$ whence $V \setminus \tau(\tau) \in \tau(\tau) \setminus \tau$ as claimed.

Since τ is a set of pairs we have $\tau(\tau) \notin \tau$. That gives $\tau = \tau^2(\tau) \in \tau(\tau)$; we have $\tau \notin \tau$ so $\tau \in (\tau(\tau) \setminus \tau)$.

Where does $\tau(\tau) \setminus \tau$ live? By the above it can't be in τ, so let's try $V \setminus \tau(\tau)$. But this is $\tau``\tau$. Everything in $\tau$ is a pair, and $\tau$ of a pair is cofinite, so everything in $\tau``\tau$ is cofinite. $\tau(\tau) \setminus \tau$ is not cofinite, so it can't be in $\tau``\tau$ and must be in $\tau(\tau) \setminus \tau$.

Where does $V \setminus \tau$ live? It turns out that $V \setminus \tau \in \tau(\tau) \setminus \tau$. Since $V \setminus \tau$ is not a pair we clearly have $V \setminus \tau \notin \tau$ so $V \setminus \tau \in \tau(\tau) \setminus \tau$ simplifies to $V \setminus \tau \in \tau(\tau)$. Now do τ to both to obtain $\tau(V \setminus \tau) \notin \tau^2(\tau) = \tau$, and this is clearly true beco's $\tau(V \setminus \tau)$ is not a pair and cannot belong to τ.

Where does $\tau \cup (V \setminus \tau(\tau))$ belong? It must be in either $V \setminus \tau(\tau)$ or $\tau(\tau) \setminus \tau$. Which one it belongs to depends on whether or not it belongs to $\tau(\tau)$, so let's examine $\tau \cup (V \setminus \tau(\tau)) \in \tau(\tau)$.

Now $\tau(\tau)$ is $V \setminus \tau``\tau$, and $\tau``\tau$ is a set of complements of pairs. So $\tau \cup (V \setminus \tau(\tau))$ is going to be a member of $\tau(\tau)$ unless it's a (rather special) complement of a pair, which it isn't. So $\tau \cup (V \setminus \tau(\tau)) \in \tau(\tau) \setminus \tau$.

In summary:

$\emptyset$ belongs to $V \setminus \tau(\tau)$;
τ belongs to $\tau(\tau) \setminus \tau$;
$\tau(\tau) \setminus \tau$ belongs to $\tau(\tau) \setminus \tau$;
$V \setminus \tau(\tau)$ belongs to $\tau(\tau) \setminus \tau$;
$\tau \notin \tau$ whence $\tau(\tau) \in \tau(\tau)$ and $\tau(\tau) \in \tau(\tau) \setminus \tau$;
$V \setminus \tau$ belongs to $\tau(\tau) \setminus \tau$;
$\tau \cup (V \setminus \tau(\tau))$ belongs to $\tau(\tau) \setminus \tau$;
V belongs to $V \setminus \tau(\tau)$.

In order to know whether or not $n \, \mathcal{E} \, m$ it suffices to know whether or not $n \, \mathcal{E} \, (m \, \texttt{REM} \, 8)$ (the remainder of m on division by 8, aka the three least significant bits of m). To this end it would help to know what natural number encodes τ of the set encoded by the natural number n. We use recursion: $\tau(n)$ is $V \setminus \tau``n$, and we can do this by recursion on $\mathcal{E}$. In this setting $\mathcal{E}$ is the relation $n \, \mathcal{E} \, m$ iff the $(n+3)$th bit of m is 1, and this relation is clearly wellfounded and supports recursion.

I have no idea how to add an internal antimorphism that is not an involution (a polarity); we would have to think about ordered pairs and I am not ready for that!

7.3 Adding Principal Ultrafilters: Models of NF0

We will do this in two ways, once by atoms and once by CO.

7.3.1 NF0 by atoms

At some point we are going to decide that one atom $\texttt{a}$ is to be $B(x)$ for some atom x and that $\texttt{b}$ is to be its complement. In deciding this we have determined

only the *intension* of that atom, because we don't yet know all the atoms that are to code sets of which x is a member. So all we can do is mark the atom `a` and attach to *all* atoms a warning:

```
if you ever discover that x is a
member of the set of atoms you are to
code, then decide to be a member of a.
```

That is to say, we can decide which atom is to be $B(x)$ eventually, but we cannot decide at any bounded stage what the Di Giorgi map is going to send it to, tho' we do construct a series of monotonically $\subseteq$-increasing approximations to it whose limit it is.

What about the complement of $B(x)$? Well, we can do the same trick of deciding on an atom which the Di Giorgi map will send to a set which will turn out to be $V \setminus B(x)$. What can we say about approximations to this set? Is it, too, the union of an increasing $\subseteq$-sequence of sets of atoms? It will be as long as we can discover that a set s does *not* have x as a member. We can discover this if s is a set created in the unsophisticated manner we started with, where we create the intension and the extension at the same time. But if s is itself a B object or the complement of a B object we won't necessarily learn this until the end of time.

This means that the creation falls into two stages, each of transfinite length. Let us call these **æons**. Aeon One is an exact analogue of the creation of the inner model of wellfounded sets in ZF, like the construction we started with. On Aeon One we decide membership in all sets created by the vanilla wand and the complement wand. (Notice that—whatever x is—$\{B(x)\}$ is always a set created by the vanilla wand, and $V \setminus \{B(x)\}$ is always a set created by the complement wand)

On Aeon Two we tie up loose ends. As just noted, if x is an arbitrary set created by the vanilla wand or the complement wand then by the end of Aeon One we have determined which things created by the vanilla wand or the complement wand have it as a member. What about things not created by that process, like $B(y)$ for some y? How would we ever decide that $x \in B(y)$? Only by deciding that $y \in x$. But *ex hypothesi* all such questions were decided during Aeon One, since x was created by the vanilla wand or the complement wand.

So the first thing that happens on Aeon Two is that we determine membership in things that are $B(x)$ or $V \setminus B(x)$ where x is an object created by the vanilla wand or the complement wand. Once we have done that we can then think about membership in things like $B(B(x))$ where x is a thing created by the vanilla wand or the complement wand. (I am taking $B(B(x))$ as representative of the quartet $B(B(x))$, $V \setminus B(B(x))$, $B(V \setminus B(x))$ and $V \setminus B(V \setminus B(x))$: they all get the same treatment). $y \in B(B(x))$ iff $B(x) \in y$. If y was created by the vanilla wand or the complement wand then this has already been decided. If not, then $y = B(z)$ for some z and we are trying to determine whether or not $B(x) \in B(z)$. But this is just $x \in z$. If z was created by the vanilla wand or the complement wand then we know the answer already. If not, then for some w we are trying to determine whether or not $x \in B(w)$. But this time we come

to rest, because this is just $w \in x$ and all such questions have been answered already, because x was created by the vanilla wand or the complement wand.

What about membership in $B^n(x)$ where x was created by the vanilla wand or the complement wand? $y \in B^n(x)$ is $B^{n-1}(x) \in y$. As before, if y was created by the vanilla wand or the complement wand we already know the answer. If not the question becomes $B^{n-1}(x) \in B(z)$ for some z which is $B^{n-2}(x) \in z$. This is a question just like the question that started this paragraph, except that we have replaced '$B^{n-1}(x) \in y$' by '$B^{n-2}(x) \in z$'. Thus we have shown that by induction all these questions can be dissolved.

Aeon One has as many steps in it as there are ordinals, but Aeon Two is a less protracted affair: just long enough to do some induction over $\mathbb{N}$.

Complicating the construction so as to allow for the manufacture of $B(x)$ for all x is a nice exercise, and it has already shown us that we will spend Aeon Two unravelling membership of atoms which in some sense are denoted by complex terms, and that we do this unravelling by recursion on the structure of the terms. This unravelling is in some sense deterministic. Once we have determined membership of sets created by the vanilla wand or the complement wand, the rest happens automatically, even if it takes countably many steps to play itself out. In particular we don't need to do any forcing.

7.3.2 A CO model for NF0

The obvious thing to try is:

$$
x \in y \text{ iff } \begin{cases} y = 4n \text{ for some } n \text{ and the } x\text{th bit of } n \text{ is } 1; \text{ or} \\ y = 4n + 1 \text{ for some } n \text{ and the } x\text{th bit of } n \text{ is } 0; \text{ or} \\ y = 4n + 2 \text{ for some } n \text{ and } n \in x; \text{ or} \\ y = 4n + 3 \text{ for some } n \text{ and } n \notin x. \end{cases}
$$

So $4n$ is finite, $4n + 1$ is cofinite, $4n + 2$ is $B(n)$ and $4n + 3$ is $\overline{B(n)}$.

Binary code	corresponding to	Set in NF0 model
0		$\emptyset$
1		V
10		$B(\emptyset)$
11		$V \setminus B(\emptyset)$
100		$\{\emptyset\}$
101		$V \setminus \{\emptyset\}$
110		$B(V)$
111		$V \setminus B(V)$
1000		$\{B(\emptyset)\}$
1001		$V \setminus \{B(\emptyset)\}$
1010		$B(B(\emptyset))$
1011		$V \setminus B(B(\emptyset))$
1100		$\{V, \emptyset\}$
1101		$V \setminus \{V, \emptyset\}$
1110		$B(V \setminus B(\emptyset))$
1111		$V \setminus B(V \setminus B(\emptyset))$
10000		$\{\{\emptyset\}\}$
10001		$V \setminus \{\{\emptyset\}\}$
10010		$B(\{\emptyset\})$
10011		$V \setminus B(\{\emptyset\})$
$\vdots$		$\vdots$

In this structure $\{y : x \in y\}$ exists for all x. Nevertheless it is not a model of NF0 because it is not closed under $\cup$ and $\cap$. Although the original Oswald model promised merely to give everything a complement (*i.e.*, it was a model of *AST*), that process also automatically created binary unions and intersections: the same process that creates x and y also creates $x \cup y$ and $x \cap y$. In the present case $B(y) \cup B(x)$ is neither low nor co-low, and furthermore it is neither B nor $\overline{B}$ of anything—and so doesn't get created. The model we have just exhibited satisfies a theory that has extensionality plus

    (i) there is a universal set;
    (ii) existence of unordered n-tuples for concrete n;
    (iii) $V \setminus x$ exists for all x and
    (iv) $\{y : x \in y\}$ exists for all x.

which is to say AST + existence of $B(x)$ for all x[13].

Observe however that if we perform here the inner model construction described by formula 5.2.2, we obtain the inner model of sets that are hereditarily low or co-low—and this *is* closed under $\cup$ and $\cap$, since it is the basic Oswald model $\mathfrak{M}_0$.

---

[13]Does it support subcision and insertion? If it doesn't then that is a reason for not including them in AST.

Thus this inner model construction gives us a consistency proof for NF_2 relative to AST + existence of $B(x)$ for all x.

It's a fact (i'm guessing) that any CO-model of AST has an inner model that satisfies NF_2. Does this shed any light on the "getting things free" matter?

I do not delude myself that there is a huge groundswell of public demand for consistency proofs of NF_2 relative to systems that are even weaker, but it is pleasing that the inner model construction in the CO setting can give rise to relative consistency results in the same way as inner models do in the traditional wellfounded setting. In both settings we find that we have more control over what happens in the inner model than we do over what happens in the enveloping model: L always satisfies choice even if V doesn't; the inner model of hereditarily low-or-co-low$^{\mathcal{E}}$ sets is a boolean algebra even if the ambient model isn't.

However what we came here for was a proper CO construction of a model of NF0. It was pointed out in [8] that NF0 has a term model: a model consisting solely of denotations of closed set abstracts, and that the theory of this term model is decidable. This observation is the key to finding a CO construction for embedding an arbitrary model of ZF as the wellfounded part of a model of NF0.

Start with a model of ZF(C). Give names to all its elements. Consider now the class of NF0 words over these names. "NF0 word"? 'V' is a word, all the names are words, and if w and u are words so are $B(w)$, $\{w\}$, $u \cup w$, $u \cap w$ and $V \setminus w$. We do not in fact want *all* these words, since there is duplication. However the word problem for this semigroup is easily solvable, so we discard all but one representative of each equivalence class. (The obvious word to choose from each class will be one of the words in that class that are in disjunctive normal form.) The collection of remaining words is in bijection with the original model. Taking this bijection to be k we can then define

$$x \in_{new} y \text{ iff } \begin{cases} k(y) \text{ is the letter V; or} \\ k(y) \text{ is the name of a set } z \text{ and } x \in z; \text{ or} \\ k(y) \text{ is a word } u \cup v \text{ and } x \in_{new} k^{-1}(u) \vee x \in_{new} k^{-1}(v); \text{ or} \\ k(y) \text{ is a word } u \cap v \text{ and } x \in_{new} k^{-1}(u) \wedge x \in_{new} k^{-1}(v); \text{ or} \\ k(y) \text{ is a word } V \setminus v \text{ and } x \notin_{new} k^{-1}(v); \text{ or finally} \\ k(y) \text{ is a word } B(v) \text{ and } k^{-1}(v) \in_{new} x. \end{cases}$$

Now this is not literally a CO construction ... perhaps we shouldn't take these things too literally.

It is clear what the engendering relation of this model is: $x \mathcal{E} y$ iff x is a generator in the word $k(y)$.

7.3.3 Complementation, adjunction, subcision and $B(x)$

Here is an Oswald-style model. Reserve the even bits for a flag. So, when asking whether or not $x \in_{\texttt{new}} y$, $x, y \in \mathbb{N}$, extract a natural number from the even bits.

If this number is 0 you say $x \in_{\mathbf{new}} y$ iff the $2x + 1$th bit of y is 1. If it is 1 you say $x \in_{\mathbf{new}} y$ iff the $2x + 1$th bit of y is 0. If the number is $k + 2$ then y is going to have finite difference from $B(k)$, in that the odd bits tell you the symmetric difference between y and $B(k)$.

I don't think it gives you binary union and intersection.

7.3.4 Models of NF0 with antimorphisms

If we want to end-extend an arbitrary model of ZF to a model of NF0 with an antimorphism we do the construction of the previous section with a couple of add-ons. We add a new constructor σ, to denote a polarity. Since σ is ___rewrite this___ an antimorphism we have $\sigma(x) = V \setminus \sigma``x$, and if it is actually a polarity we have $\sigma(B(x)) = B(\sigma(x))$, $\sigma(x \cup y) = \sigma(x) \cap \sigma(y)$, $\sigma(x \cap y) = \sigma(x) \cup \sigma(y)$, $\sigma(\{x\}) = V \setminus \{\sigma(x)\}$ and $\sigma(V \setminus x) = V \setminus \sigma(x)$. These identities give us— in addition to the reduction rules for NF0 from [8]—the reduction rules for σ that enable us always to push all occurrences of 'σ' inwards. Of course we can simplify $\sigma(w_1) \in \sigma(w_2)$, $\sigma(w_1) = \sigma(w_2)$ and $\sigma^2(w)$ to $w_1 \notin w_2$, $w_1 = w_2$ and w respectively. $\sigma(V) = \emptyset$ and $\sigma(\emptyset) = V$. '$\sigma(\sigma)$' won't simplify any further. Thus every word can be simplified into a form where the only occurrences of 'σ' are in σ (as a function graph, a set, an argument to B, $\setminus$ etc.) and $\sigma(\sigma)$. Observe that neither σ nor $\sigma(\sigma)$ can be equal to any boolean combination of Bs and singletons.

Equality and membership between words are thus decidable.

There still remains a little bit of work to do to check that the term model is extensional.

start of work-in-progress

A useful thought: σ is a set of pairs, and $\sigma(\sigma)$ is a set of complements of pairs.

It will be sufficient to show how to exhibit a witness to $w \neq \emptyset$. Without loss of generality a word is a union of intersections. So to check whether or not words are empty it is sufficient to be able to check whether intersections of $B(x)$s, $\overline{Bx}$s, finite sets, σ and $\sigma(\sigma)$ are empty.

$B(x) \cap B(y) \cap \sigma$ is either empty or a singleton.
$B(x) \cap \overline{By} \cap \sigma$ is either empty or is infinite.

The question of whether or not $B(x) \cap B(y) \cap \sigma$ is empty reduces to the question $\sigma(x) = y$, and the question of whether or not $B(x) \cap \overline{By} \cap \sigma$ is empty reduces to the question $\sigma(x) \neq y$.

end of work-in-progress

8   Limitations on CO Constructions

Discuss the recurrence problem here. Allude to [11]. If the collection of [isomorphism classes of] widgets supports a widget structure then once you add isomorphism classes for all widgets you create new widgets that need to be swept up if one is to end up with a model in which isomorphism classes exist for all widgets. There are really two problems here not one.

One is the problem of adding isomorphism classes for all widgets, co-low as well as low. This happens even without the recurrence problem. One instance would be the task of ensuring that every isomorphism class of groups is a set. This may even happen "for free" in the basic (what Sheridan calls the "$m = 0$") model.

The other is the recurrence problem proper, of which one example would be that of ensuring that every set belongs to a (set) cardinal number. This clearly does not happen in the basic model. It would be nice to know iff one can execute a straightforward CO construction of a model in which every set has a cardinal that is a set and that NC is a set. I would guess that this can be done, and it would be good to do so, to point up the difference between NC (which is tractable) and NO (which is not).

8.1   Power Set

Let's modify our construction to obtain models of NF_2 + Power set. This is actually a generalisation of what we have just done (rather than a complication of it), though this fact might not be immediately obvious. $B(x) = V \setminus \mathcal{P}(V \setminus \{x\})$, so once we have created all power sets we have created all $B(x)$s as well. Furthermore, taking power set as the next operation is in fact historically correct: it is what Emerson Mitchell did in his Ph.D. thesis, [16].

If x was created by the vanilla wand then $\mathcal{P}(x)$ will also be created by the vanilla wand, and $V \setminus \mathcal{P}(x)$ will be created by the complement wand—both of them on the very next day. For every other set x we will have to set aside two unused atoms b and c with a view to making b into $\mathcal{P}(x)$ and c into $V \setminus \mathcal{P}(x)$. Thereafter for every y whenever we decide that y is a subset of x we put it onto b and when we decide that y isn't a subset of x we put it into c.

Now let's rerun the operation and see what we get this time. On Day One we do all the creating with the vanilla wand or the complement wand that we can fit in before we run out of ordinals, and along with the wand waving we earmark lots of atoms to be $\mathcal{P}(x)$ and $V \setminus \mathcal{P}(x)$ for the other sets we are creating. This gives a rank function on these sets in an obvious way and we may as well make a note of this fact now, since the rank function will come in handy later. (A detail: for the purposes of the rank function we count only the number of uses of the vanilla and complement wands: the rank of $V \setminus \mathcal{P}(\mathcal{P}(V \setminus \mathcal{P}(x)))$ where x is a set created by the complement wand is the same as the rank of x.)

Let us now fast-forward to Day Two, and think about deciding the truth values of expressions like $s \in t$ where s and t are nasty molecular things built up out of the two operations $\mathcal{P}$ and $\mathcal{P}(V \setminus \ldots)$. We can use the following reductions:

$$
\begin{array}{lcl}
\mathcal{P}(x) \in \mathcal{P}(y) & \Rightarrow & \mathcal{P}(x) \subseteq y \\
(V \setminus \mathcal{P}(x)) \in \mathcal{P}(y) & \Rightarrow & (V \setminus \mathcal{P}(x)) \subseteq y \\
\mathcal{P}(x) \subseteq \mathcal{P}(y) & \Rightarrow & x \subseteq y \\
(V \setminus \mathcal{P}(x)) \subseteq \mathcal{P}(y) & \Rightarrow & \bot; \\
\mathcal{P}(x) \subseteq V \setminus \mathcal{P}(y) & \Rightarrow & \bot.
\end{array}
$$

Now we have to think about expressions that do not match the input to any of these reduction rules. $V \setminus \mathcal{P}(x) \in y$ and $\mathcal{P}(x) \in y$ arise from cases failing to match the input to the first two rules because the expression to the right of the '$\in$' isn't $\mathcal{P}$ of anything. Then it must be something denoting a set created by the vanilla wand or the complement wand, and in either of those cases we know what its members are and there is no more work to do.

The third rule draws our attention to cases like $\mathcal{P}(x) \subseteq y$ and $x \subseteq \mathcal{P}(y)$, and the fourth gives us cases like $V \setminus \mathcal{P}(x) \subseteq y$ and $V \setminus x \subseteq \mathcal{P}(y)$, which are in fact the same case.

This eventually reduces any question like the one we started with to one of the three irreducible cases: $\mathcal{P}(x) \subseteq y$, $(V \setminus \mathcal{P}(x)) \subseteq y$, and $y \subseteq \mathcal{P}(x)$, where in each case y is created by the vanilla wand or the complement wand.

Let us start with the first one: $\mathcal{P}(x) \subseteq y$. We know that y is created by the vanilla wand or the complement wand and x isn't created by either. We can drop one of these cases immediately, because if y had been created by the vanilla wand then so would $\mathcal{P}(x)$ and we would know the truth-value. So suppose y had been created by the complement wand. If we write 'z' for '$V \setminus y$' this becomes $z \subseteq V \setminus \mathcal{P}(x)$ where z is created by the vanilla wand. Is every element of z a subset of x? z was created by the vanilla wand so we do at least know what all its members are, and the only question is whether or not all of them are subsets of x. This generates huge numbers of questions like '$w \subseteq x$?' which sound just like the one we started off with. But this time the situation is subtly different: the rank of the ws that we have to consider is *bounded*.

The two remaining cases yield to a similar analysis and perhaps in later draughts of this (and the final version if there ever is one!) i shall spell out some of the details. (They are all in Emerson's thesis [16] but i actually worked all this out myself!) For the moment my feeling is that there isn't a great deal of profit in this, as the main aim—to get a taste of how this construction feels–has probably been achieved.

Perhaps some things will have become clearer to the reader by now, as they did to me once i had reached this stage. One thing that is not hard to see is that the clever idea of generalising the transfinite construction of the wellfounded sets, first to a transfinite construction with complements and then a transfinite construction with B objects and then one with power set, is a bit of a red herring—or at least the fact that it is a *transfinite* construction is a bit of a red herring: the hard work comes in Day Two where we have to unravel all the words in $\mathcal{P}$ and B and so on, and the fact that the construction on Day One was of uncountable length is completely irrelevant. In fact we could even replace the vanilla wand or the complement wand with two finitary operations of complementation and $\lambda xy.(x \cup \{y\})$ and thereby bring out into the open that

all the interesting work takes place on Day Two.

Another thing (and i have put this after the digression rather then before it beco's it will keep us busy for a long time) is that, mucky tho' this induction is for the power set operation, the only reason why it works *at all* is that $\mathcal{P}$ is injective and that if all we ever do is create sets by the vanilla wand, the complement wand, and power set and power-set-with-complementation then we will never create any fixed point for $\mathcal{P}$ other than V. It works because there is only ever one way to create any particular set. Sometimes, *in the beginning* as it were, there is—or was before we (promptly) corrected it—the possibility of sets with two ways of being built. For example if x is a set created by the vanilla wand, then $\mathcal{P}(x)$ could be created either by $\mathcal{P}$ or by the vanilla wand. However we saw that one coming and resolved to use $\mathcal{P}$ only on sets that had not been created by the vanilla wand.

8.1.1 A Set of all Ordinals?

We are now in a position to review the project of obtaining a CO model with a set of all ordinals. I hope the reader is willing to believe that a CO construction can be found that will give a model of NF∀ that has ordinals for all low wellorderings and the set of all such ordinals. (NF∀ is the extension of NFO that allows the existence of $\{x : (\forall y)\phi(x, y, \vec{z})\}$ where ϕ is stratifiable and quantifier-free.) It is shown in [8] that NF∀ has a decidable term model, and we can easily find CO constructions for theories with decidable term models.

8.2 How difficult is it to arrange for the graph of the engendering relation to be a set?

8.3 Cardinals

In general one can easily incorporate into the new model $\sim$-equivalences classes for any equivalence relation $\sim$ defined on low sets. The strategy is to reserve a few numbers to serve as these equivalence classes and then do the by-now customary coding on the remainder.

To illustrate, let us show how a modification of Oswald's construction will give us a model of NF_2 containing Frege-style natural numbers (where the natural number n is $\{x : |x| = n\}$).

We reserve countably many objects to be the natural numbers. Let's reserve $2n + 1$ to be the Frege natural number n, the set of all n-membered sets. We define the new membership relation by:

$m \in_{\mathtt{new}} 2n + 1$ iff m has precisely n distinct members$^{\in_{\mathtt{new}}}$;

$m \in_{\mathtt{new}} 4n$ iff the mth bit of n is 1;

$m \in_{\mathtt{new}} 4n + 2$ iff the mth bit of n is 0.

The squeamish might feel there is some circularity in the first clause. So we could say instead

$m \in_{\texttt{new}} 2n + 1$ iff $m = 4k$ and k has precisely n bits set;

Observe that there is no recurrence problem in this case: this construction does not invent any new finite sizes.

9   stuff to fit in

A conversation with Randall about doing an analogue of Kaye-Wong for the Oswald interpretation. The challenge is: "can one axiomatise the theory of those models obtaained from models of PA by the Oswald interpretation?"

In any such model, every set is small or co-small, so we have to be able to capture smallness. Then, once we've done that, we can define what the engendering relation E is; then we say that E and that, for every x, the set of E-ancestors of x is a set—probably specify small set to be safe. This will justify E-induction and recursion. Then we can define the arithmetic operations by E-recursion.

Randall suggests defining "x is small" as $(\forall Y)(x \in Y \wedge (\forall z \in Y)(z \neq \emptyset \to (\exists! w \in z)(z \setminus \{w\} \in Y) \to \emptyset \in Y))$ and that there are such Y.

You have to be very careful about the definition of finite/small. They aren't the same. We are certainly going to be in $\mathrm{NF}_2 + \neg$ infinity—for *some* concept of infinity ... so V is going to be finite in some sense. But not in the same sense of finite as the sense in which the small sets are precisely the finite sets. As i say, you have to be careful.

One can get a handle on this by asking what axioms play the rôle—in this context—of the negation of infinity played in the Kaye-Wong case. Presumably the axiom would be "no small set is infinite". We also need "every set is small or co-small" and apparatus for E-induction.

Perhaps we can get away with "$(\forall x)$(either x or $V \setminus x$ is finite)" for some suitable sense of finite. But of course they might *both* be finite!

The really odd thing is that the model of NF_2 that one gets by Oswald is both externally wellordered and internally amorphous!

In CUS define $h(x) = $ `if` $\mathrm{low}(x)$ `then` $\langle h\text{``}x, 0\rangle$  `else`  $\langle h\text{``}(V \setminus x), 1\rangle$.

Observe that every value of h is a wellfounded [hereditarily low] set, with the consequence that whenever x is a low set $h\text{``}x$ is a wellfounded [hereditarily low], so every low set is the same size as a wellfounded [hereditarily low] set, so replacement for low sets follows from replacement for wellfounded [hereditarily low] sets.

Coret's axiom is precisely the assertion "Every set is low"!!

Use again the *bon mot* about the universal set being the empty set with a party hat on. A kind of permanent saturnalia.

If $\langle M, \in \rangle$ is a model of (say) ZF, then $\langle M, \notin \rangle$ is antimorphic to it. Is there a sensible operation of *addition* one does to these two antimorphic structures to obtain the CO model?

Should we have a section on the recurrence problem, and allude to my *Philosophia Mathematica* paper, [11]?

What terms are CUS-suitable? By which i mean t is suitable if the result of substituting it for variables in a Δ_0 expression is a Δ_0 expression.

REMARK 5 *The ordering of the wellfounded sets (of the Oswald model) in numerical order coincides with the ordering of them defined by the recursion* $x \leq y \longleftrightarrow (\forall x' \in (x \setminus y))(\exists y' \in (y \setminus x))(x' \leq y')$.

The binomial coefficient $\binom{n}{m}$ is odd iff m is a subset of n in the sense of the Ackermann model. A cute fact! Is there a cute version of this fact in connection with the Oswald model?

Just discovered this amazing fact that if p^c is the largest power of p that divides $\binom{a+b}{a}$ then c is the number of carries performed in the addition of a and b to base p.

Is that any use?

Might it be possible to show that there is in general no way—internal to the CO model—of identifying the sets created by the first wand?

Might have to say something about how if you perform a naïve CO-construction on a model of NF (for example) then $k^{-1}\langle \emptyset, 1 \rangle$ and $k^{-1}\langle V, 0 \rangle$ have the same members in the new sense, and violate extensionality.

An illustration. Think of the Von Neumann ω as an $\in$-structure.

Let $\langle n, 0 \rangle$ be $2n$ and let $\langle n, 1 \rangle$ be $2n + 1$. Do a CO construction. Then $n \in_{\text{new}} m$ iff either

$$(\exists k)(m = 2k \wedge n < k) \vee (\exists k)(m = 2k + 1 \wedge k \leq n).$$

equivalently

$$(\exists k)[m = 2k \wedge n < k. \vee .m = 2k + 1 \wedge k \leq n].$$

You get (as it were) $\omega + \omega^*$. (I think!) Everything has either finitely or cofinitely many predecessors. Is the order total?

Suppose $n \notin_{\text{new}} m$. Then

$$\neg(\exists k)[(m = 2k \wedge n < k) \vee (m = 2k + 1 \wedge k \leq n)].$$

$$(\forall k)[\neg(m = 2k \wedge n < k) \wedge \neg(m = 2k + 1 \wedge k \leq n)].$$

$$(\forall k)[(m \neq 2k \vee k \leq n) \wedge (m \neq 2k + 1 \vee n < k)].$$

$$(\forall k)(m \neq 2k \vee k \leq n) \wedge (\forall k)(m \neq 2k + 1 \vee n < k).$$

$$(\forall k)(m = 2k \to k \leq n) \wedge (\forall k)(m = 2k + 1 \to n < k).$$

Is this $m \in_{\mathtt{new}} n$? It's equivalent to

$$(\exists k)(m = 2k \wedge k \leq n) \vee (\exists k)(m = 2k + 1 \wedge n < k).$$

Should we say something more about the $\in$-game? Or is that in my 2001 paper?

One nice thing about CUS is that it respects the intuition that V and the other big collections can be sets, while at the same time confirming our suspicions that there is something different about them. It explains what is distinctive about the sets that appear in so-called ordinary mathematics: they are all *low*. Are they all *hereditarily* low? Perhaps, perhaps not: it all depends on how you have implemented the various non–set-theoretical mathematical entities as sets.

It seems to be generally agreed—among those mathematicians that have an interest in foundations—that *au fond* philosophy of mathematics is what happens when mathematicians examine their mathematical practice. The difficulties we encounter in mathematical practice are typically not ontological, so we tend to feel that agonising about the ontological status of the subject matter of mathematics is not a good use of our time. I, for my part, am fully signed up to this point of view. In this context one of the interesting features of the CO construction is that it appears to confront us with an ontological question. The point of departure for many people when first exposed to CO models will be that the sets in the cumulative hierarchy are OK (somehow) but that the co-low sets of CO constructions are nothing more than an annoying mind-game. However, in explaining why this might be so one is forced to consider an ontological question of the kind that we routinely turn our backs on. However, *this* ontological question, at least, does actually arise out of mathematical practice: Alonzo Church was a mathematician, and a rather good one at that. CUS is a piece of mathematics, and the philosophical questions that it raises happen—just this once—to be ontological.

9.1 I briefly thought that CO Methods could give Models wherein all Sets are wellfounded

Define:

$n \in m$ iff
m is even and the nth bit of $m/2$ is set; or
m is odd, n is even and the nth bit of $(m-1)/2$ is clear.

What do we get? 0 is the empty set as before, and—more generally—even numbers are finite sets. Indeed every finite set is coded by an even number. 1 is no longer V (it contains only *even* numbers) and odd numbers generally contain only even numbers, and cofinitely many even numbers at that. 1 is the $\subseteq$-largest number.

Actually i don't think these things are wellfounded after all.

Consider 8. It is even, and the 1th bit of $8/2$ is set; no other bit is set. So $8 = \{1\}$. But observe that $8 \in 1$ because the 8th bit of $(1-1)/2 = 0$ is clear. So this structure is not wellfounded.

9.2   An idea of Tim Button's

Start with ZF + there is a unique atom, and use the atom as the flag in a construction of the basic CO model wherein every set is low or co-low. The situation now appears to me to be slightly more complicated than i thought at first, but here goes:

We start in a model of ZF + there is a unique atom. Let F be a function class that bijects $V \setminus \{a\}$ with V, where a is the atom. We then say

$x \in_a y$ iff either (i) $a \in y \land x \notin F(y \setminus \{a\})$

or (ii) $a \notin y \land x \in F(y)$

It might help to be explicit about what F is. One thing it could be in the function that sends $\iota^{n+1}(a)$ to $\iota^n(a)$ and fixes everything else, but it oughtn't to matter.

9.3   Classes

A possibility suggested by a question of Zuhair's .... Presumably one can set up a *k*oding function starting with a model of NBG, so that in the CO model one has proper classes?

9.4   Another idea of Tim's, May 2018

Hi Randall & Thomas

I think I have worked out a nice way to present definitional equivalence, without leaning on exterior results. But maybe I'm kidding myself.

As in my last email, here's how I interpret two wand theories in a one wand theory:

Let $t(x) = x + 1$ if x is a natural number, and $t(x) = x$ otherwise. Now interpret membership thus:

1. if $0 \in a$, then $x \in^I a$ iff $t(x) \in a$
2. if $0 \notin a$, then $x \in^I a$ iff $t(x) \notin a$

Working in the opposite direction, "unravel" that membership relation as follows.

Still use $t(x) = x + 1$ if x is a natural number, and $t(x) = x$ otherwise. Recursively define:

- $r(V) = 0$;
- $r(0) = \{0\}$;
- $r(a) = \{t(r(x)) : x \in a\} \cup \{0\}$, if a is low;
- $r(a) = \{t(r(x)) : x \notin a\}$, if a is high

Working in a two-wand setting, it is easy to prove these.

Theorem. r is a bijection from V to the hereditarily low sets.

Theorem. The hereditarily low sets model the one wand theory.

Now define:

$x \in^J a$ iff $r(x) \in r(a)$

So, using $\in^J$, I can regard V as a model of the one wand theory.

I *think *this is all that's required. I think it's immediate that $\in^I$ and $\in^J$ are inverses, but maybe I'm missing something. In any case, I've updated the paper at the dropbox link, and you can see the implementation in Section 25. `https://www.dropbox.com/sh/zp4ewt8qwqqyw7u/AABv8adpxZ7sycO6Rul194gVa?dl=0`

All the best,

Tim

9.5   Hereditarily transitive sets in CO constructions

I don't think there is anything preventing us from building a CO model containing the set of all low von Neumann ordinals. There is no paradox beco's it's not itself a low von Neumann ordinal. And it's not the lfp because it's not the set of all its transitive subsets. It is a transitive subset of itself but it's not a member of itself, so it's not the set of *all* its transitive subsets, merely *some* of them (well, all but one). In my joint article with Thierry we make the point that greatest fixed points are less paradoxical than least fixed points. Can we have a CO-model that contains the gfp for $x \mapsto$ the set of transitive subsets of x?

Might we find such a set x in the form of a solution to the equation $x \setminus \{x\} =$ class of von Neumann ordinals? Quite possibly, but bear in mind that this set is clearly self-membered. It is hereditarily transitive being a set of hereditarily transitive sets (easy) that is itself transitive. But then the model housing this object is not a model of AST beco's if there is an x s.t. $x \setminus \{x\} =$ class of von Neumann ordinals then subcision (which is a theorem of AST)[14] would give us the set of von Neumann ordinals. Isn't that OK, beco's it's the set of *small* von Neumann ordinals ...? No, beco's AST will give $x \cup \{x\}$ always and then the set of small von Neumann ordinals will give us a larger von Neumann ordinal, and then the gfp would not longer be the gfp. If we want a model of AST that contains such a gfp then the gfp will have to contain lots of rubbish. It may be that we can arrange for every member of a sequence $\langle A_i : i \in \mathbb{N} \rangle$ to be a set (perhaps even the sequence itself to be a set, because of low replacement) where A_0 is the gfp, and, for each $n \in \mathbb{N}$, $A_{n+1} = A_n \setminus \{A_n\}$, and $\bigcap_{i \in \mathbb{N}} A_i = A_\infty$ is the class of low von Neumann ordinals. There is no reason for A_∞ to be

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[14]Here we are assuming that subcision is an axiom of AST

a set of the model, and indeed it can't be, beco's if it is then $A_\infty \cup \{A_\infty\}$, $(A_\infty \cup \{A_\infty\}) \cup \{A_\infty \cup \{A_\infty\}\}$ and so on will be hereditarily transitive sets not in A[15].

Whenever X is a candidate for being the gfp, a better candidate is any Y s.t. $X = Y \setminus \{Y\}$. No reason why there should be such a Y of course. (Things like that contradict the $\forall^*\exists^*$ conjecture). The $\forall^*\exists^*$ conjecture will imply that, for all X, if there is such a Y then it is unique.

9.5.1 A set of Hereditarily Transitive sets

It would be nice to have a CO model of NF_2 (or even mere AST) with a set equal to the set of its transitive subsets. Subcision is enuff to show that there is no least fixed point, and we do have subcision in NF_2. Finding such a model would make a useful point about the paradoxical nature of lfps.

9.6 A CO model for a set theory that supports category theory

Set theory for category theory must have all local identity relations ($\mathbb{1}_X$ for all X) and all compositions of functions.

Start with a model $\mathfrak{M} \models ZF$, where ZF (or whatever the theory is) is expressed in a language with primitive pairing and unpairing. There will be a definable class bijection $k : M \longleftrightarrow (M \times \{0, 1, 2\})$.

The new CO structure has the same pairing relation, and we can define a new membership relation on M (giving us $\mathfrak{M}_1 = \langle M, \in \rangle$) by saying

DEFINITION 11

$$\mathfrak{M}_1 \models x \in y \text{ iff } \begin{cases} \text{either } \mathtt{snd}(k(y)) = 0 \text{ and } x \in \mathtt{fst}(k_1(y)); \text{ or} \\ \mathtt{snd}(k(y)) = 1 \text{ and } x \notin \mathtt{fst}(k(y)); \text{ or} \\ \mathtt{snd}(k(y)) = 2 \text{ and } x = \langle x', x' \rangle \text{ for some } x' \notin \mathtt{fst}(k(y)) \end{cases}$$

The third clause provides the restriction of the identity function to cosmall sets. Restrictions of the identity to small sets of course exist beco's of clause 1.

Observe now that in this model the composition of two *functions* always exists and is another function. Composition of two small functions is another small function. There are no cosmall functions: the only non-small functions are the restrictions of the identity—and if you compose f with the identity you just get f.

Composition of relations is a different matter altogether! The composition of two small relations is small, and the composition of two cosmall relations is cosmall, but the composition of a small relation with a cosmall relation is intermediate. A relation that is a composition of a small relation with a cosmall relation can obviously be obtained in many ways, so this will be another instance of the *freeness* problem.

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[15]This section is going to have to be rewritten in the light of a final reasoned decision on whether or not subcision and insertion are axioms of AST

There are consistent fragments of NF in which one can prove that compositions of functions exist and that $\mathbb{1}\restriction x$ exists for all sets x. Randall says that NF$_3$ is one such (with a bit of trickery). What about the theory that just says that every set has a complement, that compositions of functions exist and that $\mathbb{1}\restriction x$ exists for all sets x? Is there a CO model for this theory? It would be the minimal theory with a universal set that supports category theory. It shouldn't be too hard to get a CO model for it.

Is there a CO model for NF$_3$? Randall says 'no' and one can see why. It would be nice to have an impossibility proof.

If the coding function k is not onto then we get lots of atoms. If fact we should probably take the definition of the k function more seriously in general. How definable can it be?

9.6.1 Find a way of making the CO construction constructive

Let Ω be the generic power set of a singleton: $\Omega := \mathcal{P}(\{\emptyset\})$.

Is there a bijection $V \longleftrightarrow V \times \Omega$? Normally to obtain such a bijection one would use Schröder-Bernstein but S-B is not constructive. So that's one problem, for a start.

Anyway, let's minute that and press on. If we are only doing an NF$_2$ construction we can write something that doesn't look like a case split:

$$x \in_{\mathtt{new}} y \text{ iff } x \in \mathtt{fst}(k(y)) \longleftrightarrow \mathtt{snd}(k(y)) = 1$$

Does that work?

No, what you do is consider the individual worlds in the kripke model, and do the CO construction on each one individually.

That way you find that in the CO model the truth-value is no longer defined by stipulation at each world, but behaves instead like a molecular expression.

9.6.2 more low subsets than members?

Can we show that every set has more low subsets than members? Tarski shows that every set has more wellordered subsets than members. Mostowski collapse shows that every wellordered set is low, but this needs unstratified replacement.

However we can give a direct proof that every set has more low subsets than members. Sse i is a bijection between X and $\mathrm{Low}(X)$, the set of low subsets of X. Think about

$$\{x \in X : i(x) \text{ is wellfounded in the sense of } \langle X, i\cdot \in\rangle\}$$

This is clearly a low set but cannot be in the range of i beco's of Mirimanoff's paradox. This does not use replacement, and we can prove it in Mac. Why is it low? I'm not 100% certain but it is a set with a wellfounded extensional relation on it

We might be able to find a countermodel if Coret's axiom fails. (Clearly if Coret's axiom holds then every set has more low subsets than elements co's

every set is low!) So work in a model whose wellfounded part satisfies AC but where there are infinite Dedekind-finite sets of Quine atoms. That might do something for us..

Hang on, rub eyes, deep breath. How can V possibly have more low subsets than members?! Duh!

I'm cracking up. Why can V not have more low subsets than members?

9.7 non-freeness

The way in which we modified $G_{x=y}$ to take account of the fact that there might be two constructors rather than one can obviously be extended to the case where these are yet more constructors. However, there is an important proviso: *it must be possible to ascertain which constructor a set was made from.* We noted that no low set is self-membered and every co-low set is, but this cannot be used to tell us whether a set is low or not, since we we cannot "unlock" a set (and thereby see whether or not it is a member of itself) until we know what constructor it was made with![16] The set has to wear its constructor on its face like a cicatrice. In other words, for this story to work, *no set shall be manufacturable in more than one way.* This means that *the algebra of terms generated by the constructors must be free* (This is a separate concern from the fact that you might lasso the same pre-set twice—indeed infinitely often)

What happens if some sets can be constructed in more than one way, by using different constructors?

One thing that happens is that we would have to modify the rules of $G_{x=y}$ yet further to incorporate a stage at which `Equal` and `Notequal` negotiate about which destructors they are to use to take x and y apart. Ominously there is no obvious way to do this. Is player `Notequal` to choose a wand for one of x and y? If so, then `Equal` 's move is forced; she has to pick the same wand, so in effect `Notequal` is deciding which wand to use. But why should it not instead be `Equal` who chooses? There doesn't seem to be any good reason to offer this choice to one player rather than the other. If we allow `Notequal` to make the choice, he might be able to pick a wand that was used to construct x but not y. But if he can do this, x and y are clearly unequal anyway, so that is all right. If we allow `Equal` to choose it, she might find a wand that was used to build both x and y even tho' they are distinct. But then she will presumably be found out later, since—if x and y are indeed distinct—they cannot be obtained by applying one and the same wand to two distinct presets, the wands being deterministic. `Notequal` will be able to find a member of the symmetric difference and play that.

Here's a game-theoretic account of equality in the absence of freeness. Let us say that $C(x)$ is the collection of constructors c such that x can be made by applying c to lasso-contents $c(x)$. ("If x was made from c then it was made from preset $c(x)$"). Then $G_{x=y}$ is played as follows.

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[16] 4/i/13. I don't see this. Why?

If x and y are both empty, `Equal` wins;
if precisely one of them is empty `Notequal` wins.[17]
If neither is empty `Equal` picks an ordered pair c from $C(x) \cap C(y)$,
and loses if she can't.

This dismembers x and y into presets $c(x)$ and $c(y)$. `Notequal` now picks
either

(i) a member x' of $c(x)$ (in which case `Equal` must reply with a member y' of $c(y)$)
or
(ii) a member y' of $c(y)$—in which case `Equal` must reply with a member x' of $c(x)$.

They then play $G_{x'=y'}$.

9.8 Does NF have an iterative model?

Kaye has an unpublished prediction that—even if NF is consistent—there will be no Church-Oswald style interpretation of NF into a ZF-like theory. In the terminology used here, Kaye is saying that the sets of the NF world cannot be thought of iteratively. But we know that in any case the constructors more-or-less have to be Δ_0 beco's o/w the algorithm wouldn't give a decision procedure. So our hands are tied. If Kaye is right this would explain why the consistency problem for NF is so hard: we cannot prove NF inconsistent because it isn't, and it is hard to prove it consistent because there is no iterative analysis of the sets-of-the-NF-world.

I suspect also that there is an important truth along Kaye's lines, but the only formalisations i've been able to give of it all make it trivially true. For example: the constructors have to be Δ_0 if the recursion is to give us a decision proceedure for $\in$, and it's easy to see that no Δ_0 constructors will ever give us—for example—the set of all ordinals. But this is all pretty obvious, and i suspect Kaye would say that that is not what he meant. Another straw in the wind is a result of Bowler's [3] to the effect that NF proves, for each concrete k, that every wellfounded set is of size $< |\iota^k\text{``}V|$. If $i : V \to \mathcal{P}(V) \times W$ is injective and $\mathcal{E}$ is wellfounded, then how large must W be? There ought to be a theorem saying it's large.[18]

This raises a question in connection with NF. If some models of NF have an iterative genesis then there will be such a function $i : V \hookrightarrow S \times W$. i is a kind of omnibus uniform global destructor: that is to say, there will be a *notion of smallness*, an ideal in V such that the membership relation restricted to it is wellfounded.

There is no reason whatever to suppose that this i has any stratifiable description.

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[17] Notice that this means that we never feed an empty set into the one-armed bandit.
[18] Does Forti-Honsell have this?

So the ambiguity (non-freeness) of the construction can be seen—in this picture—as an uncertainty about the soul, the identity of a set.

There are two major issues in connection with this:

(i) do we expect our constructors to generate a model of NF in ω steps? Or are some sets going to have to await transfinite ordinals? If the second, then when we construct a model of NF by lasoos + wands we will inevitably create wellfounded sets of transfinite rank on the fly. Holmes has recently showed that every model of NF has a permutation model containing no infinite *transitive* wellfounded sets, so this seems rather unlikely. I don't much like the look of (i) beco's the second order categoricity stuff would mean (i think) that the output of the recursion after ω steps (which will be a term model for NF) will be unique, and i'm pretty sure that if NF has term models at all it has nonisomorphic ones. A term model, after all, is simply a model created using multiple wands *in only ω steps.* Is it true that if we can construct models of NF at all we can do it in at most ω steps? Well, it won't be true if NF proves the existence of wellfounded sets of infinite rank: no such set can be constructed in finitely many steps by the vanilla wand and we don't want wellfounded sets ever to be constructed by any other sort of wand. That's why Holmes' clever permutation is so important.

Let us not forget that all the constructors have to correspond to very simple operations—Δ_0 operations in fact, o/w the recursion they are plugged into would not give an algorithm. This means that if there is an iterative account of NF using only highly predicative constructors, it won't give us a model after ω steps. If it did, there would be a unique term model, and its theory would be decidable.

It shouldn't be hard to prove that if NF has a term model then it has more than one. Someone really should prove this!

(ii) What about the possibility of a set being constructed in two different ways, using two different constructors? This is complex and nasty: how does one test equality in this setting..? It may be that the universe of sets in NF is so rich that one cannot construct things in a parsimonious way that gives each set a unique provenance...

9.9   More leftovers

The following questions might be frequently asked

Q(i): *Are these CO sets the correct explication of the pre-formalised concept of set?*

Answer: Does it matter? In any case history will decide for us and we do not need to worry.

Q(ii): *Are they legitimate mathematical objects?*

Answer: Surely the answer to this must be an unequivocal "yes!" (Conway's Principle in the Appendix to part 0 of [6] p 66.

Q(iii): *Do I want to study them?*

Answer: Up to you!

Q(iv): *Are the (presumably good) reasons i had for studying sets also reasons for studying these chaps?*

Answer: This one has some bite: my response of course is that yes, they are. If not why not? A good answer to that challenge would be interesting.

Both the constructors considered so far act on the contents snared by a single lasso. Should one perhaps allow constructors that accept two inputs not one?

The two constructors we have seen so far—both the vanilla constructor that just turns the lasso-contents into a set and the complement constructor that provides the complement—do not look inside any of the sets that have been lassoed.

Both constructors so far correspond to *total functions*. Is this necessary for smooth functioning of the iterative apparatus? Or can we have partial constructors as well?

Conversation with Max. He suggests that you might need something extra to infer that these co-low sets are not constituted by their members but rather by their non-members. The mere fact that that is how the algorithm runs isn't enuff by itself. And I think the answer is that mathematical objects have this operationalist quality that means that the algorithms arising from the constructions of the objects are constitutive in precisely this way—as we felt on page **??** that the recursive story was constitutive of identity for wellfounded sets.

Similarly we will have to decide whether by (2) we really mean no more than that $\in$ is extensional—in which case we will happily allow co-low collections to be sets—or whether we perhaps mean something stronger and with ontological significance—in which case only low sets are sets and the co-low collections are something else. In this context it might be helpful to remind ourselves that the *aperçu* (2) about sets being the only mathematical structures that are determined solely by their members was only—in the first instance—a way of contrasting sets with the other things that have members, like groups, fields, multisets, lists etc, all of which have members but are not uniquely determined by them: extra structure is required.

9.10 Bfexts

We can extend the two-wand story to Bfexts, as follows: We are interested in structures $\langle X, x^*, R, c \rangle$ where x^* is a constant, R a binary relation and c is a two-colouring of X: $c(x) \in \{$low, co-low$\}$. x^* is as before. We have the condition that for every x in X there is an R-path to x^*. However, an R-path is now something different. An R-path (for x) is a map p from an initial segment of $\mathbb{N}$ to X satisfying $p(0) = x$ and for all $n > 0$ in the domain of p, if $c(p(n+1)) = $ low then $\langle p(n), p(n+1) \rangle \in R$ and if $c(p(n+1)) = $ co-low then $\langle p(n), p(n+1) \rangle \notin R$.

We no longer require R to be extensional in the old sense but in a new sense in which $R^{-1}``\{x_1\} = R^{-1}``\{x_2\} \to c(x_1) = c(x_2)$. The membership relation between these structures is as follows:

$\langle X, x^*, R, c \rangle$ is a "member" of $\langle Y, y^*, S, c' \rangle$ if either

1. $X = (S^*)^{-1} \text{``}\{y^*\}$,

2. $x^* \, S \, y^*$; and

3. $c'(y^*) = \text{low}$

or: One of the above conditions fails and $c'(y^*) = $ co-low. (Notice overloading of the asterisk)

(We have to be careful how we state this last condition since we do not wish to include as "members" of $\langle Y, y^*, S, c' \rangle$ any $\langle X, x^*, R, c \rangle$ that are iso to anything that "belongs" to $\langle Y, y^*, S, c' \rangle$.)

Once one has done this, several things become clear.

1. The binary relation *within* the relational structures is the engendering (ontological priority) relation and the embedding relation *between the isomorphism types* is the membership relation between the represented sets.

2. It's simplicity itself to add more colours to correspond to new constructors. The Bfexts-with-their-bells-and-whistles are now clearly *notations* for sets.

3. They are of course also *structures*. Isomorphism between structures can be captured by Ehrenfeucht-Fraisse games and as soon as one thinks about E-F games between Bfexts-with-bells-and-whistles one can see that the identity game is just a special kind of E-F game. It's a lot more specific of course, and we'll have to show that if `Notequal` has a winning strategy in the game to detect isomorphism then he also has one in the identity game.

4. j-equivalence is just the ability of player `Equal` to postpone defeat for j moves. Since an ability to postpone defeat for j moves is something one can capture with j alternations of quantifiers this ought to connect stratification with counting quantifiers—something i've been trying to do for years!

In general we are going to be interested in binary relational structures with an accessibility condition that have a designated element, and a colouring, If we want these things to be part of an iterative conception of set we will require the binary relation to be wellfounded. Finally, we need a kind of extensionality condition: $R^{-1}\text{``}\{x\} = R^{-1}\text{``}\{y\} \to c(x) \neq c(y)$. Actually the freeness condition imposes something much stronger than that, but this will do to be getting on with.

There is clearly something very striking about the way in which the well-founded sets are constructed by transfinite recursion. Very striking too is the fact (and it does appear to be a fact) that all the nonproblematic mathematics known to us can be interpreted in a theory of wellfounded sets. It is but a short step down the primrose path from being struck by these facts, to a state in which one believes the axiom of foundation.

On a naïve idea of unbounded (as in: On is unbounded) How can one say: iterate this construction through all ordinals and then . . . take away the number you first tho'rt of? The ordinals are unbounded aren't they? You never actually manage to ("get to") finish the construction. There is no "next stage". That's the whole point! *On* is when the "next stage" ploy ceases to work.

Another point about this construction is that you can do it not with atoms (as i do below), but by means of ordinary CS recursive datatype magic. You simply magic the things into existence. It's no harder to do the first and second wand than it is to do just the first wand. But if the naturalness of this cumulative hierarchy narrative is supposed to be a reason for believing in the existence of wellfounded sets, why isn't it also a reason for believing in the existence of the complements as well? How can the wellfounded set theory people be sure that God didn't construct all the complements as well?

I s'pose they'll say that the difference is that under the new dispensation some sets are created before some of their members. But it remains the case when you create a set that you know what its members are. (there is a well-founded relation "$x \in y \longleftrightarrow y$ is created by the vanilla wand" and we can define things by recursion on this relation.)

I don't think this is much of an argument. There may be people who worry about impredicative declarations like that of the reals say, when we don't yet know what reals there are. Deciding that therefore $\mathbb{R}$ isn't a set is nowadays felt to be a bit extreme!

What the methods of Church and Oswald prove is that God *could have* created these recursively engendered things had he felt like it . . . so how do we know he didn't? What would it be to discover that these things do not exist? It would be a discovery of a counterexample to the principle that existence is nothing more than freedom from contradiction. So, given that they exist, what reason might you have for not wanting to call them sets?

Altho' it is natural to look to this line of thinking to provide a consistency proof for *NF* eventually, for the moment it is even more natural to think of developing these constructions in *NF* itself. There is an inductively defined proper class which is the $\subseteq$-least thing containing all its subsets and closed under complementation and B, say. The fact that this object is presumably a proper class is actually not a problem, because the universal set of this model is the actual universe, and is therefore a set. The resulting model of *NF*O is not transitive of course, but that is hardly serious.

So we should be thinking about the intersection of all stratrud-closed sets closed under complements. Is this a model of *NF*?

Let's just note *en passant* that V is the only such set that is self-membered. If x is a stratrud-closed set that contains itself. Then both x and V are in x and they have the same members, contradicting extensionality.

10   An email from Roy Cook: 15.i.2016

Thomas,

I have a quick question that you might have some insight into.

So I have been thinking (as I often do) about how to construct models of set theory that (in an intuitive sense) maximize the of sets one gets.

Now, given a model $\mathfrak{M}$ of ZFC (or something sufficiently similar - i.e. a set of objects D and a subset of $D \times D$ that is meant to interpret $\in$), there are two well-known[19] constructions for getting a model with "additional" sets:

> (1) The method in your "The Iterative Conception of Set" paper (i.e. the Church-Oswald stuff) where (loosely put) we build a new model $\mathfrak{M}^*$ that satisfies the axiom of complement by taking the domain to be pairs $\langle a, b \rangle$ where a is 0 or 1 and b is a set from the original model.

> (2) The method often used in Aczel-style non-well-founded set theories, where, for example, we build a model $\mathfrak{M}'$ that satisfies some version of the anti-foundation axiom by taking the domain to be equivalence classes of isomorphic (or something weaker, depending on the exact anti-foundation axiom you want) collections of graphs of minimal rank.

Now, it seems to me that either method will produce a model of set theory in some intuitive sense, even if the initial model $\mathfrak{M}$ isn't a model of ZFC. In short, we could apply either (1) or (2) to an arbitrary model interpreting the language containing only '$\in$' as non-logical vocabulary and obtain a new model of that language. Basically, what you get out of the new model will depend on what you put into the original model $\mathfrak{M}$.

Now, if this is right, then the following question arises. What happens if we start with some reasonably well-behaved model $\mathfrak{M}$ (e.g. a model of ZFC) and then apply (1) to obtain $\mathfrak{M}^*$, then apply (2) to $\mathfrak{M}^*$ to obtain $\mathfrak{M}^{*'}$, then apply (1) again to obtain $\mathfrak{M}^{*'^*}$, and so on...

I haven't quite figured out how to handle limits of such a sequence yet, although it seems likely there would be a way. If so, then we could iterate transfinitely.

And if we iterate transfinitely, alternating between (1) and (2), then it feels like there should be a fixed point, since (a) each model is no bigger than the one before, and (b) there is an intuitive sense in which each model just adds more sets to the domain of the previous model. [Yes, this is very loose and intuitive - I might be wrong here, but it 'feels right'!]

Even if we can't iterate transfinitely, we could certainly iterate through omega, and then take the union of principles that are stably true (i.e. are true on every model in the sequence after some point) which, by compactness, would be consistent.

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[19]Okay, neither of these is all that well-known, or nearly as well-known as it should be, since most philosophers and mathematicians seem to think that the haphazard collection of "oh, and this one looks good too, and we probably need it!" axioms that comprise ZFC is somehow the single, unique, correct account of sets. But they are certainly contenders for the most well known!

Where things would get interesting, it seems to me, is when we move from $\mathfrak{M}^*$ (which is closed under complement) to $\mathfrak{M}^{*'}$, since then we would be introducing new sets based on graphs that are 'big' - that is, where the collection of nodes in the graph is equinumerous to the entire domain!

Has anyone explored anything like this? Do you have any intuition about what one might get? It seems like a cool question, but also a hard one (I have some pretty basic knowledge now about both NF-style set theories and Aczel-style set theories, but I'd have to learn a whole lot more to tackle this!). Any thoughts? Is it worth pursuing?

Roy

Extra credit: What happens if we apply the alternating sequence of constructions to a model of NF or NFU?

I suspect what needs to be said at this point is that any R-B construction of illfounded objects (such as turning every equivalence class of APGs into a set) can be incorporated into a CO construction. In this connection we need to think about illfounded objects being manufactured by bad coding functions.

11   A new introduction to CO models?

Idea: set out the Ackermann bijection and the Oswald bijection. Compose them! That elides $\mathbb{N}$ and gives you a bijection between V_ω and the term model for NF_2. Then snap your fingers and say "You can extend this to bigger models of ZF to unfold any model of ZF into a model of NF_2 whose wellfounded sets are the model you started with.

12   There needs to be a chapter on synonymy

. . . now that Tim Button has confirmed my suspicion that CUS (which he calls BLT) is synonymous with ZF.

This is a topic i am actively investigating, so I am not going to try to keep this section up-to-date. If you want the lates, Dear Reader, do email me.

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