

# An Essay on Philosophy of Mathematics

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# Contents

<b>1 u/g Lectures on Philosophy of Mathematics Lent 2020</b>	<b>7</b>
<b>2 Stratification in Set Theory and the Strong Typing of Mathematics</b>	<b>9</b>
<b>3 Vienna talk on stratification</b>	<b>11</b>
<b>4 Stuff to put in the right place</b>	<b>17</b>
4.1 What is the scope of set theory <i>sensu strictu</i> ? . . . . .	44
4.2 A Chapter on the evolution of the idea of an abstract data type . . . . .	45
4.2.1 CO Models give us the Correct Way to Understand the Limitation-of-Size Principle . . . . .	49
4.2.2 A rather good remark of Randall's . . . . .	53
4.3 Spurious detail . . . . .	59
4.3.1 Annoying Technicalities . . . . .	59
4.4 Polymorphism, not Sets vs Classes . . . . .	61
4.5 Some Polemic . . . . .	64
<b>5 Introduction: Background and Definitions</b>	<b>67</b>
5.1 Definitions . . . . .	67
5.1.1 Some simple theories of sets . . . . .	69
5.1.2 Notation for Ramsey theory . . . . .	69
5.1.3 Set-theoretic Existence Principles . . . . .	69
5.1.4 Pairing . . . . .	71
<b>6 Munich talk—to be recycled</b>	<b>73</b>
6.1 The Recurrence Problem . . . . .	76
6.2 Typing and Counting . . . . .	77
6.2.1 Burali-Forti . . . . .	79
<b>7 Definitions contrasted with Implementations</b>	<b>83</b>
7.1 Implementation and Definition [A stand-alone article in which I explain why one has to make this distinction] . . . . .	87
7.1.1 Stuff to fit in . . . . .	90
7.1.2 Letter to Michael Norrish . . . . .	93

7.2 Problem 1 . . . . .	95
7.3 Problem 3 . . . . .	97
<b>8 Emergence, Implementation and Replacement</b>	<b>101</b>
8.1 When are two implementations isomorphic? . . . . .	109
8.2 Implementation and Emergence: Homophonic Implementations .	111
<b>9 Typing and Stratification</b>	<b>113</b>
9.1 Russell-Quine Typing is more natural than you think . . . . .	114
9.2 NF, NFU and KF . . . . .	116
9.3 A discussion of $T$ functions? . . . . .	116
9.4 Pairing and Burali-Forti in NF . . . . .	117
9.4.1 Don't look inside . . . . .	120
9.5 Stratified pairing-and-unpairing . . . . .	122
9.6 Ordinals and the Extended Axiom of Counting . . . . .	124
9.7 Subversion of Stratification . . . . .	128
9.8 KF is just as good as Mac . . . . .	128
<b>10 Endogenous Strong Typing</b>	<b>131</b>
10.1 $T$ -functions on an arbitrary Abstract Data Type . . . . .	132
10.2 Lofty Indifference . . . . .	140
10.3 Sometimes it matters—or appears to matter . . . . .	147
10.3.1 Gödel's Finitisation of $\Delta_0$ Separation . . . . .	147
10.3.2 Finite Axiomatisability of NF . . . . .	149
10.3.3 Hartogs' theorem . . . . .	149
<b>11 Operationalism</b>	<b>151</b>
11.1 Antifoundationalism . . . . .	152
11.2 How did it ever start? . . . . .	153
11.2.1 Foundationalism about sets . . . . .	154
11.3 What might they believe? . . . . .	155
11.4 Foundationalism Untrue to Mathematics . . . . .	157
11.5 The Phlogiston Problem . . . . .	160
11.6 Freeing oneself from foundationalism . . . . .	161
11.7 stuff to be fitted into the correct place . . . . .	161
11.8 Operationalism in Mathematics . . . . .	163
11.8.1 A conversation with Graham White . . . . .	165
11.8.2 Equivocation and Operationalism . . . . .	167
11.9 Indeterminacy of translation . . . . .	167
11.9.1 $\omega$ and $\mathbb{N}$ . . . . .	168
11.9.2 Category Distinctions . . . . .	171
11.9.3 A Conversation with Ken Manders . . . . .	174

<b>12 The Coincidence</b>	<b>175</b>
12.1 The Burali-Forti Paradox—again . . . . .	177
12.2 Sets of size $\aleph_\omega$ . . . . .	179
12.3 The Paris-Harrington theorem: a case study . . . . .	181
12.3.1 Stratified and Unstratified versions of Paris-Harrington in NF . . . . .	184
12.3.2 Other versions of Paris-Harrington . . . . .	185
12.3.3 Concluding Random Thoughts . . . . .	185
12.4 Paris-Harrington redux: Ramsey and Paris-Harrington Again . .	186
<b>13 Miscellaneous Topics</b>	<b>189</b>
13.1 Synonymy . . . . .	189
13.2 Emergence and Reduction . . . . .	189
13.3 The Discrete and the Continuous . . . . .	190
13.4 Replacement and Lofty Indifference—Again . . . . .	191
<b>14 Appendices</b>	<b>193</b>
14.1 The law of small numbers . . . . .	193
14.2 stuff to be blended in . . . . .	204
14.2.1 A talk at BEST . . . . .	204

LET GIVES YOU OLY THAT THE PICTURE

This seems to be bifurcating. One part of it is a discussion of The Coincidence; the other half is a sustained rant about set-theoretic foundationalism, and part of *that* is a defence of set-theory-with-a-universal-set. It used to be called *Implementations, Typing and Replacement*

This book is written for mathematicians who want to think about their praxis. It is not written for philosophers, welcome tho' they are to join the conversation. Its author is a reformed foundationalist<sup>1</sup> who wishes to share with his fellow-creatures the path he discovered that led him out of Hades and which can lead them thence too, if they will only promise not to look back.

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<sup>1</sup>In this context one should bear in mind that here—as is usual—‘reformed’ is an example of what the linguists call an “alienating” adjective: (other examples are ‘fake’ or ‘imitation’: a fake Picasso is not a Picasso) a reformed foundationalist is not a species of foundationalist. [Is an ex-parrot a variety of parrot...?] Contrast “dry alcoholic” or “lapsed catholic”; a dry alcoholic is a kind of alcoholic and a lapsed catholic is a kind of catholic. Not here: foundationalism is not like alcoholism or catholicism... it is possible to throw it off: a lapsed foundationalist is not a kind of foundationalist.

## Chapter 1

# u/g Lectures on Philosophy of Mathematics Lent 2020

ADTs Barbara Liskov.  
Flew ECA.



## Chapter 2

# Stratification in Set Theory and the Strong Typing of Mathematics

A thirty-page article for Ali Sadegh Daghighi's (<a.s.daghichi@gmail.com>) book "Research Trends in Contemporary Logic" due in september 2019.

Since the intersection of the empty set of sets of widgets is the set of all widgets and the intersection of the empty set of sets of gadgets is the set of all gadgets, how do i know, when i run into an empty set in the street, whether its intersection is the set of all widgets or the set of all gadgets? Am i right to be reminded of the way in which i need to be told the domain (or codomain) of a function before i know whether or not it is total (or surjective)? It's more like the fact that gets in the way of thinking of  $\perp$ -elim as an empty case of  $\vee$ -elim. This is why mathematics is strongly typed.



# Chapter 3

## Vienna talk on stratification

Synonymy, Stratification and the Universal Set

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Abstract

Set theories that radically contradict one another (“Are there atoms or not? Is there a Universal Set or not? Is set membership wellfounded or not?”) can nevertheless be synonymous. But does that not mean that they are reporting to us on the same mathematics? So just what is at stake with these disagreements? As we uncover more of these synonymy results it is starting to look as if the parts that are *not* at stake can be captured by stratifiable expressions. It turns out that this is an echo of the strong typing of mathematics. The nonexistence of the Universal Set is no more part of Mathematics than was the idea that 3 is a member of 5.

### Rieger-Bernays Permutations

Let  $\mathfrak{M} = \langle M, \in \rangle$  be a structure for  $\mathcal{L}(\in, =)$  and  $\pi$  a permutation of  $M$ . Then  $\mathfrak{M}^\pi$  is  $M$  equipped with  $\in_\pi$  defined by  $x \in_\pi y \longleftrightarrow x \in \pi(y)$ .

A formula  $\psi$  in  $\mathcal{L}(\in, =)$  is **stratifiable** if there can be found a function  $f : \text{vbls}(\psi) \rightarrow \mathbb{N}$  such that if ‘ $x \in y$ ’ occurs in  $\psi$  then  $f$  of the variable ‘ $y$ ’ is one greater than  $f$  of the variable ‘ $x$ ’ and if ‘ $x = y$ ’ occurs in  $\psi$  then  $f$  gives the same number to the two variables.

The Pétry-Henson-Forster theorem [4] says that a formula of  $\mathcal{L}(\in, =)$  is equivalent to a stratifiable formula iff the class of its models is preserved under the Rieger-Bernays construction.

### Church-Oswald Constructions

Let  $\mathfrak{M} = \langle M, \in \rangle$  be a structure for  $\mathcal{L}(\in, =)$  and  $k$  a bijection  $M \longleftrightarrow M \times \{0, 1\}$ . Then  $M$  equipped with the relation  $x \in_{new} y$  defined by

$$x \in_{new} y \longleftrightarrow \begin{cases} k(y) = \langle y', 0 \rangle \text{ and } x \in y', \text{ or} \\ k(y) = \langle y', 1 \rangle \text{ and } x \notin y'. \end{cases}$$

is another structure for  $\mathcal{L}(\in, =)$ . This is in [19] and [1].

## A Symmetric Model for a Stratified Fragment of ZF(C)

Work in ZF(C). The symmetric group  $\Sigma(V_\omega)$  on  $V_\omega$  has countably many actions on  $V$  defined by  $\sigma_1(x) = x$ ;  $\sigma_{n+1}(x) = \sigma_n ``x"$ . A set  $x$  is *symmetric* if, for all  $\sigma \in \Sigma(V_\omega)$ ,  $x = \sigma_n(x)$  for all but finitely many  $n$ . The hereditarily symmetric sets is a model for all the stratifiable axioms of ZF but violates AC.

The APGs of the hereditarily symmetric sets are an isomorphic copy of the original model. See [7].

## Synonymy

We say  $T_1$  and  $T_2$  are synonymous iff the following happens.

Whenever  $\mathfrak{M} = \langle M, \langle R_i : i \in I \rangle \rangle$  is a model of  $T_1$  then there are  $\langle S_j : j \in J \rangle$  all definable in  $\mathfrak{M}$  such that  $\mathfrak{N} = \langle M, \langle S_j : j \in J \rangle \rangle$  is a model of  $T_2$ , and vice versa:

whenever  $\mathfrak{N} = \langle N, \langle S_j : j \in J \rangle \rangle$  is a model of  $T_2$  then there are  $\langle R_i : i \in I \rangle$  all definable in  $\mathfrak{N}$  such that  $\mathfrak{M} = \langle N, \langle R_i : i \in I \rangle \rangle$  is a model of  $T_1$ .

Another definition:

An  $\mathcal{L}_1$  theory  $T_1$  and an  $\mathcal{L}_2$  theory  $T_2$  are synonymous if there are [recursive?] maps  $\sigma : \mathcal{L}_1 \rightarrow \mathcal{L}_2$  and  $\tau : \mathcal{L}_2 \rightarrow \mathcal{L}_1$  such that, for all  $\phi \in \mathcal{L}_1$ ,  $T_1 \vdash \phi \longleftrightarrow \tau(\sigma(\phi))$  and, for all  $\phi \in \mathcal{L}_2$ ,  $T_2 \vdash \phi \longleftrightarrow \sigma(\tau(\phi))$ . We also want: for all  $\phi \in \mathcal{L}_1$ , if  $T_1 \vdash \phi$  then  $T_2 \vdash \sigma(\phi)$  and for all  $\phi \in \mathcal{L}_2$ , if  $T_2 \vdash \phi$  then  $T_1 \vdash \tau(\phi)$ .

Standard examples: partial orders/strict partial orders. Boolean rings/Boolean algebras. Nontrivial examples [6], [65], [1].

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## Stratification

Originally just to avoid the set-theoretic paradoxes. Have to get used to stratification the way you get used to constructive logic. And, like constructive logic, it has its rewards.

The coincidence: most mathematical notions are stratified, indeed homogeneous. Burali-Forti. Polymorphic ordinals.

Nice theorems concerning stratified formulae: Rieger-Bernays and the PHF theorem. Baltimore model Vu’s result. My result about AFA and Coret’s axiom. Nice proof-theoretic behaviour. Cut-elimination; confluence

## Synonymy

Many grades of synonymy. We always thought we understood it. I am just getting started, and realising that some of the results i was obtaining years ago were actually synonymy results, tho' i didn't know it at the time.

ordinary partial order/strict partial order; boolean alg/boolean rings Any binary structure and its converse, or its complement.

The partial order case illustrates v well how synonymous theories can be thought of as two takes on the same mathematics: the differences between them are presentational not mathematical. The one bundle of pre-mathematical thoughts can be mathematised in two different ways.

Kaye-Wong has a different flavour. Mathematisations of two things turn out to be in some sense the same thing. That's when we need *beschränkheits axiome*

In a set-theoretic context i am interested in  
 synonymy of unstratified set theories with stratified set theories  
 and - more specifically -  
 synonymy of theories of wellfounded sets with  
 (i) theories with antifoundation axioms  
 (ii) theories with a universal set

Rieger-Bernays Synonymy of stratified theories with unstratified theories. relies on the inverse of a permutation being in the permutation model. This ought to mean that every set theory is synonymous with its stratified fragment. consistency statements all in arithmetic and are stratified. Complicated by the fact that ... altho' if a fmla is unstratified there is a RB perm model that falsifies it, nevertheless that RB strux might not be a model of the relevant set theory.

PHF theorem again and R-B permutation methods

Ad (i) Marco Forti on Antifoundation (the philosophers of mathematics of the last 90 years would not have been trying to explain why the entire mathematical universe rests eternally on the back of a gigantic empty set) and my theorem about Coret's axiom.

Partly beco's of my theorem We are looking for results that say ZF and ZF+AFA are synonymous. Lots of old results giving equiconsistency; we want to sharpen them up. Harbingers are Benedikt's result. Basis.pdf

Synonymy of theories-with-a-universal-set with theories-with-foundation. Tim's result, with Beschänkheits axiome. CO constructions are very open-ended, and this obstructs proofs of synonymy results. One needs beschränkheits axiome

Kaye says NF not synonymous with any theory of wellfounded sets.

Medium-term goals.

- Kaye's remark about NF and CO constructions.
- Sort out the situation with arithmetic KF, Mac and the Oswald constructions.
- Prove ZF synonymous with ZF+AFA
- Prove a theorem along the lines of Every set theory is synonymous with its stratified fragment

- Prove a raft of theorems about synonymy of theories of wellfounded sets with theories with a universal set.

The point is not that there are consistent set theories with a universal set; the point is that some of those theories are synonymous with theories that prove there is no universal set. Thus the nonexistence of a universal set is not part of mathematics, it's an artefact of the formalisation. Just as silly as asking whether or not  $3 \in 5$ .

Came up in the discussion:

$ZF + V = L$  synonymous with  $ZF + V = HOD..?$



## Chapter 4

# Stuff to put in the right place

As Randall says, in geometry we identify a point with its singleton (it's the intersection of two lines which are sets-of-points).

Set-theoretic foundationalism has its roots in logicism. But where does Quine: reduction to a dyadic predicate fit in..?

Foundwash: chapter 0 in which we set out a lot of set theoretic flak

Sets are part of mathematics but they sit on top of it not underneath it. There are sets of rationals but that doesn't mean that rationals are sets.

An email to Tony Egan

Tony, thanks for this. Let me tell you a bit about why I need to think about this stuff. I am trying to write a book about Philosophy of Mathematics, or rather about one particular aspect of it. Being by now a mathematician rather than a philosopher my take on the matter is that Philosophy of Mathematics, properly understood, is simply the study of, and resolution of, the methodological problem encountered by working mathematicians in their everyday praxis. It isn't a branch of Philosophy, an esoteric skill (akin to "management Science") which can be acquired independently of any experience and bestowed on people who can then be parachuted into various parts of real life where they can tell the local inhabitants about the meaning of their activities.

Mathematics studies all sorts of things. Not just numbers, but diagrams, graphs, knots (remember Vaughan J). These different kinds of objects do different things; they "support" different operations. You can add and multiply numbers (but not sets); you can take binary union and intersection (remember Venn diagrams) of sets—but not of numbers. You can form products of spaces but not of grammars, and so on. This is all entirely uncontroversial and in some sense so obvious that it never gets taught to students. One might point out to

students that sets have membership information but not multiplicity information or order information; multisets have multiplicity information as well, and lists have order information on top of that. (The coins in your pocket are a multiset rather than a set, and certainly not a list). An understanding of these distinctions is acquired subliminally by students; it is never explicitly taught. Indeed an understanding of these matters only became explicit in the twentieth century, in the sense that it was only (starting in about the 1960s) that we acquired terminology for it. That (i think) was beco's it was only when people starting thinking about the design of programming languages that we need to be clear about what the data objects manipulated by the programming language were expected to do. The concept (and expression) \*Abstract Data Type\* (“ADT”) dates from this era. Certainly the terminology and the literature comes from theoretical computer scientists.

Next month i am scheduled to be giving some lectures on Philosophy of Mathematics for Cambridge over zoom. Of course i am going to talk about ADTs, beco's that falls clearly under my conception of Philosophy of Mathematics. You can see why i would like to find something helpful - even if brief - about the hidden curriculum, since it provides a context for the treatment of the novel material they are seeing.

One sometimes says to ZFistes who don't like NF that NF lets them have all they want: the wellfounded part of the universe-according-to-NF can be as ZF-like as you please. We haven't actually proved that every model of Zermelo set theory has an end-extension that is a model of NF but someone will one day. Reactions to this are various. (i) Some of them scoff and take up the point that it hasn't been proved. (ii) Some of them engage with the possibility but don't find it persuasive. That is the point at which to point out their cloven hoof: their reply—their *pitch* as ZF-istes is revealed to be, after all, *not* that they can offer us the things we all want (a wellfounded universe of sets etc etc) but rather that they can deny us the things they don't want us to have.

Ah. Some provoking maths tidbits. I am lecturing philosophy of maths and getting some very interesting feedback. I trotted out my usual example of a daft question:  $3 \in 5$ ? and Ben Morley (one of our Ph.D. students) said that Hindu decimal notation for numbers is also an implementation, so that—for example—the question: “Is there a power of 2 which when you write it in decimal and then reverse the digits gives you a power of 5?”. He says that this question is not only daft, but *it's daft in the same way as “ $3 \in 5$ ”* (A point that may be relevant: the elementary theory of binary strings is synonymous with PA. Albert told me something like this)

(Or again: is there a metric on the positive reals which is associative? Yes, associative?! Bonkers!)

Pursuing this line of thought: is Goldbach's conjecture daft? I remember someone (Ben Green?) saying to me that the reason why Goldbach (and, one might add, quite a lot of modern additive combinatorics) is so hard is because

you are asking questions about the *additive* properties of something defined *multiplicatively*. Yes, that makes it *hard*, but does it make it *daft*?

I say to Doug: the quixotic reduction of mathematics to set theory plays no role in actual mathematical praxis: Wiles did not prove FLT in ZF, did he! Doug says: chemists don't use fundamental particle physics in their everyday work. Explain what is wrong with this!

One of the things about philosophers of mathematics that annoys me (there are so many) has just come into focus. They talk about ordinals as tho' they were completely unproblematic objects, or at least no more problematic than natural numbers. This speaks of a radical misunderstanding.

Leeds, July 2019. When I complained (over a nice glass of Chimay Rouge provided by John Truss) to Andrew Brooke-Taylor that monism about set theory is absurd he said: well, look at what happens at the level of machine code in machines doing mathematics. Machine code is one-sorted!

The von Neumann ordinal  $\alpha + 1$  is a topology on the von Neuman ordinal  $\alpha$ .

Synonymy results point to bits of mathematics that are "formalisation-relative" to us Graham White's expression.

Nathan says: you complain about arbitrariness of implementation in set theory. But isn't the choice of language for doing Logic arbitrary? I say if that's the same arbitrariness, then formulæ must be implementations of something. Propositions, presumably. 6/vii/18

Tim Button sez: start with a boolean algebra. Consider the stone space of ultrafilters. Then the regular open algebra. POW! It's the algebra you first thought of! But it's two levels higher.

Institutionalised abuse of notation.

How do you show that a set is countable? You count it, duh! Not so fast .... You use up all your natural numbers, but that doesn't mean it isn't countable. The point is that you have to count it with countable ordinals. That way, if it really is countable then you will exhaust it by running through the countable ordinals. OK, so you finish counting it, and you still have countable ordinals left. How do you know they were countable? Well, I s'pose you started off knowing where the countable ordinals run out .... This is where the old fact about there being no canonical counting of a countable wellorder come back to bite us.

I am often struck by people in philosophy departments doing they call 'philosophy of mathematics' who write about Burali-Forti. Why do they do that? Burali-Forti is a piece of very hard mathematics and it requires a great deal of what mathematicians always call 'mathematical maturity' but which would be better called 'mathematical sophistication'. In short, you have to have done

quite a lot of mathematics to have had a chance of developing a feel for it. There are even people whom one would expect to understand it who don't, and these include, sadly, quite a lot of set theorists. There are lots of angles on this subject, and they each have something distinctive to say. The collection *On* of all ordinals is an object defined by a second-order recursion; such objects are problematic. Some of them are straightforwardly set-theoretic and result in nonexistence proofs for certain kinds of set. However *On* is not a purely set-theoretic object and so understanding it (in set theory) involves grappling with questions of implementation inside set theory ... and these are questions that many people wish to regard as solved. The trouble is that—to take the example of ZF and its congenors—the von Neumann analysis of ordinal numbers is so cute that the reader is liable to get trapped in set-theoretic foundationalism and go no further. If we think of ordinals as originally arising from (and deriving their nature from) equivalence classes of wellorderings (and this is inescapably one of the ways of encountering ordinals and has to be engaged with) then we are up against the complication that *wellorder* (unlike *set*) is a higher-order concept and so *ordinal* is problematic in a way that *cardinal* is not. Type-theoretic treatments of *On* (Russell-Whitehead, Quine) allow us to treat ordinals as polymorphic objects rather than monomorphic and there is profit in that line of thought too. There are simply too many angles for anyone with an ordinary upbringing to have been exposed to them all. My friend Rabten, a buddhist monk, tells me that the whole point of the trope about the five blind men and the elephant is that if you have only one teacher you will have only one insight. To think that von Neumann ordinals tidy up B-F is to miss a lot of the fun .... It's not a *crime*; (much worse!) it's a *mistake*. *C'est pire qu'un crime, c'est une faute*. If you want to understand the elephant that is Burali-Forti you will need a lot more than one insight.

Or you could put it like this.

Russell's paradox is complicated enough for most people, but, as paradoxes go, it's comparatively straightforward. It's purely logical, and properly understood, doesn't even involve set theory. Even if you want to think of it as set-theoretic its first-order. Mirimanoff's paradox is purely set-theoretic, but it's second-order. Burali-Forti involves notions that are not purely set-theoretic.

One wants to say that thinking that CH is a piece of set theory is a *pointilliste* error. But that's not straightforwardly true. If the reduction of reals (*via* Cauchy sequences etc) to naturals is considered to be part of mathematics, then we appeal to replacement to say that it doesn't matter how we implement naturals. Mind you, that is bounded replacement so is provable from weaker principles.

Should exploit my recent *aperçu* about PH to illustrate how mathematics really is stratified even if at first blush it doesn't appear to be.

Talking about ordinals as if they were hereditarily transitive wellfounded sets is a bit like the teleological slang used in biology. It's a convenient and racy

shorthand. Most if not all ZF-istes talk all the time as if they believe it but, in my experience, if you push them (most of them) they agree that it's merely a [mostly] convenient oversimplification which (in the hands of the *cognoscenti*) saves time and does no real harm ... to the *cognoscenti* that is. But some bystanders get confused.

The counting principle (AxCount) is not a straightforward consequence of a conception of cardinal arithmetic as that part of set theory for which equipollence is a congruence relation. It is actually quite sophisticated and untyped. Worth making this point beco's most people think it's fundamental. Well! Perhaps it *is* fundamental, and my analysis is bad because it doesn't bring it out.

Set theory is not so much a foundation as a backdrop.

That's romantic nonsense too; it's *all* romantic nonsense. For my part, i like it—the most fun you can have with your clothes on—but i have no patience with the foundationalist claims that are made on its behalf. They're mistaken and they mislead and annoy, and make set theory a lot of enemies. Enemies? Yes, enemies. There is a parallel here with Brexit, Trump and the unreasoning hostility towards the faux progressive ideas that have distracted the Western Left from their proper business for too long. The Trumpistas and Brexiteers can see that all this trendy identity politics is crap, but—not being people whose trade is ideas—they are unable to provide an explanation of this fact that silences their opponents—or even satisfies themselves. So they just get cross. Similarly, ordinary mathematicians can see that all the foundationalist claims made on behalf of set theory are crap, but they have neither the time nor the inclination to think through quite *why* they are crap; they have lives to lead, after all, and refuting these errors is not a sensible use of their time. For one thing it wouldn't improve their cred with their colleagues (“you're just picking at a scab”). So they, too, just get cross.

Gold standard for proof? Bah! Sir Otto Niemeyer<sup>1</sup> was the British Government's enforcer for the gold standard on the Dominions in the 1930s:

The heart is gold, the name is Otto;  
“Women and children first” the motto.

The gold standard was not generally regarded as a Good Thing!

They've had it their own way for so long that they have got flabby and lazy.

Set theoretic foundationalism is the belief that the whole of mathematics is poised on the back of a gigantic empty set.

But seriously. ZF is a strong theory and [much of] mathematics can be interpreted into it. So a proof in ZF of [the interpretation of] a problematic mathematical assertion is certainly a step forward. And you can welcome such a proof even if you are not a set-theoretic foundationalist. Is it the be-all and

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<sup>1</sup>[https://en.wikipedia.org/wiki/Otto\\_Niemeyer](https://en.wikipedia.org/wiki/Otto_Niemeyer)

end-all? Possibly a useful comparison here is with computer verification of proof. In both situations the proof not only authenticates the thing proved but also improves the credibility of the apparatus being invoked.

Set-theoretic foundationalism starts beco's you can reduce everything—apparently—to a dyadic predicate. See Quine “reduction to a dyadic predicate”. So you imbue this fact with metaphysical significance. Why use  $\in$ ? Why not use a symmetric predicate, as Apparently one can?

Consider  $HT$ , the set of all hereditarily transitive sets defined as the least fixpoint for  $x \mapsto \{y \subseteq x : \bigcup y \subseteq y\}$  (the set of transitive subsets of  $x$ ).  $HT$  is transitive, since if  $x \in HT$  every member of  $x$  is hereditarily transitive and so belongs to  $HT$  as well. So  $HT$  is hereditarily transitive, and so is self-membered if it is a set. Now consider  $HT \setminus \{HT\}$ . This set, too, contains all its transitive subsets, so we must have  $HT \subseteq (HT \setminus \{HT\})$  (since  $HT$  is the  $\subseteq$ -least fixed point) whence  $HT$  is not self-membered after all. Thus  $HT$  is a paradoxical object: it is a member of itself iff it is not a member of itself. This paradox seems not to have a name, even tho' it is elementary enough to expressible purely in the language of Set Theory. In particular, one of the names it does *not* bear is ‘The Burali-Forti Paradox’ and thereby hangs a tale.

We must also show that the von neumann ordinals are precisely the well-founded hereditarily transitive sets. It is a standard student exercise that von Neumann ordinals are hereditarily transitive; for the other direction we prove by  $\in$ -induction on  $HT$  that all its members are von Neumann ordinals. To keep the induction going we need to know that a transitive set of von Neumann ordinals is itself a von Neumann ordinal. But this is a standard student exercise too. The induction proves that every hereditarily transitive set is a von Neumann ordinal because  $HT$  is a least fixpoint and supports induction.

Carnap on *explication*.

They don't want to *do* mathematics, they just want to gossip about it.

Bourbaki—the most important collective author before Python.

Since for any number  $n$  and any set  $x$  there is an implementation of arithmetic that implements  $n$  as  $x$  you can never prove arithm  t  c facts about numbers by appealing to facts about the sets that implement them. All numbers would have bear the same truths!

```
Reply-To: tf@dpmms.cam.ac.uk
From: Thomas Forster <tf@dpmms.cam.ac.uk>
To: gleachkr@ksu.edu
Date: 06 Dec 2016 21:21:08 +0000
Subject: What Russell should have said to Burali-Forti
```

Dear Dr Leach-Krouse,

Google scholar has just turned up a rather promising-looking link to an article by you. I haven't been able to download the article (i'd probably have to subscribe to something!) but - as i say - it sounds promising. Hearing anyone talk sensibly about Burali-Forti is a rare treat. Google scholar pointed your paper my way beco's you kindly allude to my article with Thierry Libert - and i have now forgotten what it said!

I think you are absolutely right when you say that the moral of B-F is that the status of the collection of all ordinals is questionable. However i think that saying that it shows that On is not a set is insufficiently nuanced. After all, the collection of all (implemented) ordinals is a set in the Quine theories. You will say, of course, that the orderings that those are order types of are not genuine well-orderings - and you will be right, but then (wellordering being a second-order notion) even superficially more sensible theories like ZF have trouble with it, albeit in more subtle ways: it's just not first-order axiomatisable. This insight has been with us a long time, but increasingly tends to be overlooked beco's the Von Neumann implementation is soooo cute. Do you know the article by Rosser and Wang 'Non-standard models for formal logics'?

One of the complications is that ordinals are not *prima facie* pure sets. The trouble is there all along of course, but it crystallises as soon as you try to think of them as sets. The class of von Neumann ordinals is the least fixed point for the function sending  $x$  to the class of its transitive subsets. (I think Bernays knew this, tho' he would've said that the von Neumann ordinals is the class of hereditarily transitive sets. Interestingly there is a \*\*purely set-theoretical\*\* paradox concerning this object....

Have fun  
v best wishes  
Thomas Forster

Further to my last, i think the correct expression of the apercu that On is not a set is that if T is a second-order theory of sets that is rich enough to describe an implementation of ordinals, then T can prove that the class of ordinals is not a set. This is rather neatly illustrated by Quine's ML. It speaks of two objects: the class of order types of linear orders every subSET of which has a least element, and the class of order types of linear orders every subCLASS of which has a least element. The first is a set and the second is not.

I notice that Kreep-Louse has not acknowledged these two communications from me. Perhaps he thinks i'm a nutter who should be ignored. Or perhaps he plans to use the ideas without acknowledgement.

Actually it's pretty obviously the first.

The rationals-as-an-abelian-group are not the same as the rationals-as-a-ring. But the rationals-as-a-ring are the same as the rationals-as-a-field! That's beco's rings and fields have the same signature.

It is slightly misleading for ZF-istes to describe themselves as set-theorists. They are not interested in set theory *in general*, only in the study of wellfounded

Explain CHF moment

sets, since most of them disavow any interest in—or for that matter any knowledge of—sets that are not wellfounded. Indeed they—some of them at least—go so far as to deny that there are any sets that are not wellfounded. There is a curious echo here of the French attitude to cuisine. The French define themselves as world experts on cuisine. However french cuisine is suitable only for people who wish to eat *inter alia* suitably prepared body parts of dead animals. How are we to square this *hemispheric inattention* to vegetarianism with the claim to all-embracing french expertise in cuisine? Easy! Vegetarian cuisine is not cuisine; it is an aberration, a *cult*. Frenchmen talking to vegetarians inevitably experience CHF moments. These CHF moments are annoying for them of course, but the alternative would be for them to admit that there are *lacunæ* in their expertise in cuisine, and that is not to be borne. So it is also with the ZF-istes. NF is simply not set theory. It's *womanseeing* ([53] ch 15).

The way in which NF is disdained by so many set-theorists who have not interested themselves in its subtleties reminds one inevitably of Lady Catherine de Bourgh's piano playing “If I had ever learnt, I should have been a great proficient.” And, to be fair to her (and them) she (and they) *might*. But who knows?

Track down that lovely remark of Dennett's about how Hubel and Wiesel's title should be “What the frog's eye tells the *frog*” ... typing!

It's in [?] p. 16.

Extension element problem. Ideal divisors, points at infinity etc get invented as sets beco's we identify them with the set of problems to which they are solutions. But also integers and rationals as equivalence classes. One can even think of the representation of functions (in-extension) in this way. How are we to think (concretely) of a function, for God's sake? What does it *do*? Perhaps we can safely identify it with the collection of things that it *does*. And what does it *do*? It marries up inputs with outputs.

Typing in real life not just mathematics. What do you keep in your fridge? Food? Why not batteries and medicines? Those, too, are things that you need to keep cool to prevent them going off. However, fridges belong to a datatype that supports operations connected with food, usually storage but sometimes preparation as well. They're like saucepans, ovens, kettles ... Object-oriented programming. Mary Douglas.

At least some set-theoretic foundationalists believe in God. Do they think God is part of mathematics? [Surely they do—what mathematician doesn't?] If so, which set is (s)he?

Of course the STF people will regard such challenges as frivolous and disrespectful. This is rather like the way in which true adherents of a religion do not take kindly to challenges based on a literal reading of their assertions. The problem is not that their assertions should not be taken literally, the problem is that the religious people tend to think that beco's it's *their* language then it

is up to *them* to decide when it is to be read literally. They chide us for making light of their purported beliefs, while they—at their pleasure—trifle with them with aristocratic langour.

Similarly if you challenge a set theoretic foundationalist with “So you think  $\pi$  is a set?” they will weasel their way out of it rather the way in which Christians wriggle when trying to treat Jesus’ *hoc est corpus meum* as if it were a watertight legal document. The believers claim an exclusive right to decide how literally they are to be taken. That’s almost all right...after all it’s their language.

The ZF-istes have the casual attitude to lawbreaking typical of effete aristocracies who have been in power for too long, and who believe that laws are for other people. Observe the casualness with which they abuse notation by writing as if  $\aleph_0$ ,  $\omega_0$  and  $\mathbb{N}$  were all one and the same thing—and the contemptuous way in which challenges to this practice are dismissed. Only little people pay taxes.

On the subject of certain things not supporting certain operations...what happens if you cross a mosquito with a mountaineer? You can’t! You can’t cross a vector with a scalar.

It’s rather the way in which mathematicians (unlike almost everyone else) write ‘Let  $X$  denote a wombat...’ when what they mean is ‘Let  $X$  be a wombat...’, or perhaps: ‘Let ‘ $X$ ’ denote a wombat...”.

The foundationalist project is topsy-turvy: if you’re trying to stop the sky from landing on your head how is digging foundations going to help? Surely you should be constructing a stout roof ...?

You kick off your investigations of the hard cases by first looking at the cut-and-dried cases. This is beco’s by starting with the easy cases you can see what principles you were/are using, and see how you use them to see which way to jump in the hard cases. This sounds good, and it is, but sometimes the cut-and-dried cases are so cut and dried that you never have to examine your reasons—it’s so obvious. And there is also the danger that the cut-and-dried case was cut and dried so long ago that you have long since forgotten what your reasons were. And the context for those reasons—even if you can remember them—may be so different from the context within which you are currently working that the reasons you had then cannot be easily recovered and no useful parallels are evident.

This is why people who challenge orthodoxies are so annoying. You know they’re wrong of course, but you cannot for the life of you remember why.

Victor Sevilanov asks: if set theory isn’t there to do foundations then what is it for?

In conversation with Andrew Withy and Jeremy Seligman it became clear to me that when one insists that numbers are not sets one is not hostile to the idea

that numbers can emerge from sets. Lots of mathematical entities are magicked out of (equivalence) classes

Every biconditional has an easy direction and a hard direction ...

I know lots of people who moved from mathematics to philosophy. That is the easy direction (“A man who knows classics can learn science in a fortnight” said Jowett). I did the hard direction. Story of the RC priest who lost his faith and had to leave the priesthood ... an anglican friend said to him “you can join us now”—his rejoinder: “Madam, I have lost my *faith* not my *reason!*”

The way people pay lip service to foundationalism. LaRochefoucauld “Hypocrisy is the homage that vice pays to virtue”

Suppose i want to reason about all dedekind finite trees. [i have in fact been trying to prove something about them<sup>2</sup>] I do this in ZF sans choice. Clearly i do not expect the collection of all Dedekind finite trees to be a set, and i will in any case be quite happy to reason about them up to isomorphism. Is it a theorem of ZF that there is an ordinal  $\alpha$  s.t. every Dedekind-finite tree has an isomorphic copy in  $V_\alpha$ ? I bet it isn’t. But that’s not my point. My point is that nobody either knows or cares—and no more they should. That’s not because the kind of reasoning you want to do will not be of the kind that wants the collection of all [isomorphism classes of] dedekind-finite trees, co’s it might be. The point is that nobody, in their heart of hearts, really believes that this investigation is going on in axiomatic set theory. If they did believe that, then whether or not the collection of such isomorphism classes is a set might matter a great deal.

Trying to ride two horses. Try to refute the believer, try to rescue their students. You need different arguments.

ZF misses the point that the universal set really is an object of finite character. It also represents perfectly respectable finite objects like polynomials over the reals with rational coefficients as infinite objects.

The concept of *finite object* is perhaps not as clear as one could wish. Every proper initial segment of the second number class can be thought of as a set of finite objects. But we cannot think of all countable ordinals as finite objects simultaneously.

Some material about finite objects in philrave.tex

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<sup>2</sup>An exam question for Part III Mathematics in Cambridge in 2017 runs:  
In this question you may use excluded middle but not AC.

A set is *Dedekind* iff it is infinite but has no countably infinite subset.

(1) Show that if  $X$  is Dedekind then so too is the set of finite repetition-free sequences from  $X$ .

Define  $D$ -trees inductively as follows. A  $D$ -tree has a root  $d$  which is a member of  $D$ ; its children form a repetition-free finite sequence of  $(D \setminus \{d\})$ -trees.

(2) Prove that if  $D$  is Dedekind so is the set of  $D$ -trees.

Let's follow the causal chain. People noticed that you can interpret everything in the dyadic language  $\mathcal{L}(\in, =)$ . They then invested this possibility with metaphysical significance... *ontological* significance even. These things that are related by the binary relation we then call sets, and we think that every mathematical fact is a fact about sets and that therefore everything is a set. Never mind the fallacies involved: I have a separate question to ask. Apparently one can go further than this deduction: one can reduce not merely to a binary relation but to a *symmetric* binary relation. So why don't we rerun the process with graphs instead of sets? Why don't the crazy foundationalists think everything is a graph?

Quine: reduction to a dyadic predicate.

Foundationalism (about sets, for example) is the error of thinking that there is a Royal Road, and that it is maintained and patrolled by set theorists, complete with a breakdown service..

The Foundationalists' error is to seek certainty instead of knowledge. The reassurance and security that they crave is not to be had, and their fears are irrational; they worry that the sky might land on their heads. This is one of many reasons (and by no means the worst of them) why they don't like Set Theory with a universal set. After all, if there is a universal set then there *really is* something up there that might land on your head.

...you shouldn't worry about what happens under the bonnet. Perhaps he has a bee in it.

“Everything is what it is, and is not another thing”

When Russell and Whitehead famously prove ([101], \*54.43) that  $1 + 1 = 2$  they of course aren't really doing that at all. What they're doing is illustrating how their implementation of arithmetic inside ramified typed set theory is compliant with the specification. It is not at all like machine verification of proofs large enough to be questionable. 4-colour theorem etc etc. One of the drawbacks of set theoretic foundationalism is that it prevents you from seeing this, and encourages you to mistakenly believe that Russell and Whitehead proved that  $1 + 1 = 2$ . Indeed it encourages you to suppose that, after Russell-and-Whitehead's proof (which incidentally nobody reads) we now know something that we didn't know before, namely that  $1 + 1 = 2$ . Or, at least that we now know it in a new, more secure, sense than we'd known it hitherto. But—hold on a minute—if Russell-and-Whitehead's proof really does give us this new knowledge, why does nobody bother to check it? The obvious answer is that it doesn't offer us *that* piece of new knowledge... the new knowledge that it offers us is merely the fact that the system of PM satisfies certain basic requirements on implementations of natural numbers. And *that* fact (although pleasing to logicians-of-a-certain-stamp) is not of general interest. And that, in turn, is why nobody other than logicians-of-a-certain-stamp ever bother to read it.

Hardy said “I believe the Prime Number Theorem because of de la Vallée-Poussin’s proof of it, but I do not believe that  $2 + 2 = 4$  [sic] because of the proof in Principia Mathematica.” Rouse Ball lecture 1928 p 17

People will say that by making a fuss about the errors of foundationalism i am merely being disruptive. This is a point worth thinking about: how does it come about that this fuss can disrupt anything? Only because received practice has no answer to it. (If there were a satisfactory response they’d come out with it.) So: why do they have no answer to it? Is it beco’s there is no answer? Or merely beco’s they haven’t thought of any?

Say that all this nasty implementation-dependent stuff that the ZFistes do is done in a \*permissive\* environment.

[collate this with other stuff about  $x$  and  $\{x\}$ ]. One can get away with failing to distinguish between  $x$  and  $\{x\}$  quite a lot of the time. For example, one hears algebraists say that a group is simple iff it has no normal subgroups other than itself and the unit. What they mean is “...and the singleton of the unit” (or really the subgroup whose carrier set is the singleton of the unit). The over-loading of notation is OK, but that’s not because the two things are the same, but because they are so obviously of different types that the correct semantics can be recovered straightforwardly. The first thing your syntax module tries to do is to type everything, and—once it’s done that—life is straightforward.

It autocorrects

The homophony between ‘hare’ and ‘hair’ doesn’t lead anyone to think that the two are the same. Since they are so easy to tell apart it doesn’t matter if you mention one when you mean the other!

There are other examples of this kind of thing. The dark ‘l’ sound (as in ‘feel’) and the light ‘l’ sound (as in ‘leaf’) are distinct, yet in English we write them with the same letter. There is no context with a placeholder that can be occupied legally by both. This frees us to (thriftily) use the one symbol for both of them, since there is no danger of confusion: in any context where one of them is legal, the other is not. We can even think of the two sounds as being in some sense the same, both of them being manifestations of the one thing ... in two different contexts (back consonants vs front consonants). Thus one might think of the ‘l’ sound as an abstract entity which evaluates to a particular sound in a context. Interestingly English speakers (at least those that are not also linguists!) do in fact regard these two sounds as one and the same sound. The linguist will recognise their distinctness, and call them (two) *allophones* (of the one *phoneme*).

The parallel with our singleton-of-the-unit case is less than perfect because there is no set-theoretic concept analogous to *phoneme* under which the unit and its singleton are (as it were) *allophones* of the same *phoneme*.

There are two ways in which one might try to sell someone a new idea. One might appeal to their nature as reasonable open-minded creatures and say “try looking at it like this, hear me out, it might help, this is something that

reasonable people might disagree about". That's the way to approach people who are broadly happy with the machinery they have. However, if you are trying to sell something to people who are less than entirely happy with the machinery they are using you can use a different approach: "Yes, your suspicions are correct, it is suspect, it is a tissue of errors, and i have come to liberate you from it"

Foundationalism about sets is as silly as (tho' no sillier than) Descartes' idea that everything is a vortex. The difference is, *that* mistake didn't last as long.

I want to set my readers free from the absurd worries that were wished on them by the foundationalists. Didn't Wittgenstein say somewhere that the purpose of philosophy was to enable people to see things as they really are, or as they were all along..?

Saying that the study of wellfounded sets *is* set theory is a bit like saying that Americans speak English. They don't. They speak a *subset* of English.

There is a curious parallel with AC here. The alephs are a recursive datatype (you define the successor of an aleph by reference to the set of lengths of wellorderings blah) but they do not exhaust the cardinals any more than the inductively conceived sets (be they well-founded or church-oswald sets or Fort-Honsell sets) exhaust the sets.

There are two drawbacks to devoting your time primarily to the study of ZF. The first is that in so doing you are occupying the rhetorical high ground. The trouble with occupying the rhetorical high ground is that you win all debates automatically even *if your arguments are bad*. We all wish to save on glucose, so why spend time and energy sprucing up arguments to use in debates that you know you are going to win anyway? The second is that many things are too easy in ZF, so you can end up using extremely profligate constructions. For example:

Hey there,

So for the paper I was looking for a demonstration (which I'm almost certain exists) that some of NF's dependent products are empty. To this end, I was looking up how one gets the wellordering principle from the usual form of AC and went to Jech's little book. But I'm either being dense, or his argument (as presented) is a little circular. He goes like this:

Take a set  $S$  and a choice function  $F$  on its non-empty subsets. By transfinite recursion one defines a sequence  $a_\alpha$  with  $a_0 = F(S)$  and  $a_\xi = F(S \setminus a_\eta : \eta < \xi)$  until you've used up all the elements of  $S$ .

The thing I that rubs me the wrong way here is it's not established that you have an ordinal long enough to exhaust  $S$ . I don't see it coming out of the above argument, certainly, but I also don't see how to establish it from Choice. I'm obviously missing something?

and my reply

The first thing to notice is that deducing that  $X$  can be wellordered from the existence of choice functions going on entirely inside the double power set of  $X \times X$ , so Hartogs' is not an issue.

The idea is to use AC to build longer and longer wellorderings of bits of  $X$ . You start with the empty worder. Thereafter you take unions at limit stages, and at successor stages you use your choice function on the power set of  $X$  to plonk on the end that element of  $X$ .

OK, so you consider the inductively defined subset of the double power set of  $X \times X$ , defined as the intersection of all those members of the double power set of  $X \times X$ , that contain the empty wellordering and are closed under unions of chains, and the operation that plonks the obvious element in the end of any wellordering of successor type.

Is it induction on ‘ $x$ ’? Or induction on  $x$ ? Do we mean ‘Let  $n \in \mathbb{N}$ ’ Or Let ‘ $n$ ’ denote a natural number?

The rud function ‘ $\langle x, y, z \rangle \mapsto \langle x, z, y \rangle$ ’ is unstratified but not because of the underlying mathematics.

Good evening ladies and gentlemen.

Anybody here think  $\pi$  is a set? Really?! What do you do to sets?  $\cup$  and  $\cap$ , that sort of thing. What do you do to  $\pi$ ? You square it then divide by six, or perhaps multiply it by the length of a radius of a circle. Do you still think  $\pi$  is a set?

Mind you, if  $\pi$  is a set it must be an uncountable set!

There are these people who worry about whether or not the number 6 and Julius Caesar might be one and the same thing. (What is it like to be such a person? It always reminds me of Nagel: what is it like to be a bat?) If you think that is a genuine question then you are in the grip of a radical misunderstanding. People who think that  $\pi$  is a set are in the grip of a radical misunderstanding of the same stamp.

These crazy fundamentalists who believe that all sets were created in stages... one should parody their contortions as *creation set theory*. Has possibilities. Like creationism it's also an argument from incomprehension.

Patterson says (in his book “*Introduction to the Riemann  $\zeta$  function*” (p. 1) that “by convention” the product of the empty set of numbers is 1. Is he right? Is it only by convention? I thought i had a proof that it was 1. But perhaps all i have proved is that if there is an answer it must be 1, not that there is an

answer. Like the solution to  $x^{x^{x^{\dots}}} = 4$ . If there is a solution it would have to be  $\sqrt{2}$  but, as it happens, there is none.

So how do I prove that there is a solution to the product of the empty subset of  $\mathbb{N}^*$ ? Well, to answer this challenge one needs to have acquaintance with the graph of the version of the multiplication function that accepts lists of arbitrary length. What is this function? I can declare the string ‘`mult`’ to denote any function-in-extension I damned well please, so I declare it in the obvious way—so there’s an end of it. Isn’t there? “No”, the objector will say: “you have to persuade me that the function you have defined really is the multiplication function on lists of naturals. And you’re not going to succeed, because the function I have in mind is undefined on the empty set”. How am I going to argue with this person? We appear to be disagreeing about the extension to be associated with an intensional object that is `multiplication`. Then of course we can get into irresolvable disagreements, because we don’t really have any access to this entity. I say it’s defined on the empty set, and he says it isn’t. What can we do? There’s no way through *there*. The only thing I can say is that my extension is better (bigger!) than yours. Well, actually, that’s not the only thing. The argument that if the product of the empty set of naturals is defined at all then it must be 1 can be used to show that not only is my extension bigger than yours but that my extension to the empty subset is canonical.

One can also make the *pædagogically therapeutic* remark that if you are worried about the product of the empty set of natural numbers it is not because you are worried about multiplication, but rather because you have got your knickers in a twist about the empty set.

An exercise on Maurice Chiodo’s sheet III was: write a DFA that accepts strings from  $\{0, 1\}$  that correspond to binary representations of multiples of 3. No problem there. Of course it has three states corresponding to the three residues mod 3 that you might have. Or does it have four? What does one want to say about the start state, the state it is in when it hasn’t been fed anything? Is that to be the same as the accepting state or not? To what natural number does the empty string from  $\{0, 1\}$  correspond? If it corresponds to 0 then the start state is the same as the accepting state. If it corresponds to anything (and how big an ‘if’ that is is a matter for discussion) then it must correspond to 0. That’s because when we append a ‘0’ to it we have the string 0 which of course corresponds to the natural number zero, and this appending-of-‘0’ corresponds to multiplying-by-2, and an appending-of-‘1’ corresponds to multiplying-by-2-and-then-adding-1. Both these thoughts tell us that the empty string must point to the number 0.

But, says Harry Roberts (in his silly Father Christmas hat which he has been wearing all week—I mean: how can you take seriously anyone wearing such a hat, I ask you?!?) it doesn’t *prima facie* mean anything at all, so it’s by convention that you decide it means 0. His thought then is (I’ve monkeyed around with this a bit) is that if you wake the machine up, and then press `carriage-return` before you have entered a character from the alphabet  $\{0, 1\}$  you’d get an error message. He’s right. In fact if, at some point in a sequence

of keystrokes: ... character, carriage-return; character, carriage-return ... I press carriage-return without immediately preceding it with a character i'll get ... what? Harry says i'd get an error message. I might, i suppose. But it wouldn't be from the DFA, it would be from the DFA's *minder*, the *operating system*. Actually the O/S probably wouldn't even bother to send me an error message, on the grounds that the simplest thing to do in these circumstances is to ignore the ectopic carriage return altogether. But i think it's important that the error message [if there is one] will come from the user interface not from the machine.

How perverse is Harry being? Suppose i were to pretend that i don't know what multiplication by 0 is, and that  $x \cdot 0$  (for  $x \in \mathbb{N}$ ) is *prima facie* undefined. I could probably be talked into agreeing (with a becomingly modest display of reluctance, of course) to a convention that says that  $x \cdot 0 = 0$ , on the grounds that it makes the distributivity etc etc work. But we all think that that is perverse ... don't we?

Yes it is perverse, but perverse rather than actually *false*. If i define an operation of multiplication on  $(\mathbb{N} \setminus \{0\}) \times (\mathbb{N} \setminus \{0\})$  then how i extend it to the whole of  $\mathbb{N}^2$  is entirely up to me.

And it's surely not *by convention* that we think that when, in the course of doing a proof by resolution, we have resolved to the empty disjunction, then we have proved the false?

If Zach Weber was here he would probably say that there is a point to be made about the *ex falso*. And he's probably right.

[also in my supervision notes]

Some people memorise the decimal expansion of  $\pi$  to umpteen places, as a kind of parlour trick, or mantra. This information is not discovered by a detailed analysis of the set-theoretic structure of the set  $\pi$ ! Mind you, there are probably people crazy enough to think that it is!

Years ago, when i was a Ph.D. student, DPMMS had an events board in the entrance hall. It was formatted in columns with research group, title, room and speaker. One day someone had used this to announce "Subsets of  $\emptyset$  by Prof. F. Utterbunk". I wonder what the 'F' stood for. I can't remember what room it was in ... there was probably a barb there too.

Swinners' quotation:

"... for most of us there are plenty of research problems within our scope; and the natural thing is to add one small piece of irrefutable knowledge to another. But Mathematical Logic is not like that; there are in it almost no problems that a non-genius can do, and so scholarship rather than research is what most of its practitioners are forced into."

Sir Peter Swinnerton-Dyer, in a letter to Hugh Trevor-Roper, then Master of Peterhouse, in connection with the renewal of Adrian Mathias' fellowship.

The point is that set-theoretic foundationalism is silly in a way that is obvious to mathematicians in the clapham omnibus, but obvious in a way that they feel no need to explain or investigate: they have lives to lead and research projects to pursue after all. It is of course true (as i am always telling my students) that explanations of the obvious can be informative, but time is short and one has to make choices. It's much simpler to allow the daft questions to play directly into a narrative (already tee-ed up and ready to go) that says that logicians are wanky intellectuals who waste our time.

It is not pleasant to be in the grip of these errors, but i am not here to offer you aid or comfort. I wish you good luck in freeing yourself from them, but i am your colleague not your therapist. Nor your priest. Saving your soul is *your* job. But then, like my swiss grandfather before me, i am a protestant atheist not a catholic atheist.

If you think that everything is a set, then you certainly think that  $\mathbb{R}$  is a set, and you think moreover that every real is itself a set. The error of thinking that  $\mathbb{R}$  is a set has a name: *pointillism*. It is becoming commonplace for people to say (correctly) that the oddity of Banach-Tarski is nothing to do with AC but is pointillism. OK, so the spheres in BT are not sets of points. But if you are a set-theoretic foundationalist they've got to be sets of *something*—everything is, after all. So . . . sets of what?

There is a much stronger case for thinking of  $\mathbb{R}$  as a set than there is for thinking of each real as a set. If sets are on the table then the mathematical intuition that says that  $\mathbb{R}$  is a respectable comprehended, completed (or whatever) entity is tolerably captured by the thought that it is a set, but you really have to be a mad dog foundationalist to think that every real is itself a set.

Ordernesting is a representation theorem: every poset  $\langle X, \leq \rangle$  is isomorphic to a poset whose order relation is  $\subseteq$ . The isomorphism is  $x \mapsto \{y : y \leq x\}$ . It may be worth pointing out that for the graph of this isomorphism to be a set we need unstratified separation. If we ask merely that every poset  $\langle X, \leq \rangle$  be isomorphic to a poset whose order relation is  $\subseteq$  then IO suffices, as follows. Let  $X'$  and  $f$  be such that  $f : X \longleftrightarrow \iota^* X'$ . Then  $x \mapsto \{x' \in X' : f^{-1}(\{x'\}) \leq x\}$  makes  $\langle X, \leq \rangle$  isomorphic to a poset whose order relation is  $\subseteq$ .

But then aren't all implementations representation theorems?

Quine writes about ordered pairs in [94]. What he says is very sensible but at that stage (1960) the philosophical tradition had not been exposed to the ideas of *implementation* or *abstract data type*.

Our usage has become so corrupted that when we see (as I did for example when i saw [104] in the *Bulletin of Symbolic Logic* the other day) an article

entitled “Defining Integers” one expects to find a discussion of ways of *implementing* integers. Quite properly, [104] addresses the question of whether or not  $\mathbb{Z}$  is a diophantine subset of  $\mathbb{Q}$ .

95% of the practice of set theory is underpinned—vitiated—by two errors. The first error is the assumption that the axiom of foundation is safe (Taf’s story about pretending the negative integers weren’t there). The other error is set-theoretic foundationalism.

Actually there are *three* distinct mistakes.

- (i) The first is thinking that you need foundations if you are to be rigorous;
- (ii) the second is thinking that foundations have to be sets;
- (iii) the third is thinking that sets have to be wellfounded<sup>3</sup>.

If you make any one (but only one) of these mistakes you might just get away without anyone noticing; if you make all three you have a crazy philosophical position.

Once you have taken the fatal step of thinking that mathematics is to be founded on the theory of wellfounded sets, and have gone on to identify and list all the things that the theory of wellfounded sets is good at modelling—namely wellfoundedness, recursion, induction etc—it is easy to retrofit a whig history according to which mathematics was—all along—the study of . . . wellfoundedness, recursion, induction etc. (This ties in very well with the idea—coëval with set theory but independent of it—that mathematics is the study of finite objects.) But, by insisting that everything has to be built up from below, it shuts out the possibility of divine revelation, an absurd thing to do—indeed one might say a *fundamentally*<sup>4</sup> absurd thing to do—since Mathematics was never anything but the project to See Into The Mind Of God. God does not build things up from below: He casts thunderbolts from above, or offers us an uplifting finger from the ceiling.




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<sup>3</sup>See Fraenkel [50] p 89

<sup>4</sup>It is foundations we are talking about, after all.

Building up the cumulative hierarchy is never going to get you a view of the Mind of God. Remember what happened to the builders of the Tower of Babel. I recall at this point a conversation I had with Wayne Rasskind when we were both Ph.D. students.

Rasskind:	"Doing set theory with a universal set is a bit like believing in God."
Forster	"Do you believe in God?"
Rasskind	"I'm not sure!"

What reply does one have to people who say that the concept of wellfounded set is the correct scientific-isation of the pre-theoretic (paradoxical) notion of set? One can say the following (which is in any case important and true even if it isn't the correct answer to this particular challenge): *The datatype of **set** is the extensional datatype of minimal structure.* Whatever flavour of structure you start with, if you keep on taking reducts as far as you can you eventually end up with sets. (From the point of view of object-oriented programming it's about as boring as it gets: all the datatype **set** can do is correctly answer "is  $x$  one of your members?") Reflect now there is nothing about this conception that tells you that sets have to be wellfounded. The only axiom that arises directly from this understanding of what sets are is *extensionality*. Of course there are other axioms with compelling reasons for adoption, but those reasons have more to do with the explanatory purposes we find for sets. Foundation is just another axiom, and your acceptance or rejection of it reveals what your interests are.

The other response is to tell a story, and the story goes as follows. In the beginning there was the type of **set**, or perhaps **naked-set** to make things even clearer. There are various other datatypes that appear, such as **multiset** or **list**, but there are others that concern us more: **counted-set** and **wellfounded-set**. The syntax sounds the same in the two cases, but there are differences, as follows.

**Wellfounded set** is a subtype of **naked-set**, and is a recursive datatype in its own right, and comes equipped with a datatype of certificates. Now this datatype is free and so, given an object of type **wellfounded-set**, we can uniquely recover its certificate. (think: transitive closure). This possibility of unique recovery spares us the need for actions that would keep us constantly aware of the distinction.

If you happen to think that all naked sets are wellfounded then there is little to prevent you conflating **naked-set** with **wellfounded-set**—particularly since **wellfounded-set** is free so you would naturally discard the scaffolding. Thus, when you encounter a **naked-set** that doesn't happen to be wellfounded, you find yourself thinking that it isn't a set (= **naked-set**) at all but must be something else. The mistake is not in thinking that all **naked-sets** are wellfounded (tho' of course that is a mistake too); the mistake that matters is equivocation between **naked-set** and **wellfounded-set**.

Contrast the situation with `counted-set`. There is no canonical way of recovering a counting from a `set` that happens to be countable: a set can be counted in lots of different ways. This makes it comparatively straightforward to learn and retain the difference between `set` and `counted-set`. `counted-set` is a fancied-up ADT, an expansion of `set`. Thus when someone who thinks that all sets are countable encounters uncountable sets for the first time they don't say "These things aren't sets, they must be something else" but "I see: not all sets are countable".

Historically what seems to have happened is this. We started off with an ADT `naked-set`. Von Neumann offered us an interesting inner model of well-founded `naked-sets`, which gradually—*subliminally*—became tho'rt of as an ADT of `wellfounded-set`. Since this ADT is free, there is no pressure to distinguish between it and merely wellfounded `naked-sets`, so one equivocates between the two. Thus when you encounter illfounded sets, the part of you that has turned from thinking-about-`naked-sets` to thinking-about-`wellfounded-sets` detects a type error and complains that illfounded `naked-sets` aren't sets.

Thinking of illfounded sets as having a type different from the type of well-founded sets results in an unsolvable typing problem, co's there is no way of telling in a finite time that a set is wellfounded unless it is hereditarily finite. That doesn't mean that it's wrong to think of illfounded sets as having a type different from the type of wellfounded sets of course, but it does mean that it's something you would be very glad to have a good reason not to do!

The attempt to insist that illfounded sets are not sets invites a parallel with Hofstader's parody of Lucas' views on why computers cannot have minds, in [53] p 477. Women cannot see, they can only womansee.

The study of wellfounded sets is mathematics in the missionary position. You won't go to hell but you will miss a lot of fun.

A conversation with Richard Kaye. He, too, is concerned about abuses of type distinctions. "Tomorrow's temperatures will be hotter"; "The speed of light may be slower than we thought". <http://www.dailymail.co.uk/sciencetech/article-2672092/Was-Einstein-wrong-Controversial-theory-suggests-speed-light.html> Do these really cause confusion? Not to me, they cause annoyance. But some people might be confused. One has to decide whether to describe this situation in terms of FTPM or in terms of complex semantics.

"Ohne meine einverständniss"

Should emphasise somewhere that we are not concerned here about Martin-Löf-style type theory. The types we are interested are not types of the flavour that makes them correspond to propositions.

How do we prove that  $\aleph_0 + 1 = \aleph_0$ ? The usual proof takes a set of size  $\aleph_0$ , decorates it with a counting, and then exploits the fact that  $1 + \omega = \omega$ . Then it throws away the decoration. This is not really in the spirit of the completeness theorem or the Interpolation lemma!

Should supply original ref

### Functions from $A \times B$

A function  $(A \times B) \rightarrow C$  accepts as input not an ordered pair  $\langle a, b \rangle$  but accepts two inputs,  $a$  and  $b$ . Or, if it given an ordered pair  $\langle a, b \rangle$ , it is required to extract the two components and then throw away the pair.

A function  $f$  of two arguments (of types  $A$  and  $B$ ) is often thought of as  $f : A \times B \rightarrow C$ .

However we don't *literally* mean that it is a function that wants a single argument and wants that argument to be an ordered pair. If  $f$  is a definable function then we probably expect that it is in some sense well-typed. The absurd function  $\mathcal{A}$  that says "take two natural numbers  $x$  and  $y$ , form their ordered pair (which is of course—as it happens—another natural number), take the largest prime that divides it and return the least primitive root of that prime" is—indeed, literally—a function  $\mathbb{N} \times \mathbb{N} \rightarrow \mathbb{N}$  but there is something odd about it. Normally, writing  $f : A \times B \rightarrow C$  suggests that  $f$  looks at an  $A$  and a  $B$  but doesn't create or process their ordered pair—indeed it neither knows nor cares what such a pair might be.

Perhaps if one pays close attention to type disciplines surrounding pairing-unpairing then binary functions really do look different from unary functions, and the moral one draws from  $\lambda$ -calculus is wrong. Observe that currying/uncurrying are *unstratified* operations in the set-theoretic sense.

No! Surely the point is that a function that eats ordered pairs is not allowed to do anything to them that is implementation-sensitive!! If all it knows about the input is that it is an ordered pair then all it is allowed to do is decode it.

The black box that executes the function of two variables  $a : A$  and  $b : B$  has two ports; the corresponding function that has a single port into which one puts  $\langle a, b \rangle : A \times B$  decodes the pair before it does anything else, and thereafter only looks at  $a$  and  $b$ . However an arbitrary function  $A \times B \rightarrow C$  might not decode its input pair at all, as witness the absurd function  $\mathcal{A}$  above.

One thinks of `let` commands in this context:

`Let x = pair(u,v) in... [do stuff to u and v]`

Maybe one has to think about plural logic in this connection. [God, i hope not!]

Contrast this with functions  $A \rightarrow (B \rightarrow C)$ .

Perhaps it will help to remember that  $A \times B$  is

$$\bigwedge_X ((A \rightarrow (B \rightarrow X)) \rightarrow X)$$

so, given  $f : A \times B \rightarrow C$ , we instantiate 'X' to 'C' so that the argument to  $f$  is an object of type  $((A \rightarrow (B \rightarrow C)) \rightarrow C$ , and this of course will happily gobble up our  $f$  of type  $A \rightarrow (B \rightarrow C)$ .

Interestingly we no longer have to worry about what this  $f$  does, co's it's an argument.

So how do ideas of *implementation* and *implementation-sensitivity* play out in this context? What is it to implement an object of type  $\bigwedge_X((A \rightarrow (B \rightarrow X)) \rightarrow X)$ ? Do we implement it as a function that, when we give it a type  $X$  [by which we do **not** mean: give it an object of type  $X$ !] gives us back an object of type  $(A \rightarrow (B \rightarrow X)) \rightarrow X$ ? That would seem to be the obvious thing to do.

Needs more thought.

Perhaps the question to focus on is “What is the difference between a machine with two ports and a machine with one?” Is there any, given that the machine with one port can decode ordered pairs . . . even if it doesn’t know that that is what it is doing?

### Innate ideas

Chomskian innate grammar; innate moral sense. How these concepts “cash out” (I think that’s the kind of slang analytic philosophers use) varies from one civilisation to another. So what we have here are a couple of examples of \*implementation\*s.

Innate ideas always turn out to be hardwired acquaintance with an abstract datatype specification.

Another example that seems to me to be logically very similar (tho’ it doesn’t fall under the heading ‘innate idea’ is the idea of a living being.

All this twin-earth stuff, where people worry about whether or not water might be something other than H<sub>2</sub>O. What is the underlying concern that is driving it? Mightn’t it be better addressed by engagement with ideas about implementation?

copy in stuff from Kaikouratalk.tex

Robert Thomas writes:

Thank you for submitting the MS. I’ve enjoyed the paper and the correspondence. After your paper was accepted, I mentioned to Neil Tennant your notion of implementation:

It seems to me that Forster’s notion of ‘implementing a mathematical object by a set’ is simply that of ‘having the set be a surrogate for the mathematical object, in the set-theoretic reconstruction of the branch of mathematics in question’. The latter notion is familiar to all contemporary philosophers of mathematics, especially after the works of Benacerraf and Maddy that have focused on it. In fact, the whole programme of set-surrogacy for mathematics was what motivated Bourbaki, from the 1930s onwards.

I replied:

Like his notion of implementation, yours of surrogacy is new to me. Not clear how they differ. Can you cite chapter and verse for the

idea that sets are phoney substitutes for mathematical objects? I have thought for years that they were being used \*as\* mathematical objects and have for many years considered it an error. In courses that were sufficiently sophisticated to do so, my mathematics courses \*defined\* things in terms of sets. I never heard any talk of the things being defined being other than those sets and merely being represented by the sets, subsets, mappings of them, and so on. Even if surrogacy is common coin among philosophers of mathematics, it sure isn't talked of in PM!

He did not reply to this, which may be mainly a comment on my deficient mathematical education (University of Toronto but in another century). Anyway, for what it's worth someone thinks that everyone knows about this in spite of the obvious fact that neither I nor the second referee of your MS knew of it. He also claims that it predates Bourbaki. Odd.

Robert

#### A message from Bill Tait

I can recommend Tony Martin's review of Quine's Set Theory and its Logic (Journal of Philosophy, 67, 1970 pp. 111-114). Quine wrote a reply (J of Phil 67, 1970, pp. 247 ff. — Bill Tait

How do you get the natural numbers? Start with lots of  $\omega$ -sequences, and pare off, from all of them severally, all the structure you can until they all end up the same. Manders overspecification. Careful! You might just end up with a countable **naked-set**. Paddy Blanchette tells me that this is more-or-less what Dedekind says.

<http://www.ac-nancy-metz.fr/enseign/philo/textesph/>

When we say “ $x$  can naturally be thought of as  $y$ ” we always mean that there is a natural bijection such that .... And ‘natural’ here is a category theorists’ term of art.

A conversation with Zhen Lin 8/xi/2013.

Thinking about the difference between abstract groups and concrete groups (as per my 1a notes from Rachel's lectures). If one thinks (in NF or CUS) of an abstract group as an isomorphism class of concrete groups then one has a job to explain homomorphisms between abstract groups. One seems to need, for each equivalence class, a global family of commuting isomorphisms between members of the class. (That is to say, for any two concrete groups  $G$  and  $H$  in the class one has a designated isomorphism between  $G$  and  $H$ , and these isomorphisms commute: the bijection  $G \rightarrow J$  is the composition of the bijections  $G \rightarrow H$  and  $H \rightarrow J$ ). And that needs AC.

Look up material on **In-discrete categories** in `logicrave.tex`

The point isn't often made, but it should be. If you are working in ZF(C) you are going to have *exactly* the same problem with abstract groups as you have with the natural numbers. If you want to reason about abstract groups in ZFC what do you do? Here is the assembler code for the canonical representation of  $S_3$ , as the set of permutations (concretised as sets of Wiener-Kuratowski ordered pairs) of  $\{0, 1, 2\}$  where the numerals of course denote von Neumann naturals.

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{{{\{\emptyset\}}, {\emptyset, {\emptyset}}}}, {{\{\emptyset\}}, {\emptyset, {\emptyset}}}, {{{\emptyset, {\emptyset}}}}},
{{{\{\emptyset\}}, {\emptyset, {\emptyset, {\emptyset}}}}}, {{\{\emptyset\}}, {\{\emptyset, {\emptyset}\}}}, {{\{\emptyset, {\emptyset}\}}, {\emptyset}}},
{{{\{\emptyset\}}, {\{\emptyset\}}, {\emptyset, {\emptyset}}}}, {{\{\emptyset\}}, {\{\emptyset, {\emptyset}\}}}, {{\{\emptyset, {\emptyset}\}}, {\emptyset}}},
{{{\{\emptyset\}}, {\{\emptyset\}}, {\{\emptyset, {\emptyset}\}}}}, {{\{\emptyset\}}, {\{\emptyset, {\emptyset}\}}, {\{\emptyset, {\emptyset}\}}}, {{\{\emptyset, {\emptyset}\}}, {\{\emptyset, {\emptyset}\}}}, {{\{\emptyset, {\emptyset}\}}, {\emptyset}}},
{{{\{\emptyset\}}, {\{\emptyset, {\emptyset, {\emptyset}}\}}}}, {{\{\emptyset, {\emptyset\}}, {\{\emptyset\}}}}, {{\{\emptyset\}}, {\{\emptyset, {\emptyset}\}}}, {{\{\emptyset\}}, {\{\emptyset, {\emptyset}\}}}, {{\{\emptyset\}}, {\{\{\emptyset\}\}}}, {{\{\{\emptyset, {\emptyset}\}\}}}

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Feel better now? (Reminded of Adrian's quip about Bourbaki's definition of the number 2)

A set-foundationalist will say that *of course* that isn't enlightening, and it was never intended to be. This is the point at which to tell Wendy's "There is no 'f' in 'haddock'" joke.

This exposes a grave defect in set theory as foundations. Either you implement small things like  $S_3$  as small objects instead of as equivalence classes (which is unnatural) or you do them naturally, as above, and that seems to need AC which is not available.

Recycle from NFatnearly75.tex the *bon mot* about the universal set being the empty set with a party hat on. Make use of the word *saturnalia* in this connection.

Lambek in his review [73] of [7] writes:

"...the original type theory of Russell and Whitehead ... was replaced by the set theories ... Zermelo-Fränkel .... However in these languages one can ask such meaningless questions as whether the Klein four-group is included in  $\pi$ "

This is the Cæsar problem.

and then he quotes Turing:

"Russell's Theory of Types, though probably not providing the soundest possible foundation of mathematics, follows closely the outlook of most mathematicians" [112].

(Any theory that gives truth-values to puns is clearly not to be trusted. If you listen a little longer you will probably hear it promise to cure baldness and global warming.)

But—how is one to reply to the foundationalist who says “But yes, we all know that it is possible to ask daft questions! Numerology is full of daft questions about natural numbers!”

to which i suppose the reply is that any sensible criteria for detecting daft questions will turn out to be precisely type-theoretic. Not only do we all agree with Lambek that the question of whether or not *the Klein 4-group is a subset of  $\pi$*  is daft, we probably all also agree *why* it’s daft: it’s ill-typed. [tho’ we probably wouldn’t all describe it in precisely those terms]

In the process of giving a type-theoretic analysis of why it’s a daft question the set-theorist will find themselves appealing to implementation-insensitivity.

The daft questions are a result of overspecification.

If you think of set theory merely as a convenient way of concretising mathematics, then you can shrug off the question of whether or not the Klein 4-group is a subset of  $\pi$  on the grounds that the answer is not implementation-independent, and therefore not genuinely a question about those two objects. If you think these mathematical objects are sets then you can’t shrug it off in this way. So what *do* you do? You might bite the bullet and say that—despite all appearances—it is in fact a sensible question, and one to which we now know the answer. However this biting-of-the-bullet invites a new challenge. Nowadays we are all naturalists: a naturalist philosopher is one who regards philosophising as a worldly activity that is part of the world that his or her philosophy describes. If you are a philosopher you diagnose errors being committed by those around you; if you are a naturalistic philosopher you are under an obligation to have a story of some sort about how those errors come to be made. For my part I am a naturalistic philosopher, and I have an explanation of why set-theoretic foundationalists make the mistakes they do: it is that they are confused about the implementation/definition distinction. For your part, if you are a naturalistic philosopher who thinks that these mathematical objects are sets and that “*Is the Klein 4-group a subset of  $\pi$ ?*” is a sensible question, then the challenge for you is to explain why so many mathematicians persist in their erroneous belief that it is a batty question.

Set theory is fundamental at least in the weak sense that a lot of mathematical objects are structured sets (tho’ arguably the reals shouldn’t be—see *pointillism*) even if the things they are structured sets of are not themselves sets. Perhaps one should say something here about *gluons* or *moments*. Very striking that mathmos do not lose sleep over the way in which the sets and the operations are parts of a whole. Would they be better mathematicians if they did?

If you make the error of set-theoretic foundationalism then you think of sets as quite different things from graphs. Nobody wonders what the universe of graphs looks like. The point perhaps is that all-the-sets-there-are (cf Kipling,

[68] ch 10 “*The crab that played with the sea*”) coheres in a canonical way that all-the-graphs-there-are does not.

One does seem to be stuck with the idea that algebras are [expansions of] sets. One is *not* stuck with the idea that their *elements* are sets. The real need for set theory in mathematics is only skin-deep, by which I mean one layer deep.

The categorists are quite right that (99% of the time) one doesn’t want to attempt to concretise mathematical entities, and that the endeavour to concretise them is in some sense unmathematical. Nevertheless there are genuine mathematical questions *about concretisation* . . . the minimal number of levels you need to define an ordered pair, the lowest level of the cumulative hierarchy at which you can implement the reals etc . . .

If you really do think that a set can’t have a cardinal unless it is wellordered then you have had your head done in by implementationism so completely that you have misunderstood the notion of cardinality altogether. Sure, it’s a point about AC, but your dodgy views on AC wouldn’t have got you into trouble without the extra handicap of implementationism.

Inference to the best explanation is all very well, but in some circumstances one should be wary of people who are willing to tell us what we want to hear. ZFC plays into all the errors of set-theoretic foundationalism by making vN ordinals so appealing.

must use the word ‘binary’ as in “like identifying the operating system with the binary that runs on your machine”

Correspondence Frege-Hilbert Letter Frege to Liebmann 1900; 1903 1906 Frege on foundations of geometry.

Patricia Blanchette says: look at The Journal of Philosophy Vol. **XCIII**, Number 7 (July 1996) pp 317–336; reprinted in The Philosophers Annual 1998; reprinted in Gottlob Frege: Critical Assessments of Leading Philosophers (Reck, E. and M. Beaney, eds), Routledge 2005.

[msg to self: I have a copy on my laptop in assorted-paper-archive]

also: Patricia A. Blanchette, Frege on Consistency and Conceptual Analysis *Philosophia Mathematica* (2007) **15** (3): 321–346 doi:10.1093/philmat/nkm028 reply to Wilfrid.

She also sez:

I thought you might like this quotation from Frege’s letter to Liebmann (1900):

“Hilbert was apparently deceived by the wording. If an axiom is worded in the same way, it is very easy to believe that it is the same axiom. But it depends on the sense; and this is different, depending on whether the words ‘point’, ‘line’, etc. are understood in the sense of Euclidean geometry or in a wider sense.”

(in English, in Frege's Philosophical and Mathematical Correspondence p. 91)

I'm not sure that that really was Frege' concern, but it is a real one so here goes. Suppose ZFC proves some fact about reals. An old-fashioned pre-fomdationalist mathematician might say "That's all very well, but how do i know that these so-called reals you are talking about are the same as the reals i've been talking about all these years? Or even suitably isomorphic?" The fomdationalist pronouncement that "they just are" cuts no ice. The traditional mathematician will reply "I see: you are asking me to be irish about it: asking me to say "All right, i won't start from here, i'll start from over there.". But i am starting from here; it's where we *all* started from, years ago."

And reals are not sets, sorry. Every thing is what it is and is not another thing.

Shelah solved the Whitehead conjecture by exhibiting a counterexample:  $V = L$  solves  $\mathcal{SH}$ ? by exhibiting a Suslin line. But suppose it had gone the other way? How do we know that a ZFC proof of the nonexistence of a particular mathematical object isn't merely a proof that the set theoretic universe doesn't, after all, reliably contain *simulacra* of everything? For example, suppose someone were to say "I am unimpressed by your proof that there is no set of reals order-isomorphic to the second number class. How do i know that your proof applies to genuine out-in-the-real-world reals?" How would one respond? (And one *should* respond: this is a serious honest challenge that deserves a serious honest answer.) The answer is that the proof doesn't depend on thinking of the individual reals as *sets*.

This example points the way to the answer. If reals really were pure sets we'd be OK, but they aren't. We would have to establish that it gave the same results for *all* interpretations of real arithmetic into set theory. Well, it presumably does, but that has to be proved. But is even that true? ZFC proves the completeness theorem after all, so any consistent theory (such as an unsound but consistent theory of arithmetic) will have a model in ZF. So how can we tell which models count for this purpose? Presumably those that arise from "the arithmetic of ZFC".

Explain the relation between implementation and Carnap's idea of *explication*.

The set-theoretic foundationalists will say that set theory has a lot to tell us about analysis. This is true, but what set theory has to tell us about analysis comes from clever definitions of sets of (sets of)<sup>n</sup> numbers; it doesn't come from thinking of the numbers themselves as sets.

Once you replace 'definition' by 'implementation' *passim* you then have to make the downstream change of redesignating your theorems as theorems about implemented-ordinals, not about ordinals. Of course this reveals them to be much less interesting that you had thought, but it is a price worth paying for getting back into good odour and regaining control of your brain.

## 4.1 What is the scope of set theory *sensu strictu*?

Although this is an interesting question in itself my purpose in raising it here is merely to make the point that even if we decide that ordered pairs, function, wellordering and other notions are set-theoretic notions, there will still be ideas that are not set-theoretical in nature, and which will oblige us, if we wish to be set-theoretic foundationalists, to find implementations of them into set theory.

“Set theory is that branch of mathematics whose task is to investigate mathematically the fundamental notions ‘number’, ‘order’ and ‘function’, taking them in their pristine simple form, and to develop thereby the logical foundations of all of arithmetic and analysis; thus it constitutes an indispensable component of the science of mathematics.”

Zermelo ([119] p. 200)

Do we take this to mean that Zermelo thought that “number”, “order” and “function” are set-theoretic notions...? Perhaps it’s not a fair question. The modern way to ask this question is to ask what we think the language of set theory is. For my part, I prefer a narrow picture: I take it that set theory is that part of Mathematics that goes on in  $\mathcal{L}(\in, =)$ . Many people who make a living by researching into sets think that ordinals and ordered pairs are sets too, and Zermelo (see above) seemed to be of the view that real numbers were part of set theory. Nevertheless, the general view seems to be that the language of set theory is  $\mathcal{L}(\in, =)$ . On the face of it this would appear to mean that people who think that *ordinal* is a set theoretic notion, and who have a narrow view of the language of set theory must believe that ordinals are particular sets—presumably the Von Neumann ordinals. I shall write of this *false consciousness*

$A \sqcup B$  is not a set-theoretic construct!!

What if we decide that the language of set theory has extra gadgets? Pairing, unpairing, functional application, domain and range? Is the resulting confection to be many-sorted or one-sorted? If one-sorted then we either have to implement (for example) pairing-and-unpairing once and for all, or we leave it unimplemented, so that ordered pairs are sets—but sets about whose members we are not allowed to ask questions! That’s not absurd, but it is odd.

What about manifolds? Branes? I don’t think that there is any sane stopping point on that road other than the place where it starts: the only sensible answer to the question “What is set theory?” is the one with which this section begins. These other objects can be implemented as sets, and altho’ the implementations are so nice they have been hardwired into our brains, that does not mean that the concepts themselves are set-theoretic.

If you think that pairing/unpairing is part of Set Theory doesn’t that mean that you think the implementation question is answered? So doesn’t it mean that you believe replacement..?

## 4.2 A Chapter on the evolution of the idea of an abstract data type

ADTs are what happen when you mathematise the external world. Actually they were always there, all along, but that's when they became obvious even to halfwits.

On Sunday 01 Apr 2012 09:31, T.Forster@dpmms.cam.ac.uk wrote:

- > (i) Is there a decent historical article anywhere
- > outlining the emergence of the idea of the \*Abstract
- > data Type\* (aka ADT)?

Hi, Tom,

There is a lot of literature from the 70s and 80s. Could not find a purely "historical" overview. But these references might be helpful, also via their bibliographies.

- DANA

(1) [89]

**Abstract:** This paper surveys ways in which the ideas and concepts developed in the research field of abstract data types have undergone a vigorous process of generalization that has led to the development of axiomatic notions of logic and of expressive multiparadigm logics. On the one hand, the generalization from equational specifications to specifications in any logic requires general metalogical concepts making precise what logics are.

Beginning with the notion of institution, several notions have been proposed to cover different needs arising in this task; we discuss these notions and summarize in particular the main ideas of the theory of general logics, which is a specific line of work in this area. On the other hand, the extension of equational logic in several directions to make specifications and programs more expressive has given rise to powerful multiparadigm logics in which other specification and programming paradigms can be cleanly combined with the equational one; we discuss several of these extensions, including rewriting logic, which unifies equational, Horn, object-oriented, and concurrent specification and programming.

These two lines of research converge in the idea of a logical framework, that is, an expressive enough logic in which many other logics can be represented. We use notions from the theory of general logics to make precise the concept of a logical framework, and summarize our recent work on the use of rewriting logic as a logical framework as a promising particular approach.

Finally, we explain how these general concepts can help us achieve the practical goal of formal interoperability, that is, how they can provide a mathematical foundation to rigorously interoperate the different formalisms and tools that we need to use in order to formally specify and verify systems.

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the Information Technology Promotion Agency, Japan, as a part of the Industrial Science and Technology Frontier Program New Models for Software Architecture sponsored by NEDO (New Energy and Industrial

(2) [25]

**Book Description:** This text expands the traditional course focus to examine not only the structure of a data object, but also its type. This broader focus requires a new paradigm for classifying data types. Within each classification, the different ADTs are presented using axiomatic specifications. Various implementation alternatives are discussed for each ADT and algorithms are written in a pseudo-code based on the Pascal–Modula-2–Ada model. Next, the Big- $O$  complexity of each implementation is discussed and each ADT is used in an application. Classic algorithms provide applications for some of the ADTs; implementation of a previously defined ADT is the application for others. The result is a clear, logical presentation that gives students a solid, practical foundation in current software engineering principles. Applications are included to demonstrate how the ADTs are used in problem-solving. Proven paedagogical features such as detailed examples, highlighted definitions, numerous illustrations, and exercises teach problem-solving skills.

(3)

[4]

**Abstract:** The notion of Abstract Data Type (ADT) has served as a foundation model for structured and object oriented programming for some thirty years. The current trend in software engineering toward component based systems requires a foundation model as well. The most basic inherent property of an ADT, i.e., that it provides a set of operations, subverts some highly desirable properties in emerging formal models for components that are based on the object oriented paradigm.

We introduce the notion of Abstract Behavior Type (ABT) as a higher-level alternative to ADT and propose it as a proper foundation model for both components and their composition. An ABT defines an abstract behavior as a relation among a set of timed-data-streams, without specifying any detail about the operations that may be used to implement such behavior or the datatypes it may manipulate for its realization. The ABT model supports a much looser coupling than is possible with the ADT’s operational interface, and is inherently amenable to exogenous coordination. We propose that both of these are highly desirable, if not essential, properties for models of components and their composition.

To demonstrate the utility of the ABT model, we describe Reo: an exogenous coordination language for compositional construction of component connectors based on a calculus of channels. We show the expressive power of Reo, and the applicability of ABT, through a number of examples.

From Roger Bishop Jones

The idea of abstract data type is important both in object oriented and in functional programming languages.

As far as the object oriented paradigm is concerned the idea is said to originate with Barbara Liskov: [78]

In 1973 Robin Milner started work on what would become Edinburgh LCF, an important aspect of which was the idea of using a typed functional programming language as a “meta-language” for programming proof search and checking, and the use of an abstract data type of theorems to ensure that any computation of a theorem would indeed yield a theorem of LCF.

As far as I am aware this was not published until 1978.

Some of the history here was written up by Mike Gordon in a paper with which you must already be familiar (obtainable from his web site).

There is a general survey paper by Cardelli and Wegner:

On understanding types, data abstraction, and polymorphism. Luca Cardelli and Peter Wegner.

Dick Crouch suggests reading “Thinking in JAVA” by Eckel.

Jacques Carrette writes:

This is not really a historical article, but rather a modern look at the idea, but I would nevertheless highly recommend [22]

The bibliography is extensive, and cites all the classic works on the topic.

On the axiom of foundation...

The axiom of foundation is a bit like those wartime prefabs and Nissen huts that I remember from my youth—put up in a hurry as a quick fix to a long term problem, and still in use long after superior alternatives became available.

(Fränkel, Bar-Hillel and Levy)

“Thus one can accept Axiom IX not as an article of faith but as a convention for giving a more restricted meaning to the word ‘set’, to be discarded once it turns out that it impedes significant mathematical research”

[50] p. 89.

People like me find ourselves saying that category theory seems to be a useful way of expressing things, even if it doesn’t have anything of its own to say. *It’s all notation.* But—as we keep saying—there are mathematically significant points about notation.

Must find something useful to say about intrinsic and extrinsic curvature, relating somehow extrinsic curvature to properties of an implementation.

A sphere has intrinsic curvature, a cylinder does not. Nor does the torus. Or (better) the sphere has nonzero curvature whereas the cylinder and the torus do not.

There is a certain amount of equivocation going on. The torus is the cartesian product of two copies of the unit circle ... but how do we think of the unit circle?

Is it merely a topological space, with the obvious topology? Or do we think of it as equipped with a metric (the obvious one)? It's when we think of it as equipped with a metric that we can sensibly ask whether or not the product (the torus) has curvature. So let's do that. Think of the torus as the product of two copies of the unit-circle-as-a-metric-space-in-the-obvious-way. Then, as it happens, it has zero curvature (it's flat). However, if we embed it somehow in  $E^3$  we find that the subspace  $T$  of  $E^3$  to which it corresponds *does* have curvature—at least if the metric on  $T$  is the metric it inherits as a subspace of  $E^3$ . What happens if we give it the metric of geodesics that “lie inside the surface”? My guess is that even then it's curved. At points on the “inner” rim the curvature is negative; on the outer rim it's positive. But that is an artefact of the embedding. Presumably you can embed the torus into a space of higher dimension in such a way that the image is manifestly not curved. (I can't see how, but never mind).

Casting the net a bit wider, isn't this a moral one could apply in philosophy of mind? Distinguish between properties of the representation and properties of the thing being represented? You know, the old saw about ideas being located in time but not in space distinguishes them from brain states which are located in both. Yawn...

Here it is:

“Mental events are located in time but not in space; physiological events are located in both time and space. Therefore mental events are not physiological events.”

There is clearly *something* going on here. (Not a whole lot, admittedly). How does this compare with arguments to the effect that wave-particle duality must be wrong because particles are located in space and time but waves are only located in time?

It now seems to me that this situation is exactly the same as the set-theoretic properties of ordinals being properties of the implementation not the datatype. The curvature of the torus is a property of an *expansion* of the torus—“expansion” in the model-theoretic sense. But the implemented ordinals in set theory are objects in an expanded ADT—ordinals plus some set-theoretic structure: it's exactly the same.

How does one respond to the insight that “ $2 \in 17$ ?” is a daft question? Is it to cease to be a realist about numbers? Or is it to adopt a Vienna-school-style category distinction? Compare one's reaction to “This stone is thinking about Vienna”. Why not be a structuralist about stones too? Well, physical objects are located in space and time, and they have endogenous properties—unlike numbers (it is said) so that is not an option.

How much of the charm of structuralism for IN and IR comes from the fact that their second-order theories are categorical?

The Cæsar problem is yet another example of misreading an engineering problem as a metaphysical one.

You don't have to be a Viennese logical positivist to think that all contentful philosophical activity must somehow, eventually, be connected with mundane human activity. Similarly, any genuine metaphysical problem in mathematics will eventually, somehow, one way or another, manifest itself as—so to speak—a stone in the mathematician's shoe. Had the Cæsar problem been a genuine philosophical problem we mathematicians would have noticed.

Sorts: as in many-sorted logic; they name the partial domains that make up a model. A number of mathematical theories are naturally many-sorted (e.g., “point” and “line” in plane geometry, “vector” and “scalar” in the theory of vector spaces over an arbitrary field, “object” and “arrow” in the theory of categories); if there are altogether finitely many sorts to a language, they can be replaced by predicates on a single sort—at the occasional cost of increased complexity in the axioms (disjunctions).

Polymorphic counting. Can we imagine an animal that has numerals for counting dingbats—and numerals for counting wombats—but has no way of saying that there are the same number of dingbats as wombats?

The problem is not AC, it's pointillism; AC is merely the stain that makes the pathology visible. Banach-Tarski is the disaster you see in the microscope.

If you stain pointillism with AC, you get these Banach-Tarski-shaped splodges in the microscope.

#### 4.2.1 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 [19] (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.

Every countable transitive model of ZF is a member of  $HC$ . There are  $2^{\aleph_0}$  complete extensions of ZF and every one has a transitive countable model so there must be at least  $2^{\aleph_0}$  things in  $HC$ !

If NF is not set theory what is it? It's certainly part of mathematics. It certainly doesn't fail to be part of mathematics in the way in which implementing UNIX on my laptop fails to be part of mathematics.

So you are going to tell me that 0 is the empty set... and that 1 is  $\{\emptyset\}$ . How can you tell? What did those ordinals do to give you that idea? Nothing about their behaviour *qua* ordinals tells what sets they have to be. It doesn't even tell you which sets they have to be *on the assumption that they are sets*.

They write ' $\forall n \in \omega$  for ' $\forall n < \omega$ ' but they don't write ' $\forall n \in k$  for ' $\forall n < k$ '. Again, no-one defines "*f*-dominates-*g*" by

$$(\exists n \in \omega)(\forall m)(n \in m \rightarrow g(m) \in f(m)).$$

Are they being inconsistent in their usage? They would say not, co's there's a difference between  $x \in \omega$  and  $x \in m$ . But is this difference a mathematical difference or a stylistic difference?

Writing ' $\omega$ ' for ' $\aleph_0$ ' can cause real confusion in the minds of unsophisticated readers if this muddle is perpetrated in a sentence they are trying to read in which there is an address that can be occupied by a cardinal or an ordinal—as is the case with infinite exponent partition relations.

Again, the point is not so much that writing ' $n \in \omega$ ' for ' $n \in \mathbb{N}$ ' is wrong; the point is that it is *bad practice*. And it would be bad practice even if *per impossible* it were true that  $\omega = \mathbb{N}$ . It's bad practice in the way that writing in assembler (when you don't have to) is bad practice. Never call by value when you can call by name.

Writing ' $\omega$ ' when you mean ' $\mathbb{N}$ ' will confuse readers who are not signed up to logician-speak. Suppose you are an algebraist or a number theorist who looks up something logical and finds ' $\omega$ ' where they should find ' $\mathbb{N}$ '. They get confused. And quite possibly annoyed. This choice of notation says nothing about the material; it says something about the utterer.

Must satirise the idea that everything is a set as *monochrome*. black-and-white. Or perhaps say it's OK as a thought experiment, but not as a foundation for mathematics. It's one thing to have the thought-experiment of being trapped on a desert island with one of my prettier students; trying to realise it would get me into serious trouble. So it is with sex, i mean sets.

Writing ' $\omega$ ' for the set of natural numbers might not automatically mean that the writer believes that the first infinite ordinal is the same object as the set of natural numbers; it might merely be a signal that the writer belongs to a particular mathematical community, namely set theorists of a particular stamp. It may even be an example of what i have elsewhere called *lexical choice semantics*. When Randall Holmes, in the paper in which he proves  $\text{Con}(\text{NF})$  writes ' $\omega$ ' where he means ' $\aleph_0$ ' (and this despite my reproaches!), he was doing it in order to look and sound like a ZFiste. After all, he is writing for an audience of ZF-istes, and he doesn't want to alarm them from the outset by pointedly sounding different; they're very *skittish*.

Dammit, if  $\aleph_0$  and  $\omega$  are the same thing, then it ought not only to be OK to write ‘ $\omega$ ’ when you mean ‘ $\aleph_0$ ’ (as so many miscreants do) but it ought also to be OK to write ‘ $\aleph_0$ ’ when you mean ‘ $\omega$ ’—and nobody does that! And why not, one might ask? Don’t bother to look for a mathematically sensible answer, co’s there isn’t one. I can’t help suspecting that one of the reasons why people write ‘ $\omega_n$ ’ when they mean ‘ $\aleph_n$ ’ is nothing more complicated than the fact that they don’t know how to draw the symbol ‘ $\aleph$ ’. [How many people know how to write ‘ $\aleph$ ’?] Looking at some logical literature from the 1960’s (Mark Wilson has just given me his copy of Springer LNM 72) it occurs to me that another [related] contributory factor is the fact that in the days of golf-balls (mere typewriters) your typewriter was more likely to have a Greek golf ball (giving you a ‘ $\omega$ ’) than a Hebrew golf-ball (giving you an ‘ $\aleph$ ’). Why did Jensen write his combinatorial principle with a ‘ $\square$ ’? Because he had a typewriter that was designed for a modal logician!

Writing ‘ $\omega$ ’ for all three of  $\omega$ ,  $\aleph_0$  and  $\aleph_0$  makes sense only if you hold a particular philosophical position. It’s a crazy thing to do otherwise. Come to think of it, it’s crazy anyway. And it’s crazy not because the philosophical position on which it rests is crazy (tho’ it is) it’s a crazy policy beco’s it’s divisive. It’s a notation that rests on a quite particular reduction of mathematical objects to sets. I suppose it could be defended on the grounds that it’s merely a notational convenience, but that’s not the real reason.

rewrite this next bit

Socially it serves as something that says “look, i’m a set theorist” but even i (a set theorist myself) find this elbow-tugging tiresome, and non-set-theorists may find it even more so. They may even find it confusing (“why am i being told this?”).

It’s a notation that is designed not to say anything about the subject matter, but about the speaker.

Zachiri is showing me an Ehrenfeucht-Mostowski proof that involves a family of constants indexed by  $\mathbb{Z}$ . Naturally this involves a compactness argument where, for each finite subset  $X \subseteq \mathbb{Z}$ , we prove consistent a theory  $T_X$  mentioning only constants whose subscripts are in  $X$ . What he *in fact* does is prove the consistency only of those theories  $T_X$  where  $X$  is an interval of  $\mathbb{Z}$ . This is perfectly OK of course, but note that his reason for this specialisation is not *mathematical* but *notational*. It’s much easier to notate the proof if  $X$  is an interval than if it is a mere subset.

Peter Lumsdaine sez that yes, everything is indeed inductive. Even reals are strings of 0s and 1s. This is, in another guise, the idea that all mathematical objects are finitely presented.

He also says that if it really doesn’t matter that the category of sets according to NF is not cartesian closed (for the usual reasons) because there is a cartesian closed category lurking inside there somewhere.

Being an NFiste is a bit like being a vegetarian. In both these cases the minority (NF-iste, vegetarian) is acknowledged by the majority party as being in

some sense a legitimate part of the landscape, albeit a part that is uncontroversially *in error*. However it is important to remember that this acknowledgement of legitimacy is not a recognition of the rights of the minority; the majority tolerates the minority beco's the majority wishes to be thought of as magnanimous and inclusive. Their magnanimity and tolerance is something they owe to themselves, not to the other party; the other party is in error and does not have the same rights to take part in the debate as the majority has. The majority owns the space. The demands on one's magnanimity and capacity for inclusion scale linearly with the size of the minority, and of course no-one wishes to expend, on magnanimity and inclusion, time and glucose that can be better spent on Doing One's Own Thing. ZF-istes and meat eaters tolerate NFistes and vegetarians of course, but the fewer there are of them the happier they'll be. And the minorities are well advised to not presume too much on the tolerance and magnanimity of the majorities. In any interaction between a minority member and a majority member there eventually comes a point at which the meat-eating ZFiste says "Thank you! This has gone on long enough—please shut the \*\*\*\* up and go and sit in the corner and let me get on with my life"<sup>5</sup>.

There is an edge to the toleration and magnanimity, an edge of resentment. ZFiste meat-eaters resent vegetarian NFistes because of the lurking suspicion that *they just might be on to something*.

[or put it another way: being a NFiste is a bit like being a vegetarian. You are tolerated *of course*, but you are resented. Altho' the Zfistes/meat-eaters have definitely decided not to take your ideas seriously, and in *that* sense have come to a conclusion, they haven't been able to *entirely* banish the thought that *we just might be on to something*]. They've taken a policy decision to ignore it, but they haven't been able to convince themselves that it absolutely isn't there.

This is a discomfiting tho'rt beco's it raises the possibility that *one might have to start doing things differently*. And nobody wants to do that. Chesterton (once) said something intelligent: "The object of opening the mind, as of opening the mouth, is to shut it again on something solid.". It's not just that one *disagrees* with vegetarians/NFistes, that you think they're wrong, it's that you *want* them to be wrong.

That is very human, but it is of course irrational. The rational policy is to want to know when you are wrong, the better to put things right. A policy of wanting vegetarian NFistes to be wrong, all-to-humnn tho' it is, is not rational. And, to be fair to (some of) the people who act in this irrational way, they know it's irrational—and, at some level—they know they're doing it. And that of course is even more annoying! And what do you think about people who annoy you? You get cross with them!

ZFistes have made a lifestyle choice to not think about NF. The trouble is that that lifestyle choice compromises their ability to do their job properly. It's not like a lifestyle choice to not do fluid mechanics or percolation.

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<sup>5</sup>As one ZFiste with whom i was organising a conference said to me when i persisted in trying to make a contribution "I have had enough of your aggression"

### 4.2.2 A rather good remark of Randall's

“An intuitive picture of set theory: we start with a universe of bare objects, and we want to identify sets of these objects with objects in the universe. The guiding metaphor is that we implement a set by attaching a “label” (the concrete object which “is” the set) to the collection of objects. Certain collections must go unlabelled, as Russell's argument shows. Let's back off from Russell's class itself, and consider its less alarming complement: *the set of all sets which are elements of themselves*. When we try to determine whether this set is an element of itself, we find that we have no grounds for making a decision; it is an element of itself exactly if it is an element of itself. But there is no immediate paradox; we can consider it a little more calmly. On reflection, we observe that there is something wrong with the predicate “is an element of itself” (quotes used as delimiters here; this is not a character string!) *a priori*—before consideration of any paradoxes. When we test a collection  $A$  to see if it has this property, we look at the “label” we have attached to the collection and check to see if it is found among the members of the collection. My claim is that this is not information about the collection at all—it is information about the *labelling scheme*—which is essentially arbitrary! In a computer science context, the labelling scheme is an implementation of a data type “set”; the property “is an element of itself” is seen to be a property depending on the specific implementation of the type—asking whether the collection  $A$  “belongs to itself” can be viewed as an illicit peek at the inner workings of an abstract data type! This observation suggests that the condition “ $x \in x$ ” (and the condition “ $\neg(x \in x)$ ”) should be ruled out because they do not really define properties of sets, before we ever get to the point of seeing that one of them is paradoxical. Moreover, this insight can be extended; what needs to be avoided is any query about the relationship between an object  $x$  in its rôle as a bare object and its rôle as (the label of) a set. Further reflection reveals that each object has other potential rôles as well: if each element of the collection labelled by  $x$  is being considered in its rôle as (the label of) a set, then  $x$  is being considered as (the label of) a set of (labels of) sets; each object can play rôles in the definition of a property as a bare object, a set, a set of sets, a set of sets of sets, and so forth. No object can be considered in more than one of these rôles in the definition of a property of objects without violating the “security” of the labelling scheme. But this condition is exactly the stratification condition of *NFU* or *NF*! We get *NFU* instead of *NF* because we do not know whether we will need to (or be able to) use every object as a label; unused labels are interpreted as “individuals”. However, nothing prevents us from having an object  $V$  which labels the universe; we will then have  $V \in V$  as a particular fact; what we cannot do is abstract from this to a property “ $x \in x$ ” of general objects  $x$ . The theory remains untyped. Notice that the map  $\lambda x. \{x\}$  which takes objects to their singletons clearly involves information about the labelling scheme!”

He goes on “Specker's argument shows that if we want AC, we will end up with more individuals than sets; but it is not a violation of intuition to assert that not everything in the world is a set (it is merely a little odd that we can

actually prove it)!” but that doesn’t matter so much.

There is a sort-of Wittgensteinian point to make about understanding the equivocation between types that is legitimated (by AC for example) so that we can then go back to our old practice.

$R(x, y, z)$  says there is a partition  $\Pi$  of  $x$  s.t. any two distinct pieces of it that are the same size are the same size as  $y$  and if there is a piece that is not the same size as  $y$  it is the same size as  $z$ ; if there is no such piece then  $z$  is empty.

(This can be made purely set-theoretic: we do not need ordered pairs, because the pieces are disjoint.)

Equipollence is a congruence relation for  $R$ . This give us modular arithmetic. This is neat: no need to talk about bijections between sets of ordered pairs—which would have been necessary had we gone down the road of  $(\exists w)((x \setminus z) \sim (y \times w))$ . But there is a price to be paid. We would like to postpone thinking about cardinalities of sets of ordered pairs as long as possible because we could in principle have a stratification problem with ordered pairs.

Observe that we need choice to prove

$$(\forall x, y)(R(x, y, \emptyset) \rightarrow (\exists w)(x \sim (y \times w))).$$

(Russell knew this: he wrote about it in [100] (Introduction to Mathematical Philosophy) but where is the point first made?)

This is related to Lagrange’s theorem. We say a group  $H$  is an extension of  $G_1$  by  $G_2$  iff  $H$  has a normal subgroup isomorphic to  $G_2$  and the quotient is isomorphic to  $G_1$ . To prove  $|H| = |G_1| \cdot |G_2|$  we need choice.

How can we tell that ordered pairing is *not* a set-theoretic notion (in the sense that, say,  $\subseteq$  is a set-theoretic notion)? Is it because there is more than one way of defining it and no single way is obviously the “right” way? (Here, as so often, one should trust one’s mathematical intuitions). That is to say, the language of set theory has a partial automorphism that swaps pairing functions. What do we mean by “partial automorphism”? One free-associates to things like Coret’s Lemma. (On that note, there must be a general result known to group theorists along the lines of “properties of  $F(X)$  [where  $F(X)$  is obtained from  $X$  in a manner straightforward enuff so that any group acting on  $X$  acts naturally on  $F(X)$ ] of a certain flavour are invariant under the action of  $\text{Symm}(X)$  acting on  $F(X)$ ”.)

Suppose  $\mathfrak{A}$  is a structure of some kind, with carrier set  $A$ . What is the relation between  $\text{Aut}(\mathfrak{A})$  and  $\text{Symm}(A)$ ? Of course  $\text{Aut}(\mathfrak{A})$  must be a subgroup of  $\text{Symm}(A)$ . But there must be more one can say than that. If the structure we put on top of  $A$  to obtain  $\mathfrak{A}$  can be characterised set-theoretically then  $\text{Aut}(\mathfrak{A})$  should surely be a quotient (or do we mean subgroup?) of  $\text{Symm}(\bigcup^n A)$  for some  $n$ . For example, let  $\mathfrak{A}$  be a total order of  $A$ , tho’rt of as a set of Wiener-Kuratowski ordered pairs—or perhaps an ordernest, since the point can be made

either way. That makes  $\mathfrak{A}$  a subset of  $\mathcal{P}^n(A)$  for some small  $n$ .  $\text{Symm}(A)$  acts on  $\mathcal{P}^n(A)$  in the obvious way. Does this not give a homomorphism from  $\text{Symm}(A)$  to  $\text{Aut}(\mathfrak{A})$ ? No, because an arbitrary permutation of  $A$  might not give an automorphism of  $\mathfrak{A}$  . . . tho' it will move it to something of the same signature with the same carrier set.

But it does seem to me that we need to get 100% straight how automorphisms of  $\mathfrak{A}$  and  $\text{Symm}(A)$  interact.

One difficulty with the Quine pair is that it does not have the feature all other pairing functions have, namely that if we get  $\text{Symm}(A)$  to act on  $\mathcal{P}^n(A)$  in the obvious way we find that the action of  $\sigma$  on  $\langle a, b \rangle$  is to send it to  $\langle \sigma(a), \sigma(b) \rangle$ . This is because when forming the Quine pair of  $a$  and  $b$  we have to “look inside”  $a$  and  $b$ .

Representing orders by ordernesting works just as well as non-Quine pairs (Randall says that the von Neumann ordinals are their own ordernesting, and the unique solution if we have foundation. If we have foundation then extensiality enforces linearity.)

Suppose we have a structure  $\mathfrak{A}$  with carrier set  $A$ .  $\text{Symm}(A)$  acts on  $\mathfrak{A}$  in the “obvious” way. By implementing ordered pairs, relations etc we can concretise  $\mathfrak{A}$  as a substructure of  $V(A)$ . We want  $\text{Aut}(\mathfrak{A})$  to manifest itself as a subgroup of  $\text{Symm}(A)$ . Think of this as an extra constraint on implementations of mathematical objects in set theory.

One might wonder whether the disjointness condition is a sign that we aren’t really thinking about sets here but objects in a suitable category (“finite products”). In some ways 1-equivalence is more natural than equipollence. It takes care of the tiresome disjointness condition on cardinal addition. However it means that we have to think of the congruence relation as being one on tuples not on single objects the way bijection is. I mean: if the pair  $x$  and  $y$  is disjoint, so is  $j(\pi)$  of the pair. The usual way in which group theorists think about the action of permutations on tuples makes it clear that they are not thinking of them as Wiener-Kuratowski—unless of course they don’t think of  $j^2\pi$  as a different permutation from  $\pi$ . But then perhaps they don’t! I think this may be another case for radical translation.

Anyway, to get back to “is pairing a set-theoretic notion?” and “how can one tell?” perhaps this is a pointer that a more natural notion is a two-sorted language for sets and permutations acting on sets.

Some people have typing intuitions that are more pressing. That’s the word to use . . .

Randall says that the two roots of typing are more historically intertwined than I have been suggesting. One input into the CS tradition is Church’s 1940 paper [does he mean [?]?] which goes back to Russell-and-Whitehead. Richard Bornat says that the CS idea of a datatype comes from mathematics. But which mathematicians?

Mathematicians would benefit from taking the idea of abstract data type even more seriously than they do. It is equivocations over set/counted-set that make choice look spuriously plausible. I find that in order to explain to my third-years why you need AC to prove that a union of countably many countable sets is countable one needs the notion of the datatype **counted-set**.

Do we want to say anything about how the proof that every countable limit ordinal is of cofinality  $\omega$  requires us to reason about wellorderings and not merely about ordinals?

We should explain the locution ‘arithmetic of  $T$ ’ and the parallel ‘set theory of  $T$ ’. When we assert that, for example,  $\text{con}(T_1)$  is provable in the arithmetic of  $T_2$  then it is the virtual arithmetic we have in mind. These consistency assertions are not implementation-sensitive.

Some things that emerge from sets (and whose emergence can be iterated) need higher-order set theory: set theory with several levels. They concern stratified predicates. A virtual theory of sets of widgets and sets-of-sets of widgets etc is a different device from a virtual theory of sets as in the *set-theory-of-T* using relational types.

Might be able to make good use of the word ‘substrate’.

Mathematicians much more likely to read Polya than Wittgenstein or even Brouwer.

Do we need to say anything about stratification and locality?  $k$ -definite machines. Look at stuff in `logicrave.tex`.

Charlotte Angas Scott wrote about von Staudt  
See an article by Mark Wilson: The royal road from geometry/Frege Nous  
1990’s

“Mathematicians are philosophers who are not interested in ontology”  
—Plato, in the republic somewhere....

So when is a set-theoretical result about an implementation of a mathematical entity (a *simulacrum*) a theorem about the entities being simulated, and not just a theorem about the simulacrum? Is setlikeness the key, here, too?

When we say that a pairing-with-unpairing suite must be setlike we can’t mean that it is a setlike function from  $V \times V$  to  $V$ . We couldn’t even express such an idea until we have implemented pairing. We have to curry it! What we must mean is that if we think of it as a function  $V \rightarrow (V \rightarrow V)$  then it is a setlike function whose values are setlike functions.

Hang on . . . even *that* isn't OK. Because we can't talk about a setlike function having values that are functions until we have implemented functions (and ordered pairs!) And these functions are defined on the whole of  $V$ .

Still need to say something about the fact that “everything is an ordered pair” is not implementation-invariant, despite being well-typed. Does this mean that the preservation theorem should say that well-typed things are preserved by Rieger-Bernays permutation? This plugs in to the thinking about dodgy pairing functions like Wiener-Kuratowski  $\langle\{x\}, y\rangle$ . This is setlike—even in NF—(at least in the sense that if you think of it as a function  $V \rightarrow (V \rightarrow V)$  then it is a setlike function whose values are setlike functions. This pairing function denies the global existence of composition of relations. And what's more, so does everything RB-equivalent to it.

Implementing wellorderings as sets of ordered pairs. If you are a serious-minded foundationalist then the problem with the empty wellordering is really annoying.

The strong typing needs to be acknowledged and embraced. A lot of the tangles people get into over AC are the result of fallacies of equivocation over types.

Is there a stratified proof of Borel determinacy?

How smooth are the mutual interpretations of ZF+ foundation and str(ZF)+IO?

In proceeding to the inner model  $HS$  we seem to destroy all unstratified information. But it's all still there! See Vu Dang's Ph.D. thesis [?].

One of the advantages of a youth misspent studying NF is that the need to check whether or not anything is stratified keeps constantly in one's mind the question of how things look in primitive notation. At that in turn reminds one that the interpretation into primitive notation can be pretty arbitrary.

Fit in somewhere the stuff about Chesterton from axiomsofsettheory.tex

Equivocation: we implement objects but we also implement theories of objects. (overloading?)

The psychological literature equates counting with cardinality.  $\Rightarrow$  axiom of counting.

The kind of typing equivocations that i had been writing about (sequences of pairs vs pairs of sequences) . . . do they rely on natural isomorphisms between types?

This matters!!

The possibility of the implementation is mathematically important but the details of it are not. And the fact that the presence of the possibility is mathematically significant does not make it a fact that the possibility should be worked through.

Also should try and tie together the thought that it's a good idea to use *urelemente* to implement mathematical objects whose internal set-theoretic structure (in the implementation) is irrelevant . . . with the thought that permutations propagate in one direction only . . . to  $\mathcal{P}(x)$  but not to  $\bigcup x$ .

Set theory as a basis for mathematics, well, yes. Very little of mathematics is purely set-theoretical, but the parts that aren't can at least be *implemented* in set theory. Very well, so we find within set theory suitable *simulacra* for the suite  $\mathcal{S}$  of mathematical entities we wish to study, and then we reason about their implementations, the *simulacra*. Our reasoning issues in theorems, which are supposedly about the original mathematical entities. That, after all, was the point of the exercise. What we tend not to keep an eye on is the extent to which our success in proving theorems *in set theory* about general mathematical objects (that are not themselves purely set-theoretic) depends on our choice of implementation. What would we do if we could prove the twin primes conjecture for von Neumann naturals but not for Zermelo naturals? Well, that's obviously not going to happen, but it's worth explaining to oneself why not.

This last point has an important corollary. Since the proof of the twin-primes conjecture isn't going to depend on naturals being implemented in one way rather than another, talk involving the implementation can have no part to play in the explanation or proof. Thus we won't be satisfied with a proof that relies on any one interpretation. For example (and one of my pet hates) we should not be satisfied with a proof that  $<_{O_n}$  is wellfounded that relies on ordinals being von Neumann. More on this: nobody would dream of proving the least-number principle for  $\mathbb{N}$  by appealing to the fact that the order relation on von Neumann naturals is wellfounded. See p. 50.

But now there is something funny going on. I was proclaiming that it's not going to matter how you implement naturals when you are attempting to prove the twin primes conjecture, but then I complain about attempts to prove that ordinals are wellfounded by appealing to the von Neumann implementation! But it's not that odd really: the point being made in both cases is that one should reason as far as possible with the abstract object and not with the implementation.

So what was the point of implementing the damned things in the first place?

### Lying to children

Imre's point, that has been keeping me awake at nights, is this: "We don't flag uses of the axiom of pairing, so why do we have to flag uses of AC?"

One reason why we might want to flag AC or excluded middle is that we might be in a context where we want to make a point about a legitimate alternative view. And you can believe this even if you think, as Imre does, that AC is just straightforwardly true.

OK, so let's suppose—for the sake of argument—that AC is just straightforwardly true, and is as true as the axiom of pairing. Let's also agree that there are situations where we have a specific need to flag the uses of—for example—AC and T non D. These situations are fairly well understood, and i won't labour the details. Imre's position is that there is typically no other reason to flag use of AC or T non D. So his challenge to me is to explain to him what is special about AC. I cannot say that the difference is that it is mathematically questionable, because we have agreed to park that issue. I have to come up with something else. The best answer i can come up with at the moment is a political/pedagogical one, namely that since they are going to have to learn to spot uses of AC later on it is fibbing to present material to them in a way that denies there is an issue.

But what are axioms for, and what is a proof? Can we accept as a proof something that doesn't bring out into the open the axioms it uses? The answer to this cannot be straightforwardly 'no' because o/w we would have to repudiate all the proofs in non-axiomatic mathematics.

[insert stuff here about pairing from my 2016 paper]

## 4.3 Spurious detail

### 4.3.1 Annoying Technicalities

One can't help feeling that if one only conceptualises the subject matter properly then all the rules/axioms one had would be sensible rules/axioms that encapsulate principles and there wouldn't be any annoying technicalities that are there only to make things work. I have written elsewhere about how the axiomatisations of set theory usually allow for *graceful downward compatibility*: each axiom corresponds to a principle—as above. In those circumstances dropping any one axiom is usually a sensible thing to do. In general if one has satisfactorily conceptualised—and satisfactorily axiomatised—a subject area then each axiom has a manifest sensible job to do and one gets sensible results if one considers subsystems obtained by dropping one axiom or another. It does fewer things, but whatever it does it does sensibly. If one hasn't conceptualised properly then one probably (and i think this is Ken Manders' point) has lots of spurious distinctions and one has to repair it by lots of structural rules that erase them. (tho' i am being a bit hasty—that's not what the structural rules in sequent calculus do).

What marks off the annoying technicalities from the sensible axioms is the fact that you don't get anything sensible if you drop them: if the axiom is an annoying technicality then discarding it doesn't result in an interesting subsystem, it merely results in the system not working properly. (Perhaps this means that

the basic theory for any subject (“meccano set 0”) is the collection of annoying technicalities). There are axioms/rules to be found in set theory and in sequent calculus that are of the annoying-technicality variety. The annoying technicalities of sequent calculus are the structural rules; the annoying technicality in set theory is the axiom of pairing. One can try to make it look like a sensible principle by pretending that replacement is an infinitary version of it (it follows from replacement and power set) but few will be convinced. Dropping pairing doesn’t make for a sensible set theory. Indeed it is only very recently and with great ingenuity that any models have been found for  $ZF \setminus$  pairing. See Mathias [80].

However there will always be people who want to see what happens when you drop an annoying technicality. Boys will be boys won’t they? If you give an 8-year-old boy a toy tractor he will take the wheels off (if he can) and put them on again—the wrong way round, as like as not. And why not!—it’s natural. Usually no permanent damage is done to the toy, but it’s only very occasionally that an interesting new toy emerges. (“Hopeful monster”) What do you think is going to happen if you give a grown logician a system of sequent calculus, with nice logical rules and nice structural rules?

rearrange

Nobody has suggested that it might be interesting to see what can be done in a set theory without the axiom of pairing. However plenty of people have suggested dropping structural rules from sequent calculus. They take the structural rules off and put them back on—back-to-front of course—or perhaps lose them altogether. What is the result?

The first answer has to be that there is nothing about the process of taking the wheels off and putting them on the wrong way round that tells us that the results will always be bad, nor that they will always be good. (Naturally there is a biological literature about this!) The second thought will be that the role played by the wheel-in-the-wrong-place—always assuming it *has* a role in the new scheme of things—cannot be expected to bear any sensible relation to its role in the earlier scheme of things. The new mutant entity might be a sensible modification of the original entity, but it might be something entirely different. A good (and topical) illustration of this is Linear Logic. Classical logic manipulates propositions; linear Logic is obtained from a sequent formulation of classical logic by monkeying with the rules of weakening and contraction. As I remarked earlier, there is no *prima facie* reason to expect any sensible outcome, but—as it happens—there is one. However that sensible outcome is a calculus that manipulates not *propositions* but *resources*. My first reaction to this development is to say that Linear Logic is not a *logic* (only engineers call stuff like that ‘logic’) but this isn’t really (or shouldn’t be allowed to become) a turf war over who owns the word ‘logic’ (there are more engineers than logicians, after all). However the fact that Linear Logic manipulates resources not propositions should be recorded in our nomenclature somehow, and calling it a *resource logic* may be a sensible way to proceede.

Hiatus

Is the desire to re-establish identities between the spurious(ly distinguished)

entities a source of axioms? Can we place Quine's invention of NF ("restoring severed connections") as a reaction to what Holmes calls the "hall of mirrors" of TST in this context?

The annoying technicalities are the things that tell you that the spuriously distinct things are in fact the same. Or are those the principled axioms?

I've just been trying to explain to two 1a's about the natural bijections in (i) currying and (ii) between  $X \rightarrow \mathcal{P}(Y)$  and  $\mathcal{P}(X \times Y)$  and (iii) between  $(A \rightarrow B) \times (A \rightarrow C)$  and  $(A \sqcup B) \rightarrow C$  and so on. It seems to me that—quite apart from the difficulties with the notation—they also have an entirely separate difficulty in understanding what the problem is supposed to be in the first place. Anyone who can understand the bijection between  $(A \rightarrow B) \times (A \rightarrow C)$  and  $(A \sqcup B) \rightarrow C$  quite possibly does it by thinking in a way that postulates a single mathematical object of dual aspect. That is to say that they probably think of the curried function and the uncurried function as one and the same mathematical object. (And they think of the two objects (i) the function from  $(A \sqcup B)$  to  $C$ , and (ii) the corresponding pair of functions  $A \rightarrow B$  and  $(A \rightarrow C)$  as one and the same object.) And indeed—in an operational sense—they are the same object: you can use them for the same things. If that is how you are thinking of these matters, then you will not be able to understand currying, or at least it might look to you like a mere notational quibble. You can't see in front of you two distinct well-lit things waiting to be joined up by some act of yours, you see only one. *No entity without identity* indeed.

Ditto subsets. There is this object which we can think of indifferently as a subset and as a function. If they are thinking of the two objects as one, then one can make use of the idea of *casting*: the one object can be cast from one type to the other, and the bijection is the act of casting. This can make for difficulties. If you are thinking of a subset of  $X$  as a function from  $X \rightarrow \{\top, \perp\}$  then—again—you can't see two distinct things for the bijection to connect.

Partial orders and strict partial orders: they contain the same information. Williamson on converses [115] makes essentially the same point. Once we have formalised a language we need to axiomatise in such a way that the connections between the spuriously separated entities become clear. Cross-modal matching anyone?

Partitions-and-equivalence-relations could be another example. (This reminds me again of Ben Garling's remark that paedagogical difficulties often overlie the scars of old foundational questions.) However there i think the partition seems more fundamental, and the difficulty is not of the same nature as with currying/uncurrying.

## 4.4 Polymorphism, not Sets vs Classes

*...in which we discuss what the mathematical rôle is of the set theorists' distinction between sets and proper classes, and whether some of the hygienic purposes thus served could be better served by other means.*

Useful form of words we should use in connection with Rosser’s axiom of counting and the possibilities of polymorphism: the abode of peace (dar-es-salaam, or *comfort zone*) is characterised not by the fact that when you are in it the objects around you are sets rather than proper classes, but instead by the fact that types are monomorphic rather than polymorphic. That is to say, by the fact that you have complete freedom to manipulate things.<sup>6</sup> See the material in chapter ??.

The typed nature of Mathematics is an obvious given: it is by building on that that we can organise our mathematical practice properly, not by importing set theory.

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

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

The only reason why the abode of peace can be characterised as that area where everything is a set is that if everything-is-a-set then you have the desired freedom of manipulation—you never have to worry about the possibility of something turning out to be a proper class—and it is this freedom of manipulation that makes it the abode of peace. It is much more illuminating to think of freedom of manipulation in terms of type disciplines than in terms of set existence. Indeed it is in precisely the fact that typed programming languages interfere with programmers’ freedom of manipulation (and thereby prevent them from making silly mistakes) that the appeal of those languages lies.

Mathematics is certainly conducted in the abode of peace—most of it anyway: one expects nothing less. Mathematicians really do not want to have to worry about whether or not the collections of interest to them are proper classes, and there is no taste for polymorphic natural numbers.

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

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<sup>6</sup>We have to be careful how we use the word ‘polymorphic’. Even in NF, where the ordinals are polymorphic in the sense i mean, there is only one **sort** of ordinals. It’s not a many-sorted theory.

### A message from Mohan:

Turns out there are two flavours of type theory.

Structural: you have a set of 'ground types' and various type constructors (product, sum, etc.) which generate all of your types  
Nominal: Types correspond to names [at least, as far as I can gather]

Most programming languages need to be modeled with the nominal version, because you can create arbitrarily many distinct 'record' or 'struct' types which are 'like', say,  $\text{Int} \times \text{String}$ , but which have different names and therefore are distinct.

If you are using types for the purposes of disambiguation (which I am), maths can't be modelled with the structural version, because

a)

$\text{Group} = \text{Set} \times \text{Function}$

$\text{Metric Space} = \text{Set} \times \text{Function}$

b)

you need Group and Metric Space to be distinct types, because otherwise notation defined in groups and notation defined metric spaces (e.g. " $A \times B$ ") will collide, causing ambiguity.

But IMO the nominal version doesn't really work either... although it's rare, you do find types with the same name (for example, in IIB Functional Analysis I encountered 'rings' which were distinct from the rings of algebra). This is normal for maths; cf. the many meanings of 'prime'. Further, I don't think that there are hidden namespaces here ... or at least, if you want to account for other ambiguous things (like 'prime'), namespaces aren't a good solution. So I think that the right thing is:

Whenever you say

An  $X$  consists of a  $Y$  together with a  $Z$ .

this generates a new type.

I.e. for maths you need an essentially dynamic approach...

[ That was longer than I meant it to be – hope it was of some interest!]

anyway, thanks again;

Mohan

Mohan says: "I defy you to come up with a theorem and a proof of it wherein the real number 0 and the integer 0 both appear and where it matters that they are different". Randall's riposte to this is : "I defy you to come up with a theorem and a proof of it wherein the real number 0 and the integer 0 both appear and where it matters that they are the same!" That is tricky ... because you do need a casting function.

## 4.5 Some Polemic

If you believe—as i do—that mathematics is simply anything done with sufficient rigour, then higher set theory and illfounded set theory are part of mathematics. There’s no way of preventing it. And never underestimate the capacity of great mathematicians to make idiots of themselves by pronouncing anathemas against novelties. There were people who not only didn’t think complex numbers were part of “ordinary” mathematics, but were not part of mathematics at all. They were just wrong. If you have a complex-number-problem with illfounded sets, get over it. Quite how you get over it is up to you. After all, as Brian of Nazareth said, “We are all individuals: you’ve got to work it out for yourselves”.

Blend these two

To say that such-and-such “is not part of Mathematics” (or perhaps, more weasel-ly “not part of *ordinary* Mathematics”) is to offer a terrible hostage to fortune and to expose oneself to the danger of ridicule by those who come later. (Arthur Clarke) There were Old Farts who said that complex numbers were newfangled nonsense that wasn’t part of mathematics. Now there are New Farts (or New Age Farts? Young Farts?) who say that transfinite numbers are newfangled nonsense that aren’t part of mathematics. There is nothing new under the sun. Oh but it’s different this time they say—as lemmings would say if they could talk. We were told that the axiom of choice was not part of mathematics; … Kronecker said that blah. Poincaré said that Set Theory was a disease from which mathematics would recover … *und dafür ist er nun tod* as my parents would say. Set theory marches on.

The Rolle-Saurin debate; the Jurin-Berkeley debate …

The History of Mathematics is littered with the corpses of people who—gifted with hindsight—have died laughing at pronouncements like these. It’s no use complaining that it’s somehow unfair of them to have hindsight: historians have hindsight—that’s their thing. History is the exercise of hindsight.

[of course this isn’t entirely fair: when the Great and the Good say that some new development is tosh they are usually right, and the simplified *post hoc* histories—needing a continuous thread (a *yarn*, in fact)—do not tell the backtracking narrative that records all the blind alleys: it’s only historians of science who know about Blondlot’s *N*-rays. So we don’t get a representative sample. However it took mere decades to rumble *N*-rays and Set theory has lasted longer than that. Mistakes get detected on shorter timescales …]

# Preface

The theme of this book is the natural typing of Mathematics. I set out to examine how it arises in part from a (generally unacknowledged) operationalist view of Mathematics; I examine how that operationalism is connected to Category theory and the ideas of *abstract datatype* in Computer Science, how the operationalism is in conflict with foundationalism; and how the typing arises partly from the need to respect implementation-insensitivity (as I wrote about in [36]). I also examine how the strong typing is connected to the notion of typing to be seen in Russell-and-Whitehead and in Quine [92]. Given their apparently completely unconnected roots it is remarkable how well these two ideas of typing sit together; this fact deserves explanation and I attempt to supply it.

In many ways this book is a sequel to [36]: it draws on the ideas and results of that book, which readers of this volume will find useful. Nevertheless it is not just a spin-off/merchandising device to get the remaining copies of [36] to sell before they get pulped. Nor is it the sequel I had envisaged writing at the time I wrote [36]; that sequel would have contained applications of the theory of interpretations to less tractable (because less mathematical) subject matter, and would have had to include a treatment of the recent and exciting work on the various flavours of interpretation, biinterpretation, synonymy etc  $\llbracket$ ,  $\llbracket$ ,  $\llbracket \dots$

This book has grown out of various talks, some of them invited: “*Is Mathematics Stratified?*” was the title of my last-minute talk at BLC in 2004 (an invited speaker dropped out) and eventually became chapter 10; “*The two roots of type distinctions*” was written for a meeting of the *Groupe de Contact, Algèbre et Logique* at the ULB in December 2007 and eventually became chapter 12 after a pupal stage as [39].

It is a pleasure to have the opportunity of thanking colleagues and correspondents and the odd seminar participant . . . Adrian Mathias, Graham White, Randall Holmes, Mohan Ganesalingam, Peter Johnstone, Marcel Crabbé, Martin Hyland, Robin Milner, Dana Scott, Piers Bursill-Hall . . .

I have benefitted enormously from a visit Mark Wilson made to the department at Canterbury as an Erskine fellow. He opened my eyes to the 19th century literature on the legitimation of the *free creativity* of mathematics and the *extension element problem*. (points at infinity, Kummer’s ideals and so on.)

Then—and this is the real point of the book—I discuss several phenomena in mathematics where *both* these typing disciplines seem to have something useful to say, and examine in each case how the two analyses interact/overlap

etc. This involves a treatment of Burali-Forti, Paris-Harrington and a few other titbits.

This is a book that could only have been written by an NFiste. The path from philosophy into mathematics that i found myself on was not one that i sought. But i am not complaining. I have found the accident of having NF as a focus to my studies to be an incredibly fruitful experience. Having as a focus to one's studies a problem that is essentially foundational has the effect that one is being introduced all the time to issues which those foundations touch, and means that one is being reminded regularly of issues that many other wish to—and can afford to—sweep under the carpet. It has forced me to learn algebra, model theory, combinatorics, Logic, and type theory.

## Chapter 5

# Introduction: Background and Definitions

### 5.1 Definitions

Structures are denoted by letters in upper case  $\mathfrak{FRAKTUR}$  font with their carrier sets denoted by the same letter in capital Roman font.

$\mathcal{L}(\in, =)$  is the language of set theory.

$\iota$  is the singleton function.

$f \upharpoonright x$  is  $\{\langle a, b \rangle : a \in x \wedge f(b) = a\}$

$\langle x, y \rangle$  is the ordered pair of  $x$  and  $y$ . (implemented or unimplemented?)

The Wiener-Kuratowski ordered pair of  $x$  and  $y$  is the set  $\{\{x\}, \{x, y\}\}$ .

$j(f)$  is the function that sends  $x$  to  $f''x$  (which is  $\{f(y) : y \in x\}$ )

$X \rightarrow Y$  is the set of partial functions from  $X$  to  $Y$ .

$[n, m]$  is the set  $\{x \in \mathbb{N} : n \leq x \leq m\}$

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

We will write abstract (unimplemented) functions in `verbatim` font in the style `cardinal-of`, `ordinal-of`, `natural-number-of`.... Precisely which functions from sets-to-sets these things correspond to will depend on how we have implemented the relevant mathematical entities as sets: ‘`cardinal-of`’ ‘`ordinal-of`’, ‘`natural-number-of`’ etc. are not part of the language of set theory.

When working within set theory we write ‘ $|x|$ ’ for the cardinality of  $x$ . Beth numbers.  $\beth_0 = \aleph_0$ ;  $\beth_{\alpha+1} := 2^{\beth_\alpha}$ . Thus  $\beth_1$  is the cardinality of the continuum.

‘ $NC$ ’ denotes the collection of all cardinals, implemented somehow. (This is a Rosserism, from [98].) In the axiomatic set theory of that work  $NC$  is actually a set. There is no assumption in this work that  $NC$  is a set, so the use of ‘ $NC$ ’ to denote the collection is not intended to create that impression. The phrase is being borrowed from [98] simply because we need a notation for this collection, and the Z/ZF(C) tradition does not supply one. It may be worth noting that

it is not only in NF that  $NC$  can actually be a set. We shall establish this on p. 110.

A formula of set theory is *stratifiable* iff by assigning type subscripts to its variables we can turn it into a wff of simple type theory. That is to say, a wff  $\phi$  is stratifiable iff we can find a *stratification assignment* (henceforth “stratification” for short) for it, namely a map  $f$  from its variables (after relettering where appropriate) to  $\mathbb{N}$  such that if the atomic wff ‘ $x = y$ ’ occurs in  $\phi$  then  $f('x') = f('y')$ , and if ‘ $x \in y$ ’ occurs in  $\phi$  then  $f('y') = f('x') + 1$ . Variables receiving the same integer in a stratification are said to be of the same *type*. If  $n$  successive integers are used, the formula is said to be  $n$ -stratified. There is a notion of a *canonical stratification* which assigns each variable the lowest possible type and the integer they receive is its *type*. We write ‘*type-of* ‘ $x$ ’ for the concrete natural number given to the variable ‘ $x$ ’ by the canonical stratification. A formula with one free variable, and that being assigned type  $n$  in the canonical stratification, is an  $n$ -formula. If  $\Phi$  and  $\Psi$  are two closed stratifiable formulæ, then we can assign integers to their variables independently, and so the canonical stratification for ‘ $\Psi \wedge \Phi$ ’ will be that function whose restrictions to the two sets of variables are the two canonical stratifications. If  $\phi$  is a stratifiable formula then ‘ $\#\phi$ ’ is that expression in the language of type theory which is the result of incorporating into  $\phi$  as type subscripts the integers used in the canonical stratification.

A *function* is said to be stratified iff it is represented by a stratifiable expression  $\phi$  such that  $\forall x_1 \dots x_n \exists! y \phi$ . This idea is less natural than one might think, for the class of stratified functions is not closed under composition: singleton and binary union are both stratified, but their composition,  $x \mapsto x \cup \{x\}$ , is not. The largest class of stratified functions of unbounded arity closed under composition is the class of **homogeneous** functions, and the smallest class of functions closed under composition and containing all stratified functions is the class of **weakly stratified** functions. It is simple to check that a function is homogeneous iff there is a stratifiable expression  $\phi$  such that  $\forall x_1 \dots x_n \exists! y \phi$  wherein all the  $\vec{x}$  and  $y$  have the same type, and equally simple to check that a function is weakly stratified iff there is an expression  $\phi$  such that  $\forall x_1 \dots x_n \exists! y \phi$  wherein such failures of stratification as there may be involve the  $\vec{x}$  only. Thus we can apply the adjectives ‘homogeneous’ and ‘weakly stratified’ to formulæ as well as to functions. The **height** of a word  $W$  will be the number of types needed to stratify it.

The significance of this notion of homogeneity is that if  $F$  is a homogeneous relation then NF proves that the graph of  $F$  is a set. Further the closure of a set under a homogeneous function is also a set. Mere stratification is not sufficient. Thus the graph of  $\in$  (which is stratified but inhomogeneous) is not a set, and the transitive closure of a set (its closure under  $\in$ ) is not always a set. Nevertheless the “skewed-membership” relation  $\{\langle\{x\}, y\rangle : x \in y\}$  is a set, and it contains in some sense the “same” information. This phenomenon of sethood of “skewed” versions of things that cannot themselves be sets is frequently encountered, and is very important.

If  $\Phi(\vec{x})$  is a formula where such failures of stratification as there are involve

the free variables only—that is to say, we can give subscripts to the bound variables which obey the stratification rules—then  $\Phi$  is said to be *weakly stratified*. Weakly stratifiable formulae are obtained from stratifiable formulae by identifying free variables.

A  $\kappa$ -like total order is a total order of a set of size  $\kappa$  all of whose proper initial segments are of size  $< \kappa$ .

But we've already defined  
“weakly stratified”

### 5.1.1 Some simple theories of sets

An essential reference here is Mathias' magisterial [82].

**DEFINITION 1** *KF, TST, Mac, NF, NFU, CUS*

‘ZF’ will denote Zermelo-Fraenkel set theory without choice or foundation; ZFC is ZF with choice, ZF + foundation is ZF with foundation and ZFC + foundation is ZFC with foundation. Analogously ‘Z’ will denote Zermelo set theory without choice or foundation; ZC is Z with choice, Z + foundation is Z with foundation and ZC + foundation is ZC with foundation.

TCl (“transitive closure”) is the axiom that says that every set has a transitive closure; TCo (“transitive containment”) is the axiom that says that every set has a transitive superset.

$\text{str}(\text{ZF})$  is the theory axiomatised by the stratifiable axioms of ZF; similarly  $\text{str}(\text{ZFC})$ .

CUS is...

### 5.1.2 Notation for Ramsey theory

When  $x$  is a finite set of naturals  $\text{butlast}(x)$  is  $x$  shorn of its top element.

$[X]^n$  is the set of unordered  $n$ -tuples from  $X$ .

$\alpha \rightarrow (\beta)^\gamma_\delta$  says: take a set  $A$  of size  $\alpha$ , partition the unordered  $\gamma$ -tuples of it into  $\delta$  bits. Then there is a subset  $B \subseteq A$  of size  $\beta$  such that all the unordered  $\gamma$ -tuples from it are in the same piece of the partition.

Ramsey's theorem asserts that  $(\forall nmk \in \mathbb{N})(\exists l \in \mathbb{N})((l \rightarrow (k)_m^n)$ .

For Paris-Harrington we will also need the notion of a **relatively large** subset of  $\mathbb{N}$ :  $x \subseteq \mathbb{N}$  is **relatively large** if  $|x| > \min(x)$ .

We speak of set ‘monochromatic’ for a colouring rather than ‘homogeneous’ because we have already used ‘homogeneous’ to describe stratifiable formulae all of whose free variables are of the same type.

### 5.1.3 Set-theoretic Existence Principles

From NF studies we have the concept of a *setlike* function.<sup>1</sup>

Copy back to here the later definition of setlike ...

---

<sup>1</sup>The idea (tho' not the terminology) goes back to [23]. Coret uses the word ‘admissible’. We need a more specific word for it, since it is going to be re-used... and ‘admissible’ is already overloaded.

## B I G G A P H E R E

(ii) Is the definition of ‘setlike’ for a partial function satisfactory? Or do we need to say something like: “ $f$  is setlike iff it has an extension to a total function  $V \rightarrow V$  which is setlike”.  $T$  is a setlike function in the universe  $NC, \mathcal{P}(NC), \mathcal{P}^2(NC) \dots$  but considered as a partial function  $V \rightarrow V$  it might not be.

Setlike relations?

Supply all the details here

In set theories with a universal set there is the possibility of some permutations being not merely *setlike*, but being actually *sets*. Naturally Rieger-Bernays constructions that use permutations-that-are-actually-sets preserve stratifiable sentences as before, but they also preserve all members of a slightly larger class and again there is a converse due to Holmes, see [44] theorem 5.5. That theorem applies only to NFU + AC. There must be a similar result for NF, and there is something about it in one of my books.

Is there a notion of “setlike” applicable to theories that are not set theories? Is the nice primitive recursive pairing-and-unpairing gadget for natural numbers setlike in some sense? Yes, in the sense that if  $X$  and  $Y$  are (semi-)decidable subsets of  $\mathbb{N}$ , then  $X \times Y$ ,  $\text{fst}^{\text{“}}X\text{“}$  and  $\text{snd}^{\text{“}}Y\text{“}$  are also (semi-)decidable.

We shall see that in NF the singleton function  $\iota : x \mapsto \{x\}$  is setlike but is not locally a set:  $\iota \upharpoonright x$  is a set only if  $x$  is rather special. We say of such an  $x$  that it is *strongly cantorian*.

Composition of setlike functions is setlike. But what about functions with more than one argument? The function that takes two arguments  $x$  and  $y$  and returns  $x \cup \{y\}$  seems to be 1-setlike:  $\{x \cup \{y\} : x \in X \wedge y \in Y\}$  is always a set in KF (check it!) but  $\{x \cup \{x\} : x \in X\}$  is not. So we have to check on the process of identifying arguments.

**LEMMA 1**

1.  $\bigcup$  is setlike.

2.  $\mathcal{P}$  is setlike:

3.  $\iota$  is setlike:

*Proof:*

1.  $\{\bigcup y : y \in x\} \subseteq \mathcal{P}(\bigcup \bigcup x)$ ; In fact i think  $(j^n \bigcup)(x) \subseteq \mathcal{P}^n(\bigcup^{n+1} x)$ .

2.  $(j^n \mathcal{P})(x) \subseteq \mathcal{P}^n(\bigcup^{(n-1)}(x))$ .

3.  $j^n(\iota)(x) \subseteq \mathcal{P}^n(\bigcup^{(n-1)} x)$ .

In fact

in NF any total function defined by a *stratifiable* expression is setlike; if it is defined by a *homogeneous* expression it is locally a set.

Have to be careful in KF because we don’t have any replacement.

### 5.1.4 Pairing

We can define  $\mathbb{N}$  to be the intersection of all sets containing the singleton of the empty set and closed under  $\text{succ}$  where  $\text{succ}(x) =: \{w : (\exists y \in w)(w \setminus \{y\} \in x)\}$ . In doing this we make no use of pairing functions.

Similarly we can define the set of inductively finite sets as the intersection of all sets containing  $\emptyset$  and closed under unions with singletons:

$$FIN = \bigcap \{X : \emptyset \in X \wedge (\forall y)(\forall x \in X)(x \cup \{y\} \in X)\}$$

It is a simple matter to check that

**LEMMA 2**  $(\forall x)(x \in FIN \longleftrightarrow (\exists n \in \mathbb{N})(x \in n))$ .

Since the members of  $\mathbb{N}$  can be proved to be pairwise disjoint each member  $x$  of  $FIN$  belongs to a *unique*  $n \in \mathbb{N}$  and we write this  $n$  as ' $|x|$ ' and speak of it as *the cardinal of*  $x$ . We do this by induction on  $FIN$ . It remains to be established that we have a genuine implementation of the arithmetic of natural numbers here—that is to say that  $|x| = |y|$  iff  $x$  and  $y$  are equinumerous—but this cannot be done until we have found a way of expressing equinumerosity in the language of pure set theory, and to do that we will need to implement pairing+unpairing.

Now suppose we have two functions  $f$  and  $g$ , both total, whose ranges are complements:

$$\begin{aligned} &(\forall x \exists ! y)(y = f(x)); \\ &(\forall x \exists ! y)(y = g(x)); \\ &f `` V = V \setminus g `` V. \end{aligned}$$

Given such an  $f$  and  $g$ , any set  $z \subseteq V$  is  $f `` x \cup g `` y$  for unique  $x$  and  $y$ , so we can take  $z$  to be the ordered pair of  $x$  and  $y$ . However we wish to do this in a noncircular way, without using ordered pairs!

To do this we define two functions:

$$\begin{aligned} \theta_1(x) &:= \{\text{succ}(n) : n \in x \wedge n \in \mathbb{N}\} \cup (X \setminus \mathbb{N}); \\ \theta_2(x) &:= \theta_1(x) \cup \{0\}. \end{aligned}$$

Observe that we can define  $\theta_1 `` x \cup \theta_2 `` y$  with a set abstract that makes no use of pairing functions.

Note that, although this pairing function was discovered by Quine and is also the usual pairing function used in NF, there is no deep connection between these two facts: Quine pairs can be used in any axiomatic set theory that has the axiom of infinity (see [99]), and Wiener-Kuratowski pairs are perfectly serviceable in NF.

We will be making much use of the fact that if  $\theta_1$  and  $\theta_2$  are definable and homogeneous then the expression

$$x = \langle y, z \rangle$$

(where ' $\langle \rangle$ ' is the Quine pair) is homogeneous.

This definition is not constructively robust. Could change it

Allen Hazen calls this *ad-junction*.

Much more detail needed

Do we need to write this out ...?

**REMARK 1** (*Zermelo set theory*)

If  $\theta_1$  and  $\theta_2$  are setlike then Quine pairing and unpairing are setlike.

*Proof:*

To obtain  $X \times Y$ , proceed as follows. For each  $y \in Y$  consider the function  $I_y : x \mapsto \theta_1 ``x \cup \theta_2 ``y$ . The function  $x \mapsto \theta_1 ``x$  (which is  $j(\theta_1)$ ) is setlike because  $\theta_1$  is. For any set  $b$  the function  $a \mapsto a \cup b$  is setlike (check it!) so in particular the function  $a \mapsto a \cup \theta_2 ``y$  is setlike, and the composition of setlike functions is setlike. So  $I_y$  is setlike.

Then we have to show that the function  $I_X : y \mapsto I_y ``X$  is setlike. But this is just  $j(I_y)$ .  $\bigcup$  is setlike by lemma 1 and the composition of two setlike functions is setlike so the function  $x, y \mapsto \bigcup(I_X ``Y)$  is setlike. But this is precisely the pairing function. ■

We will have to show that the two unpairing functions are setlike too.  $x \mapsto x \cap B(0)$  and  $x \mapsto x \setminus B(0)$  are both setlike. We want  $\theta_1^{-1}$  and  $\theta_2^{-1}$  to be setlike.

Supply the missing details

## Chapter 6

# Mathematical Objects arising from Equivalence Relations, and their Implementation in Quine's NF

### ABSTRACT

Interpretations and synonymy. (Cardinals don't have to be Frege cardinals. Other implementations possible.) Big sets in NF and CUS. [equivalence classes only for LOW sets in CUS(\*)] Versions of CUS are going to be synonymous with versions of ZF. Point the way with [65]. NF a bit stronger and a bit more mysterious. Stratification. The Iteration Problem for stratification echoes (\*) above. Also with CUS. Burali-Forti in NF.

### Introduction

To implement a suite of mathematical objects into set theory is to provide an interpretation  $\mathcal{I}$  from a theory  $T$  of those objects into a theory  $T'$  of set theory. In recent years logicians have become increasingly interested in studying interpretations between theories as mathematical objects in their own right<sup>1</sup>, not least for what they can tell us about relations between theories.

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<sup>1</sup>My Doktorvater Adrian Mathias used to say that a logician is that kind of mathematician that thinks that a formula is a mathematical object.

## NF

The brief the organisers gave me specifically mentioned NF and natural numbers, and the two are joined in this context because of the possibility held out by NF (being a set theory with a universal set) of implementing natural numbers in a—natural!—way as equipollence classes.

This is a fruitful restriction, but a restriction nonetheless, and in several ways: NF is not the only set theory that allows a universal set, since there are also CUS and Positive Set Theory. CUS (see [19]) and its variants resemble NF in allowing large sets and in allowing the implementation of cardinals and suchlike as equivalence classes. At present it is still unclear whether or not NF is consistent<sup>2</sup>, but we do at least know that NFU is consistent, and it does all the good things NF does about large sets, thereby facilitating the implementation of mathematical objects as equivalence classes.

The second restriction is to entities arising from equivalence relations. It is worth noting *en passant* that not all entities in need of implementation arise from equivalence relations. Typically, a formal theory—of widgets, as it might be—will need to be able to quantify over ordered pairs of widgets. And there is no obvious way of thinking of ordered pairs as equivalence classes—certainly not as equivalence classes of thing that they are pairs of.

The third restriction is that even those mathematical entities that do arise from equivalence relations do not have to be implemented as equivalence classes, or even as restrictions of them.

Many mathematical entities arise from equivalence relations, and it is natural to feel that such entities should in principle be implementable in set theory as equivalence classes. Obvious examples are cardinals (equivalence classes under equipollence), ordinals (isomorphism classes of wellorderings); an (abstract) group arises from an isomorphism class of concrete groups; points-at-infinity arise from the equivalence relation “parallel-to” on lines. One could go on. As mentioned above, NF set theory has the pleasing feature that most mathematical entities arising from equivalence classes can in fact be implemented in NF straightforwardly as those equivalence classes. In theories with unrestricted separation (such as Zermelo and Zermelo-Fraenkel) the equivalence classes cannot be sets because they would lead to a universal set and thence—by unrestricted separation<sup>3</sup>—to the Russell Class. Things work in NF because NF does not have unrestricted separation (it has separation only for stratifiable formulæ) but it does have comprehension for stratifiable formulæ—which Zermelo and ZF do not. NF thus lacks *unstratified* separation, but does have stratified comprehension. Swings and roundabouts.

NF is now axiomatised by Extensionality plus a comprehension scheme for stratifiable expressions:  $\{x : \phi(x, \vec{t})\}$  (parameters are allowed) is a set. It cannot be emphasised too loud or too often that NF is a one-sorted set theory: the variables in the formulæ do not have types; they are given types only while one

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<sup>2</sup>though the situation may well have changed by the time this document goes to the printer.

<sup>3</sup>Separation is the principle that every subcollection of a set is a set.

is checking their host formula to see whether or not it is stratifiable (tho' one can—and does—say that, in ' $x \in y$ ', ' $x$ ' has a type one lower than the type of ' $y$ '). At no stage do the sets of which NF talks to us ever have types.

As the reader can see, it is easy enough to explain what the stratification condition on formulæ *is*, but it's rather more difficult to explain what it *means* deep down. It certainly *looks* like a mere syntactic trick, but there is a completeness theorem that provides a semantic motivation: it turns out that stratifiable expressions are precisely those preserved by all applications of a construction known as *Rieger-Bernays* permutation models<sup>4</sup>. That much is fairly straightforward. It also turns out—more significantly but also more obscurely—that all (or nearly all) mathematical notions can be implemented into set theory in such a way that the relations-between-sets that implement the relations-between-the-original-mathematical entities get captured by stratifiable expressions—and those that can't are in some interesting but obscure sense special. It's not entirely clear how to express this fact, it's less clear what its significance is, and even less clear still whether these two facts (the completeness theorem and the implementability) make stratification an appropriate criterion for set existence. After all, the fact that stratification is important and natural doesn't obviously make it the correct criterion for a formula to have an extension, for its real significance may lie elsewhere.

Stratification arises fairly straightforwardly from a syntactic device of Russell-and-Whitehead that is designed to forestall Russell's paradox. However there is also an endogenous typing in Mathematics which on the face of it has nothing to do with set theory. It found its way from mathematics into the theory of programming languages, where it is commonplace to have a typing discipline that distinguishes **naturals** from **floating-point reals**, and both of them from **booleans** and **arrays**. The striking fact is how often these two notions of typing coincide in practice and how well they fit.

What this neat fit means for NF studies is that, for any naturally occurring kind of mathematical entity (at least those which arise from equivalence classes) there will be an NF-implementation such that

- (i) the entities themselves (the tokens) are implemented as sets;
- (ii) the collection of all of the tokens is implemented as a set, and
- (iii) (at least some of) the natural operations on them are captured by homogeneous formulæ and their graphs implemented as sets.

The weasel words in brackets in (iii) are needed because of complications with quotients; for example: " $G_3$  is the quotient of  $G_1$  over its normal subgroup  $G_2$ " is stratifiable but not homogeneous.

In particular the three expressions in the language of set theory that say

- (a) " $x$  and  $y$  are equinumerous";

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<sup>4</sup>This is very much in the spirit of the results in classical model theory that say things like: universal sentences are precisely those preserved under substructure; universal existential sentences are precisely those preserved under colimits.

- (b) “ $x$  is the set of all things equinumerous with  $y$ ”; and
- (c) “ $x$  is an equinumerosity class”

are all stratifiable—and (a) is homogeneous. The fact that (a) is homogeneous implies that—in NF—the graph of the equinumerosity relation is a set; the fact that (b) is stratifiable implies that every (Frege) cardinal is a set (i.e., every set has a Frege cardinal and that cardinal is a set) and the fact that (c) is stratifiable implies that the collection of all (Frege) cardinals is a set. Observe that ‘ $y = |x|$ ’ (“ $y$  is the cardinal of  $x$ ”) is stratifiable. However the type of ‘ $x$ ’ is one less than the type of ‘ $y$ ’, so ‘ $y = |x|$ ’ is not homogeneous. This echoes the fact that if  $y$  is the cardinal of  $x$  then  $x$  and  $y$  are objects (*prima facie*) of different abstract data types. In contrast “ $x$  and  $y$  are cardinals and  $x < y$ ” is homogeneous. It turns out (I shall spare the reader the technical details) that all the usual operations on natural numbers turn out to be homogeneous.

## 6.1 The Recurrence Problem

Despite what was said above about how Mathematics can be implemented into set theory in a stratifiable way, complications can arise if it happens that the flavour of mathematical entity whose implementation we are discussing (as it might be *widgets*) has the feature that the family of isomorphism classes of widgets supports a widget structure. This widget structure of the family of widgets can result in stratification conflicts. There are many examples of this kind of thing, and Feferman writes about them in [30] (tho’ not in terms of stratification conflicts). The collection of all categories supports a category structure; the family of all [isomorphism classes of] semigroups supports a semigroup structure, namely direct product; the family of wellfounded binary structures supports a wellfounded relation, namely end-extension; and so on. In [30] this phenomenon is mentioned as a motivation for developing a foundation in which one can talk about universal objects: the set of *all* groups, of *all* cardinals, and so on. The best and simplest illustration of what one might call the *recurrence problem* comes from cardinal arithmetic—specifically the arithmetic of  $\mathbb{N}$ , the natural numbers.

Natural numbers count [finite multiplicities/collections of] concrete objects—at any rate in the first instance it’s concrete objects that they count. But once natural numbers have appeared, we have the possibility of counting [finite multiplicities/collections of] *natural numbers*. Can we count these new collections with the same natural numbers used to count concrete objects? Or does that counting require a novel abstract data type of a new kind of number?<sup>5</sup> The answer will depend on how squeamish you are. Computer scientists are used to a situation where two distinct types of *widget* and *wombat* will give rise to the two different types of *widget-list* and *wombat-list*; we say of the constructor

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<sup>5</sup>Sheep farmers in the Pennines, when counting sheep, use distinctive numerals (which are in fact *p*-celtic words) instead of the ordinary english words they use for counting everything else. I have not been able to find a linguistics literature on the possibility of using different suites of counting-words for objects of different flavours.

`list` that it is *polymorphic*. However the output of the `length` function applied to lists is always taken to be *monomorphic*: the same kind of number is used to measure both the length of lists-of-widgets and the length of lists-of-wombats. The polymorphism of lists is lost in the progression to natural numbers. Most of us are happy with monomorphic natural numbers. It seems natural enough: two multiplicities have the same cardinal iff there is a bijection between them, and there seems to be no reason why two multiplicities of different types should not have a bijection between them. And, indeed, the natural numbers of NF are monomorphic. However there is a ghost of the primordial polymorphism still tangible in NF. The cardinal number of  $\{m : m < n\}$  (where  $n$  is a given natural number) is a perfectly ordinary natural. So there is a function sending the natural number  $n$  to the natural number  $|\{m \in \mathbb{N} : m < n\}|$ . Specker called this function  $T$ :  $Tn = |\{m : m < n\}|$  for  $n$  a natural number. ‘ $m = Tn$ ’ is stratifiable but inhomogeneous, and the graph of  $T$  is not provably a set. Rosser’s axiom of counting from [98] says—in NF—that this function is the identity, and we now know that Rosser’s counting principle is not a theorem of NF. Outside NF it says that among the sets belonging to a natural number  $n$  is the set  $\{m : m < n\}$  of natural numbers smaller than  $n$ .

## 6.2 Typing and Counting

In the garden of Eden there is equinumerosity but there are no cardinals. We can say that two sets over here are the same size, and that two sets over there are the same size. Can I say that these two sets over here are the same size as those two over there without taking one of them over there to check? We can as long as we have a way of saying what that cardinal “is”, perhaps by identifying it as the cardinal of a particular set that somehow stands out from the crowd, because we can then appeal to transitivity. If there is some distinctive flavour of set such that no two distinct sets of that flavour are equinumerous, then one can uniquely identify a given cardinal by saying that it is the cardinal of this particular set of that distinctive flavour. Thus it comes to pass that, out in the real world, determining a cardinality for a set involves bijecting that set with a set of known cardinality, a *canonical* set, one might say.

That’s when the trouble starts: how do you know which canonical set to try to biject your candidate with? After all, if you start trying to biject your candidate with the wrong canonical set you might not learn the exact cardinality of your candidate, but instead learn only whether its size is greater or less than that of the canonical set you happened to use. If you guess the wrong canonical set you have to try again with another—bigger or smaller—one.

The solution to this is to have a kit of canonical sets that *cohere*—that are *nested*, in the sense that the ordering of the canonical sets by size is the same as the ordering of them by  $\subseteq$ —set inclusion. That is to say, if  $X$  is a canonical set, then the next canonical set is  $X \cup \{x\}$ , for some  $x$ . (We call this the result of adjoining  $x$  to  $X$ .) Thus, when one is building a bijection between a candidate and a canonical set, at any stage when one has bijected part of the candidate

set with a canonical set  $X$ , one pairs the next member of the candidate with that  $x$  such  $X \cup \{x\}$  is the next canonical set. The effect of this is that, when one is trying to ascertain the cardinality of a finite set, one builds a bijection between the candidate and the union of the canonical sets, and records when the candidate runs out of members to be counted—for that is the stage at which one has a bijection between the candidate and one of the canonical sets... and the job is done. We recognise this as the process of *counting*.

Let us now think a bit about the union  $\mathcal{U}$  of all the canonical sets. It may be finite or infinite—we don’t know which in general—but there are two things that we do know. (i)  $\mathcal{U}$  must come equipped with a wellordering, so that at each step in the counting process one pairs off a member of the candidate with the next member of  $\mathcal{U}$ . (ii) If our natural numbers are polymorphic, then the members of  $\mathcal{U}$  must be of the same type as the members of the candidate. This is because each canonical set has the same cardinal as any candidate set with which it is in bijection (that is what it is for, after all) and that cardinal has a type. That is to say, the [extension of the] type must be a *counted* set or—one might say, thinking about type—that it must be a *recursive* datatype. And we can say a bit more: one naturally expects that, when one adjoins an  $x$  to a canonical set  $X$  to obtain the next canonical set  $X \cup \{x\}$ , the  $x$  is obtained from the  $X$  by means of the constructors of the datatype.

(A datatype is recursive iff its extension is a counted set, indeed a *recursively enumerable* set))

However when we pick members of the candidate in building our bijection we can do it nondeterministically. Indeed we might have to do it nondeterministically, since the candidate might not be downward closed under the engendering relation. (That is to say if these are *cardinals* of sets, rather than *ordinals* of lists).

However, in the psychological literature it is always taken as read that the initial segments used to count finite sets are one and all initial segments of  $\mathbb{N}$ . That is to say, we give  $\mathbb{N}$  the job of being  $\mathcal{U}$ : to ascertain the cardinality of a candidate finite set one enumerates it with natural numbers, and once one runs out of members of the candidate the last natural number one has used is the cardinality of the candidate. For this to work it is neccessary that the cardinals that count sets of natural numbers should be the same type as the cardinals that count sets of widgets or dingbats or whatever ADT it is that the candidate is a set of.

Hmmm You can build a bijection between lists of different types... Actually if it is lists we’re giving numbers to then they are ordinals not cardinals, but it doesn’t make much difference.

There doesn’t seem to be any reason why you shouldn’t have a bijection between [multiplicities of] things of different types.

The following ML code is well-typed even if our naturals are polymorphic:

```
fun bij(l1,l2) = if null l1 then if null l2 then true
                  else false
                  else if null l2 then false
```

```
else bij(tl(l1),tl(l2))
```

The idea that—more generally—each ordinal counts the set of its predecessors seems to be in Cantor [16], indeed to be coeval with the idea of ordinal itself.

The recurrence problem turns up also in CUS. The Grand Scheme with CUS is to construct from a given model of ZF a new model with big sets while maintaining the wellfounded sets of the new model as a model of ZF—in fact an isomorphic copy of the original model. Sets (in the new model) that are the same size as wellfounded sets (in the new model) are said to be *low*. In Church’s original construction he provides a universal set. Sheridan [103] shows how to add the (graph of the) singleton function as a set. Church shows how, for more-or-less any equivalence relation  $\sim$ , to add  $\sim$ -equivalence classes *for low sets*. (Equivalence classes for non-low sets are not provided.) He explicitly does this for equipollence (obtaining cardinals—and also generalisations that he calls *j-cardinals*) and, altho’ he doesn’t say how to do it for arbitrary equivalence relations, it is clear that he knows how to, and that he could have done had he chosen to. There is some suggestion (for example at the end of [40]) that the theories satisfied by the models furnished by these constructions are synonymous with the theory of the model that is operated on. If correct, this would account for the comparative straightforwardness of the constructions that give us the universal set, the singleton function, the *j*-cardinals of low sets and so on, for that straightforwardness would be a reflection of the fact that these new entities are just pointless epiphenomena of no mathematical substance. But that is yet to be established.

However it is significant that the construction generally does not provide *global* equivalence classes, but merely equivalence classes of *low* sets. And the difficulty we experience in providing global equivalence classes seem to arise only where there is a recurrence problem. Why does recurrence cause a problem? Suppose the family of all equivalence classes of widgets supports a widget structure. One adds—by a Church-style coding trick—equivalence classes of the widgets in one’s field of view. But then in consequence of the Church construction there are now *new* widgets, and they might not all fit into the equivalence classes so far provided. So one repeats the construction... with potentially the same unsatisfactory result. There doesn’t seem to be any global argument that would tell us that this process should reach a fixed point. Thus it comes about that no known enhancement of Church’s construction gives us a model containing a set of all ordinals. NF alleges that there is such a set, and thus in effect assumes that the recurrence problem has been solved. No wonder the consistency problem for NF is so hard.

pointless epiphenomena with  
no mathematical content??  
Tim B probably has estab-  
lished it now.

### 6.2.1 Burali-Forti

The most notorious example of the recurrence problem is undoubtedly the Burali-Forti paradox. Recall that an ordinal is a mathematical object associated with a wellordering: every wellordering has an ordinal, and two wellorderings

have the same ordinal iff they are isomorphic. One can think of ordinals as equivalence classes of wellorderings (which is how they fall under the rubric of this essay) and that indeed is precisely how they are implemented in NF.

The Burali-Forti paradox arises because every set of ordinals is naturally wellordered by  $\langle O_n \rangle$ , the order relation on ordinals, and so must have an ordinal. Notorious it may be, but its notoriety has not resulted in it receiving close attention—rather in it being avoided and—in consequence—remaining poorly understood ... and certainly never mentioned in front of the children. The elephant in the drawing room? Wrong elephant.... It's more like the elephant in the old Indian trope about the five blind men. You have to approach it from more than one point of view if you are to have any hope of understanding what is going on.

In the hope of obtaining some enlightenment, and on the assumption that the reader is familiar with the ZF analysis of Burali-Forti, let me concentrate on an analysis from an NF perspective.

Let us take as our point of departure the idea that natural numbers are *prima facie* polymorphic—and indeed that ordinals, too, are *prima facie* polymorphic. That is to say, when `widget` and `gadget` are different types we not only distinguish (as we must) between the two types `widget-list` and `gadget-list`, we even distinguish between `length-of-widget-list` and `length-of-gadget-list`, even though both these types are flavours of natural number! Our point of departure is to behave like Pennine sheep-farmers. Our first move is to identify, to coalesce into one type all these types that look like natural numbers. In particular we are going to regard the numbers-that-count-sets of [whatever] as the same types as the numbers that-count-sets-of-numbers-that-count-sets-of-[whatever]. Our second move is to say further that, not only is  $|\{n : n < x\}|$  a number of the same type as  $x$ , but that it is actually *identical* to  $x$ . This is Rosser's Counting Principle. Initially at least we perform this coalescence only on types of natural numbers (= finite ordinals)—it does seem to be safe there. However we can contemplate (a third move of) extending it to the ordinals (I suppose one should then call it Rosser's *Extended Counting Principle*) and this is when the trouble starts.

I emphasised earlier that NF is a one-sorted theory: in particular it has only one type of ordinal. We can in any case prove from first principles that every set of ordinals (and there is now only one kind of ordinal) is wellordered by the comparative-magnitude relation for ordinals (which we can write ' $\langle O_n \rangle$ '), so in particular every initial segment  $S$  of the ordinals is wellordered by  $\langle O_n \rangle$ , and now Rosser's Extended Counting Principle will tell us—if we let it—that the order type of  $S$  is the least ordinal not in it.

NF tells us that the collection  $NO$  of all ordinals is a set, and is wellordered by  $\langle O_n \rangle$ . And its length? If Rosser's Extended Counting Principle were to be believed then the length would have to be the first ordinal not in  $NO$ , and there is of course no such thing. Clearly Rosser's Extended Counting Principle fails for at least some big ordinals.

What does this failure mean for NF? We must not forget that NF is a one-

sorted theory, so we cannot go back to our state-of-nature in which ordinals were polymorphic; the effect is rather that—sometimes—where we would expect to find a single ordinal we instead find a whole hall of mirrors of them. For example: *NO*-ordered-by- $<_{On}$  has an ordinal, which we shall call ‘ $\Omega_0$ ’. What about the ordering of the ordinals below  $\Omega_0$ ? Rosser’s Extended Counting Principle would lead us to expect that its order type should be  $\Omega_0$ , but it evidently cannot be. It’s a distinct ordinal, which I suppose we will have to call ‘ $\Omega_1$ ’ … and the ordering of the ordinals below  $\Omega_1$  is of order type  $\Omega_2$ , and so on. Morally of course all these  $\Omega_n$  belong to distinct abstract data types, but since NF is one-sorted they are one and all compelled to belong to the one *same* type, so NF can express this insight only in a veiled or muted way. Dana Scott wrote to me, when I was a Ph.D. student, that NF is really a type theory, and it must have been this that he had in mind.

There are echoes here of the axiom of reducibility, but that is too difficult a topic for a brief note such as this.



## Chapter 7

# Definitions contrasted with Implementations

I would like to start off by insisting on some terminology: what we try to do to cardinals, ordinals (and other mathematical entities) when we come to set theory is not to *define* them but to *implement* them. We don't need to *define* cardinals: we know perfectly well what a cardinal is: a cardinal is that thing that two sets have in common (*i.e.*, to which they are related in the same way) precisely when they are equinumerous. Inattention to the distinction between definition and implementation can result in absurdities. For example, with the von Neumann implementation of cardinals and ordinals into set theory it happens that the three distinct mathematical objects (i) the ordinal  $\omega$ , (ii) the set  $\mathbb{N}$  of natural numbers, and (iii) the cardinal number  $\aleph_0$  are all implemented as the same set. (The set itself has a purely set-theoretical characterisation as the set of wellfounded hereditarily transitive finite sets). These three mathematical objects are all distinct, and—however convenient it may be to implement them in set theory by the same sets—it would make no sense to attempt to *define* them to be the same. It's the same mistake as trying to define  $\pi$  to be  $22/7$ .

lifted from munich.tex

Blend into this text the material around page 51

Confusing definition with implementation does more than merely annoy pedants. It can—and all too often does—lead people into the error of thinking that they have proved something about a suite of entities (for example that the order relation on ordinals is wellfounded) when the only thing they have actually proved is the corresponding fact about the sets that implement those entities. I have even seen it claimed that, given that the order relation on (von Neumann) ordinals is  $\in$ , then the wellfoundedness of the order relation on ordinals follows easily from the axiom of foundation (which of course says that  $\in$  is wellfounded). By resolving to use the word ‘implement’ instead of ‘define’ we arm and protect ourself against such craziness and we set off on the right foot.

kunen

If you are suffering under an error of attachment to the concept of definition then you should feel yourself free to continue thinking that there is some definition going on, but it's not a definition of (as it might be) *cardinals*, but a

definition of an *implementation of cardinals*

The point is, if you think you are defining 3 and 5 rather than implementing them, you cannot appeal to implementation sensitivity/insensitivity to explain the difference between daft questions and sensible questions.

In connection with Suppes on definition i should track down the allusion of Belnap's (on p 134 of [?] Strawson (ed) Philosophical Logic (OUP) that there is no operation \* on rationals such that  $(a/b) * (c/d) = (a + c)/(b + d)$ ).

You have to be careful how to state that!

(See [3], [8], [9] (the chapter entitled “The General Theory of Definitions”), [56], [79] (see section 5 of the “Formalized Theories” chapter (ch. 11) and [118].))

Central to this book will be an appreciation that a lot of what in vulgar mathematical parlance is called *defining* is better thought of as *implementing*.<sup>1</sup> Defining something is a process of getting clear what that something is, the better to recognise it and make use of it. We define *groups*, *rings*, *fields* and so on. That is an example of the same activity as was known to (and characterised by) Aristotle. We define groups (rings, fields and so on) in the same sense as we define *mammal* or *fish*. That is to say we are recording our determination to use the terms ‘group’, ‘ring’ and so on to denote particular things which we can identify independently of our use of language. A definition is a record of a resolve to use language in a particular way, it is not a determination that two entities are the same entity. In contrast, what are we doing when we “define” ordered pairs? It’s not as if there are these things just lying around, like the way groups were just lying around, and we decide to give them this label. It’s more as if there were these things that we called ordered pairs, and we didn’t really know what they were, and now we *do* know what they are. But even that isn’t quite like it either. When we define acids to be electron acceptors we are recording a discovery we have made about phenomena: the discovery that the substances that burn holes in our labcoats do so because they accept electrons. But we are not *discovering* that the ordered pair of  $x$  and  $y$  is the set  $\{\{x\}, \{x, y\}\}$ . What we are doing is identifying in the world of sets a construct that behaves in the way an ordered pair is supposed to behave, and saying that in the game we are playing (where the stick in your hand is a magic wand and the twigs on my head are a crown) the set  $\{\{x\}, \{x, y\}\}$  is the ordered pair  $\langle x, y \rangle$ . We do not *define* ordered pairs; that is quite the wrong word: we *implement* them.

When we discover that acids are just electron acceptors we understand how it comes that they burn holes in labcoats. When we discover that natural numbers are wellfounded hereditarily transitive finite sets we don’t come to understand anything about  $\mathbb{N}$  that had hitherto been obscure.

Once we are clear about the difference between  
 • defining things (as in Aristotle);

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<sup>1</sup>The chapter on definition in [107] is strongly recommended.

- discovering that acids are just electron acceptors;
  - implementing ordered pairs as WK pairs
- ... we will be less likely to trip ourselves up.

One can make a Benacerraf-like point [11] to say that if you think you are defining ordered pairs then people who think  $\langle n, m \rangle$  is  $\{\{n\}, \{n, m\}\}$  and people who think it is  $(n+m)(n+m+1)/2 + m$  are disagreeing: at least one of them is wrong.

I went to a seminar led by Koji Tanaka on Hilbert's lectures on the foundations of geometry. Koji thought that from such a reading group he would learn something about the way in which the axiomatic method has evolved in the twentieth century. I was struck by the axiom of *Vollständigkeit*, or whatever it's called<sup>2</sup>—the one that enforces the least-upper bound property, second order completeness etc. Unger's translation:

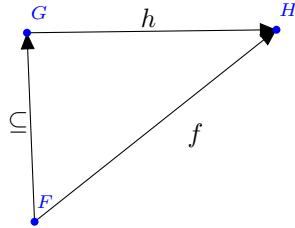
“Axiom of completeness. To a system of points, straight lines, and planes, it is impossible to add other elements in such a manner that the system thus generalized shall form a new geometry obeying all of the five groups of axioms. In other words, the elements of geometry form a system which is not susceptible of extension, if we regard the five groups of axioms as valid.”

This is not an axiom in the style we are used to nowadays. [in this connection one should definitely look at [49] which—according to Felgner—has an axiom of restriction in the same style.] Nowadays we have a notion of a formula being true in a structure. (This works even if the formula is higher-order). We then say that we have axiomatised widgets iff we have a set  $T$  of formulæ such that you are a widget iff every member of  $T$  is true in you. We call such a  $T$  an *axiomatisation of the theory of widgets*. There is a consequence of this which the tradition nowadays accepts without turning a hair: *there might not be any such set  $T$ !* Indeed there are standard examples of widgets for which we—demonstrably—cannot find such a set  $T$ . Here one has to be careful: the standard examples of unaxiomatisable theories (which are things like finite groups) are actually not examples of things that cannot be axiomatised *at all* but merely examples of theories that lack a first-order axiomatisation. The theory of finite groups has a higher-order axiomatisation. To get a theory that has no axiomatisation *at all* one has to go to things like *free groups*—and the proof that the theory of free groups is not axiomatisable is *hard*. Too hard for me!—i have no idea how the proof goes.

Let's take one of these examples: free groups, say. The fact that we cannot axiomatise the theory of free groups doesn't mean that we cannot *define* them. Manifestly we can. A Group  $G$  is free on a subset  $F \subseteq G$  if, for every group  $H$ , every map  $f : F \rightarrow H$  extends uniquely to a homomorphism  $h : G \rightarrow H$ .

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<sup>2</sup>Wikipedia calls it the *axiom of line completeness*.



We have a *definition* of free groups, but not a *theory* of free groups. And what is more, we are not confused by this contrast. Even if we cannot characterise free groups in terms of things that are true in them (or in them + their power + higher power sets ...) we can still characterise them precisely by talking about their interaction with other groups. As Patrick Girard said during the seminar, we are quite happy with the concept of *well-formed formula* which is not captured by axioms but by a definition.

My guess is that this distinction between axiomatising widgets and defining them—which comes so naturally and so clearly to us—was not evident to Hilbert at all. Quite when the difference became clear—and people started doing things the way we do them now—is something i don't know. But now that i can see that the distinction was not made by Hilbert's generation, a lot of things start to become clearer. Some of the writing on set theory in the 20's and 30's starts to make more sense to me. If you are trying to describe the universe of sets that you desire, the obvious answer to the question “which sets do you want?” is “All of them!” so you will present—as if it were an axiom—a maximality claim about the universe of sets that has the same flavour as the *Vollständigkeits Axiom*. Although modern set theorists still want to have as many sets as possible, they don't attempt to phrase axioms in this way because they know it can't be done. Earlier writers (i think of the rather unsympathetic review [42] i wrote of [32] the collected Finsler) used to.

## 7.1. IMPLEMENTATION AND DEFINITION [A STAND-ALONE ARTICLE IN WHICH I EXPLAIN WHY ONE HAS TO MAKE THIS DISTINCTION]

### 7.1 Implementation and Definition [A stand-alone article in which I explain why one has to make this distinction]

Ben Garling once said to me that expository/pædagogical problems often sit on top of the scars of old foundational problems.

Perhaps it is best to dive in head-first with an example.

I find on my shelves a variety of textbooks on mathematical logic offering an account of ordinals:

van Dalen Doets and de Swart [113] define an ordinal to be a transitive set of transitive sets;

Drake and Singh [28] say an ordinal is a transitive set linearly ordered by  $\in$ ;  
Drake [27] says an ordinal is a transitive set on which  $\in$  is a connected relation;  
Mendelson [88] 4th edition says an ordinal is a transitive set wellordered by  $\in$ ;  
Ciesielski [20] weaselly says (p 44) “A set  $\alpha$  is said to be an ordinal number if ...”

One could go on but it’s too depressing.

Mind you, Hajnal-Hamburger [58] is an honourable exception.

Why am i making such a fuss about these purported definitions? The best way to respond to this challenge is to contrast them with some definitions that are completely satisfactory. These “definitions” of ordinals do not explain anything. In contrast our definition of acid ...

Believing that ordinals are hereditarily etc etc is not like believing that acids are electron acceptors (and were all along). (One difference, as Paddy Blanchette points out to me, is that we and the Ancient Greeks have access to the same acids, the same *stuff*<sup>8</sup> that you can get your hands on). The idea that ordinals are abstract mathematical objects (as they were for Cantor, Hardy, Veblen, Mahlo ...) before the foundationalists got hold of the idea they were hereditarily ...) is not a hilarious misunderstanding like the phlogiston theory, it’s a perfectly sensible point of view which enabled them to do some genuine mathematics that we have retained unmodified. Was the decision that ordinals are hereditarily-transitive-sets-wellordered-by- $\in$  a discovery, like the discovery that acids are really electron acceptors, and like, too, the discovery [105] that (i really like this example) that EDRF is nitric oxide? Clearly not: the discovery that acids are electron acceptors explains all the phenomena that prompted us to invent the concept of acid in the first place; thinking of ordinals as hereditarily transitive sets doesn’t explain anything.

So how should these authors have set out their stall?

We can define von Neumann ordinals—that is straightforward. Actually let us call them ‘counter sets’ (as Quine does in [96]) even if only in order to leave

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<sup>8</sup>Well, perhaps not  $\text{CF}_3\text{SO}_2\cdot\text{OH}$  or  $\text{H}_2\text{FSbF}_6$  ... but you get the idea.

the word ‘ordinal’ unmolested *pro tem*, so that things that started off distinct can continue to be notated distinctly. A *counter set* is either  $x \cup \{x\}$  for  $x$  a counter set or is an arbitrary union of counter sets. Job done. Defining *ordinal* is a subtly different matter. When Quine was defining counter sets the expression ‘counter set’ was up for grabs, and he could use it to denote anything he chose. In contrast when we define ordinals we are in a situation much more like that with *acid*—there is a preexisting usage that we are trying to capture; we have some sort of idea what ordinals are: an ordinal is that object of which two wellorderings both partake iff they are isomorphic.

So: what is going on is something like the following:

- (i) We define ordinals;
- (ii) We define counter sets;
- (iii) Then we implement ordinals as counter sets.

Let’s consider these in order.

(i) The attempt to define ordinals as that-which-pairwise-isomorphic-wellorderings-all-partake-of might fall foul of some of the *desiderata* in chapter 8 of [107] (for example the non-creativity clause—how does one know that there *is* a thing that two isomorphic wellorders both partake of?)—but that is not a grave problem. Whether there really are such things is an abstract ontological question that perhaps has no answer, but it is at least consistent that there should be such things. However one can actually say slightly more. Whenever we have a theory  $T$  and a formula  $\sim$  of  $T$  with two free variables s.t.  $T$  proves that  $\sim$  is an equivalence relation then, the theory  $T'$  obtained by expanding  $\mathcal{L}(T)$  by adding a function letter ‘ $f$ ’ and adding an axiom  $(\forall xy)(f(x) = f(y) \longleftrightarrow x \sim y)$  is not only consistent relative to  $T$ , but is actually a conservative extension of it. This is a standard consequence of the completeness theorem for first order logic, well-understood even if not widely known outside the literature on the  $\epsilon$ -calculus. Adding  $\epsilon$ -terms gives a conservative extension, even when one adds *extensionality*:

$$(\forall \vec{y})((\epsilon x)F(x, \vec{y}) = (\epsilon x)G(x, \vec{y}) \longleftrightarrow (\forall x)(F(x, \vec{y}) \longleftrightarrow G(x, \vec{y})))$$

In fact this analysis using the  $\epsilon$ -calculus tells us that even stipulating the further condition that  $(\forall x)(f(x) \sim x)$  gives us a conservative extension.

(ii) is straightforward.

(iii) Then, when one is doing set theory and one wishes to implement all of mathematics in this theory, one can opt to implement ordinals as counter sets. Opting thus requires us to equip counter sets with a lot of operations so they are fit for their new rôle as implementations-of-ordinals. For a start, every wellordering must “have” an ordinal, (“has” in this setting means “is isomorphic to” since counter sets—once equipped with  $\subseteq$  and thought of as ordernestings—are wellorderings) and to establish that every wellordering “has” an ordinal in this sense one needs the axiom scheme of replacement (or at any rate enough of it to prove the Mostowski collapse lemma).

## 7.1. IMPLEMENTATION AND DEFINITION [A STAND-ALONE ARTICLE IN WHICH I EXPLAIN WHY ONE H

The rest of the story can be read in any textbook on set theory.

The foregoing might read oddly to a ZFiste. After all, in the study of wellfounded sets it matters a very great deal that the ordinals have the properties they do, that they play a central rôle in the definition of  $L^4$  that “ $\alpha$  is an ordinal” is absolute, and so on. [need more examples here]. To a ZFiste i may seem to be making pointless and naïve attempts to call into doubt some very delicate—but very secure—mathematics into which a lot of work has been put over many years (and by better mathematicians than me, one might add for good measure). But the fact that counter sets/Von Neumann ordinals—whatever you want to call them—have the central rôle in the study of wellfounded sets that they do *doesn't mean that those important objects are ordinals*; a lot of their importance derives from their peculiarly cute set-theoretic structure—and you don't have to be an ordinal to be important.

It might be worth making some observations about how cute it is. Blah ordernesting.

I am making a fuss about the failure to distinguish implementations from definitions because it matters; it can lead to the bizarre doctrine of *foundationalism* which—in one of its forms—is a belief that all mathematical objects are sets. The garden gate for the primrose path to foundationalism is the thought that it would be very nice to interpret all of mathematics into set theory. It would indeed, and for all the reasons usually given—a universal language for mathematics ensuring portability of ideas across different parts of mathematics and so on. (The case is often over-egged by the mistaken thought that one needs *foundations* in order to achieve *rigour* but the project is a good and useful one even without this extra hard sell. In any case this point is orthogonal to the case i am trying to make.)

So, one starts looking for *simulacra* within the world of sets for all the objects one encounters in mathematics, simulacra that will implement the objects of interest. (There is of course a question about which axiomatic set theory one is going to use for this project, but that question, too, is orthogonal to the case i am trying to make). Consider now the mental processes of a reader contemplating any of the definitions of the simulacra of ordinals given above [...]. The reader is not told that they are being given definitions of the simulacra that are to implement (in this case) ordinals; instead they are told that they are being given definitions of the ordinals themselves. If you think that what is being given you is a definition of ordinals you naturally think that you have thereby learnt what ordinals are. So what are they then? Sets of a certain kind. Thus the project to interpret all of mathematics into set theory becomes the error of thinking that all mathematical objects are sets. And this error will arise quite naturally from the project to implement mathematical objects as sets if one does not attend to the distinction between implementation and definition.

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<sup>4</sup>Smallest class closed under this and that and the other and containing all ordinals... not: all cardinals, or all groups, or all BFEXTS.

### 7.1.1 Stuff to fit in

If you think the challenge was to ascertain the true nature of (e.g.) ordinals then you may well think that von Neumann solved it, and you will find NF to be a tiresome late applicant for a post that has already been filled. And it's no good for an NFiste to say "you don't know what you're talking about co's you don't know the literature." Reading the literature won't help. [After all i wouldn't stand for the parallel retort from the dialetheists when i'm having a go at them: the dialetheist literature doesn't contain an explanation of why their errors aren't errors.]

I suppose it's not strictly fair to say that these things are not proofs *at all*; one would have to allow that they are proofs—or at least *parts of proofs*—as long as they are equipped with certificates that the implementations they reason about are suitably *faithful*.

You can't really believe that the wellfoundedness of  $\langle O_n \rangle$  is a consequence of the axiom of foundation. You'd have to believe that ordinary mathematical induction (on  $\mathbb{N}$ ) is likewise a consequence of the axiom of foundation. I always thought it was because  $\mathbb{N}$  is the  $\subseteq$ -least set containing 0 and closed under successor. Silly me.

If natural numbers really are sets, then it is to their nature as sets to which we must look for an explanation of their nature to obey mathematical induction. But their nature as sets is clearly no part of the explanation: we knew about natural numbers and induction long before we were onto their nature as sets. How different from acids: once we get the idea that acids are electron acceptors it is to their nature as electron acceptors that we look for an explanation of their ability to burn holes in carpets and—lo!—we find such an explanation. But beware! The crazy foundationalists might try saying that their account of naturals numbers as von Neumann ordinals does show how their compliance with induction arises from their nature as sets. How does one reply? One replies that they have justified a principle of  $\in$ -induction on (finite) counter sets.

duplication

Track down Frege's remark about how Hilbert's independence proof proves nothing about euclidean space. It may turn out to be the same point i'm trying to make about how proving things about *simulacra* proves nothing about the things being simulated. As in: in some models of ZF the simulacrum of  $\mathbb{R}$  is wordered; does that prove that  $\Diamond(\mathbb{R} \text{ can be wordered})$ ? (Always assuming that makes any sense!)

Maybe it's time to look again at Suppes and at what the Greeks had to say. Presumably Hilbert knew Aristotle on definition. Ed Mares says: read the appendix to [18].

Very striking that in Suppes' excellent Introduction to Logic [107] where in ch 10 he discusses ordered pairs, he never implements them.

Something of the flavour of aristotelian definition of species-within-a-genus remains in mathematics. A monad is a semigroup with a unit. A group is

## 7.1. IMPLEMENTATION AND DEFINITION [A STAND-ALONE ARTICLE IN WHICH I EXPLAIN WHY ONE H

a monad with an inverse. A field is a ... ADT declarations could fairly be described as definitions in the aristotelian sense.

### Aldo writes

Look at Belnap's [8] ("On rigorous definitions" Phil Studies **72** pp 115-46 early 1990's, don't remember the year exactly). That should give you a starting point. Also the Belnap and Gupta [9] book (The revision theory of truth – MIT Press) is relevant. They talk about truth but revision rules can be turned into a full-fledged general theory of definitions.

I wrote the entry "definition" for the Routledge Encycl of Phil, and that's the most recent stuff I could come up with (the entry itself, by the way, might give your student a starting point and pointers to the lit.)

Cheers,

- Aldo

### Allen Hazen writes

Dear Thomas

Nuel Belnap (the "relevance logician": in Philosophy Department, University of Pittsburgh) has written on definitions: I'm not sure how much of it is published. If I get a chance, I'll try to come up with bibliography. (His joint book with Anil Gupta, [9] has a chapter called "The General Theory of Definitions," which is largely about partial and circular definitions: this stuff is very interesting but may not be what you are interested in): the readable introduction is Gupta's [very Belnap-influenced] article "Remarks on definitions and the concept of truth," in "Proceedings of the Aristotelian Society" 89 (1988-1989), pp. 227-246.

For the classical case, there is a nice clean treatment of the definitions of predicates by universally quantified biconditionals and functions by universally quantified identities in Shoenfield.

Hope this is helpful (and I'll try for another Belnap reference later).

Be well,

Allen Hazen

### Charlie Silver writes

Thomas, I know what you mean about that '57 Suppes stuff on definitions. When I read it about a million years ago, I thought it was good too. Mates's \*Elementary Logic\* ([79] 2nd edition, 1973, Oxford Press, NY) has a bit on definitions, which he ties to formal theories, pp. 197–202. I can't locate my copy of Suppes at the moment, to compare the two, but from memory: Suppes says more about definition, but at a more elementary level. Mates covers a good deal of the same ground as Suppes, but at a slightly higher, more sophisticated level. (Mates wrote his book as an attempted improvement over Suppes'.) Mates's stuff leads nicely to conservative extensions of theories and can be followed by

## 92CHAPTER 7. DEFINITIONS CONTRASTED WITH IMPLEMENTATIONS

the conditions on conservative extensions in more advanced books, like Shoenfield. Unfortunately, Mates doesn't go into great detail. He just has the six and one half pages I cited earlier. (There's a typo on the second line of p. 202; " $\chi$  of  $T'$ " should be " $\chi$  of  $T$ ") You might also look up Beth's \*The Foundations of Mathematics\*, but his terminology is more old fashioned. As I recall, he uses tableaux, also. (There's some philosophical stuff on definitions in Kripke's Naming and Necessity, but you may not want to get into philosophical matters. Kripke's interesting, though.)

Good luck.

Charlie

There is a good survey article by Aldo Antonelli in the new Routledge Encyclopedia of Philosophy. There is also a worthwhile paper by Nuel Belnap—'On rigorous definition'—in Philosophical Studies 1993. Finally, a big part of Gupta and Belnap's work on truth is concerned with definition. You might find it useful to have a look at some of that (e.g. [9]). This should at least give you a start.

Best regards,

Mic Detlefsen

### Steve Yablo writes

Hi Thomas – remember me, the paradox without self reference guy? I don't know exactly where the student's interests lie but I can make two suggestions. One is [9]. Two is my own self, [118] "Definitions, Consistent and Inconsistent" in Philosophical Studies 1993. The first has a theory of definitions in the spirit of the Gupta/Herzberger revision theory of truth. The second tries to extend the series: explicit definition (" $x$  is  $F$  iff  $x$  is a dog"), positive circular or "inductive" definition (" $x$  is  $F$  iff  $x$  is 0 or the successor of an  $F$ "),....The next entry is negative circular definitions (" $x$  is  $F$  iff  $x$  is 0 or the successor of a non- $F$ "), and after that unconstrained circular definitions (" $x$  is  $F$  iff .... $Fx$ ...." where  $Fx$  can occur positively or negatively or both). Someone else who's worked on the logic of definitions is Jaako Hintikka; he's working more in a model-theoretic framework. Hope that isn't completely irrelevant to your needs!

Steve Yablo

Yep. Joseph Shoenfield, "Mathematical Logic," Reading MA (Addison-Wesley), 1967. Section 4.6, "Extensions by definitions," pp. 57–61.

If you want something to hit undergraduates with, I think Benson Mates's "Elementary Logic" (New York: Oxford U.P., first ed. 1965) has a decent discussion in section 5 of the "Formalized Theories" chapter (ch. 11), but I don't remember how it compares to the stuff in Suppes's textbook. (If you aren't familiar with it, the Mates book was written as a text for undergraduate philosophy students, back in the pre-Derrida days when they were expected to actually learn some logic: readable, well-written, and reliable.)

From `silver_1@mindspring.com` Wed Nov 15 13:39:06 2000

## 7.1. IMPLEMENTATION AND DEFINITION [A STAND-ALONE ARTICLE IN WHICH I EXPLAIN WHY ONE H

tf wrote

“Excellent! I bought a second-hand copy of Mates in 1994 because i always buy old logic books when i see them—partly because of things like this! I bought a copy of Beth in 1969 when my father gave me 100 quid to spend on books. Thanks for the Mates tip tho’.”

I think Mates’s book is excellent. It leads nicely to conservative extensions of theories.

(If you get interested in different kinds of definitions, you may actually enjoy a kind Kripke introduces in \*Naming and Necessity\*. He distinguishes between fixing the \*meaning\* of something (for example, the meaning of “one meter”), versus just fixing its \*reference.\* I personally like Kripke a lot. He’s clear and always interesting (whether one thinks he’s right or not). Many Brits in philosophy don’t like him because of conflicts between him and Michael Dummett, whom I gather is somewhat of a god where you are.)

Aristotle Posterior Analytics.

David Charles: Types of definition in Plato’s Meno Karasmanis-Judson Remembering Socrates

Also Aristotle on meaning and essence

### 7.1.2 Letter to Michael Norrish

Let us start from the modern rescencion, with all its overladings, unspoken assumptions and hacks.

Morally, of course, an ordinal is something that arises from the isomorphism relation on wellorderings. It is true that we can—and should—also think of them as a cunning rectype— $\mathbb{N}$  with an extra constructor—but it is from wellorderings that they arise both mathematically and historically. How are we to implement these things? The naïve first tho’rt is to take them to actually *be* the equivalence classes, and this of course is what Russell-and-Whitehead actually do. Another strategy could be to pick a canonical representative from each class to serve as the ordinal. Which representative? The answer always given is: Von Neumann ordinals. What is it that is so nice about Von Neumann ordinals? One cute fact (Randall Holmes draws attention to this) is that Von Neumann ordinals are their own ordernesting. (??). Ordernests are an idea (due i think to Sierpinski) that represents an ordering  $\langle X, \leq \rangle$  as a family of subsets of  $X$  (namely the initial segments). So when does it happen that the ordernesting of an ordering  $\langle X, \leq \rangle$  is the carrier set  $X$  itself? Precisely when  $X$  is a von Neumann ordinal and  $\leq$  is  $\in$ .

Observe that the feasibility of this implementation relies on every wellordering being isomorphic to a Von Neumann ordinal. Of course, the world being the kind of place it is, this is a nontrivial constraint that is not always met. The usual way to meet it is to appeal to Mostowski’s collapse lemma (which says that every wellfounded relation has a homomorphic image that is  $\in$  restricted to a transitive set) is a consequence of replacement. You don’t need full replacement

of course, but you do need *some*, and in particular Zermelo set theory cannot prove that every wellordering is isomorphic to a von Neumann ordinal.

Be that as it may, most modern set theorists work in situations where it can be assumed that every wellordering is isomorphic to a Von Neumann ordinal. Since they also assume the axiom of choice, this implementation extends to an implementation of cardinals. Every cardinal is an aleph if we have AC, and we can implement alephs as initial ordinals. Thus we can use the symbol for the first infinite ordinal (' $\omega$ ') to denote also the cardinal  $\aleph_0$ . Further, under the Von Neumann implementation, every ordinal is implemented as the set of its predecessors. This means that ' $\omega$ ' can denote also the set of finite ordinals and finally—since the finite ordinals are naturally isomorphic to the (inductively) finite cardinals—the set  $\mathbb{N}$  of natural numbers. I think that as a result a lot of set theorists have become genuinely confused about the nature of these mathematical entities. How can you not be if you use the one symbol to denote three completely distinct things? No wonder they are all crazy. It's Set Theory's own version of the Mystery of the Trinity: it would do anyone's head in.

The easy availability—and cuteness—of the Von Neumann ordinals has actually done a lot of damage and caused a lot of confusion. It has resulted in people giving spurious proofs. A good example of this is the assertion that the order relation on ordinals is wellfounded. One sees people claiming to prove this result by pointing to the fact that the order relation on Von Neumann ordinals (which is of course  $\in$ ) is wellfounded, because of the axiom of foundation. This is precisely as sensible as proving the least-number principle for  $\mathbb{N}$  by appealing to the fact that natural numbers are wellfounded as sets. Nobody in their right mind would ever think that *that* was a proof. See p 50.

The proof that the order relation on ordinals is wellfounded requires a bit of thought.

OK, Von Neumann ordinals are the obvious candidate if you want to implement ordinals by taking representatives. Is there any other candidate that has this uniform universal property? None known to me. It has to be admitted that in some sense the canonical representative of the wellorderings of length  $\omega$  is the wellordering of the ordinals below  $\omega$  but (i) this assumes Rosser's counting principle; (ii) it's circular (or at least recursive) and (iii) if we treat it as a recursive declaration we get back von Neumann ordinals again. So the executive summary is that if ordinals are to be representatives then von Neumann ordinals are the only show in town.

What if we are interested in implementing only an initial segment of the ordinals? Are there canonical representatives then? For example if we are interested in countable ordinals only then we can restrict our attention to wellorderings of  $\mathbb{N}$ . Is there a way of assigning to each countable ordinal a wellordering of  $\mathbb{N}$  of that length. No; one would need choice. (If there were such a canonical assignment one would have an injection from the set of countable ordinals into the reals, and it is known that this cannot be done with a little bit (not much!) of AC).

OK, so if you can't have von Neumann ordinals you have to give up on having ordinals as representatives. One thing you can still do, if you have foundation, is

*Scott's trick.* If you have a mathematical entity ('wombat-of') that arises from an equivalence relation ('equiwombat') you can take the wombat of  $x$  to be the set of all things of minimal rank that are equiwombat-with- $x$ . It is known that there are models of ZF minus foundation and minus choice that do not admit any definable global "classifier" function, a function  $f$  s.t.  $(\forall xy)(f(x) = f(y) \longleftrightarrow x \text{ and } y \text{ are equipollent})$ .

One thing one can always do (tho' one hopes very much that one will not be reduced to it!) is to implement equiwombat classes *locally*. That is to say, for each set  $x$ , one looks at the equiwombat classes inside—as it might be— $\mathcal{P}^2(x)$  and then hopes that these implementations *cohere* in a suitable sense. In some set theories (KF, MacLane without foundation and perhaps a few others) this is what one has to do for many wombats. Certainly for ordinals.

So what are the problems before us?

1. The project of obtaining naturally, given a family of wellorderings, a canonical wellordering whose length/order-type is the sup of the lengths/ordertypes of the members of the family.
2. Why the order relation on ordinals defined as a rectype of infinite character is total.
3. Why are ordinals as rectypes the same chaps as ordinals as isomorphism classes of wellorderings?

## 7.2 Problem 1

This is related to the challenge of showing that every set of ordinals has a bound, and this is a nontrivial task since—as we know—there are consistent set theories (NFU and its congeners) in which the collection of ordinals is a set. OK, so in those cases where every set of ordinals has a bound, how do we prove it? One way of proving it would obviously be to prove 1!

"The quotient as witness"

Making disjoint copies by means of IO.

Suppose I have an indexed family  $\{\mathcal{A}_i : i \in I\}$  of structures. I can obtain disjoint copies of all the  $\mathcal{A}_i$  as follows. For each  $\mathcal{A}_i$  replace its carrier set  $A_i$  by  $A_i \times \{i\}$ , and modify the relations in the obvious way. Call the resulting structure  $\mathcal{A}'_i$ . The  $\mathcal{A}'_i$  are now all disjoint.

In particular we can do this if the  $\mathcal{A}_i$  are all wellorderings. So what we can next do is create a new wellordering, out of "slices" of the  $\mathcal{A}'_i$ . To be precise, its first point is the set of first points of all the  $\mathcal{A}'_i$ , and so on. (One can do this without talking about ordinals). It might be worth sketching how one does this. It is elementary to establish that between any two wellorderings is a canonical bijection between one and an initial segment of the other. These canonical bijections form a commuting system of maps between the  $\mathcal{A}'_i$ . Thus any point, in any of the  $\mathcal{A}'_i$ , is linked to points in some (but possibly not all) other  $\mathcal{A}'_i$ .

Indeed this defines an equivalence relation on the union  $\bigcup_{i \in I} A_i \times \{i\}$ . The carrier set of our new “supremum” wellordering is the set of all these equivalence classes, and the wellordering of the equivalence classes is what you think it is. Let’s call this new structure  $\mathcal{A}_\infty$ .

(You probably guessed all that, but i just thought i’d let out a yelp so you can be sure that the two of us are barking at the same hatstand.) Now let’s stand back and see what we have assumed.

We have, in each structure  $\mathcal{A}_i$ , replaced the carrier set  $A_i$  by  $A_i \times \{i\}$ . Of course the index  $i$  for the structure  $\mathcal{A}_i$  could be taken to be  $\mathcal{A}_i$  itself, since it doesn’t really matter what it is. What does matter is that we replace each carrier set  $A$  by  $A \times \{a\}$  for some  $a$ , and that all the little  $a$ s are distinct. That is to say, we have to be able to assign to each carrier set  $A$  a singleton, and all the singletons must be distinct. This means that we have to have an assumption (called **IO** by people who concern themselves with these matters) that **every set is the same size as a set of singletons**. The mathematical (as opposed to set-theoretical) meaning of **IO** is that *you can make pairwise disjoint copies of everything in sight*. It is this principle that fails in NFU.

OK: let’s assume **IO**, since this disjoint-copy project is not getting anywhere without it. The next thing we have to do, now that we know that this  $\mathcal{A}_\infty$  exists, is show that it really is what we think it is, to wit a wellordering of minimal length into which all the  $\mathcal{A}_i$  inject isomorphically. This means that we have to exhibit some injective maps. If this is going on in set theory the existence of these maps is a set-theoretic existence claim. The maps exist as long as one has unrestricted separation (every subclass of a set is a set) but there are theories (NFU is an example) where, altho’ we can prove the existence of the composite worder, we lack the separation necessary to prove the existence of the maps in virtue of which the composite worder is the sup. Bear in mind that—apparently—**IO** does not by itself imply the existence of the needed maps. You need something extra, the separation. The extra separation is roughly what one needs to prove Rosser’s counting principle. The assertion that the ordinal  $\omega$  is monomorphic (which is the original version of the counting principle in [98]) is

I think this is wrong: IO does

suffice. Check it

Another construction (that you might use) that relies eventually on those same set existence principles is the idea that i used to call “the quotient as witness”. This idea is best illustrated in the simplest case, namely natural numbers. Suppose i have sets of all finite sizes; how am i to obtain a countable set? Well, i organise the finite sets that are available to me into equipollence classes. Then the collection of equipollence classes [the quotient] is the witness to the existential assertion that i am trying to prove.

The problem from the point of view of sensitive souls like you and me is that the quotient object might have the wrong type for the task of being the supremum you want. For example in the case of cardinals the supremum of all the natural numbers of type  $\alpha$ -card turns out to be of type something like  $\alpha$ -card-card. However this is how we prove in ZF, from the existence of a Dedekind-infinite set, that there is an infinite countable set (and thence an

implementation of arithmetic). The slight worry (i am reminded here of the princess and the pea) is that the infinite set whose existence is proved in this way isn't really a pure-as-the-driven-snow set, since morally it is a set not of sets, but a set of *cardinals*.

For you, since you are interested only in countable ordinals, these problems do not arise, or arise only in a form that can be dealt with. For you the question is: if i have a [countable] family of countable wellorderings/ wellorderings of  $\mathbb{N}$ , then i can produce a [countable] sup. The answer to this is yes, because of countable choice. Since we are using countable choice the existence proof is not constructive, and I don't know if this matters for you. Perhaps you can just brutally do a `mk_theorem` or whatever does it nowadays.

Proving that the order relation on ordinals (conceived as a rectype) is total is \*hard\*. Do you know Witt's theorem? Sometimes called 'Bourbaki-Witt'? It says that if  $\langle P, \leq_P \rangle$  is a chain-complete poset and  $f : P \rightarrow P$  satisfies  $(\forall x)(x \leq f(x))$  then  $f$  has a fixed point. The idea is to iterate  $f$  (starting at the bottom element—i think you need there to be a bottom element) until you reach a fixed point. As part of the proof you have to show that the iterated images of  $\perp$  are totally ordered by  $\leq_P$ . What is going on here? Of course what is happening is that you are creating an implementation of the ordinals inside  $P$ .  $\perp$  is 0,  $f$  is successor, and the fact that  $\langle P, \leq_P \rangle$  is chain-complete gives you sups. (If there is no fixed point you have injected all of  $On$  into  $P$  and that is impossible because of Hartogs' theorem.) Proving that the iterated images of  $\perp$  under  $f$  are totally ordered is of course the problem of showing that the engendering relation of the rectype of ordinals is a total order. It's surprisingly tricky, but i imagine that you have already thought about that quite hard. I learnt this from PTJ's lectures years ago, and i put into *Logic, Induction and Sets* the proof that he lectured. I don't know if his proof is the original one, but i suspect it is—and that's something that it would be easy to check. PTJ also distributed a new proof, shown to him by someone called Patariaia, which it might pay us both to look at.

The moral seems to be that whenever you have an inflationary function from a chain-complete poset into itself that has no fixed point (as it might be the powerset function from the poset  $\langle V, \subseteq \rangle$  into itself) then you have an implementation of the ordinals.

### 7.3 Problem 3

To have implemented a rectype of ordinals would be to have a collection (set or class, we won't show our cards yet) called  $On$  with an injective function  $S : On \hookrightarrow On$  and a function *sup* that takes bounded subsets of  $On$  and gives back members of  $On$ . Everything in  $On$  is either *s* of something or is *sup* of some subset of  $On$ . I said 'bounded' didn't i.... So  $On$  had better be equipped with an ordering.

> These are complex questions which too few people worry about, and it  
 > will do me good to write something down! However it won't be until

```
> next week, co's i'm supervising all today and then i'm bunking off
> to Manchester for the w/e to see my old mate Wendy and her toyboy,
> play too much bridge and drink too much. These things happen!
```

On

Thu, 26 Apr 2012, Michael Norrish wrote:

```
> On 25/04/12 22:44, Thomas
> Forster wrote:
>> This all seems fairly straightforward. The next
> bit is where we seem to get into a tangle:
> >>> perhaps this is
> the thought you need. You could have a >>> constructor that takes
> a function num -> cord and gives back a cord. That's the
> obvious (well, *one* obvious) thing to do. It's true that you
> need countable choice to ensure that this constructor never
> gives you $\\omega_1$ but my guess is that you'll want countable
> choice anyway.

> >>> I'm afraid I don't see what the RHS of my
> definition would be. But yes, I'm happy to use (even full)
> choice. I'm not sure what the word 'definition' is doing
> here. What i have in mind is: what extra constructor does one
> have to add to the >> naturals to get countable ordinals? And *one*
> answer is: a >> constructor that takes a function num -> cord and
> gives you a new >> cord, secretly its sup. >> So, I could
> certainly define a new algebraic type >> cord = ZERO | SUC of cord
> | SUP of (num -> cord)
> > I can then give an inductive definition
> of <> > ----- > 0 < SUC c > > c1 < c2 > -----
> SUC c1 < SUC c2 > > (?i. f(i) < SUC c) > -----
> f < SUC c > > (?i. c < f(i)) > -----
> think this is total, irreflexive and transitive. I should then be
> able to define an equivalence by >> x ? y <=> (x < y) ? (y
> < x) >> Can I then prove a bijection between these guys and the
> equivalence classes of wellorders?
> > What happens if I replace
> "num" in the original definition by type variable alpha? >>

> There's stuff like this in the Isabelle distribution (but certainly
> not any connection to wellorders). >>> Contrast the situation in
> set theory where you have this nice canonical >>> wellorder and can
> then ignore all the other members of equivalence >>> class. John
> Harrison's existing HOL formalisation of this stuff does >>> the
> same: he uses Hilbert Choice to pick out canonical representatives
> >>> as the segments grow. In either of these worlds, you can just
> use union >>> to construct limits. (John's ordinals are sets of
> pairs, and again, >>> union "Just Works".) >>> Well, you do in
> *some* set theories of course, specifically ZF, but >> not in
> Zermelo or NF or NFU. The existence and availability of von >>
```

> Neumann ordinals is a massive red herring that has put many people  
> >> off the scent. One even hears people saying that ordinals are >>  
> hereditarily transitive sets wellordered by \$\\in\$. Horrible hacky >>  
> confusion of entity with implementation. Ugh Argh! > > I totally  
> agree. And my claim that John's approach just works is not quite  
> true either. He can't demonstrate the existence of a canonical  
> ordinal corresponding to ? + 1 because his canonical representation  
> of ? may have used up all the natural numbers. (You can't tell one  
> way or the other because of the use of Hilbert choice throughout.)  
> > >> When first attacking this, I thought I should just prove the  
> theorem >>> that if one has an arbitrary set of wellorders, then  
> there is an >>> equivalent-up-to-isomorphism set whose members are  
> consistent over all >>> the underlying elements. Perhaps I should  
> have put more effort into >>> this, but it suddenly seemed to turn  
> into a hard proof and I gave up. >>> I'd be very happy to be told  
> how to get this result out. > >> Let me think about. First i have  
> to turn back into a set theorist for >> a moment. The challenge is,  
> You give me a hatful of ordinals, each of >> them an isomorphism  
> class of wellorderings, and you expect me to come >> up with a  
> wellordering whose length is the sup of the lengths of the >> stuff  
> you gave me? Do i read you right..? > > Yup. I'm willing to  
> accept a side condition along the lines of "the hat doesn't already  
> contain all the possible wellorders over the underlying set". > >>  
> Let me think a bit about this. The obvious first answer is NO but  
> >> there may be something clever that does \*something\* > > If the  
> answer really is NO, it would seem to really crimp the whole "get  
> ordinals by quotienting wellorders" approach. > >> Could i trouble  
> you to cast an eye over >> >>  
> www.dpmms.cam.ac.uk/~tf/TMStalk2012.pdf >> >> I would be glad of  
> your tho'rts > > The examples are certainly very nice. Here, as in  
> the other notes, I'm a bit disturbed by the language about the  
> Counting Principles, where you have things like "an ordinal is just  
> the length of its predecessors arranged in the right order." > >  
> Here you're saying "length" instead of "order type" (a point made in  
> your other notes). This is fine as a definition of "length" (or  
> "order type"), but the language seems to be assuming that "length"  
> is already a well-defined notion in your audience's heads. > >  
> Michael



## Chapter 8

# Emergence, Implementation and Replacement

*Memo to self:*

*In connection with the fact that implementation-insensitivity for On implies replacement but implementation-insensitivity for  $\mathbb{N}$  does not it's worth making the point (i'd forgotten this!) that  $\mathbb{N}$  arises from finite sets and the class of finite sets is a proper class, but it also arises from the class of hereditarily finite sets, which typically is a set. We can't do the same for ordinals because the class of hereditarily wellorderable sets is a paradoxical class. (Suppose it were a set: think about the set of von Neumann ordinals in it. That's a hereditarily wellordered subset that is not a member)*

When trying to work out how much set theory one needs to prove implementation-insensitivity for abstracted widgets there are two things one needs to worry about. One is the question of whether the abstract widgets arise from a class or a set of things, and whether or not the class of entities thus abstracted is a set ( $\mathbb{N}$ ) or a class (On, BFEXTs). The other is the typing status of the assertion to be considered.

*WRT the first, it has only just occurred to me that sometimes one can simplify a [proper] class to a set. For example,  $\mathbb{N}$  arises from operations on [the class of] finite sets. But it can all be seen as arising from hereditarily finite sets—which form a set. Indeed HF is a subset of the class of finite sets that is elementary for the purposes of arithmetic. We can't do the same for ordinals.*

Any set theory  $T$  strong enough to prove the completeness theorem will have lots of implementations into it of arithmetics, and lots of interpreted arithmetics corresponding to those implementations. Naturally there is no restriction in that setting to sound theories of arithmetic. One particular arithmetic is privileged by being called the *arithmetic of  $T$* , and that arithmetic is the set of those sentences in the language of arithmetic which hold true of the cardinals of finite sets according to  $T$ . This is the arithmetic which one might say emerges from  $T$ . In this section we are concerned with interpretations-into- $T$  of this

“endogenous” (emergent) arithmetic, and not with interpretations-into- $T$  of any other arithmetics. The critical difference between the endogenous arithmetic of  $T$  and all the other arithmetics that one might interpret into  $T$  is that part of the kit of an interpretation-into- $T$  of the endogenous arithmetic of  $T$  is a *classifier*: a function  $f : \text{finite sets} \rightarrow V$  s.t  $(\forall x, y)(f(x) = f(y))$  iff there is a bijection between  $x$  and  $y$ . Of special interest here will be the question of what difference can be made by our choice of classifier.

In all cases we expect that the theory of the abstract entities implemented by the classifier does not depend at all on the choice of classifier. The Riemann Hypothesis is not going to be provable in ZF if we use von Neumann naturals while refutable if we use Zermelo naturals. Indeed this implementation-insensitivity seems so obvious that the matter is never raised and no proof of it is ever offered. The time has come to correct this omission. It will turn out that the explanation has a lot to do with the axiom scheme of replacement.

We start off with some useful technicalities, repeated from an earlier chapter to make this extract self-contained.

**DEFINITION 2** A classifier for an equivalence relation  $\sim$  is a function  $f$  s.t  $(\forall xy)(f(x) = f(y) \longleftrightarrow x \sim y)$ .

We define an operator  $j$  (for ‘jump’) on functions so that  $(j(f))(x) = f``x$ . And  $f``x$  is of course  $\{f(y) : y \in x\}$

From NF studies we have the concept of a *setlike* function.<sup>1</sup>

**DEFINITION 3** A (unary) function  $f$  is

- 1-setlike if  $f``x$  is a set for all  $x \subseteq \text{dom}(x)$ ;
- n-setlike if  $j^n(f)$  is 1-setlike;
- setlike if  $j^n(f)$  is 1-setlike for all  $n$ ;
- locally a set if  $f`x$  is a set for all sets  $x$ .

An  $n$ -ary function  $f$  is 1-setlike if  $f``(X_1 \times \dots \times X_n)$  is a set whenever  $X_1 \dots X_n$  are. In particular a pairing function is setlike as long as  $X \times Y$  is a set whenever  $X$  and  $Y$  are.

Evidently a composition of  $n$ -setlike relations is  $n$ -setlike.

In a model  $\mathfrak{M}$  a 1-setlike function defined on a set  $X$  can “see” all the subsets of  $X$  that are present in  $\mathfrak{M}$ . A setlike function defined on a set  $X$  can “see” everything in the natural model of TST whose bottom type is  $X$ . To put it another way, if  $f$  is a setlike 1-1 function then the two natural models

---

<sup>1</sup>The idea (tho’ not the terminology) goes back to [23]. Coret uses the word ‘admissible’. We need a more specific word for it, since it is going to be re-used... and ‘admissible’ is already overloaded.

of Typed Set Theory,  $X$ ,  $\mathcal{P}(X)$   $\mathcal{P}^2(X) \dots \mathcal{P}^n(X)$  and  $f``X$ ,  $\mathcal{P}(f``X)$ ,  $\mathcal{P}^2(f``X)$   $\dots \mathcal{P}^n(f``X)$  are isomorphic.

This last fact is related to the fact that the concept of *setlike* arose from the need to state correctly a completeness theorem for Rieger-Bernays permutation models. If  $\mathfrak{M}$  is a structure for  $\mathcal{L}(\in, =)$  (the language of set theory) and  $\sigma$  is a setlike permutation of the carrier set of  $\mathfrak{M}$ , then the structure formed of that same carrier set and the binary relation  $x \in \sigma(y)$  satisfies the same stratifiable formulæ as does  $\mathfrak{M}$ . There is a converse too: any sentence  $\phi$  that is preserved by all Rieger-Bernays constructions using setlike permutations is equivalent to a stratified sentence. In this setting, where we are studying a model  $\mathfrak{M} = \langle M, \in_M, = \rangle$ , it is permutations of the carrier set  $M$  that we are interested in, not arbitrary functions living inside  $\mathfrak{M}$ , so the concept of *setlike* was applied in the first instance to permutations.

Need a reference for this

That may have been the genesis of the idea, but it turns up as the correct concept to use to shed light on a rather amusing (but profound) *aperçu* of Mathias. If the cartesian product  $A \times B$  exists, for all  $A$  and  $B$ , irrespective of what our ordered-pairing-and-unpairing kit is, then replacement follows. Randall Holmes' comment on this is that the existence of  $A \times B$  should be (or perhaps, *is always tacitly assumed to be*) part of the specification of the ADT of ordered pairs. But this is simply to say that the pairing and unpairing functions must be 1-setlike. In fact, if Holmes' analysis is the right way to go, then one probably wants pairing/unpairing functions to be fully setlike.

The following obvious observation might help set the scene:

**REMARK 2** *The axiom scheme of replacement is the assertion that every function is setlike.*

*Proof:*

The Right-to-left implication is immediate. For the other direction... A 1-setlike function is simply a function for which replacement holds. If replacement holds then every function is 1-setlike. If  $f$  is 1-setlike then  $j(f)$  is defined. But then, if we have replacement,  $j(f)$  is 1-setlike. This gives us an induction ensuring that  $f$  is actually setlike, and not merely 1-setlike. ■

We now prove a series of lemmas that show that, according to even quite weak set theories, 1-setlike is the same as setlike.

**REMARK 3 (Mac Lane Set Theory)**

*Suppose our pairing and unpairing functions are setlike. If  $f$  is 1-setlike, then  $f$  is locally a set.*

*Proof:* Suppose  $f$  is 1-setlike. Then  $f``x$  is a set if  $x$  is;  $x \times f``x$  is a set since our pairing function is setlike, and  $f|_x$  is now a subclass of  $x \times f``x$ , and it will be a set because we have  $\Delta_0$  separation. (Always assuming the definition of  $f$  is simple enough). ■

In the same spirit we have

**REMARK 4** (*second-order Zermelo*)

*Suppose our pairing and unpairing functions are setlike. If  $f$  and  $g$  are both 1-setlike, so is  $\lambda x.\langle f(x), g(x) \rangle$ .*

*Proof:*

Since  $f$  and  $g$  are both setlike,  $f``x$  and  $g``x$  and therefore (since pairing/unpairing are setlike)  $f``x \times g``x$  are both setlike, whence  $\{\langle f(y), g(y) \rangle : y \in x\}$  is a set by second-order separation. ■

**COROLLARY 1** (*second-order Zermelo*)

*Suppose our pairing and unpairing functions are setlike. If  $f$  is 1-setlike, then  $f$  is locally a set.*

*Proof:*

Take  $g$  to be the identity in remark 4. ■

We have to be careful with remark 3: a function can be setlike without being a set if it is defined by bits of syntax not in the language we are using. Any external  $\in$ -automorphism  $\sigma$  is perforce setlike, since  $\sigma(x)$  has to be  $\sigma``x$ , so  $\sigma``x$  is always defined. And of course we need (full) separation.

**REMARK 5** (*Mac Lane Set Theory*) : (*Coret [23]*)

*If  $f$  is 1-setlike then  $j(f)$  is 1-setlike.*

*Proof:* Let  $f$  be 1-setlike, and let  $x$  be a set. We want  $\{f``y : y \in x\}$  to be a set. It is certainly a subset of  $\mathcal{P}(f``\bigcup x)$  which is a set by Power Set and Sumset since  $f$  is 1-setlike. So it is

$$\{z \in \mathcal{P}(f``\bigcup x) : (\exists y \in x)(y = f``z)\}$$

which is a set by separation. ■

**COROLLARY 2** (*Mac Lane Set Theory*)

*Every 1-setlike function is setlike.*

*Proof:*

If  $f$  is 1-setlike then, by remark 5,  $j(f)$  is 1-setlike as well. This powers an induction that shows that  $j^n(f)$  is 1-setlike for all  $n$ , which is to say that  $f$  is setlike. ■

We allude above to a notion of *typing* which is explained elsewhere in this document but which, too, we reprise as part of the effort to make this section self-contained. The typing we are concerned with arises [for example] when we have new entities that arise from equivalence relations—e.g. arithmetic of natural numbers arising from equipollence between finite sets and operations for

which equipollence is a congruence relation. The new language is [potentially] typed in the sense that [for example] one is not [might not be] allowed to place a ‘ $\in$ ’ to the left of a variable ranging over numbers. Some assertions in this language are so strongly typed that the number variables can be rewritten out of them altogether: the assertion that addition of natural number is commutative is such an example. There are some sentences from which number variables cannot be removed, and which accordingly require us to decide on a classifier for equipollence... an example would be the assertion that every natural number has only finitely many predecessors. This particular sentence is well-behaved in the sense that its truth-value does not depend on the choice of classifier; it is well-typed in the above sense—no integer variable is preceded by an ‘ $\in$ ’ for example. Then there are assertions like Rosser’s Axiom of Counting, that says that every natural number  $n$  has precisely  $n$  predecessors. To make sense of this expression one has to actually use a classifier, but one would expect that its truth-value does not depend on a choice of classifier. Finally there are assertions—such as ‘ $3 \in 5$ ’—whose truth-value emphatically does depend on choice of classifier, and which are clearly untyped. The purpose of this section is to relate the typing to insensitivity-to-choice-of-classifier.

**THEOREM 1** (*Zermelo, KF?*)

*Let  $b$  and  $p$  be two classifiers for an equivalence relation  $\sim$  on a class  $X$ , and let the two implementations  $p``X$  and  $b``X$  both be sets. Then there is a setlike bijection  $\pi$  between  $b``X$  and  $p``X$ , and  $p$  and  $b$  are both setlike.*

*Proof:*

First we find the bijection between  $b``X$  and  $p``X$ . To what element of  $p``X$  should we send  $b(x) \in b``X$ ? Clearly we send it to  $p(x')$  for any  $x'$  s.t.  $x \sim x'$ . It doesn’t matter which, because we will always get the same answer.

We have thus defined a total function  $p``X \rightarrow b``X$ . We need to show that it is onto. Well, we can define analogously a function going in the opposite direction, and it is clear that these two functions are mutually inverse.

Let us call this bijection  $\pi$ . We want to establish that  $\pi$  is setlike. Because of lemmas 3 and 5 it will suffice to show that  $\pi$  is 1-setlike. Suppose  $P \subset p``X$ ; we want  $\pi``P$  to be a set. Now  $\pi``P$  is a subcollection of the set  $b``X$ , so we can aspire to use separation to prove it a set. And indeed we can: it is  $\{y \in b``X : (\exists x \in P)(y = b(x))\}$ .

■

This covers some familiar cases such as finite-sets-and-natural-numbers, but it doesn’t cover ordinals—the collection of all ordinals is not a set because of Burali-Forti. What happens if we do not know that the implementations  $p``X$  and  $b``X$  are both sets?

Well, everything is the same up to the point where we want  $\pi``P$  to be a set. This time there is no set to hand of which  $\pi``P$  is a subcollection, so there is no obvious way to exploit separation.

So if  $b``X$  and  $p``X$  are proper classes there is some work to do. Is it sufficient that  $b$  and  $p$  be setlike? Consider the relation  $(\exists X')(P' = p``X \wedge B' = b``X)$  (upper case ‘ $X'$ ’ because we don’t mind if  $X'$  is a proper class). This is a formula with two free variables that defines a 1-1 function. But are its domain and codomain the whole of  $\mathcal{P}(p``X)$  and  $\mathcal{P}(b``X)$ ? Given an arbitrary  $P' \subseteq p``X$  how do we know that there is a bridging witness  $X'$ ? We seem to need a principle that says that whenever  $f$  is a surjection from a proper class  $A$  to a proper class  $B$ , and  $b \subseteq B$  is a set, then there is a set  $a \subseteq A$  with  $f``a = b$  ... and this is precisely collection!

Thus everything is all right if we have replacement, and we can actually prove that replacement is not only sufficient but is necessary.

#### REMARK 6

*Suppose that whenever  $b$  and  $p$  are two classifiers for an equivalence relation  $\sim$  on a class  $X$  then the function  $\pi$  defined above as  $p \cdot b^{-1}$  is 1-setlike.*

*Then replacement follows.*

*Proof:*

Let  $h$  be an arbitrary bijection between two sets  $A$  and  $B$ . Find a class  $X$  and a surjection  $X \rightarrow A$ . Call it  $p$ . Then let  $b$  be  $h \cdot p$ . Then  $\pi$  is just  $h$ , and is 1-setlike by the assumption. So  $h$  (which was arbitrary) is 1-setlike. This is replacement. ■

(\*)

However there are special cases where we do not need replacement. One of them is ordinals: here the equivalence relation is order-isomorphism between wellorderings. The collection of wellorderings is a proper class, and the range of any classifier (i.e., On) is also a proper class. Nevertheless, because of special features of wellorderings, we can prove that if  $p$  and  $b$  are two classifiers for order-isomorphism then not only is there a bijection between the two classes of pink and blue ordinals, but this bijection is 1-setlike. This we show as follows. Let  $P$  be a set of pink ordinals. Since  $P$  is a set, there is an ordinal  $\alpha$  bigger than any member of it.

Fix a wellordering  $\mathcal{A} = \langle A, <_{\mathcal{A}} \rangle$  of length  $\alpha$ . For each ordinal  $\beta \in P$ ,  $\mathcal{A}$  has a unique initial segment of length  $\beta$ , and the collection of such initial segments is a set by separation. Then, since  $b$  is setlike, the set  $B$  of blue ordinals of members of this set is a set, and  $B$  is the image of  $P$  that we sought.

However we have needed an extra assumption, the asterisked observation which seems innocent enough, but i suspect it needs replacement.

Summary of this section. Of course it’s not just cardinals-of-sets, and set<sup>n</sup>-of-cardinals-of sets... that we have to consider, but cardinals-of-sets<sup>n</sup>-of-cardinals and sets of them and so on. The claim will be that the bijection  $\pi$  is setlike in the sense of extending to a family of bijections between each pink type and the corresponding blue type.

But there is also a quotient of this complex family of types, a quotient that arises from our determination that natural numbers should be monomorphic. And we want our  $p \cdot b^{-1}$  to extend to family of isomorphisms between these too. Is that harder? All types cardinal-of-(something) are coalesced (cardinals are monomorphic). What conditions do we need on  $p$  and  $b$  to ensure that the two quotient families of types are isomorphic? More to the point, what set-theoretic principles do we need?

The stricter the typing the easier it is to show that typed formulae are invariant. So we should be aiming to prove that stratified replacement suffices to prove invariance of the strongly typed formulae but that full replacement is required to prove implementation-insensitivity of the less strongly typed formulae.

There is the “obvious” proof that the strongly typed sentences are invariant, namely by appeal to the fact that the occurrences of the  $bs$  and  $ps$  can be eliminated. Executing the translation requires the classifier to be setlike. If the range of the classifier is a set you get this free (= can do it in KF)

The hardest case is when the range of the classifier is a proper class and the sentences are weakly typed. Then you need replacement.

Degrees of freedom

Is the domain of the congruence relation a set or a class?

Is the range of the congruence relation a set or a class?

Are the formulae whose invariance we seek to prove, strongly typed, weakly typed or untyped. We don’t worry about untyped, co’s they’re never invariant

Six Boxes

	strongly typed	weakly typed
ADT is a set	stratified $\Delta_0$ separation	full separation
ADT a class; quotient a set	?stratified? replacement	replacement
ADT a class; quotient a class	?stratified? replacement	replacement

(Fit in here the observation that equality holds only within types, and that it is characterised by supporting a rule of substitution)

Consider  $(\forall n \in \mathbb{N})(n = |\{m : m < n\}|)$ . For this to be given a truth-value at all, we have to decide on a classifier for equipollence. (Perhaps one should say that Rosser’s Axiom of counting is not *one* assertion, but a *family* of assertions... indexed by classifiers). However we earnestly hope that that truth-value does not depend on our choice of classifier!

What set-theoretic axioms do we need, and what conditions on classifiers, to ensure that Rosser’s Axiom of Counting holds?

Well, a claim that two cardinals are the same is a claim that bijections of a certain sort exist, and that is a set existence claim. Which set-theoretic axiom do we reach for to underpin this claim? In this case, where we desire a bijection between two given sets, separation will do, since the desired bijection is a subset of the cartesian product of the two sets.

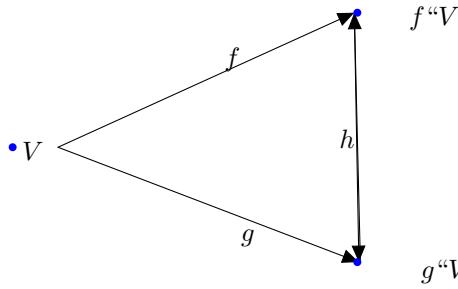
Thus in this respect the axiom of counting behaves exactly like strongly typed assertions like the commutativity of addition, altho' the first requires an actual classifier whereas the second does not. In both these cases for the proof of implementation-insensitivity we need only separation but not replacement. However, it turns out that a finer analysis can give us more information. Specifically to prove implementation-insensitivity of the axiom of counting and Fermat's little theorem we need *unstratified* separation.

We alluded above to the idea of *the arithmetic of  $T$* , where  $T$  is a set theory. Of course there is nothing special about natural numbers; any suite of abstract entities arising from an equivalence relation in the way natural numbers arise from finite sets will give rise to an analogue of the arithmetic of  $T$ : cardinal arithmetic, ordinal arithmetic.... Another, less well-known example even has its own special name. When  $T$  is a set theory we can consider what  $T$  proves about relations between (isomorphism classes of) *Set Pictures* aka *accessible pointed digraphs* ("APGs"). This gives rise to a theory that my *Doktorvater* Adrian Mathias calls the *lune* of  $T$ . The lune of a set theory  $T$  is another set theory, of course, and the relations between a set theory and its lune can be very interesting. Again, the lune of  $T$  is an *emergent* theory, like the arithmetic of  $T$ .

If  $T$  has the axiom scheme of replacement then any classifier for the isomorphism relation on wellfounded APGs then we can use Mostowski's collapse lemma to show that the wellfounded sets that the pictures were pictures of were already sets according to  $T$ . However if  $T$  does not have replacement it can happen that  $T$  can prove the existence of pictures of sets whose existence it does not prove. For example, Zermelo set theory does not prove the existence of any sets beyond  $V_{\omega+\omega}$ , but it knows about set pictures for all sets in [the very much larger collection]  $H_{\beth_\omega}$ .

## 8.1 When are two implementations isomorphic?

In this section



Consider the simple case of an equivalence relation  $\sim$  on the universe, in a context where the universe is not a set. Suppose further that we have implemented the equivalence classes in two ways, by means of two functions  $f$  and  $g$  which satisfy  $(\forall xy)(x \sim y \longleftrightarrow f(x) = f(y))$  and  $(\forall xy)(x \sim y \longleftrightarrow g(x) = g(y))$ . Need a commutative diagram here. Suppose further that  $f$  and  $g$  are 1-setlike. We seek a function  $h$  making the diagram here commute, and we want  $h$ , too, to be 1-setlike. How do we do it?

If  $f''V$  and  $g''V$  are both sets we're all right: we can construct  $h$  as a set by using only the Zermelo axioms, since it is a subset of  $f''V \times g''V$ . [unless our pairing functions are extremely perverse] Although this is only a special case it matters and we will return to it. The interesting case is the general case where these two objects are not sets.

If we are allowed collection then (without separation) we can show that  $h$  is 1-setlike.

Suppose  $Y \subseteq g''V$ . We want  $h \upharpoonright Y$  to be a set. We cannot rely on  $g^{-1}Y$  being a set. However, if we have the axiom scheme of collection there will be  $Y^* \subseteq V$  s.t.  $(\forall y \in Y)(\exists y' \in Y^*)(g(y') = y)$ . But then  $f''Y^*$  is a set, and is  $h''Y$ .

I think if we work a little harder we can show that  $h$  is actually setlike by the same method.

What happens if we have the KF axioms but no collection? For  $y \in g''V$  we

want to define

$$h(y) := \bigcup \{x \in A : (\exists u)(f(u) = x \wedge g(u) = y)\}$$

where  $A$  is a suitably chosen set. This is a  $\Sigma_1$  separation so it cannot be done in KF. But even if we upgrade to Zermelo (so we have full separation) we still have a problem with finding a suitable set  $A$  from which to separate. If  $f``V$  is a set we can use it to be  $A$ .

And it is a simple matter to show that our final construct  $h \upharpoonright Y$  does not depend on our choice of  $Y^*$ .

Here is a useful nugget.  $V_{\omega+\omega} \cap L$  is a model of Zermelo set theory. This model satisfies GCH (so that every infinite set is of size  $\beth_n$  for some  $n$ ) and also satisfies the negation of Mathias' formula  $M$ , in that every set of infinite sets all of different sizes is finite. It allows two implementations of **cardinal-of**: (i) the usual one in which **cardinal-of**( $x$ ) is the appropriate initial Von Neumann ordinal; (ii) an *ad hoc* but nevertheless setlike implementation in which **cardinal-of**( $x$ ) is (the Von Neumann) natural number  $2n$  if  $x$  is a finite set of size  $n$  and is (the Von Neumann) natural number  $2n+1$  if  $x$  is a set of size  $\beth_n$ . Both these implementations are setlike. However, according to (i)  $NC$  is not a set, and according to (ii) it is. It is an immediate consequence that if we take these two implementations for  $f$  and  $g$  (as above) then the (unique)  $h$  is not setlike, even tho' its converse is!

What is the strength (when added to Zermelo plus classes) of the assertion

If  $f : V \rightarrow V$  is a function such that  $(\forall xy)(f(x) = f(y) \longleftrightarrow x \sim y)$   
then the range of  $f$  is a proper class.

Surely it depends on what  $\sim$  is...?

But can it happen that  $f$  and  $g$  are both setlike but that  $h$  is not?

The following lemma will be useful in any project to lift an isomorphism between two set models of a first-order theory to an isomorphism between the corresponding models of the corresponding second-order theory.

**LEMMA 3 (KF)**

*If  $x$  and  $y$  are equinumerous sets, so are  $\mathcal{P}(x)$  and  $\mathcal{P}(y)$ .*

*Proof:* (Until further notice our ordered pairs are Wiener-Kuratowski.) First we note that if  $f$  is a bijection between  $x$  and  $y$  then the graph of  $f$  is a set by separation, even stratified  $\Delta_0$  separation (as in KF). Let  $f$  be a set of ordered pairs that bijects  $x$  and  $y$ . If  $x' \subseteq x$  then  $f``x' \subseteq y$  so  $f``x'$  is a set by separation. So  $j(f)$  is a bijection between  $\mathcal{P}(x)$  and  $\mathcal{P}(y)$ . And it is a set—again by separation—being a subset of  $\mathcal{P}(x \times y)$ . This last object is a set according to KF—at least if our ordered pairs are Wiener-Kuratowski. ■

What we can't do is prove that (Specker's)  $T$  is an isomorphism (even if it is!) where  $Tn =_{df} |\{m : m < n\}|$

## 8.2 Implementation and Emergence: Homophonic Implementations

We need to sort out the question of which of the various things we want to implement into set theory are things that arose from set theory in the first place. Cardinality and wellordering seem to emerge from set theory in a way that some other mathematical notions—*group*, *ring*, *field*—do not. How can one tell? It seems reasonable to work on the assumption that if *As* emerge from *Bs* then we will not acquire a concept of *As* until we have acquired a concept of *Bs*. But we had natural numbers before we had sets, and despite this one is tempted to say that cardinality emerges from sets. Presumably the answer is that cardinals arose from some pre-theoretical precursor of sets? This fact will constrain the ways in which we can sensibly implement these notions in set theory. This is the stuff underlying the discussion of implementations of pairing functions in NF—and I really haven’t thought it through. I think the importance of homophonic implementations becomes clear here.

[*HOLE At some point one has to have a discussion of why the Specker definition of exponentiation is correct!!! It seems there are special constraints on the implementation of predicates and relations of emergent properties. Cardinal exponentiation isn’t just any old piece of mathematics queuing up to be implemented somehow into set theory along with the rest of them. It is something that emerges from set theory.  $2^{|x|}$  just is  $|\mathcal{P}(x)|$ —isn’t it!?*? Well . . . , possibly not. We certainly knew about exponentiation long before we knew about power sets . . .

*Tie this in with the discussion of the right way to implement ordered pairs. Relational algebra sort-of emerges from set theory too, which is why we aren’t free to opt for perverse pairing functions that deny the existence of the identity.]*

There is an important difference between mere implementations (arithmetic into typed lambda calculus for example) and the kind of implementation that is adjoint to emergence. Implementing cardinals in a set theory, where the cardinals are the cardinals of sets—as in the expression “arithmetic of *T*”.

In [36] I used the word **implementation** to describe an interpretation that sends equality to equality. This terminology is a bit misleading, because one *implements* entities but interprets *theories*. Too bad: I shall have to stick with it. We shall need another bit of notation. Consider now the—frequent—situation where we are interested in interpretations from  $T_1$  (in language  $\mathcal{L}_1$ ) into  $T_2$  (in language  $\mathcal{L}_2$  where  $\mathcal{L}_2 \subseteq \mathcal{L}_1$ ). Let  $\mathcal{I}$  be such an interpretation; if  $\mathcal{I} \upharpoonright \mathcal{L}_2$  is the identity we say that  $\mathcal{I}$  is **homophonic**.<sup>2</sup>

One standard setting in which we are interested in homophonic implementations is where  $\mathcal{L}_2$  is the language of set theory and  $\mathcal{L}_1$  is the language of set theory + pairing-with-unpairing;  $T_2$  will be an axiomatic set theory (such as

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<sup>2</sup>If we interpret the theory of widgets + wombats into the theory of widgets, where wombats are a new suite of emergent entities, does this mean that the interpretation is automatically homophonic? Is equality between wombats the same as equality between widgets? Clarify this . . .

ZF or NF) and  $T_1$  will be  $T_2 +$  various banalities about relational algebra, existence of cartesian products and the like. Typically in these cases we want the interpretation to be homophonic. (Arithmetic of the natural numbers is another setting where we want a homophonic implementation of pairing.)

## Chapter 9

# Typing and Stratification in Russell and Quine

Altho' the topic of this chapter has roots that can be traced back earlier, it is probably safe to take as the point of departure Russell's [102].

Type distinctions à la Russell comprise the type hierarchy elaborated by Russell and Whitehead in PM, and the hierarchies of languages that follow from Tarski's famous analysis of Truth in the 1933 paper [108]. They are accepted as an—unfortunately necessary—part of the mathematicians' and linguists' toolkit. No-one really grumbles about the language-metalanguage distinction, but the regimentation of sets into levels is not welcomed. Altho' it is not denied that the type distinctions—by making ' $x \notin x$ ' an ill-formed formula—cauterised Russell's paradox very effectively, it is felt that this ruse causes the mathematical universe to fragment in a way that does not respect our intuitions of the unity of nature.

Important facts about Russell-style type distinctions: (i) the types are linearly ordered; (ii) they are formally disjoint rather than cumulative; (iii) the order relation on the types has to be wellfounded. (iii) is underlined by Yablo's paradox, which arises in a strongly typed language where the types are totally ordered—but in order type  $\omega^*$  so there is no bottom type. See [37].

The intention is that in a well formed formula such as ' $x \in y$ ' the two variables cannot be give the same type. This is to prevent ' $x \notin x$ ' from being wellformed. However there is nothing to prevent ' $y_1$ ' and ' $y_2$ ' being given the same type in ' $x \in y_1 \wedge x \in y_2$ ' and parsimony invites us to give the two  $y$  variables the same type. We can *identify greedily*. If we do this we end up with a typed language with types indexed by the natural numbers (or the integers) and every variable has a natural number for a type. Thus in ' $x \in y$ ' the (integer that is the) type given to ' $x$ ' must be one lower than the (integer that is the) type given to ' $y$ '.

In Russell's original treatment the type of the variable was an integral part of the variable and was (usually) written as a superscript. Thus the theory was

many-sorted first-order. Can we set it up as one-sorted first-order with lots of type predicates? The problem is that there are infinitely many types, so there is no way of saying that every object belongs to a type. See [17] pp.19 *ff.*

Altho' it seems to be purely a type discipline within  $\mathcal{L}(\in, =)$  (the language of set theory) the significant rôle played by that language in the explicit reductionist (“logicist”) project of Russell and Whitehead means that it is in principle to be found everywhere in Mathematics.

## 9.1 Russell-Quine Typing is more natural than you think

A reader who has no special interest in this topic, and who therefore merely does the obvious—pick up some Russell-and-Whitehead by osmosis, and perhaps casts an eye over Quine’s original paper (now conveniently to hand in [93])—could be forgiven for thinking that Russell-Quine typing is purely a syntactic trick. Indeed lots of readers are, thus, hereby forgiven. However, this assessment—even when forgiven—remains wide of the mark. The error might not be particularly grave (quite how much genuine semantics there is to this apparatus is a fair question) but it is widespread. Accordingly—before we get down to the exploration of the relations with the endogeneous strong typing, which is the project of this book—it might be helpful to bring out and dust off some facts about the semantics of Russell-Quine typing that deserve wider currency than they enjoy at present, thereby putting it in a clearer light and preparing the reader for what is to come.

Marco Forti has persuaded me that the best point of departure for set theory is an *aperçu* of di Giorgi’s. Di Giorgi thought of a structure for the language of set theory as a set  $A$  of atoms (things with no internal structure) together with an injective map  $i : A \hookrightarrow \mathcal{P}(A)$ . We then define a membership relation between the members of  $A$  by  $a_1 \in_i a_2$  iff  $a_1 \in i(a_2)$ . Thus an atom  $a$  “codes” the set of things that are in  $i(a)$ , and sets of the model are those elements of  $\mathcal{P}(A)$  that are coded by atoms.

A famous theorem of Cantor’s tells us that this injection cannot be surjective. Suppose every subset of  $M$  is coded by an element of  $M$ . Consider the set of all those elements of  $M$  that code subsets of  $M$  and are not members of the subsets they code. Call it  $X$ . If every subset of  $M$  is coded by a member of  $M$  then  $X$  is coded by some element  $x$ . We then get a contradiction by asking whether  $x$  is a member of  $X$ .

This is not the usual way in which Cantor’s theorem is presented: I present it this way because i prefer to think of Cantor’s theorem as a constraint on our ability to code things: naïve set theory is inconsistent.

The inconsistency of naïve set theory is one of those fundamental metaphysical disasters that befall intelligent life, like original sin except—if anything—worse. The endeavour to recover from it colours all our experience, rather in the way in which everything has to be seen in the context of our endeavours to

rebuild the Tower of Babel, or to reconstitute the two-backed beasts.

Cantor's theorem tells us that not every collection of sets can be coded by a set. Moreover it seems to be the only thing—the only *elementary* thing—we can say that constrains which collections of sets (of atoms) can be coded (by atoms). In particular, for any subset  $A'$  of  $A$  we can devise  $i$  so that  $A'$  is in the range of  $i$ , that is to say, so that  $A'$  is coded by an atom.

Now evidently we can make a decision about *which subsets of  $A$  are to be coded by atoms* while leaving open *which atoms are going to code those subsets*. After all, if we compose an injection  $i : A \rightarrow \mathcal{P}(A)$  on the right with a permutation  $\pi$  of  $A$  then we have a different injection, but one that makes the same decision about which subsets of  $A$  are to be sets of the model. These two injections will be two distinct ways of putting into effect the one decision about which sets are to be coded.

How do these two models differ (the one corresponding to an injective map  $i : A \hookrightarrow \mathcal{P}(A)$  and the other corresponding to  $i \cdot \pi$ )? Which sentences are preserved by composing with a permutation in this way? It is natural to seek to ascribe a special status to the formulæ which are invariant under this change. It turns out (see [34]) that the sentences preserved are precisely the stratified sentences.

Klein  
See *setlike* p 69

Marco Forti likes to make the point that there are no deep mathematical reasons why the set theory used for the foundationalist project should have been ZFC-with-foundation rather than ZFC-with-antifoundation. This *aperçu*—that the choice we made is pure historical accident—is a good one, and it deserves wider currency than it enjoys. It is not my purpose here to get embroiled in a discussion about the axiom of foundation. Taking Forti's insight to be that *these two theories capture the same mathematics* one naturally asks what differences there are between them. They are mutually interpretable of course, but for our present purposes more to the point is the observation that ZFC-with-foundation and ZFC-with-antifoundation have the same stratified theorems. See [41].

I think there is some duplication here with the region around p 6.

Let ZFB be ZF + “every set is the same size as a wellfounded set”.  
Then the following are true.

*Every sentence true in every (Rieger-Bernays) permutation model of a model of ZF is a theorem of ZFB. (i.e., ZFB is the theory of Rieger-Bernays permutation models of models of ZF)*

*ZF and ZFAFA are both extensions of ZFB conservative for stratifiable formulæ.*

*The class of models of ZFB is closed under creation of Rieger-Bernays permutation models.*

Categorists have other notions of equivalence - e.g. Morita equivalence  
Say something about synonymy

## 9.2 NF, NFU and KF

KF arises from TST by the direct limit construction. I suspect that a belief that Mac is sufficient for mathematics could be turned into a belief that KF is sufficient. After all, if mathematics is strongly typed and the only purpose of set theory is to implement mathematics then as long as set theory has separation for things in the image of the interpretation it can be strongly typed.

Vu makes the point that if you use the combinatorial definition of BQO not the algebraic one using  $H_{\aleph_1}(X)$  then you can do all of known BQO theory in KF + DC. (He also says that if you couch it in the language of posets not preorders you don't even need DC.)

## 9.3 A discussion of $T$ functions?

Specker's use of the letter ' $T$ ' to denote the function on cardinals that he needed to spell out in order to display his refutation of AC in NF must have seemed to him like a nonce notation: use once and throw away. Disposable needles for junkies. However the operation is of great importance so the need for the label has not gone away, and nobody has come up with a better one. Not only that, but it turns out that it is a special case of a general phenomenon, and these other instances have come to be known as  $T$ -functions as well. There is a type-raising operation on ordinals and also on relational types of wellfounded extensional relations and those, too, are notated with a ' $T$ '.

Thus the primary use for the letter ' $T$ ' in this setting is to indicate a (setlike) stratified (but inhomogeneous, since it raises types by 1) function defined on a particular relational type.

We now have a fairly good picture of these generalised  $T$  functions. See [45]. However, there is another way in which they can be seen to arise.

We start with an illustration.

Cardinals emerge from sets, so we can interpret a theory of sets-plus-cardinals in a theory of sets. Then we can repeat the process, so that a second kind of cardinal, more ethereal than the first, emerges from the new world of sets and sets-of-cardinals. In the typed language these cardinals are of a different type from the cardinals that first emerged. We can repeat this as often as we like, as in [36]. When we finally implement cardinals as sets, all these infinitely many varieties of cardinal all get implemented as the one data type. [Our cardinals are *monomorphic* in the terminology of chapter ??]. However some complications can remain. Consider [for example] the (singly virtual) cardinal number  $n$  [ $n$  a natural number] and the (doubly virtual) cardinal  $|\{m : m < n\}|$ . These two get implemented as objects of the one datatype of cardinals. However there is no guarantee that the two of them get implemented as the same single object of that datatype, because the equation  $n = |\{m : m < n\}|$  (which would ensure that they do) does not arise from a well-formed formula of the many-sorted language. What happens is that there is a function  $T$  on the datatype of cardinals such that, at least,  $Tn = |\{m : m < n\}|$  is true.

The situation is much more general.  $T$  functions arise from ill-typed equations—explain this properly

Is the reason why  $T$  functions tend not to be trivially the identity anything to do with the fact that there is no type-lowering pairing function?

## 9.4 Pairing and Burali-Forti in NF

[“homophonic” sounds like a quite gratuitously long word for an entirely banal condition, but in fact the condition is not banal at all. The BFEXT interpretation of set theory into itself is an example of an important interpretation that is not homophonic.] Although the question of the best way to implement ordered pairs (homophonically) in Quine’s NF is well understood by the small and valiant band of NFistes it has never been given a thorough treatment in the literature—though it was discussed (briefly) in the closing pages of Lake’s Ph.D. thesis: [72].

Let us hark back to the  $T_1$  and  $T_2$  of page 111 and take  $T_2$  to be NF, a pure set theory. We first take  $T_1$  to be NF + the raw axioms for pairing and unpairing. The implementation of pairing must be a formula  $P(x, y, z)$  (with three free variables) in  $\mathcal{L}(\in, =)$  satisfying

$$(1) (\forall xy)(\exists!z)(P(z, y, z));$$

and

$$(2) (\forall z)(\forall x)(\forall x')(\forall y)(\forall y')(P(x, y, z) \wedge P(x', y', z) \rightarrow x = x' \wedge y = y').$$

Any  $P$  satisfying this will be said to be a *pairing relation*.

Next suppose that  $T_2$  contains elementary (binary) relational algebra. This theory has operations like composition ( $R \circ G$ ), inverse ( $R^{-1}$ ) and boolean operations on relations (thought of as their graphs) over any fixed domain. This theory contains assertions like

$$R \subseteq S \rightarrow R^{-1} \subseteq S^{-1};$$

$$R \subseteq S \rightarrow R \circ T \subseteq S \circ T.$$

(Bearing in mind that it is implementations in NF that we are considering) let us make at this stage the observation that if the composition of two relations  $R$  and  $S$  is to be a relation then  $R \circ S$  which is of course

$$\{z : (\exists x \in R)(\exists y \in S)(\exists abc)(P(a, b, x) \wedge P(b, c, y) \wedge P(a, c, z))\} \quad (9.1)$$

had better be a stratified set abstract. That is to say, ‘ $(\exists x \in R)(\exists y \in S)(\exists abc)(P(a, b, x) \wedge P(b, c, y) \wedge P(a, c, z))$ ’ must be stratified. This requires that ‘ $P(-, -, -)$ ’ be stratified and that ‘ $a$ ’, ‘ $b$ ’ and ‘ $c$ ’ all receive the same type in any stratification of 9.4. (One consequence of this will be that ‘ $x$ ’, ‘ $y$ ’ and ‘ $z$ ’ all receive the same type, though not necessarily that that type is the same as the type given to ‘ $x$ ’, ‘ $y$ ’ and ‘ $z$ ’). In particular

**In ‘ $P(-, -, -)$ ’ the first two variables must receive the same type.** (2)

It doesn’t tell us anything about the type of the third variable.

Similarly uncontroversial will be the expectation that every relation should have an inverse. However this won’t tell us anything new. Consideration of the expression

$$R^{-1} = \{z : (\exists z' \in R)(\exists ab)(P(a, b, z') \wedge P(b, a, z))\}$$

$$(\forall R)(\exists S)(\forall xy)(\langle x, y \rangle \in R \longleftrightarrow \langle y, x \rangle \in S)$$

expand this a bit

will tell us that the first two arguments to ‘ $P(-, -, -)$ ’ must receive the same type in any stratification. Again, it tells us nothing about the type of the third argument.

This insight enables us to answer the point often made by people encountering Cantor’s theorem in NF for the first time. If we try to prove that a map  $f : X \rightarrow \mathcal{P}(X)$  is not onto we find ourselves considering the diagonal set

$$\{x \in X : (\forall w \in f)(\forall X' \subseteq X)(P(x, X', w) \rightarrow x \notin X')\} \quad (9.2)$$

For us to be confident that the diagonal set is genuinely a set we would need ‘ $P(x, X', w)$ ’ to be stratified with ‘ $x$ ’ one type lower than ‘ $X'$ ’ and this of course we do not have.

However we can prove an analogue, which for many purposes is just as good: in some sense it will enable us to recover the same Mathematics.<sup>1</sup> Clearly  $\{\{y\} : y \in x\}$  is a set, being the denotation of a stratified set abstract. Next we attempt to prove that no function  $f : \iota^{\text{“}X\text{”}} \rightarrow \mathcal{P}(X)$  can be surjective. This time the diagonal set is

$$\{x \in X : (\forall w \in f)(\forall X' \subseteq X)(P(\{x\}, X', w) \rightarrow x \notin X')\} \quad (9.3)$$

which can be seen (even before we eliminate the curly brackets in ‘ $\{x\}$ ’) to be stratified. So we have proved that

**THEOREM 2** *There are fewer singletons than sets.*

But what about the singleton function—surely it is a bijection between  $\iota^{\text{“}V\text{”}}$  and  $V$ ? Yes, but its graph isn’t a set. And this is because, as we saw earlier, the two components of the ordered pair must be given the same type.

It may be worth thinking a little bit about what would happen were we prepared to change our implementation of ordered pair so that ‘ $P(x, y, z)$ ’ were stratified with ‘ $x$ ’ one type higher than ‘ $y$ ’. Then the set abstract in 9.4 would be a set and the proof would succeed.

---

<sup>1</sup>This is a very important claim, and the obstacle to proving it is that it is not entirely clear what it means.

It seems that we would have shown that  $X$  is indeed smaller than  $\mathcal{P}(X)$ . But in what sense of “smaller than” would we have shown that  $X$  is smaller than  $\mathcal{P}(X)$ ?

Notice that although our implementation of ordered pairs has a pairing and two unpairing functions with the correct inverse properties—and it even provides for the existence of cartesian products (to keep Holmes and Mathias happy)—it nevertheless no longer ensures that the composition of (the graphs of) two relations is a (graph of a) relation. This means that our candidate for implementation-of-equinumerosity fails to be transitive, and is therefore not an implementation of equinumerosity—since equinumerosity is transitive. So we would not have an implementation of cardinal arithmetic into set theory. We would have proved something in Set Theory but that something would not be a translation of an assertion of cardinal arithmetic.

So we have proved something, a theorem of set theory indeed. It’s just that the theorem we have proved is not in the range of the translation into the language  $\mathcal{L}(\in, =)$  of set theory of an expression in the language of cardinal arithmetic

[That is to say, we can use an implementation of pairing-and-unpairing which gives different types to the two components and thereby sends “The graph of the singleton function is a set” to a theorem of NF. However, no such implementation can send to a theorem of NF the assertions “Every relation has a converse and an ancestral” nor “the identity relation restricted to a set is a set”. Constructing interpretations into NF of theories that deny these assertions is not high on our agenda.]

*[HOLE Should Say something about about how a skewed pairing function like this really isn’t a pairing function at all. Notice that merely being skewed doesn’t prevent a pairing function from supporting the existence of Cartesian products, which Randall says is part of the spec. Also we need to Say something about how a skew-pair would bugger up some stratifications and prevent some satisfaction relations from having graphs that are sets. Should spell this out for Zuhair]*

The choice of pairing functions that tradition has made for NF has resulted in our force making certain choices about which assertions about relations and functions we wish to come out true. Faced with a choice between making every set the same size as its set of singletons and ensuring that equinumerosity was an equivalence relation we decided to go for the pairing that makes equinumerosity an equivalence relation. This was certainly the correct thing to do, but can we explain why?

1. if you are trying to implement a theory of pairing + unpairing then the pairing + unpairing operations don’t have to be stratified;
2. If you are trying to implement pairing + unpairing + existence of cartesian products then it has to be stratified (but doesn’t have to be homogeneous);
3. If you are trying to implement pairing + unpairing + existence of cartesian products + relational algebra (converse + composition) then it has to be

This is true, is it not, Dear Reader? Is not the transitivity of equinumerosity a given? It’s something that set theory has to stump up, it isn’t something that set theory can choose not to prove.

This needs to be rewritten  
If sets are to be a category  
we have to have composition  
and identity!

stratified and the two coordinates have to have the same type. However this does not require that the pair be of the same type as its components.

So in NF the question is: given that we want an interpretation of NF + pairing-and-unpairing + various other nice principles (like existence of products of relations + converse etc etc) into NF ... What is the best we can do? The answer is that if our pairs are type-level then we can do existence of converses, composites, cartesian products and ancestrals, but we can't do then existence of  $\iota$  as well. This last one can be seen as in some sense ill-typed if we screw up our noses enuff. We can't do existence of  $\iota$  if we want to have composition of relations. Would this object—this set of skew-pairs—even be correctly described as the graph of the  $\iota$  relation?

So skew-pairs (as Zuhair calls them) give you that every set is stcan but it means that not all relations have squares. WK pairs or Quine pairs don't make everything stcan but they ensure that relations have squares and that all identity relations exist. It's pretty obvious which one prefers, but it's not enuff to have found the right answer, you also need a good story about why it's the right answer.

Part of the story might be that the skew-pairs are tellng you things that are outright *false*. Not everything is stcan. And Cantor's theorem in NF is not the translation of a fmla of cardinal arithmetic. All this needs to be spelt out.

#### 9.4.1 Don't look inside

I believe we can explain the difference, and that we do it as follows. There are various banalities about pairing, relational algebra and functions that we can express in a strongly typed system which not only regards the ordered pairs as having no internal set-theoretic structure, but even regards the *components* of the ordered pairs as having no internal set-theoretic structure.

For example, the assertions that the composition of two relations always exists does not require us to look inside the components of the ordered pairs. Similarly equinumerosity.

The assertion that

If  $x$  and  $y$  are equinumerous and  $y$  and  $z$  are equinumerous, then  $x$  and  $z$  are equinumerous.

—although it requires us to look inside  $x$ ,  $y$  and  $z$ —does not require us either to look at any *members* (as opposed to *components*) of any ordered pairs, nor (tho' this is an entirely separate point) to look inside any of the components of the ordered pairs that we mention.

Nothing must be allowed to override the requirement on an implementation that it respect those banalities about pairing, relational algebra and functions that can be captured in this strongly typed way.

Contrast this with the desideratum for an ordered pair function of making  $x$  and  $\iota``x$  turn out to be the same size. We will find that if we state this

properly we will be looking inside one of the components of an ordered pair—specifically to state that it is a singleton. It is worth making the point here that the expectation that  $x$  and  $\iota^{\alpha}x$  are the same size relies on an appeal to an instance of the axiom of replacement. The failure of the singleton function to be a set according to all implementations of ordered pair satisfying (2) is in fact exactly what we want. We do not want to include in our spec for the implementation of the pairing function that it should make  $x$  and  $\iota^{\alpha}x$  appear to be the same size. That is not a job for *the implementation of pairing*: that is a job for *the set existence axioms*.

The point is not that all well-typed banalities should be accommodated. It should be conceded that the existence of compositions and converses of relations does depend on set existence axioms—albeit fairly trivial ones. The formula asserting existence of transitive closures of relations is also well-typed, in that it does not require us to look inside the components of the ordered pairs it discusses. However, it does require a bit more set theory—enough to perform inductive definitions—and so one should not expect an implementation of ordered pair to automatically deliver the existence of transitive closures. The point is rather that no well-typed banality should be sacrificed as part of an attempt to accommodate a less-strictly-typed assertion (such as the existence of the graph of the singleton function) which might be thought desirable.

[thinking about this later—new year's day 2009—it seems to me that if we think an implementation of pairing-with-unpairing ought to show that every set is strongly cantorian then what we are really trying to do is to find an interpretation of set-theory-with-pairing into set theory where the interpretation of ' $\in$ ' is homophonic. Should Say something about this co's now (august 2013) i no longer know what i meant!]

I think the consideration I invoked a few paragraphs ago—that we cannot require of our pairing function that it deliver the truth (or falsehood) of any general assertion about sets, functions and relations that involve looking into the internal structure of components of ordered pairs—is completely general in the sense that analogous considerations apply to implementations of other mathematical entities.

However, these general considerations have left some points open. We have decided that the formula  $P(x, y, z)$  (whichever formula it should turn out to be) that says that  $z$  is the ordered pair of  $x$  and  $y$  must be stratified with ' $x$ ' and ' $y$ ' receiving the same type. It doesn't tell us what type ' $z$ ' should be given relative to ' $x$ ' and ' $y$ '. For example, the Wiener-Kuratowski ordered pair is perfectly acceptable in NF. We have to be more careful with Wiener-Kuratowski triples and  $n$ -tuples for higher  $n$ . The usual definition of ordered triples in the Wiener-Kuratowski style makes  $\langle w, x, y \rangle$  the Wiener-Kuratowski pair  $\langle w, \langle x, y \rangle \rangle$  where the embedded pair is Wiener-Kuratowski. This triple is unsatisfactory, since it makes ' $x$ ' and ' $y$ ' two types higher than ' $w$ '. This results in higher-degree versions of the difficulties which led us to assert (2) above. A much better solution is to take  $\langle w, x, y \rangle$  to be  $\langle \{w\}, \langle x, y \rangle \rangle$ , where once again the two pairs are Wiener-Kuratowski: this makes ' $x$ ', ' $y$ ' and ' $w$ ' all the same type. A similar manoeuvre can be used for quadruples and higher types. This

is the implementation used by Hailperin in [57].

## 9.5 Stratified pairing-and-unpairing

The Wiener-Kuratowski pair “raises types by two”: in any stratification of

$$z = \{\{x\}, \{x, y\}\}$$

‘ $z$ ’ receives a type two higher than ‘ $x$ ’ and ‘ $y$ ’. We can produce other pairs that raise types by only one. The following is due to Holmes

$$\langle x, y \rangle_1 = \{\{x', 0, 1\}, \{x', 2, 3\}, \{y', 4, 5\}, \{y', 6, 7\} : x' \in x \wedge y' \in y\}$$

defined yet?

There are eight constants that are used in this expression, and this is no accident. If  $P(x, z, y)$  is a stratifiable formula in the language of simple type theory with ‘ $x$ ’ one type higher than ‘ $y$ ’ and ‘ $z$ ’, then consider what happens in a model of TST whose bottom type has... Humph!! All this shows is that it must look inside its arguments ...

Arithmetic shows that there can’t be a pair that raises type by 1 just mentioning letters and set braces, because  $3^2 > 2^3$ : there are more pairs than subsets of a type of size 3.

It’s also really easy to show that there is no type level or lowering pair definable in TST using... arithmetic. In a finite model, merely observe that  $n^2 > n$  for  $n > 1$ , and the size of type 2 will be greater than 1.

We should note that in NFU we can even prove that there is no pairing relation  $P(x, y, z)$  where ‘ $z$ ’ is one type **lower** than ‘ $x$ ’ and ‘ $y$ ’.

Suppose there were. Let us write ‘ $\langle x, y \rangle$ ’ for the unique  $z$  such that  $P(x, y, z)$ ; then the map  $x \mapsto \{\langle x, x \rangle\}$  is an injection from  $V$  into  $\iota''V$ —contradicting theorem 2 which told us that there are more sets than singletons.<sup>2</sup>

It appears to be open how much comprehension one needs if one is to establish this result. Existence of unordered  $n$ -tuples for all  $n$  isn’t enough: observe that if the universe consists of a single Quine atom then the ordered pair of  $x$  and  $y$  can be taken to be the unique object belonging to both  $x$  and  $y$ .  $\text{pair}(x, y) = \bigcap(x \cap y); \text{fst}(x) = \{x\}; \text{snd}(x) = \{y\}$ . Something similar works if the universe consists of precisely two Quine atoms.

Randall Holmes writes:

“I do not know the history. I seem to recall that earlier work was done on it. Isn’t there a paper in which someone showed that there is no pair with a type shift less than two upward which is definable using just set braces and letters? Maybe Tarski?”

Paradox follows from a type lowering pair because the resulting theory of functions allows self-application of functions and Curry’s paradox. Details follow.

---

<sup>2</sup>This observation is in Rosser [99]. Does he give the same proof...?

Suppose  $\langle x, y \rangle$  is a pair which is  $n$  types lower than  $x$  and  $y$ . Without loss of generality we can suppose  $n = 1$ , as  $\langle \iota^{n-1}(x), \iota^{n-1}(y) \rangle$  will be a pair lowering type by 1 otherwise. For any term  $T$  of the same type as  $x$ , we can then define  $\{\langle x, T \rangle : x = x\} = (\lambda x.T)$  at the same type as  $x$ . Note that for functions  $f$  of universal domain,  $f(x)$  is definable in the usual way with  $f$  and  $x$  at the same type:  $f(x) =$  the  $y$  such that  $\langle x, y \rangle \in f$ , and since  $\langle x, y \rangle$  is one type lower than  $x$ ,  $f$  is at the same type as  $x$ . Curry's paradox follows: any function  $f$  has a fixed point  $= F(F)$  where  $F = (\lambda x.f(x(x)))$ . We can for example define a fixed point of the set complement function in this way; enough said!

My own contribution [62] was to show that there is a pair raising type by just one, but by the result noted above this actually involves some constants:

$$\langle x, y \rangle_1 = \{\{x', 0, 1\}, \{x', 2, 3\}, \{y', 4, 5\}, \{y', 6, 7\} : x' \in x \wedge y' \in y\}$$

Arithmetic shows that there can't be a pair raising type by 1 just mentioning letters and set braces, because  $3^2 > 2^3$ : there are more pairs than subsets of a type of size 3".

However, the pair that is always used in NF is the Quine pair. (see p 71.)

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

For any natural number  $n$  the set  $[0, n - 1]$  of natural numbers less than  $n$  is finite (that is to say, is in  $FIN$ ) and we prove this by induction on ' $n$ ') and its cardinal— $|[0, n - 1]|$ —is therefore a natural number by lemma 2. Do we have

$$|[0, n - 1]| = n? \quad (\text{AxCount})$$

The obvious way to prove this would be by induction. How does induction work? We prove that every natural number is  $F$  if  $\{x : F(x)\}$  contains 0 and is closed under `succ`, for then every natural number is in  $\{x : F(x)\}$ ;  $\mathbb{N}$  is the intersection of all sets containing 0 and closed under `succ` after all. For us to prove by this method that every natural number is  $F$  we require  $\{x : F(x)\}$  to be a set. Is it, in this case? The set abstract in question is

$$\{n : |[0, n - 1]| = n\}$$

and we observe that the second occurrence of ‘ $n$ ’ must be given a type two types higher than the first, so the set abstract is not stratified, does not denote a set, and the induction will not succeed. Note that the particular value 2 that appears as the difference in type levels does not depend on our choice of implementation of ordered pair.<sup>3</sup>

Where?

repetition

This is presumably in [98] somewhere. Find the proof

more detail here

Nevertheless we still have the fact we proved earlier, namely that  $\| [0, n - 1] \|$  is a member of  $\text{IN}$ . We will write ‘ $T^2n$ ’ for this member of  $\text{IN}$ . Why ‘ $T^2n$ ’ not ‘ $Tn$ ’? Why don’t we define this  $T$  function so that  $Tn = \| [0, n - 1] \|$ ? The point is that (check it!)  $\| [0, n - 1] \|$  is two types higher than  $n$  not one. For technical reasons it is more sensible to have as our defined term something that raises by one type than it is to have something that raises by two.

It is easy to check that  $T$  is actually an automorphism. We prove by induction on natural numbers that every natural number is a value of  $T$ .

The assertion  $\text{AxCount}_{\leq}$  above (that each natural number counts the set of its predecessors) was dubbed “The Axiom of Counting” by Rosser [98], who was the first to notice that NF withheld the obvious inductive proof of it.

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

## 9.6 Ordinals and the Extended Axiom of Counting

Is it not? I concede below that this is partly a matter of taste

Ordinals emerge from a theory of wellorderings and isomorphisms between them. Wellordering is not a purely set theoretic notion any more than bijection between sets is, and to discuss wellorderings in set theory we have to decide how to think of wellorderings as sets. There are two standard ways of doing this. We can

- (A) think of a wellordering of a set  $X$  as a special kind of subset of  $X \times X$ , and if we are to do this we have to make an implementation decision about ordered pairs.
- (B) think of a wellordering of a set  $X$  as the set of (domains of) its initial segments—*ordernestings*; this encodes a wellordering of  $X$  as a subset of  $\mathcal{P}(X)$ .

Do we take the possibility of (B) as (i) a demonstration that wellordering is a concept from pure set-theory? Or do we (ii) regard (B) as a way of implementing wellorderings inside set theory? My feeling is strongly for (ii) but my inability to produce any strong arguments for this position suggest to me that this matter is at least in part a question of taste, one about which reasonable people might

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<sup>3</sup>... though it does depend on our implementation of cardinal; see ??.

differ. Either way, we still have to find a way of making *isomorphisms* between *wellorderings* into set-theoretic objects and the obvious way to do this is to think of them as sets of ordered pairs, so we are going to need ordered pairs whether we go for (A) or for (B).

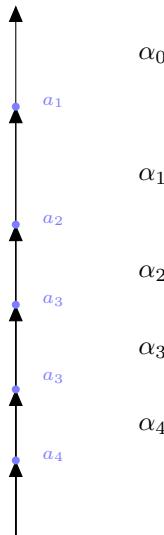
In type theory and the Quine systems the cute implementation of ordinals as wellfounded-hereditarily-transitive-sets-wellordered-by- $\in$  is not available. The point is not just that the formulæ are highly unstratified; the point is that we can exhibit wellorderings which are not isomorphic to any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

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

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

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

Say something about here about disappearing things into notation?



**THEOREM 3**  $<_{O_n}$  is wellfounded.

*Proof:* Let  $\alpha$  be an ordinal. We will show that the ordinals below  $\alpha$  are well-founded. The long arrow represents a wellordering  $\langle A, <_A \rangle$  of length  $\alpha = \alpha_0$ . If (per impossibile<sup>4</sup>) there is a family  $\{\alpha_i : i \in I\}$  of ordinals with no least member (and all of them  $< \alpha$ ) then, for each  $i \in I$ ,  $\langle A, <_A \rangle$  has a (unique) proper initial segment of length  $\alpha_i$ . For  $i \in I$  let  $a_i$  be the supremum of that

<sup>4</sup>As Prof Körner points out, a well-known Swedish-Italian mathematician.

(unique) initial segment of  $\langle A, <_A \rangle$  of length  $\alpha_i$ . Then  $\{a_i : i \in I\}$  is a subset of  $A$  with no  $<_A$ -least member.  $\blacksquare$

This result is nontrivial: it's not always true that the family of isomorphism types of widgets has a widget structure. Recall linear order types without wellfoundedness; not linearly ordered.

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

#### **DEFINITION 4 The Axiom of Counting:**

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

#### **The Extended Axiom of Counting:**

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

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

There are three assertions we have to distinguish:

- (i) Every ordinal counts the set of its predecessors;
- (ii)  $R$  and  $RUSC(R)$  (aka  $R'$ ) are isomorphic; and
- (iii) Every wellordering is isomorphic to the wellordering of its initial segments under end-extension.

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

The equivalence of (i) and (ii) is a more complex matter. Chief of these extra complexities is explaining the exponent on the ' $\iota$ '. see section ??.

Q: Which of these assertions is the one we need to add to NF to prove  $\text{Con}(\text{NF})$ ?

A:  $(\forall n \in \mathbb{N})(n = Tn)$  where ' $T$ ' denotes the usual type-raising (set-theoretic) operation.

Ward Henson, who was the first person to consider this function applied to ordinals rather than cardinals (see [59]), was properly sensitive to the difference between ordinals and cardinals, and he wrote the operation on ordinals with a ' $U$ ' rather than a ' $T$ '.<sup>5</sup> We noted that  $||[0, n-1]||$  is two types higher than  $n$ . How

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<sup>5</sup>Nowadays it usually written with a ' $T$ ', using overloading.

many types higher than ‘ $\alpha$ ’ is the ordinal of the set of ordinals below  $\alpha$  ordered by magnitude? Let’s calculate it. Ordinals are implemented as isomorphism classes (which turn out to be sets, since their defining condition is stratifiable) of wellorderings. So we consider the set of ordinals below  $\alpha$ , and we wellorder it by magnitude. This gives us a set (‘ $A$ ’ for the moment) of ordered pairs of ordinals, and we take its equivalence class (again, set) under isomorphism, and this is the ordinal we want. It will of course be one type higher than  $A$ . But what is the type of  $A$  relative to the type of  $\alpha$ ? The answer to this will depend on our choice of pairing-unpairing machinery! If we are using Quine pairs it will be one type higher than  $\alpha$ , but if we are using Wiener-Kuratowski pairs the difference will be three! Thus if we are using Quine pairs and implementing ordinals as equivalence classes of wellorderings then the order type of the ordinal below  $\alpha$  is two levels higher than the level of  $\alpha$ .

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

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

If  $\alpha$  counted the length of the ordinals below  $\alpha$  we would be able to prove the Burali-Forti paradox. Therefore any *true* (4)-like assertion about the length of an initial segment of ordinals *must* involve a  $T$ -function, and with the exponent  $k \neq 0$ . (This is in sharp contrast to the case with natural numbers, where the assertion that each natural number counts the set of its predecessors appears to be consistent—albeit strong. See section ??) The appearance of the  $T$  function here is therefore *not* an artefact of our choice of implementation for ordered pairs or wellorderings: it is a genuine manifestation of the underlying mathematics. It is true that we might not have had our noses rubbed in it had we not casually assumed there was a set of all wellorderings, but it was there all along anyway. The Burali-Forti paradox may be the *bearer* of the bad news, but it is not its author.

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

to be continued

## H I A T U S

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

PHF theorem belongs here; my theorem about ZFB belongs in the chapter on the cosmic coincidence.

## 9.7 Subversion of Stratification

(The expression is Holmes'.) Subversion of stratification is achieved by identifying things with their singletons. Allude to my BQO trick here?

Subversion of stratification: if every set is strongly cantorian then every formula is equivalent to a weakly stratifiable formula.

For what it's worth there is an analogue for sets  $x$  s.t.  $\iota^n \mid x$  exists. See [www.dpmms.cam.ac.uk/~tf/stratificationmodn.pdf](http://www.dpmms.cam.ac.uk/~tf/stratificationmodn.pdf)

## 9.8 KF is just as good as Mac

This is probably in the wrong place

One of the drawbacks of not having one's fundamental concepts sorted is that one is liable to commit fallacies of equivocation.

Sets are a much less straightforward concept than modern set theorists would have you believe, and it's not at all clear that one wants to believe aussonderung. One shouldn't forget set theory's roots in Analysis. What intuitions about set existence might those roots foster?

They would foster the entirely sensible idea that

If  $X$  is a set of implemented mathematical objects [widgets], and  $\phi$  is the implementation in set theory of a concept expressible in the language of widgets, then  $\{x \in X : \phi(x)\}$  is a set. (A)

(A) grants us enough separation to give us sets to perform whatever implementation-tasks it was for which we wanted the sets in the first place. It gives us the set of zeroes of a polynomial, the set of poles of a meromorphic function, the set of sets-of-uniqueness for Fourier series, and more of that nature. It is useful and sensible; Unfortunately it also sounds cumbersome and pedantic, so it's much simpler and snappier to say

Every subclass of a set is a set. (B)

(B) sounds like a simplified version of (A) but it's simpler only in the sense of being shorter and snappier. It's actually a lot more powerful than (A), for it grants us the existence of sets of sensible mathematical objects meeting conditions that are not themselves mathematically sensible. I have in mind sets like  $\{x \in \mathbb{R} : x \notin x\}$ . Let us call these sets *monsters*. (A) doesn't commit us to believing in the existence of  $\{x \in \mathbb{R} : x \notin x\}$ ; (B) does. It should be emphasised that it is the *intensions* that we are worried about. It might be that the monster  $\{x \in \mathbb{R} : x \notin x\}$  turns out to have the non-monstrous extension  $\mathbb{R}$ . How about the monster  $\{x \in \mathbb{R} : x \in \langle x, x \rangle\}$ ? Is this, too, just  $\mathbb{R}$ ? It may be, if we happen to have implemented pairing and the reals in such a way that this happens. One can indeed implement pairing and the reals in such a way, but that is to

miss the point. The challenge is not to extricate oneself in each case; the aim must be to never find oneself in this kind of mess in the first place. It's not a good idea to have a principle that says that this intension has a extension, and then cross your fingers and hope that the extension turns out to be sensible. The reason why these [alleged] sets are monstrous is that their existence is not required by the mathematical agenda that set theory was supposed to be acting out, and the thinking behind (A) will not lead us to think that they should be sets.<sup>6</sup>

There is an illuminating parallel here with the situation *vis à vis* the Banach-Tarski paradox. One cannot say too often that the problem with Banach-Tarski is not the axiom of choice; the problem comes with thinking of the sphere as a set. This move imports into the discussion of dissection puzzles lots of entities that the original proposers of these puzzles would never have countenanced, not have permitted in their solutions. The people who started set theory in the hope that it would help them make progress with Analysis would not welcome the set of all reals that are not members of themselves. In neither case is the novelty a useful contribution to the problem that the original parties thought they were looking at.

So how do we come to believe (B)? Is it really just by simplifying (A) and then forgetting what you did? There is probably a bit more to it than that; one can come to believe (B) by committing a fallacy of equivocation, that is to say by mistaking (B) for (A). I think that is what has in fact happened. The widely-held belief in unrestricted separation started off as enthusiasm for (A) above, that was subsequently misdirected. This fallacy of equivocation is particularly easy to commit if, deep down inside, you don't really think that set theory has a life of its own, but that sets exist only insofar as they are formulations of particular mathematical objects such as the real line. And, given set theory's roots in Analysis, that is precisely what many people do indeed, deep down inside, actually believe. And—if the only sets you are interested in are those sets that arise as implementations of those mathematical objects in which you are interested—it will be very easy for you to *think* you believe (B) when what you *actually* believe is (A). It makes so little difference to you!

One needs to think a bit about what happens if one really does go for (A) instead of (B). Can one capture this restriction by means of a syntactic device that is idiomatic in a set-theoretic context? My candidate for such a restriction is stratification. And stratification is precisely what distinguishes KF from Mac Lane.

So the thesis might as well be: KF is adequate for whatever-it-is-that-Mac-Lane-is-adequate-for. More generally, if  $T$  is a set theory adequate for mathematics then so is the theory  $\text{str}(T)$  which is  $T$  but for a restriction of its set existence axioms to stratifiable formulæ. Now comes the intriguing reflection: It is an open question whether or not KF proves the nonexistence of a universal

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<sup>6</sup>Something to think about here...set of reals satisfying an illtyped condition:  $\{x \in \mathbb{R} : x \in \langle x, x \rangle\}$  vs set of reals satisfying a purely set-theoretic condition:  $\{x \in \mathbb{R} : 0 \in x\}$ . Does this difference matter? Are they equally monstrous?

set or of a set containing wellorderings of all lengths<sup>7</sup>.

Moral: “Ordinary Mathematics” has nothing to say about whether or not such large classes can be sets.

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<sup>7</sup>The current best bet is that it doesn’t

## Chapter 10

# The Endogenous Strong Typing of Mathematics

Cardinals are a natural example to consider, but they are technically fiddly since equipollence involves the notion of an ordered pair, which is not purely set theoretic. This is more annoying than one might at first think, since it means that if we are to repeat the process we have to have a notion not only of ordered pair of set but also ordered pair of cardinal.

Let us work in a ridiculously weak set theory, so that we cannot prove for example that everything belongs to infinitely many sets. (KF-without-sumset should do). Let us say a set is green if it belongs to no more than 5 sets. Consider the relation:  $x \sim y \longleftrightarrow x \text{ and } y \text{ have the same green members}$ .

This example has been contrived so that the equivalence classes are liable not to be sets.

To enter into the spirit of the thought-experiment we will have to pretend to think that the relation of having-the-same-green-things-for-members is *such* an important equivalence relation that we will want to invent a suite of *widgets* so that we can think of two things related by this relation as partaking of the same widget. There will be a new expanded language for sets+widgets, and it has variables ranging over widgets, and—for each relation for which having-the-same-green-things-for-members<sup>1</sup> is a congruence relation—a relation symbol with widget variables for arguments. The interpretation from this new language  $\mathcal{L}'$  back into the language  $\mathcal{L}$  of set theory sends variables over widgets to variables over sets, sends '=' between two widget variables to ' $\sim$ ' and sends each new relation symbol  $\mathcal{R}$  of  $\mathcal{L}'$  (that arose from a relation  $R$  for which having-the-same-green-things-for-members was a congruence relation) to the formula of  $\mathcal{L}$  which captured  $R$ .

(Notice that we are *not implementing* widgets as sets at this stage. Granted, there is an obvious way to implement  $[x]_\sim$ —namely as  $\{z \in x : \text{Green}(z)\}$  and we will exploit it later—but not just yet.)

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<sup>1</sup>Don't ask me, I can't think of one either. It doesn't matter anyway.

Suppose we want to spice up  $\mathcal{L}'$  to a language with variables ranging over sets of widgets? We will have to have a new suite of variables to range over sets of widgets, and a symbol ‘ $\mathcal{E}$ ’ to put between variables that range over widgets and variables that range over sets-of-widgets. (Let’s use symbols in `mathfrak` for this purpose.) The interpretation back into  $\mathcal{L}$  will send ‘ $\mathfrak{x} = \mathfrak{y}$ ’ to

$$(\forall x \in X)(\exists y \in Y)(x \sim y) \wedge (\forall y \in Y)(\exists x \in X)(x \sim y)$$

It will send ‘ $X \mathcal{E} \mathfrak{y}$ ’ (where ‘ $X$ ’ is a variable ranging over widgets and ‘ $\mathfrak{y}$ ’ is a variable ranging over sets of widgets) to  $(\exists x' \in y)(x \sim x')$ .

We can now consider an equivalence relation on sets-of-widgets which is the exact parallel to our  $\sim$  relation on sets.

[the defect in this illustration is that it doesn’t give rise to a  $T$  function. One good thing has come out of it tho’: it has brought it home to me that the only equivalence relation on sets-of-widgets we can be happy with are 2-stratifiable, because  $\mathcal{E}$  is defined only between widgets and sets-of-widgets. We could define a second one between sets-of-widgets and sets-of-sets-of-widgets, but we don’t seem to be able to move *downwards* so smoothly.]

*(to be continued …)*

This move is made in section 4 of Quine *Reification of Universals* in [93]

*(or possibly tear it up and start again …)*

At some point we have to have a section on

## 10.1 $T$ -functions on an arbitrary Abstract Data Type

[This complements the section on  $T$ -functions in stratified set theory as in [45].]

Some ADTs  $\mathcal{A}$  admit endomorphisms that send an element  $\alpha$  of that type to a new element that is the type arising somehow from the family of sub- $\mathcal{As}$  of a thing belonging to  $\alpha$ . Ordinals, BFEXTs, .... Tends to arise naturally in cases where the family of all wombats admits a wombat structure. Is the power set a sort of  $T$ -function? ... on isomorphism types of APGs? Too far-fetched, perhaps. But only beco’s typically one expects  $x = Tx$  to look plausible to people who do not have type-checkers built into their midbrains. Needs to be thought about

Let’s start with the simplest and most straightforward illustration. If  $n$  is a natural number,  $Tn$  is the cardinality of the set of natural numbers below  $n$ . We notice two things immediately.

- (i) In terms of the typing discipline that arises from implementation, ‘ $Tn$ ’ has a different type from ‘ $n$ ’.
- (ii) These two entities ought to be identical.

So the generalisation we are looking for will identify a connection between objects of one type and objects of a systematically related type, and the connection will be one that we will be tempted to think of as identity.

So, let's seek this generalisation, using the  $T$  function on naturals as a point of departure. We start off with a universe of sets that has been equipped with a notion of ordered tuple, and relations thought of as sets of ordered tuples. Into the language for describing this universe we can interpret successively

- (i) a language that has variables ranging over cardinals and has predicate letters for relations between sets for which equipollence is a congruence relation; then
- (ii) a language further extended to have variables ranging over sets of cardinals and has predicate letters for relations between sets of cardinals for which equipollence is a congruence relation; finally
- (iii) a language further extended to have variables ranging over cardinals of sets of cardinals.

It is at stage (iii) that we can talk about both a natural number  $n$  (being a cardinal of a set) and  $Tn$  (which will be the cardinal of the set of natural numbers below  $n$ ) and notice that we expect them to be the same. What exactly do we mean by ‘expect them to be the same’? We mean that if we have satisfactory implementations of (i), (ii) and (iii) into the original basic language of set theory then we find that  $n$  and  $Tn$  are implemented as the same set. Why is this? Once we have implemented both the singly virtual cardinals (cardinals of sets) and the doubly virtual cardinals (cardinals of sets of [singly virtual] cardinals) as the same sets we find that  $T$  is an automorphism of the natural wellordering of this family of sets, and should therefore be the identity. ‘should’ is correct here; one might expect ‘must’ but the proof that an automorphism is the identity needs an induction and inductions need assumptions of set existence.

...thinking aloud ... so there is a  $T$  function on ordinals that arises from implementations and we expect to implement it as the identity because of the rigidity of the ordinals. There doesn't seem to be an implementation-style  $T$ -function on cardinals altho' there is a type-raising one, and we expect to be able to implement that as the identity simply because we expect every set to be stcan.

As soon as we so much as *start* to reason formally about mathematical objects we have to be fairly explicit about what sort of things our mathematical objects can do and what we can do with them.

An acknowledgement that we have to come clean, sooner or later, with answers to all questions of the kind “what can these chaps do and what can we do with them?” is honest and wholesome. However it brings dangers with it, and foundationalism is what happens when we have the mistaken thought that we have to come clean about all such questions at once; mistaken because there is always a certain amount of slack in the system, and we should delay as long as we can any pronouncements on what our objects can do: (we *typecheck lazily*).... We still haven't settled what our datatype of trees is for example, or knots.

Normal mathematical practice is quite strongly typed, and this is true despite the fact that this typing is rarely remarked on. We don't remark on it precisely because we are practical people and want to solve problems, not create problems where none exist, so why worry about typing? The typing is accordingly typically not explicit, but it is very useful nevertheless. An unspoken typing discipline enables us to disambiguate notations that would o/w be ambiguous:

- It's the natural typing of Mathematics that enables us to get away in Analysis with writing both ' $f(A)$ ' and ' $f(\pi)$ '. The point is not only that we are cued by the capital Roman letter to expect a set of reals to be in play, and that we know that  $\pi$  is a real: the point is that no real can simultaneously also be a set of reals: it's forbidden by Leviticus.
- When people say that a group is simple iff it has no normal subgroups other than itself and the unit, they aren't labouring under the illusion that 1 and  $\{1\}$  are the same thing. Quite the contrary: 1 and  $\{1\}$  are so utterly different that there is no possibility whatsoever of confusing the one with the other, so it is therefore safe to use the same notation for both<sup>2</sup>.

The typing is both syntactic and semantic.

Programming languages such as ML have type-checkers that pick up this kind of mistake faster than you can say 'Jack Robinson'. Interestingly, it's not hard to see how could could write an interface that will pass on to the user suggestions like

"I found a '1' (which is of type int) where I expected  
to see a thing of type set. Did you perhaps mean '{1}'?"

It may even be that someone has already written such a program. It's the very ease with which this error can be detected and corrected that means that we don't have to worry about the costs incurred in making it. Type-checking software of this kind will clear everything up for us.

To summarise: the point is not that we use a name for the one as a name for the other because we have difficulty telling them apart; we use a name of the one as a name for the other because it is so easy to tell them apart that we know we can rely on our interlocutors' fault-tolerant pattern matching software to tidy it up for us.

List here some uses of 'formally disjoint' points to strong typing. Typing comes in Formal disjointness is what solves the Cæsar problem. The set of numbers is formally disjoint from the set of humans.

I was thinking about group extensions the other day and my mind started to wander. Suppose we have two groups  $G$  and  $H$ . Their product,  $G \cdot H$  is also

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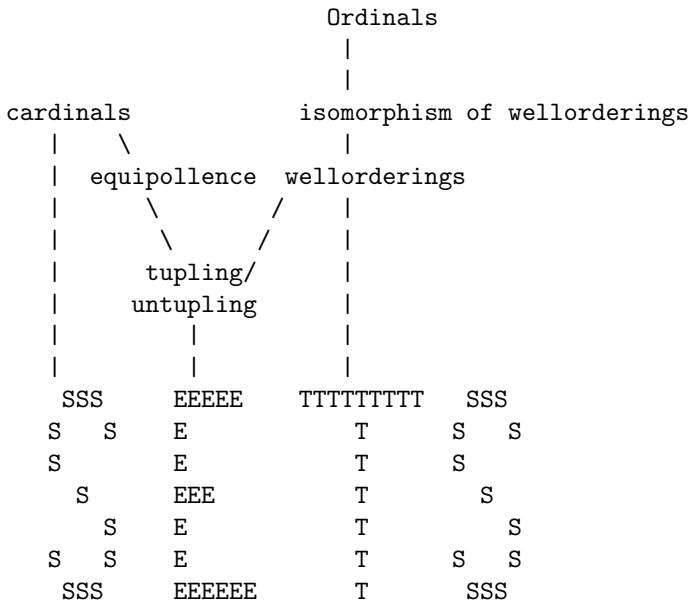
<sup>2</sup>In this connection ... the transposition that swaps  $a$  and  $b$  is customarily notated ' $(a, b)$ '. But if we are thinking of permutations as their graphs we should write ' $\{(a, b)\}$ ', or even ' $\{\langle a, b \rangle, \langle b, a \rangle\}$ '. Is there anything illuminating we can say about this?

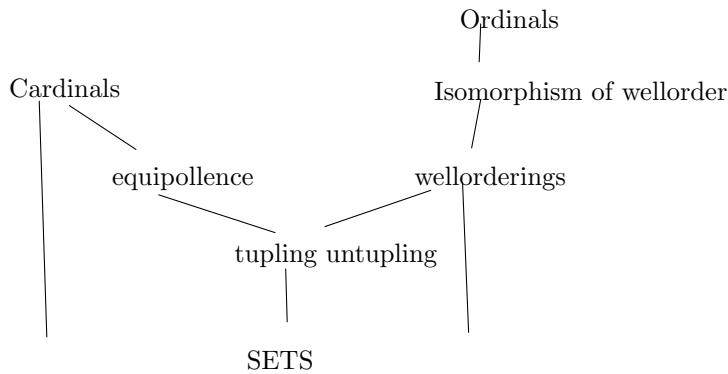
a group. (Or do we write it  $G \times H$ ?).  $G$  and  $H$  are both normal subgroups of  $G \cdot H$ . Why? Well,  $\{\langle g, \mathbf{1}_H \rangle : g \in G\}$  is a subgroup of  $G \cdot H$  which is iso to  $G$ . There is a certain amount of overloading going on: we can think of the subgroup relation as holding between concrete groups thought of as decorated sets, or we can think of it has holding between the abstract entities of which the concrete groups are manifestations/concretisations.

various levels of rigour, curiously like the interlocking network of registers supported by natural language.

Countable choice says there is a global embedding from the type `countable set` into the type `counted set`.

This has to be fitted in somewhere





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The typing of Mathematics is part of folklore, and—being folklore—is not explicitly set out anywhere. Some of the informal folk-typings are stronger than others. At one extreme we find people whose sensibilities have been so brutalised by years of writing assembler code<sup>3</sup> that they will think nothing of subjecting a variable to both arithmetic and boolean operations in one and the same breath. Most mathematicians will realise that there is something not quite nice about this but it probably won’t bother them. The feeling that the natural number 1 is a different object from the real number 1 will come naturally to many people: the second is a floating point object while the first isn’t; however the idea that natural numbers that measure lengths of *X-lists* are different objects from the natural numbers that measure lengths of *Y-lists* will appeal only to the most refined of palates. It is no part of my project to say quite where on this scale of squeamishness lies the correct notion of typing in Mathematics; my point is merely that there *is* a folk notion of typing, that it appeals to an intuition that is authentic and grounded in genuine mathematics, that it deserves to be taken seriously and finally that philosophy of mathematics must engage with it.

If you don’t think mathematics *really* is typed, but that typing is an error, then where do you think that error comes from? Is it the result of a strategy of reconfiguring our language so that we can’t ask questions to which we can’t currently find the answer, because of our profound and pervasive ignorance? Is the idea that once we discover the true nature of mathematical objects we will

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<sup>3</sup>One of students said to me years ago “Assembly-language programmers are the proletariat of the information age”.

be able to ascertain whether or not  $3 \in 5$ ? If that is the reason, why is the typing system so neat? Is it because our ignorance is systematic? and in a way which it would be illuminating to study? (see p. 158)

Where does the endogenous typing of mathematics come from? It has several sources, not all of them clearly distinct. One source is an unavowed but pervasive operationalism that identifies mathematical objects with the uses to which we put them, and draws type distinctions (lines in the sand) in order that we shall not cross them and wander into territory where we make unreasonable demands on mathematical entities by asking them to do things they cannot do. Some of it comes from a Klein-like view of Mathematics as that-which-is-invariant under something. If we take a Kleinian view of mathematics blah Tarski blah equivalence relations blah [36] blah typing.

Some people have stronger typing intuitions than others. I think mine must be on the strong end of the scale. I remember being very bothered when i first encountered trigonometrical functions at the age of about twelve. How could you get a real number out of an angle? Functions from angles to angles were obviously all right. Functions from reals to reals were all right. But angles to reals? I cannot now reconstruct the intellectual content of the puzzlement (indeed i probably could not have given an account of it even at the time) tho' the phenomenal content is still with me. Of one thing i can be quite certain: the root cause was a typing intuition that i had not properly embraced or thought through.

Violations or weakenings of the strong typing are associated with reductionist programmes like the reductionist programmes associated with set theory.

But in mathematical praxis they are associated with increased strength or complexity or expressive power. Untyped assembler language is extremely powerful. (But hard to use: “C++ has all the expressive power of assembler with all the ease of use of . . . assembler”.) Another famous example is the use of a number as both a number and code for a program in the proof of the undecidability of the halting problem.

When in Applied Mathematics do we iterate trig functions? Isn’t there a typing discipline that prevents a variable from being both a value and an argument to a trig function? Trig functions can be applied only to objects of type `angle`. Trigs are logs and we iterate logs, as Marj Bachelor has pointed out to me, but i think we iterate trigs only when applied to reals—and then they are trigs not logs. Consider the pendulum equation:

$$\frac{d^2x}{dt^2} = -k \cdot x.$$

If you solve this you find that  $t$  is a thing of type `angle`, and  $x$  is a thing of type `trigvalue`.

Does differentiating change the type of a variable? Dunno. Perhaps it doesn’t...

$$\frac{d^2x}{dt^2} = -k \cdot \sin x$$

seems less strongly typed ...

iterating trig functions. Simon Wadsley sez that there is an Analysis I exercise that iterates *cos* to obtain a fixed point for *cos*. But that's not a counterexample, beco's finding a fixed point for *cos* was unnatural already.

<https://math.stackexchange.com/questions/704904/what-does-sin-sinx-meansays>  
look at

[https://en.wikipedia.org/wiki/Frequency\\_modulation](https://en.wikipedia.org/wiki/Frequency_modulation)

In this connection we might notice:

Paris-Harrington less typed than Ramsey. See section 12.3

Let us say that  $f$  is an implementation of ordinals (resp. cardinals) if  $f$  is a total function and, for all  $x$  and  $y$ ,  $f(x) = f(y)$  iff  $x$  and  $y$  are isomorphic (resp equipollent). Equipollence classes give us an implementation of cardinals of *inductively finite sets* that raises by one type. However, it is easy to obtain implementations that actually *lower* type. NF supplies us with wellorderings of length  $\omega$  that are definable by stratified set abstracts without parameters. (Note that no such thing is possible in ZF!) One such is  $\iota\text{IN}$  where  $\text{IN}$  is the quotient of the set of inductively finite sets under equipollence. Every inductively finite set is equipollent to a unique initial segment of this, so we take the cardinal of  $x$  to be the sumset of (the carrier set of) that unique initial segment. This is an implementation of cardinals that lowers type by one.

Recall that Rosser's axiom of counting is the assertion that each natural number counts the set of its predecessors. Quite what this means in pure set theory will depend on how we implement cardinals. However, as long as our implementation of cardinals does not lower types it will be equivalent to the assertion that every inductively finite set is strongly cantorian and also to the assertion that every countably infinite set is strongly cantorian. However if we have a type-lowering implementation of cardinal-of-inductively-finite-set then the equivalence breaks down.

If there is a type-lowering implementation of ordinal then we can prove the extended axiom of counting and infer Burali-Forti. How about a type-lowering implementation of cardinal? (of all sets, not just inductively finite sets) Suppose NCI is finite. Then NC really is the size of a set of singletons of singletons, so there is a function (whose graph is a set) that sends everything to singletons and sends two things to the same singleton iff they are the same size.

Typed lambda calculus is less expressive than untyped lambda calculus.  
Assembler language is more powerful than typed languages.

(Of course in biology the apparently very strong typing that separates base sequences from amino acids disappears on close inspection. The distinction between catalyst and substrate disappears. (o/w we would never have been able to have the expression *autocatalysis*). Of course biology works in untyped lambda calculus. The theory of computable functions is ultimately untyped too. Locally there may appear to be a type discipline but globally there isn't. Biology

is written in the most unspeakably horribly hacky machine code. And—which is worse—it’s uncommented! (Since there is no intelligent interference in the process of evolution there is no reason for the code to be commented, or to be easy to discompile. Nobody has to maintain it so nobody has to read it! The code is so *ad hoc* that it couldn’t possibly have been written up from a spec; it must have emerged by trial and error. This is the most detailed argument against intelligent design i have yet come across. I hereby promise that should we ever discover that introns are actually comment lines I shall believe in God.)

In this context it’s quite striking that living organisms allow any sensible systems-theoretic description at all. The fact that they do admits such a description is not of course a watchmaker argument but merely another instance of the unreasonable effectiveness of mathematics.

If i may quote myself, from my notes on countable ordinals:

“The following fact about ordinals is of fundamental importance. For any ordinal  $\alpha$ ,  $\alpha$  is the order type of the set  $\{\beta : \beta < \alpha\}$  of ordinals below  $\alpha$  in the obvious ordering (of definition ??.) This fact is so cute that it has become the basis of the standard implementation of ordinal arithmetic into set theory. In this implementation (due to Von Neumann) each ordinal is simply taken to be the set of ordinals below it.

There is a slight niggle over this, and it concerns polymorphism. Usually we take lists to be polymorphic: for each type  $\alpha$  there is a type  $\alpha\text{-list}$ . However once we apply the `length` constructor to objects of any of these types we get objects of only the one type: `int`. We don’t get a polymorphic family  $\alpha\text{-int}$ , and nobody would normally suggest that we should. However, if one were an extreme purist one might note that, strictly speaking, Euler’s totient function (for example) is properly defined only for those `ints` that are `ints` of lists of `ints`, not on `ints` that are `ints` of lists of `wombats`, for example. However this purism is obviously extreme, since it’s pretty clear that all these types are isomorphic and we will happily make do with only one type of `ints`. This will enable us to mince that fact that for any natural number  $n$ , the set  $[0, n - 1]$  of its predecessors is of length  $n$ . As we have seen, Rosser [98] called this the **Axiom of Counting**. The axiom of counting (for `IN` at any rate) is fine; it is the extension of this observation to ordinals that is ultimately problematic. And the problem it ultimately leads to is the Burali-Forti paradox.

If we accept the transfinite version of Rosser’s Axiom of Counting then the length of any initial segment  $A$  of the ordinals is the least ordinal  $\alpha$  not in  $A$ . So what is the length of the (indisputably wellordered) collection  $On$  of all ordinals? It would have to be the least ordinal not in  $On!$  It seems that in order to avoid Burali-Forti

one has to adopt a stronger typing system that distinguishes between ordinals-from-(infinite)-lists-of-as and ordinals-from-(infinite)-lists-of-bs—for at least some as and bs. However the point at which hygiene compels one to adopt this stronger typing machinery comes a long way beyond  $\omega_1$ .”

Dfn of the ctbl ordinal  $\Gamma_n$  is not as strongly typed as ...

That’s an interesting case: the identification of particular ordinals by means of fixed points compels the source and target ordinals to be the same type, so is less strongly typed.

However the fixed-point stuff is well-typed in the NF sense. The typing discipline that arises from interpretations comes in two grades: the strong and the weak. In the weak typing there is only one type of ordinals. In the strong typing discipline ordinals of wellorderings of sets are of a different type (“ordinals<sub>1</sub>” say) from ordinals of wellorderings of sets-of-ordinals<sub>1</sub>. The types are different because they have different identity criteria.

In ML we have only one type of natural number (`int`), and a polymorphic length function that takes lists to `int`s, and all its values have the same type. The two lists `[true, false]` and `[0, 1]` have lengths of the same unitary type, `int`.

It is natural to have a monomorphic type of naturals rather than a polymorphic type because there is a canonical way of identifying naturals across the type boundaries. In contrast there is no natural way of identifying the reals that arise from preference relations over servings of chocolate mousse with those reals that arise from preference relations over servings of ice cream. This may be no more than the fact that the reals as an ordered set (or even an abelian group under addition) have lots of automorphisms but the ordinals do not. In that respect they are like the naturals: there is a canonical way of identifying ordinals across types.

## 10.2 Lofty Indifference

In the world of set theory there are only sets, so if we want to talk about numbers or functions or other mathematical objects we need sets that are *simulacra*—implementations—of those mathematical objects. It is clearly important that it is unimportant how the simulacra work as long as they do in fact work<sup>4</sup>. Usually a choice of implementation is indeed unimportant as desired, in that nothing seems to hang on it: typically an implementation is chosen in chapter one and then promptly forgotten.<sup>5</sup> However there are times when it appears to matter a great deal, and then we have to go back and think again about what might be the criteria for an implementation to be good. Since these extreme times are

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<sup>4</sup> “It matters not whether the cat is red or white as long as she catches mice”—Deng Xiaoping.

<sup>5</sup> If you are going to forget it as soon as you’ve done it then why do it? Because what is important is not the way in which it is done but the fact that it can be done at all.

rare, we have not had to think about this problem very much. Some recent—and very elementary—developments in set theory have brought this question to the fore once again.

Remark 1 in section 1 of Chapter 2 of [?] reads (with three passages omitted)

*On peut considérer la notion de couple comme un signe fondamental [...].*

*Mais on peut aussi exprimer la notion de couple à l'aide des autres signes fondamentaux [...]: il suffit de prendre comme définition de  $(x, y)$  l'ensemble*

$\{\{x\}, \{x, y\}\}$

*[ldots]. Toutefois cette seconde méthode met l'accent sur un aspect de la notion de couple qui est parfaitement dénu d'intérêt. Il est donc préférable de s'en tenir à la méthode adoptée plus haut, la seule et unique question ayant une importance mathématique étant en effet de connaître les conditions pour que deux couples soient égaux.<sup>6</sup>*

Godement asserts in his *Cours d'Algèbre* [54] that given the axioms for set theory that he supplies in the first part of his book then—however one implements ordered pairs—one can always prove that  $x \times y$  exists.

Soient  $X$  et  $Y$  deux ensembles; on peut démontrer (À l'aide des méthodes du §0) qu'il existe un ensemble  $Z$  caractérisé par la propriété suivante: pour que l'on ait  $z \in Z$ , il faut et il suffit qu'il existe  $x \in X$  et  $y \in Y$  tels que  $z = (x, y)$ .<sup>7</sup>

(ARDM sez:

He doesn't give a proof; wisely, as the statement, made at that stage of development of his system, is false (see my discussion in "Bourbaki and the scorning of logic", section C, page 17 of logban44.)

In section 3 he introduces a scheme of union in which the scheme of replacement is embedded; so at that point his statement becomes true.)

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<sup>6</sup> "The notion of an ordered pair may be taken as a fundamental sign [...].

But the notion of an ordered pair can also be expressed in terms of the other fundamental signs, [...]: we could define  $(x, y)$  to be the set

$\{\{x\}, \{x, y\}\}$

[...]. However, this second method emphasizes an aspect of the notion of an ordered pair which is totally devoid of interest, and it is therefore preferable to keep to the method adopted above. The one and only question of mathematical importance is to know the conditions under which two ordered pairs are equal.

<sup>7</sup> It's also on page 51 of the English language edition, at the start of section 2 of Chapter 2:

Let  $X$  and  $Y$  be two sets. It can be proved (using the methods of §0) that there exists a set  $Z$  characterized by the following property: in order that  $z \in Z$  it is necessary and sufficient that there should exist  $x \in X$  and  $y \in Y$  such that  $z = (x, y)$ .

As it happens Godement is mistaken: the axiom scheme of replacement is needed if one is to sustain this stance of *lofty indifference* to choice of implementation of ordered pair, but his system does not include replacement.

Mathias supplies a proof of this striking and inconvenient fact. I will say no more about Godement’s mistake here: the question of how the *Bourbachiste* school in France came to make this mistake—and others like it—is an interesting question in its own right, and one to which Mathias has applied himself ([81], [82], [83]) to some effect, but it is not a *mathematical* question so much as a question in the sociology of Science, and as such is not our concern here. I am concerned with Godement only to the extent that I want to record his [54] as a place where an “ordinary mathematician” (i.e., not a sad warped logician such as your humble correspondent) makes the point that our mathematics should not be in any way affected by the way we choose to implement ordered pairs. The point itself seems uncontroversial enough *prima facie*, but I can imagine that it will be contested in the light of what I have to say below, and I want to get on the record the fact that it is the kind claimed by—at least some—“ordinary mathematicians”.

Mathias then comments:

“To an algebraist, that might be true. But to a set-theorist interested in doing abstract recursion theory, it is very natural to ask whether a given set is closed under pairing. For that reason an economical definition of ordered pair is desirable, such as is furnished by Kuratowski’s definition: otherwise one might find that the class of hereditarily finite sets is not closed under pairing, or even that no countable transitive set is [...]”

I take Godement to mean that he doesn’t care how ordered pairs are implemented, he cares only that the implementation be fit for purpose. That’s why you need to be able to individuate them (which he says) and then of course you need to be able to take them apart and put them together again (which he doesn’t say, presumably because he thinks that’s too obvious to need stating). I don’t actually care very much whether or not that’s what he really means; it would be a sensible thing for him to mean, so I shall assume he meant it. One should be nice to the French.<sup>8</sup>

It is Mathias’ comment that interests me. Exactly what is it that the set theorist cares about, according to him? He mentions the endeavour of doing abstract recursion theory—in set theory. As he says, it is natural to ask whether or not a given set is closed under pairing. This is certainly true in the sense that it matters a great deal whether there is a pairing function under which the given set is closed: if there is one, then one can use it. If not, one is stuck. But an ordered pair of sets is not *prima facie* a set, and of course the same concern arises in connection with the encoding of any of a host of other important and relevant entities that are not *prima facie* sets—such as sequences or formulæ.

Is Mathias saying that it is mathematically important what the pairing function is? I don’t *think* that is what he means. I think that—at least if suitably

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<sup>8</sup>By the time this book appears—if it ever does—it will be more than two hundred years since we were last at war with them.

prodded—he would say that what is mathematically important is the question of whether or not there are ways of coding ordered pairs and sequences *etc.*—simple enough to be manipulable within the constraints of the typically fairly restricted syntax in play in these circumstances—in such a way that  $V_\omega$  or  $V_{\omega+\omega}$  or  $H_{\aleph_1}$  are closed under the formation of these objects. [And all these questions actually boil down to fairly simple questions about cardinality]

As work of Mathias (anticipated in faint hints in Forster [36]) has shown, a stance of lofty indifference about implementations of ordered pairs (indeed about implementations in general) cannot be distinguished from a belief in the axiom scheme of replacement. Since in many of the situations where Mathias works one does not have the full scheme of replacement it might matter a great deal which implementation of (as it might be) tupling-with-untupling one uses. For example, one might be unable to employ any tupling-with-untupling function that is not stratified, or not homogeneous, or not  $\Delta_0$ . But it is still the case that *as long as one has one that works* it doesn't matter which one one actually employs. In this setting the mathematically important question is whether or not there is available an implementation of (as it might be) tupling-with-untupling that is compliant with the special constraints imposed in the wake of the restrictions on replacement. (Questions like this have arisen in the past. Rosser [99] showed years ago—at a time before Specker had shown that the axiom of infinity was a theorem of Quine's NF—that NF admitted a homogeneous (“type-level”) ordered pair iff it proved the axiom of infinity.)

Normally decisions about which flavour of ordered pair one is to use are regarded as implementation questions not as mathematical questions. That is, if their nature is ever examined at all. It seems to me that although Godement is surely right: the question of *which implementation one uses* is indeed (as he claims) not a mathematical question; in contrast the question of *which implementations are available* is very much a mathematical question, and a mathematical question of some importance. Might this be what Mathias really means? Is this another region where the frontier between Mathematics and Metamathematics is being redrawn?

This equivalence is quite general.

Implementing entities of type  $A$  in an environment where there are supposed only to be entities of type  $B$  is a common task not only in mathematics or computer science (which is where the “implementation” terminology comes from) but is a task confronted by anyone with a reductionist strategy of any kind anywhere in philosophy. And in that more general context one sees—albeit much less clearly—the same phenomenon of type distinctions that arise in the elementary setting of implementations in set theory of other mathematical entities. Implementations always give rise to typing disciplines. Consider the case of ordered pairs for illustration. The intuition to which Godement is giving expression is that information about the set-theoretic structure of an ordered pair should be irrelevant to its job as an ordered pair. This gives rise to the following typing system. There are two types: **set** and **pair**.

- Any variable that appears to the right of an ‘ $\in$ ’ must be of type **set**;

- Any variable that appears
  - (i) to the right of an expression of the form ‘ $\langle x, y \rangle =$ ’ or
  - (ii) to the left of an expression of the form ‘ $= \langle x, y \rangle$ ’

must be of type **pair**.

- Any variable occupying the blank space in

$$\begin{aligned} x &= \text{fst}( ) \quad \text{or in} \\ x &= \text{snd}( ) \end{aligned}$$

must be of type **pair**.

... with the effect that in no well-typed formula can ‘ $\in$ ’ appear immediately to the left of an expression like ‘ $\langle x, y \rangle$ ’. Notice that there is no suggestion that formulæ violating this condition are *illformed* and therefore not proper formulæ but mere strings; the suggestion is merely that they are *ill-typed*. We will soon see circumstances in which we need to consider such expressions and reason with them, but these circumstances are rather special.

What we are really doing is interpreting the language of set-membership-plus-pairing-and-unpairing into the language of set-membership-*tout court*. There is usually a completeness theorem in these cases:

**A formula is well-typed iff its truth-value is invariant under all implementations.<sup>9</sup>**

The stance of *lofty indifference* that I spoke of above is neither more nor less than a belief in this completeness theorem. It turns out that, for many of the suites of entities that we wish to implement in set theory, the completeness theorem is equivalent to the axiom scheme of replacement.

The easy direction of this biconditional is the right-to-left direction: it’s the left-to-right direction that is hard. The significance of Mathias’ observation is that—in the case of pairing functions—it gives an easy proof of the hard direction.

Let us take an illustration from some mathematics that is more familiar. Euler’s totient function.  $\phi(n)$  is  $|\{m < n : (m, n) = 1\}|$ . This object— $\{m < n : (m, n) = 1\}$ —is a set. Indeed it will be a pure set. (A pure set is a set whose transitive closure contains nothing but sets.) Quite which pure set it will turn out to be will depend on how we have implemented natural numbers as sets, and we don’t want to have to think too much about which pure set it is. Altho’ we don’t want to care about which set it will be, we definitely do not want the cardinality  $|\{m < n : (m, n) = 1\}|$  to depend on our implementation.

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<sup>9</sup>We have to be very careful how we state it: the assertion that everything is an ordered pair is on the face of it well-typed, but its truth-value seems to vary with implementations. We may have to state something like *invariant for surjective implementations* or *invariant for non-surjective implementations*. Or, again, perhaps the correct notion is not-having-truth-value-changed-by-composing-the-implementation-on-the-right-with-a-permutation-of-the-universe-of-sets.

To banish these two worries we want to ensure that, for any finite set  $x$ , all the sets  $\{m < |x| : (m, |n|) = 1\}$  that we obtain as we vary our implementation of natural number turn out to be the same size. What we want to ensure is that, for any finite set  $x$ , the set of pink-flavoured cardinal numbers of subsets of  $x$  is in one-one correspondence with the set of blue-flavoured cardinal numbers of subsets of  $x$ . As long as the collection of pink naturals and the set of blue naturals are both definable (even with parameters) there will be a definable bijection between them—it will be what set theorists tend to call a *function class*. To banish our worries earlier in this paragraph what we need to know is that all restrictions of this function class to subsets of the pink or of the blue naturals exist. This is because if the set  $\{m < n : (m, n) = 1\}$  of pink naturals is to be the same size as the set  $\{m < n : (m, n) = 1\}$  of blue naturals then the obvious bijection must be a set. Otherwise the two sets will be seen to be the same size only from outside.<sup>10</sup>

But surely we can prove that the pink finite ordinals are isomorphic to the blue finite ordinals? They are both  $\aleph_0$ -like total orders, and any two  $\aleph_0$ -like total orders are isomorphic as posets, and we can prove this even in KF.<sup>11</sup> Isn't that enough? No! They aren't just  $\aleph_0$ -like total orders, but expansions of  $\aleph_0$ -like total orders obtained by adding an ordinal-of function. The isomorphism that we obtain between pink and blue finite ordinals cannot be relied upon to send the pink ordinal of  $\langle X, < \rangle$  to the blue ordinal of  $\langle X, < \rangle$ . The problem is not that one of the steps (base case or induction step) fails, the problem is that the induction might be illegitimate because of a failure of set existence.

But surely we can prove the thing we want by induction on finite sets. The set of blue cardinals of things-that-inject-into-the-empty-set is the same size as the set of pink cardinals of things-that-inject-into-the-empty-set, and the induction step is straightforward enough.

We could tell this story humorously by spinning a fantasy in which God (pace Kronecker) gets fed up with doing cardinalities and decides to privatise them. Or he gets leant on by the Tories. So there is the Pink-Cardinal company and the Blue-Cardinal company and all the rest of them, and of course there is the regulator: *Ofcard*. *Ofcard* licenses a cardinal-provider only if it has a function  $f$  (defined on all sets) for which equipollence is a congruence relation. (More correctly:  $f$  is a classifier for equipollence—which is not the same). That's

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<sup>10</sup>This is in conflict with some remarks on p ???. Ah! We don't need replacement for this because the two things we want to prove to be isomorphic are *sets* and we can obtain the isomorphism by separation.

<sup>11</sup>There is a discussion that this motivates. KF proves this, but only in the sense that it proves an *implementation* of it: it proves it for functions implemented as sets-of-WK-ordered pairs. It doesn't prove it for all implementations. It can't of course, because replacement fails. We could say that it proves  $\Diamond(\text{any two } \aleph_0\text{-like total orders are iso})$ . So let us write ' $\Diamond\phi$ ' for "There is an implementation in which  $\phi$ '. This is no good, because ' $\Diamond$ ' cannot be pulled up past ' $\wedge$ ':  $\Diamond\phi \wedge \Diamond\psi$  doesn't imply  $\Diamond(\phi \wedge \psi)$ . So perhaps we should take  $\Diamond\phi$  to be "It is provable that there is an implementation in which  $\phi$ '. It may be that ' $\Diamond$ ' can be pulled up past ' $\wedge$ ':  $\Diamond\phi \wedge \Diamond\psi \rightarrow \Diamond(\phi \wedge \psi)$

for starters: clearly all providers must agree on when two sets are the same size. One of the other things Ofcard wants to ensure is that for any two providers **Pink** and **Blue** of cardinals and any set  $X$  of cardinals there is a bijection between the set of **pink** cardinals of members of  $X$  and the set of **blue** cardinals of members of  $X$ .

The foregoing is a jumble:

I now think that what is going on is this. In a theory  $T$  we cannot of course prove that all implementations of arithmetic are isomorphic. This isn't a point about replacement, it's a point about there being lots of different theories of arithmetic. However can prove that any two implementations of *the arithmetic-of-T* are isomorphic. This is because of separation: the isomorphisms are just subsets of  $\mathbb{N} \times \mathbb{N}$ . What about widgets of unbounded character? Presumably we can prove in  $T$  that any two implementations of the ordinal-arithmetic-of- $T$  are iso. It seems that replacement has no rôle. But there might be something with funny illtyped things like the axiom of counting. This is still unclear to me.

Here is another way to argue for replacement. We wish to be loftily indifferent. Lofty indifference in fact demands more than mere elementary equivalence for the several implementations of wombats that we get: we actually want isomorphism. Think about entities arising from equivalence classes, for the moment (for the sake of simplicity) as it might be cardinals. We want there to be a function that, for all  $x$ , sends the green cardinal of  $x$  to the red cardinal of  $x$ .

Might be worth spelling out exactly what is to be preserved ...

If we have WK pairs and power set we can do it locally without replacement

Of course we don't expect this function to be a set (ZF will certainly prove that its graph is a proper class) but we do want it to be a set locally. How do we obtain it? Consider the function that sends each set  $x$  to the pair  $\langle |x|_g, |x|_r \rangle$  of the green cardinal of  $x$  and the red cardinal of  $x$ . The range of this function is an isomorphism between red cardinals and green cardinals. Replacement will tell us that this is a set locally.

If we want to express this without quantifying over implementations there is a trick that we can use, at least in the cases of ordinals and natural numbers. If there is an isomorphism between green natural numbers and red natural numbers then, for every finite set  $x$ , there is a bijection between the green-natural-numbers-below- $|x|$  and the red-natural-numbers-below- $|x|$ . Now the natural numbers have this curious feature that, for any natural number  $n$ , the set of natural numbers below  $n$  is also a finite set and its cardinal will be another natural number, which I shall write ' $Tn$ '. If we want all implementations of natural numbers into our set theory to be isomorphic we are requiring that  $Tn$  should depend only on  $n$  and not on the implementation. One way of ensuring this is to require that  $Tn$  should actually be identical to  $n$ . This is Rosser's **Axiom of Counting** from [98].

Clearly the converse holds too: if the red natural  $n$  and the green natural  $n$  both have  $n$  predecessors then at any rate they have the same number of predecessors. Then if we send each red natural number  $n$  to the green natural

number with the same number of predecessors this bijection will in fact be an isomorphism—in the sense that it preserves the “cardinal-of” relation.

It is easy to be distracted by superficial similarities between this observation about lofty-indifference-and-isomorphism and other more familiar ideas.

1. The point is not that the cardinals are categorical. Or that they aren’t! If the natural numbers of a model of  $T$  are nonstandard then they are nonstandard whatever the choice of implementation. (Quine’s expression “numerically insegregative”).
2. The second point that this sounds like is Gödel’s point about capturing the second-order categoricity of the cumulative hierarchy inside ZF. However Gödel’s point is an argument for replacement only in the context of an endeavour to axiomatise the cumulative hierarchy.

What kind of implementations are we allowing? Do we not have to insist that the graph of the “cardinal-of” relation has to be a set?

For formulæ that are not well-typed according to this scheme we never care about the truth-values that an implementation gives to them. Or hardly ever: there are special circumstances where we care a great deal, or at least it seems that we have to. Let us bundle up into the next section a discussion of those cases, so we can get them out of the way.

## 10.3 Sometimes it matters—or appears to matter

*Here we need a section that discusses the appearance of pairing functions in things like proofs of Hartogs’ theorem, finite axiomatisations of set theories, finitability of schemes. The general theme here is that we get a different proof for each implementation, and the uniformity that there is to the suite of proofs is visible only in the expanded language.*

### 10.3.1 Gödel’s Finitisation of $\Delta_0$ Separation

The best-known and simplest case where it matters what our implementation of ordered pair is comes in Gödel’s proof that that  $\Delta_0$  separation can be finitised. Gödel shows that there are finitely many operations such that a set closed under them (and satisfying certain other conditions which do not concern us here) is a model for  $\Delta_0$  separation. The usual proof relies heavily on the fact that the implementation being used is the Wiener-Kuratowski pair. How can this be? The claim that is being made—namely  $\Delta_0$  separation—is an assertion purely in the language of set theory, with no mention of pairing and unpairing. How on earth can it matter which flavour of ordered pair we use?

What Gödel actually proves is that, if the domain of sets is closed under a certain finite family of operations  $\mathcal{F}$  then, for  $\phi \in \Delta_0$ , and all  $A$ , and all tuples  $y_1 \dots y_k$ , the set

$$A \cap \{\langle x_1 \dots x_n \rangle : \phi(x_1 \dots x_n, y_1 \dots y_k)\}$$

exists. This is proved by structural induction on  $\phi$ . We define  $n$ -tuples in such a way that the 1-tuple  $\langle x \rangle$  is precisely  $x$ . Then the  $n = 1$  case tells us that

$$A \cap \{x : \phi(x, y_1 \dots y_k)\}$$

exists, for all  $A$ , for all  $k$ -tuples  $\langle y_1 \dots y_k \rangle$  and all  $\phi \in \Delta_0 \dots$  which is what we are trying to prove. However we can obtain it only as a corollary of the more general higher-degree claim. This is because of the possible presence of quantifiers inside  $\phi$ . The quantifiers are admittedly bounded, but there is no bound on the number of bounded quantifiers that there might be.

[*HOLE If there were no quantifiers at all we wouldn't need this excursion through  $n$ -place relations. For example, think of the finite axiomatisability of NF0.]*

The proof by structural induction on  $\phi$  involves various cases for the assorted connectives and the two restricted quantifiers. The inductive steps involve operations on sets of ordered tuples: higher-degree converses of various kinds. The  $\mathcal{F}$ s are so chosen that closure under them is enough to deal with the various cases of the induction. In none of this does the choice of implementation of ordered pair matter.

For each  $\Delta_0$ -implementation of tupling and untupling, we have a finite axiomatisation. We can set up this axiomatisation entirely in the language of set theory. (If we couldn't there wouldn't be much point, after all!). Then, for each of these finite axiomatisations, we find—lo and behold! surprise surprise—that it implies all instances of  $\Delta_0$ -separation. If we try this with several of these axiomatisations—having carefully thrown away all references to tupling and untupling by carefully casting everything in an unembellished language of set theory—we might eventually start to get suspicious and ask how it happens. For each of these axiomatisations we can give an explanation of why it is an axiomatisation, and that explanation will involve making the tupling implementation explicit.

It's worth giving a little thought to what happens when we do this. We are doing more than merely deriving each instance of  $\Delta_0$  separation. What we do is point to the implementation, and then elaborate a proof by structural induction that every instance of  $\Delta_0$  separation is a theorem. So we are actually proving a result in a metalanguage: every instance of  $\Delta_0$  separation is a consequence of this set of axioms. Indeed we can do this simultaneously for all the implementations that arose in this way from implementations of tupling and untupling, so we can prove a theorem that says:

**THEOREM 4** *Whenever we have a  $\Delta_0$  implementation of tupling and untupling, then the result of writing them into the following axioms [which will use tupling and untupling] will give an axiomatisation of  $\Delta_0$  separation.*

But now the allusions to tupling and untupling functions disconcert us much less, since it is now clear that we are proving a result in a metalanguage, and in a metalanguage we expect to find new terminology and new vocabulary.

### 10.3.2 Finite Axiomatisability of NF

This is a case very similar to section 10.3.1. We seem to need to prove that  $\{\langle \vec{x} \rangle : \phi\}$  is always a set, even tho' what we actually want is an assertion that says nothing about ordered pairs at all. This seems to contradict the conservativeness of the extension of set theory that the typed language-with-pairing+unpairing represents. But it doesn't contradict conservativeness at all: we don't prove a theorem about the original language by reasoning *in* the extended language, we prove it by reasoning *about* the extended language. So the basic fact is that the set theory in the typed extended language is finitely axiomatisable. We then obtain the finite axiomatisability of NF by deciding on an implementation.

We prove that, if  $\phi$  is an axiom of NF then, for any implementation of tupling, there is a deduction of  $\phi$  from the finite list of axioms. Therefore, by the completeness theorem, there is a deduction of it from the axioms.

### 10.3.3 Hartogs' theorem

Much of the same seems to go for Hartogs' theorem.

Hartogs' theorem is a theorem in the language of set theory + pairing-with-unpairing + worders + functions. It asserts that, for any set  $x$ , there is a wellordered set  $y$  which cannot be injected into  $x$ . (It is not a theorem of pure set theory because it employs the ideas of *wellordering* and *injection* which are not purely set-theoretic.) The statement of the theorem makes no reference to ordinals, and yet ordinals feature essentially in the proof. In this respect it resembles the finitisation result of the previous section (section 10.3.1), which established a theorem about pure set theory (without pairing) by essential use of pairing-with-unpairing. The set  $y$  we exhibit which cannot be injected into  $x$  can be seen to discharge its duty only once we see it as a set of ordinals.

Given the set  $x$ , consider the collection  $z$  of ordinals that are the lengths of wellorderings of subsets of  $X$ . If our implementation of ordinals is sufficiently well-behaved and we have sufficient comprehension principles then this object will indeed be a set. This set  $z$  is an initial segment of the ordinals, and is naturally wellordered by magnitude and so—by the Extended Axiom of Counting—its length in that ordering is the least ordinal not in  $z$ . So  $z$  cannot be the size of any subset of  $x$ ; it is indeed the set  $y$  that we have been seeking. Notice that we do not need to know how we implement ordinals for this construction to work. Nevertheless the witness we exhibit (to the claim that there is a wellordered set too large to be injected into  $x$ ) does its job only in virtue of being a set-of-ordinals-(under some implementation or other).

There are proofs that conceal the rôle of ordinals, or at any rate go some way towards concealing it. One can construct the transitive collapse of the (wellordered) set of isomorphism classes of wellorderings of subsets of  $x$  and then argue that this (wellordered) transitive set cannot be injected into  $x$ . However to construct this set requires replacement, and it is hard work to prove that it cannot be injected into  $x$ . Peter Johnstone supplies a proof like this in [63].

There is a way of proving Hartogs' theorem without talking about ordered

pairs, and I shall exhibit it, since it evades (one is reluctant to use the word ‘solves’) whatever problems accompany the need to think about implementations of ordered pairs. A wellordering of a subset  $X'$  of  $X$  is a family of subsets of  $X'$ , totally ordered by inclusion and satisfying various special conditions [...]. We can define what it is for two such wellorderings to be isomorphic

This really does need to be spelled out!

(again, without talking about ordered pairs). We then consider the set  $\Omega(X)$  of isomorphism classes of these wellorderings. This is in one-one correspondence with the ordinals of wellorderings of subsets of  $X$ . Indeed we can think of it as the set of such ordinals. We now appeal to the extended axiom of counting to say that since it is an initial segment of the ordinals the length that it has—as a set of ordinals—must be the first ordinal not in it. But that ordinal is the first ordinal not the length of any wellordering of any subset of  $X$ . But it is the ordinal of a wellordering of  $\Omega(X)$ . So  $\Omega(X)$  is not the size of any subset of  $X$ . However it is clearly wellorderable. So we have found a wellordered set too big to be injected into  $X$ .

I want to record here—for use later on—that at no point in this do we use replacement. However we have been reasoning explicitly about ordinals, and this is not a theorem about ordinals. Accordingly the interpolation lemma will tell us that there is a more parsimonious proof. (We have also been using the Extended axiom of counting.) [HOLE Is there a connection here to be made with the expressibility in prop'l logic of the pigeonhole principle? There is a proof but you can't see why it is a proof ... ?]

Should clarify that we can set up this discussion—and make the very same points—about proving Hartogs’ in a spiced-up set theory that has primitive pairing-and-unpairing.

Interpolation tells us that there must be proofs of Hartogs’ that don’t use ordinals. Indeed there are—lots! One for every implementation of ordinals into sets. But Set Theory can’t see that these proofs are all the same proof. Another one in the eye for Set Theory!

I think this is what one wants to say about Hartogs’: there is one definitive proof of it, but it is not a proof in Set Theory with pairing + unpairing. Pure set theory has lots of proofs of Hartogs’, but none is any better than any other, and they are all different. There is one for each classifier for the equivalence relation of order-isomorphism of wellorderings. The unity of this family is not visible in Set Theory with pairing + unpairing. Of course the richer your theory of sets + pairing + unpairing the more proofs you have. There are proofs in ZF that won’t run in Zermelo, and [probably] proofs in Zermelo that are not in MacLane [tho’ i can’t think of one off the top of my head]. In KF there are no proofs at all.

# Chapter 11

# Operationalism

*Irrefragibility, thy name is Mathematics. Mathematics is where the proofs are. Scientific standards have turned austere indeed [...] if anyone is to fuss about foundations [for her]. Where might he find foundations half so firm as what he wants to found?*

Quine, The Foundations of Mathematics in [97] p 22<sup>1</sup>.

We start by rubbishing foundationalism

‘Diagnosis’ is a terribly loaded word, associated generally with a desire to make pronouncements from a position of power and superior knowledge, as when the parent accuses the discontented child of being paranoid: “this is not an insult, it is a diagnosis”. Unless the person being diagnosed *presents* (as I learnt to say when I was an EEG technician) then it actually *is* an insult—or perhaps the words ‘intrusion’ and ‘impertinence’ are better … as when Israel’s supporters “diagnose” her detractors as *antisemites*, or when the Soviet Union diagnosed dissidents as having ‘political schizophrenia’ and thoughtfully provided them with special psychiatric hospitals. These are definitely not the models that i want to follow! The people in the grip of the errors i want to write about here do not regard themselves as being in the grip of error, and they aren’t seeking my help. I am careful not to address myself to them; I address myself rather to the bystanders, the people who suspect that foundationalism is not, perhaps, all it is cracked up to be, and perhaps even has a few deficiencies one might profitably look at, and who might be in the market for a few thoughts from an outright sceptic. I am also addressing the *students* of the people who are in the grip of these errors, for whom there may be hope, beco’s they might not yet have been comprehensively gripped themselves.

Dr Johnson on foundationalists:

“ Is not a foundationalist, my Lord, one who looks with unconcern on a man struggling for life in the water, and when he has reached ground, encumbers him with help? ”

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<sup>1</sup>The last line even scans.

The parallel here is that foundationalism doesn't help you do Mathematics; it might (if you are in the grip of the error of mistaking the search for truth with the search for certainty) help you feel better about it. *Encumbers you with help*, indeed.

This parallel deserves to be explored further. The error of thinking that Mathematics needs foundations (when all it needs is rigour) is parallel to the error of thinking that we need certainty (when actually what we need is wisdom). Richard Rex has a theory about how the search for certainty (which in our tradition at least, seems to first raise its ugly head with Descartes) actually goes back to theological questions about certainty of salvation. Apparently we have Luther to thank for it.

## 11.1 Antifoundationalism

I want to be brief, as brief as possible. I want to free myself, my students and my readers from foundationalism (particularly about sets) but without presuming to even try to start a new ism. Far too often the project to free the world from a particular error takes on a life of its own. It was said that The Prophet merely wanted to free his people from the errors of Polytheism and had (initially at least) no desire to set up a new montheistic religion. More recently—and nearer home—the project of Russell, Moore, Ramsey and co<sup>y</sup> to free themselves and us from the cloying vapours of German Idealism became the project of Analytic Philosophy which, more than 100 years later, is—like German Idealism itself—still with us, and busily manufacturing errors on its own account.

Actually foundationalism is not so much a philosophical position as a cult. Sometimes I worry that, next time a naked-eye comet comes along (remember Hale-Bopp? [https://en.wikipedia.org/wiki/Heaven's\\_Gate\\_\(religious\\_group\)](https://en.wikipedia.org/wiki/Heaven's_Gate_(religious_group))) they will commit mass suicide. They'll hop on the comet and it will dock at the Great Conveyor belt that is the cumulative hierarchy which will then power them all the way up to Their Maker. This is no laughing matter: some of these people are my friends.

Synopsis.

How did it happen? set theory in 1960's. Formalisation good; foundationalism bad; most mathmos aren't foundationalists even if they think they are.

Crazy rather than contentious

There is more to metamathematics than foundationalism. I am not having a go at metamathematics. Metamathematics is part of mathematics after all.

The phlogiston problem

Foundationalism is untrue to mathematics: we mathematicians need to rid ourselves of it beco's it doesn't help us do mathematics.

How to rid yourself of it—parallels with tobacco. The first thing in giving up tobacco is to recover the sense of disgust that you felt when you first inhaled the stuff.

## 11.2 How did it ever start?

The first of the many errors that mark the human condition was the error of mistaking the search for certainty (bad) for the search for knowledge and understanding (good). This error was probably committed as soon as we became aware of the fact that we were seeking knowledge and understanding. The first product of this error was religion (well, monotheistic religion at any rate). Radical scepticism is another product, and blah foundationalism a response to radical scepticism [must think this thru'] Foundationalism is an answer to the wrong question. And any answer to a wrong question is always likely to be unhelpful. I wouldn't start from here; i'd start from over there.

Try this:

The error of thinking that *the search for knowledge and understanding* was really *the search for certainty* made it possible for religion to distract us from the proper concern of large-brained omnivores, namely *natural history*. Religion is the first fruit of this error, but it's by no means the only one. Worries about radical scepticism, and later an inclination towards foundationalism are later unwelcome developments of the same stamp. The Cæsar problem is a kind of radical scepticism.

Arguments for formalisation (“formal methods” in the CS community) can sometimes be mistaken for arguments for foundationalism. The reasons why formal methods are useful in Computer Science parallel exactly the reasons why formalisation is useful in mathematics. Formalisation puts you in a position where you can reason carefully and thereby be sure that any errors you do make are at least not errors of reasoning. There are no additional benefits to be gained from foundationalism. It is very striking that most mathematicians simply *do not have* any views about foundations<sup>2</sup>. The minority that do are either antifoundationalist, or profess a kind of foundationalism whose rôle in mathematics parallels that of the established churches (chesterton) in life in general. It's a way of parking a problem.<sup>3</sup>

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<sup>2</sup>Striking, yes, but not at all surprising: they are far too busy getting on with their mathematics to worry about such things: mathematics—like any other worthwhile activity—is more fun to do than to talk about. After all: the only people who bother with pornography are people who aren't getting enough sex.

<sup>3</sup>It may be pure coincidence that both foundationalism-about-sets, and Catholic doctrine about life in general require of their votaries—as part of the price for valet-parking the problems of mathematics or of morality/life-in-general—that they assent to certain propositions which—if taken literally—would be repudiated by any reasonable person. But perhaps it might not be coincidence; it may be that that very act of *parking a problem* (in the way they both enable their votaries to do) is a piece of bad faith that has to be paid for by eating a certain amount of dirt. But then perhaps i am being unduly optimistic in thinking that all such sins are punished in this life not the next.

Well, Richard Rex says it goes back to Luther—see above

### 11.2.1 Foundationalism about sets

*“Set Theory is the Michelson-Morley æther of Mathematics”*

Doug Bullock

(or perhaps the string theory: explains everything)

Set-theoretic foundationalists are like ants who have been zombied by *Ophiocordyceps unilateralis*; they clamp their mandibles onto the empty set and then the cumulative hierarchy comes sprouting out of the top of their heads. And then they die; or their brain dies; or something, i forget exactly what. It's a ghastly parody of the emergence of Minerva, with a much less happy ending. *Quem Iuppiter vult perdere, dementat prius.* [or perhaps: those whom the gods wish to destroy they first make into set-theoretic foundationalists]

Part of solving any problem is making the preliminary decision: “what is the best way to think about these phenomena?” and set-theoretic foundationalism is the doctrine that the answer is “as sets!”. When you put it like that it sounds absurd of course, but is that really fair? Have i not perhaps exaggerated the claims of set-theoretic foundationalism in such as way as to make it look ridiculous? Well, if that is not the claim made by set-theoretic foundationalism then what is? That thinking of mathematical objects of sets can sometimes be jolly useful? Of course it can.

I make a lot of fuss about the error of thinking that just because replacement doesn't seem to be needed in current mainstream mathematics (it does, actually, but let that pass) that doesn't prevent it from being important for other reasons. The point that *mathematics is time-invariant even tho' what preoccupies mathematicians changes over time* can also be made with regard to the perceived rôle of set theory. It was always inevitable that *my* generation (at least)—growing up in the 1960's—were going to have an exaggerated idea of the importance of set theory... simply because in the 1960's set theory was the most exciting thing happening in Logic—forcing and large cardinals. However, foundationalism about sets was always a bad idea; and—now that the one generation [“talking about my g-e-neration”] that really had an excuse for being seduced by it is reaching retirement age—the rest of us can just quietly get back to our mathematics.

Sometimes—and one finds this for example when trying to prove things by induction—one has to prove more than one needs. One finds this also when tearing down idols. My proposition is not that foundationalism about sets is *false*; my proposition is the much stronger one that it is *absurd*... and, further, that—never mind what they say—no mathematicians actually believe it.

But perhaps they *believe* that they believe it. They're not mistaken about their mathematics—some avowed set-theoretic foundationalists (Harvey Friedman, Steve Simpson) are seriously good mathematicians and don't make gross errors about mathematics; what they are mistaken about is *their beliefs about mathematics*. They misreport their beliefs: they hold false beliefs about their

own beliefs. Foundationalists suffer from a kind of false consciousness: they profess beliefs that they cannot possibly hold. They may say that they think ordinals are hereditarily transitive sets wellordered by  $\in$  but on closer questioning most of them will admit that they don't mean it.<sup>4</sup> It may be that there are parallels with another important equality that nobody takes *literally*: the relation between blood and wine in the Christian sacrament; if there are, then the vast early modern literature on this latter topic could be usefully recycled.

But then any recursively axiomatised mathematician runs the risk of having false beliefs about his/her beliefs.

Perhaps, for them, it's a metaphor for something. Presumably something sensible, indeed... but what?

### 11.3 What might they believe?

There are various doctrines that go under the name of foundationalism, and various other related doctrines which do not go under that name but can get confused with it. One can clear the air by displaying some of them, in decreasing order of plausibility.

1. The Axiomatic Method is a Good Thing;
2. All of [formalisable] mathematics can be formalised in a language with equality plus a single binary relation symbol.
3. All of [formalisable] mathematics can be interpreted in some axiomatic set theory  $T$  in such a way that the things we believe to be true come out as theorems of  $T$ .
4. All of [formalisable] mathematics can be interpreted in some suitable extension  $T$  of ZFC (ZFC + large cardinals of certain kinds—which I will here call “the theory of wellfounded sets”) in such a way that the things we believe to be true come out as theorems of  $T$ .
5. All mathematical objects are really sets.
6. All mathematical objects are really wellfounded sets.

We all agree on (1), just as we all agree on cricket, warm beer, apple crumble, and marmite. How could one not? Much of the appeal of later elements in the list derives from their being confused with (1). Fallacy of equivocation.

(2) appears to be true, but it's not clear what its significance is. It certainly appears highly significant if you confuse it with any of 3–6.

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<sup>4</sup>My Forster grandmother—staying in a hotel in France—met an Englishwoman who said to her in hushed and mystified tones “My dear: I don't know what they *mean*, but what they say is that *they want an aperient before dinner!*”. She had presumably misheard ‘apéritif’ as ‘aperient’ ... an old word meaning ‘laxative’.

(3) seems to me to be a genuinely interesting possibility. Is it the same as (4)? It will be if, given any two consistent set theories, one can be sensibly interpreted in the other. I don't know enough about interpretations to know.

I am not alone in finding (4) implausible. Categorists tend to find it implausible because ZF(C) does not seem to equip them to make the kind of universal assertions ("the category of all categories") that they want to make. Nevertheless there are people who appear not to find it implausible: there even seem to be people who talk as if they believe (5) or even (6). For my part, the mere contemplation of (5) and (6) undermines my will to live.

Thinking that natural numbers are hereditarily transitive finite sets is not like thinking that acids are electron acceptors. You could persuade an Ancient Greek eventually that an acid is an electron acceptor, ("OK, i see what you mean, you've convinced me") just as an Ancient Greek geometer would accept Gauss' construction of a regular heptadecagon. But you'd never persuade Ramanujan (for whom the natural numbers were his personal friends) that his friends, all along, were never anything more than hereditarily transitive finite sets. That's because that equation doesn't explain anything about number theory, and you'll never persuade him that it does. If you really believe that natural numbers are hereditarily transitive finite sets then you have to believe that Ramanujan was radically mistaken about his subject matter. I'd love to be a fly on the wall when you try to persuade him.

At this point there is a mistake that is easily made. It's the mistake of thinking that because one can axiomatise set theory and implement arithmetic in it then one has explained why the truths of arithmetic are true—because one has obtained them as theorems of set theory. This is a mistake because the proof that one obtains never depends on one having implemented natural numbers as hereditarily transitive finite sets. One could equally well have implemented them as atoms. I'm not advocating that, but i emphatically am advocating taking on board the fact that it is a possibility—and one that is feasible and could be sensibly pursued. Indeed, as one warms to the idea, it starts to look as if it would actually be quite a good idea: the manifest success of the project would cure one of the idea that the internal set-theoretic structure of the sets that implement (e.g.) rationals tells us anything about rationals. Frege apparently said something along these lines to Hilbert.

Just as foundationalism is parallel to belief in the catholic church, so antifoundationalism is called forth by the errors of foundationalism in the way that anticlericalism is called forth in catholic countries by the power of the church.

Something that all atheists know perfectly well, and which (most—but not all) believers will admit (even if grudgingly) is that you do not have to be a believer in order to be an authentic moral agent. And you don't have to believe in sets in order to be an authentic mathematician. So even if set-theoretic foundationalism is correct, it has no mathematical force.

If we adjoin the roots of  $x^3 - 2 = 0$  to  $\mathbb{Q}$ , then we have a structure in which there are 3 objects we cannot tell apart (the cube roots of 2). But as soon as we implement this field in set theory we *can* tell the roots apart!! Foundationalism forces its votaries to say things which they know perfectly well to be false. Another way of making the same point: the complex numbers have an automorphism swapping  $i$  and  $-i$ . But the universe of wellfounded sets has no nontrivial automorphisms. So complex numbers are not sets. Or: their arithmetic structure cannot arise from their set-theoretic structure. The only way you can allow them to have this automorphism is to refuse to look inside the objects that are  $i$  and  $-i$ . But if you aren't allowed to look inside them then you aren't treating them as sets: sets have members ... that's what they do!

I know it sounds like a quibble—indeed (I admit) it might actually *be* a quibble—but if you've set things up properly then people have no way in to quibbling. The fact that foundationalism leaves the door open to quibbling is not a feature, it's a bug.

In this section we will make the point that arguments for formalisation are mistaken for arguments that Mathematics needs foundations. Foundationalism about sets is the commonest form of foundationalism. The point to make there is that if you take strong typing seriously you have to look askance at foundationalism. Thinking of everything as a set makes ' $3 \in 5$ ' look like a sensible question, which it isn't.<sup>5</sup> My impression is that although lots of mathematicians think they are foundationalists, their practice belies their words. Formalisation is good, but foundationalism is bad. Do not mistake the common currency argument for the study of set theory for an argument for foundationalism.

Connect with 1-6 page 155

## 11.4 Foundationalism Untrue to Mathematics

Geometrically reals are points: they don't have internal structure. Set theory does not do justice to this intuition.

Thinking of natural numbers concretely as finite von Neumann ordinals flies in the face of the whole direction of mathematics, which is always (when confronted with something) to find the correct abstract way of thinking about it. But of course the same accusation can be thrown in the face of all attempts at foundationalism. They are counterproductive and they miss the point.

You don't think that the real numbers  $e$  and  $\pi$  are sets, so why do you think that  $\omega$  is a set? Of course if you are an on-duty set-theoretic foundationalist you will say that they are all sets, but my question is put to an ordinary working mathematician *qua* ordinary-working-mathematician-merely-trying-to-get-on-with-their-mathematics, rather than to an ordinary working mathematician

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<sup>5</sup>Make here the connection with Ken Manders' idea that mathematisation always adds spurious structure.

posing as a set-theoretic fomdominalist<sup>6</sup>. The ordinary working mathematician does not for one moment think that  $e$  and  $\pi$  are sets, and they should not allow themselves to be spooked into thinking that  $\omega$  is any different.

If you are foundationalist you are forced to concede that the question ' $3 \in 5?$ ' is just as sensible as the question whether or not 3 is a primitive root of 5. The problem of whether or not 3 is a member of 5 is mirrored in the literature in philosophy of mathematics in Frege's *Cæsar Problem*: Is the number 1 the same object as Julius Cæsar? (see p 137)

Given the theological tone of much of foundationalism, it is hardly surprising that some of the problems that foundationalism encounters can be seen in theology proper; the  $3 \in 5$  problem is a case in point. According to Christians, the best—perhaps the *only*—way to be sure of being a Good Person and of going to heaven is to let Jesus into your life and Follow His Example In All Things—the *Imitation of Christ*. Not a bad idea, by any means, and it seems to work—by the standards one sets for this sort of theory—really quite well. For people around at the moment that is. But it prompts an awkward question: what about the salvation-status of people who were born and died before Jesus was around? The helpful suggestion that one's life should be The Imitation Of Christ is not available to them. Naturally there have been theologians clever and persistent enough to cook up an epicycle that will deal with this problem.

The perseveration of philosophers with the unnecessary question of the Cæsar problem (and with its kin) is undoubtedly one of the reasons for the almost complete disregard of the philosophical literature by working mathematicians: mathematicians never themselves lose sleep over this question, and they are not impressed by the example set by folk who do. And it's worth asking why they don't. The reason is of course that they think of mathematics as being typed: Working mathematicians all exploit the typing of mathematics in their praxis. It's typically not made explicit (the phrase 'formally disjoint' is used only rarely to flag up the strong typing that it records) but it's there all the time and it banishes the Cæsar problem in a way the Viennese positivists would be proud of.

However if you are foundationalist about sets then you do have to answer questions like ' $3 \in 5?$ ' and its kin. My guess is that in explaining why this is a daft question and 'Is 3 a primitive root of 5?' is not a daft question you will discover that you weren't a foundationalist after all. On the other hand you might be so thoroughgoing a foundationalist as to have altogether lost touch with the qualitative difference between these two questions and no longer feel that one was daft and the other not. The parallels for this are not encouraging: one of the reasons why hydrogen sulphide is so dangerous to us is that it anaesthetises the olfactory nerve, so you don't realise you are still inhaling it.

If you are a serious-minded thoroughgoing foundationalist about sets you believe that everything is a set. I don't think that there are such people, at least not on the loose in mathematics departments. For example, consider the objects

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<sup>6</sup>Perhaps 'fomdomite' is better.

that appear in discussions of  $\kappa$ -Souslin subsets of  $\mathbb{R}$ . These objects are at one other examples: trees, sequences, (as in BQO theory and the same time *both*  $\kappa$ -sequences of ordered-pairs-of-naturals-and-ordinals—below- $\kappa$  and ordered-pairs-of- $\kappa$ -sequences-of-naturals-with- $\kappa$ -sequences-of-ordinals—below- $\kappa$ . It is universal practice in this research area to equivocate. But reflect below- $\kappa$  It is universal practice in this research area to equivocate. But reflect that if you are a set-theoretic foundationalist what you officially do is implement ordered pairs and wellordered sequences as sets, so that everything you deal with is a pure set. There are standard ways of doing this implementation and—as often as not—it is tacitly assumed that it has been done. Now, a thoroughgoing set-theoretic foundationalist would find a way of implementing these new objects as sets. Nevertheless I have never seen any such implementation announced in the literature and i would be astonished to find one. What is going on? (In the following discussion i am going to call these objects **wombats**—we've got to call them *something*!) The indecision that is being described here is to be distinguished from the practice of many professed foundationalists who announce an implementation of their favoured mathematical entities—reals or naturals or whatever—as sets, and then proceed to reason about them as reals or naturals, ignoring their supposed set-theoretical nature altogether. There at least an implementation is announced, so a notionally authentic foundational move is being made—even if it is not followed through. In the wombat case there is not even a pretence.

Now why might people not defend the von Neumann picture of ordinals as a legitimate datatype equipped with a total order wherein each element can be thought of both as itself and the set of its predecessors? After all, we equivocate over wombats, and we might well want to conceptualise ordinals in such a way that clubsets of ordinals just are the same as the normal functions that enumerate them, and this move is no different in flavour from the move made *in re* wombats. But then it's a different concept of set that is involved. This datatype can be magicked into existence by Conway's principle, but it isn't a datatype of sets.

It seems to me that what is going on (with wombats) is the following. The Mathematicians who study this material pretend to themselves that they are set-theoretic foundationalists. Indeed they make significant gestures in that direction: they know how to implement-as-sets a wide variety of mathematical fauna: ordered pairs, wellordered sequences, and a host of other *prima facie* non-set-theoretic objects. However, confronted with a new kind of mathematical object—the wombat—that arises as a Janus character from two entities of a two kinds both already implemented, they do not bother to find a way of implementing it as a set but merely equivocate between the two kinds of object-already-implemented. This is a kind of half-way house, and unsatisfactory in the way half-way houses typically are.<sup>7</sup> If you don't need to implement wombats then why did you need to implement ordered pairs? What caused the impetus-to-implementation to suddenly wither? Why draw the line in the mud *there*?<sup>8</sup>

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<sup>7</sup>Full of discharged criminals, nutters, junkies and whores.

<sup>8</sup>Perhaps the answer will be that they know they could do it if they had to, so it's all OK. This is a weaker claim than the usual “we know how to do it, so we'll pretend it's been done.” That's almost OK. The weaker version is rather like claiming all the Pascalian advantages of

The answer of course is that you never needed to implement ordered pairs or wellordered sequences in the first place. The mathematicians who decline to implement wombats are not—foundationalist protestations notwithstanding—actually behaving like foundationalists. Nor should they. Their mathematical practice is sound; their description of it is inaccurate. False consciousness again!

## 11.5 The Phlogiston Problem

There is a persistent problem in philosophy of science concerning the status of entities postulated by obsolete scientific theories. The problem arises because it seems pretty clear that one can continue to use the language of the obsolete theory to say things that are true, so the language still appears to denote, somehow. Are there truths about phlogiston? Jason G pointed me at something [69] by Kitcher but it looked pretty weak to me. There is also [85], [86], [87].

This is a problem for philosophy of mathematics too. When Russell and Whitehead said that ordinals were isomorphism classes of wellorderings they were just wrong? Similarly Veblen and Cantor did not know, poor things, that ordinals were hereditarily transitive sets, nor did they know that this meant that—whatever they were doing—they weren’t proving facts about ordinals. So the theorems they proved (sorry, *tho’rt* they’d proved!) about ordinals weren’t proofs at all, or at any rate not proofs about ordinals? “Every set of ordinals is wellordered by magnitude” (which is our theorem 3.) Perhaps they shouldn’t get credit for it. They were presumably proving *something*, but foundationalists have a problem in explaining just what it was, since (on the foundationalists’ account) they can’t’ve been proving facts about ordinals—but they presumably weren’t engaged in an entirely content-free exercise either. So just what were they doing? The foundationalist has to explain quite what those people were up to (given that it wasn’t ordinal arithmetic) and why it was nevertheless useful. If you think a bit about how to give an account of how what-they-did did at least *anticipate* genuine proofs about ordinals, you might be led back to sanity.

Of course one can turn this on its head and say that set-theoretic foundationalism is itself a phlogiston theory: it invents things (sets/phlogiston) to explain phenomena that confront us.

However it’s much more fun to drop the phlogiston problem in the foundationalists’ lap instead of leaving it in one’s own. Let *them* explain what sense Cantor, Veblen and co were making if they were talking about phlogiston instead of ordinals. I know of no literature on this at all. None. It’s probably not worth reading anyway.

Of course there is a phlogiston problem with many-valued logics as well.

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believing in God on the grounds that you could believe in God if you chose. I don’t think God would be impressed by this version of fictionalism. He—like us—might feel tempted to say “*Hic Rhodos, hic salta*”.

But the phlogiston problem is of interest in its own right anyway. A[n apparently] mathematical theory that doesn't do what the people who started it thought it would do ... what is its status? Doesn't one's answer depend to a certain extent on one's philosophy of mathematics? One might want to say that it's just an empty formalism.

## 11.6 Freeing oneself from foundationalism

For a mathematician, taking on board the rebarbative demands of foundationalism-about-sets is a bit like learning not to be disgusted by the taste of tobacco smoke. The key to survival is to rediscover, to *recapture* the disgust that you once felt and had learnt to overcome. One is reminded of Bob Newhart on tobacco:

“And then you set fire to it ... ??”

see [http://monologues.co.uk/Bob\\_Newhart/Tobacco.htm](http://monologues.co.uk/Bob_Newhart/Tobacco.htm) Thinking of setting fire to things, will set theoretic foundationalists say i'm trying to attack a straw man? That nobody believes the things i'm accusing them of believing? Isn't that exactly what i said? Grr! They will say that they don't even *pretend* to believe what i'm saying they don't believe, and go on to say that set-theoretic foundationalism is a more nuanced and sensible enterprise than i am making it out to be. Yeah Right. Like what? The defence of set-theoretic foundationalism must be that it is a fruitful novel connection rather like the invention of cartesian geometry...?

## 11.7 stuff to be fitted into the correct place

Mathematics is not consciously typed, but the typing is there: it's *institutionally* typed.

An object is mathematical once you can exhibit it as a value of a function. That's why beginners can have problems with currying: a curried function takes values that are functions, and if you don't think of functions as mathematical objects then you won't like that.

stipulate vs define.  $x \mapsto x \cdot \sin(1/x)$ . We stipulate/define that it is 0 at 0.

As Lukas Brandtner says, it's very disconcerting that the rank of  $\mathbb{R}$  is not invariant under change of implementations!

“God created the integers; all else is man-made”. You might equally well say that the stork brought them. That sounds like a terrible idea until you consider the alternative: foundationalism is the error of thinking that, now you are grown up, you must not think of anything as having been brought by the stork. In fact it's simplest to just resign yourself to the fact that the reals really were brought by the stork.

ADT = signature/similarity type

Bear in mind that to most working pure mathematicians, there is a danger of taking ADTs *too* seriously. They don't really want to think of the rationals-as-an-ordered-set and the rationals-as-a-field as two completely separate things. Even once we prise the rationals-as-an-ordered-set apart from the rationals-as-a-field they call out to each other across the void, clamouring to be reunited. That is the sound of the mathematics crying out in pain, and it is unwise—*unmindful*—to ignore it altogether.

The ADT story doesn't seem to them to capture the intimate nexus between these two structures. In fact the ADT story probably looks to them like a perfect illustration of Manders' phenomenon. (I remember peddling this line to Ben Garling, and him looking at me as if I was completely bonkers)

That doesn't mean that they don't regard mathematics as strongly typed; it means rather that they don't regard the typing as an appropriate object for mathematical study. "If Logic is the source of his hygiene, it is not the source of his food" which I think is André Weil.

Have a look, perhaps, at [26].

Observe that if you implement  $\mathbb{Z}$  as equivalence classes of ordered pairs of naturals then  $-1 = \{\langle 0, 1 \rangle, \langle 1, 2 \rangle, \langle 2, 3 \rangle, \langle 3, 4 \rangle \dots\}$  so it is the graph of the successor relation on  $\mathbb{N}$ ; the integer 0 is the same set as the identity relation on  $\mathbb{N}$ ; every integer is a binary relation, and—in a curious echo of Church numerals in  $\lambda$ -calculus—addition of integers is relational composition! This is tolerable if we are thinking of these sets as *implementations* of the mathematical entities under consideration, but if integers really are sets, then the integer 0 really would be the same thing as the identity relation on  $\mathbb{N}$ , and this is surely a *reductio ad absurdum*. If the integer 0 is the same thing as the identity relation on  $\mathbb{N}$  then we can surely all stop worrying about whether or not the number 1 is Julius Caesar (It is—of course!—the predecessor relation on  $\mathbb{N}$ .)

Nobody in their right mind thinks these are mathematical facts. " $-1 = S$ " indeed. Do me a favour. It's a problem for foundationalists because they have no way of explaining why it is not a mathematical fact.

We must distinguish between arguments for formalisation (four legs  $\rightarrow$  good) and arguments for foundationalism (two legs  $\rightarrow$  bad).

Formalisation is heir to the problems Ken M speaks of. OTOH it's jolly useful. It helps avoid errors, and strongly typed mathematics helps even more (ATT story).

Foundationalism about sets sounds like a good idea: a kind of common currency argument. But you can obtain all the benefits of a common currency without being a set-foundationalist.

Operationalism is opposed to foundationalism. What does foundationalism about sets actually mean? There are various things it could mean. Might it

mean that any theorem in mathematics, however expressed, is a theorem in set theory clothed in different language? Surely this cannot be what was meant, for the terms in the mathematical theorem can typically be translated back into set theory in lots of different ways, with the effect that the theorem is a rephrasing of lots of distinct theorems in set theory. “So”, one can ask, “Which theorem in set theory did you have in mind?” If the reply is to be “all of them, because they are equivalent”, one finds oneself believing the axiom of replacement. But some foundationalists about set theory deny it. So just what do they mean?

## 11.8 Operationalism in Mathematics

The idea that the meaning of the connectives can be captured by introduction and elimination rules is clearly operationalist. Martin says that it's in Prawitz but may be earlier. Indeed the whole idea of “meaning as use” is operationalist.

Endothelial derived relaxing factor (“EDRF”) turns out to be *NO*, nitric oxide. (This is not like the evening star turning out to be the morning star: ‘EDRF’ is operational, ‘NO’ is not. [well, it’s not quite operationalist because we are not identifying it in terms of its effect on our instruments.] see [105]. Operationalism (about widgets) is the practice of conceptualising widgets in terms of the instruments you use to investigate them. The contrast is with philosophies that strive to conceptualise widgets in terms that reflect their Inner Nature. My favourite example of operationalism is actually a parody—a wonderful parody—emanating from the Medical School at the University of Cambridge in the 1970s (author unknown) “The superego is that part of the ego that is soluble in ethanol.” This doesn’t characterise the superego in a functional way, in terms of the rôle it plays in the mind, but in terms of its response to the chemicals on our bench. Another—slightly more mainstream—example is DNA: “deoxyribonucleic acid”. Why ‘acid’? Because it’s that part of the cell nucleus which dissolves in alkali (the proteinaceous part gets digested by acid and the DNA is what’s left). In both these cases you are describing the phenomenon in terms of the way it responds to your equipment (in these two cases, the reagents you have on your bench). ‘Slow-wave sleep’ is so-called because of the way it shows up on an EEG machine. REM (rapid eye movement) sleep ditto, since EEG machines detect eye movements very well; *diabetes mellitus* is so called because of the sweet taste of the urine produced by the people who suffer from it. And so on.

What has this got to do with Mathematics? Quite a lot, I think, and that is what I want to explore. Mathematicians who make the mistake of reading the philosophical literature get sucked into ontological questions: “Am I a platonist ...? ...Is fictionalism a good idea ...?” when of course what they *should* be doing is examining their navels—I mean their own *praxis*. When they do this a lot of them discover that they are operationalists: they naturally think of mathematical objects in terms of what they do (rather than in terms of what they are, which is always likely to be intangible).

A lot, but probably not all of them. I am often brought up sharp by the reali-

Must track down Frayne’s Bon Mot about how one could identify a work of art with the set of opinions that are held about it.

sation that what I think of as Mathematics is something that in most mathematical cultures is called *Pure Mathematics*, and the realisation that pure mathematicians are a minority within the people who call themselves mathematicians. My inclination to operationalism may even be specific to a minority-within-a-minority—that minority<sup>2</sup> consisting of logicians-who-have-been-exposed-to-theoretical-computer-science. But then again it might not.

There seem to be two sources of operationalism in my mathematical neighbourhood. Category theory is obviously operationalist in outlook; and the *datatype*-speak of theoretical computer science is highly operationalist too. What I want to do is get straight the relations between them, and their philosophical roots.

It took me a while to realise it, but apparently category theory was not inspired by operationalism (and didn't have any explicitly foundationalist programme, tho' this may be another way of saying the same thing). Apparently category theory grew out of the needs of algebraic geometers—specifically the need to properly understand *natural maps*. (History nicely summarised in [75].) I have no first-hand knowledge of this history but I been told closely similar versions of this narrative independently by so many people who do know about category theory that I have concluded that it must be true. Of course the fact that operationalism wasn't a motive force behind the inception of category theory doesn't prevent the operationalist tone of category theory from being very attractive to (pure!) mathematicians. Many mathematicians feel that category theory speaks to their concerns in a way that set theory doesn't, and this is probably no accident. Set theory appears to make claims about what mathematical objects are, and thereby plays into the tedious debates about the ontological status of mathematical entities, and time spent in such debates is time spent on something that doesn't seem to help them with their job of being mathematicians. Could it be that operationalism is a rather good philosophy of mathematics?)

(If it is a good philosophy of mathematics, this points up an interesting contrast between mathematics and the natural sciences: operationalism in physics is (for reasons I won't go into) almost universally regarded as a Deeply Bad Idea. (Admittedly there are lots of perfectly respectable physical concepts that *start off* operationalist but all of them subsequently acquire a theoretical underpinning that makes them device-independent: operationalism-about-widgets is a respectable point of departure, even if it's not a good place to be when the music stops). But then is Mathematics like the rest of Science? I have the unworthy suspicion that Mathematics is not *in the least* like the rest of Science. There are counterfactuals in Science and there are no counterfactuals in Mathematics;<sup>9</sup> Science has the concept of a *law of nature*, of *experiment*: Mathematics has nothing like these. So how did anyone ever get the idea that Mathematics was like Science? Because natural selection has given us a brain that was configured for doing Science, and we—poor perseverating creatures

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<sup>9</sup>people will say that there are independence proofs in mathematics. Are these not counterfactuals? Explain why not.

that we are—we insist on using it when confronted with Mathematics, for want of anything better to bring to bear. If all you have is a hammer, every problem looks like a nail, as the old cliché has it.)

The other source of operationalism in Mathematics—in my conditioning history at least—is the theoretical computer scientists' concept of an *abstract datatype*. This way of thinking is so obviously operationalist that no further comment is needed. The question however remains:

**Who started this Abstract Datatype lark, and why? And what are the connections with category theory? And what rôle does operationalism play in all this?**

What is most striking in all this is the fact that you never hear the word 'operationalism' in connection with Philosophy of Mathematics. The philosophers who interest themselves in Philosophy of Mathematics have no idea what mathematicians actually *do* and so have never been able to ascertain the extent to which mathematicians are operationalist; most mathematicians for their part are in any case too busy doing mathematics (or applying it, if they are applied mathematicians) to have any time to do Philosophy of Mathematics, and—if they can find the time—they tend to use the vocabulary that has been pre-cooked for them... by a lot of philosophers who have no idea what they (the mathematicians) are actually doing. The people who are doing the Mathematics don't have the vocabulary and the people with the vocabulary don't do any Mathematics.

### 11.8.1 A conversation with Graham White

Is there a mediæval theory of aspects—dreamed up in order to explain how God can have various contradictory properties—which could be exploited in the process of explaining *casting* in CS? One object can belong to two datatypes?

Strong typing in CS is certainly born of operationalism.

**tf to Graham White**

Graham, i remember asking you in when i was last down in London about Operationalism in Mathematics. I want to pick your brains.

My first encounter with it was when i learnt ML and HOL: constructors and destructors, that sort of thing. This idea that mathematical objects are constituted by what they \*do\* seemed to me to be immensely fruitful. It also seems to me that none of the people who nowadays purport to do philosophy of mathematics (or at any rate very few of them) ever seem to have heard of it. I have always assumed that this is because they are all philosophers and therefore don't know owt about nowt—in particular they don't know owt about computer science—which is where operationalism surfaced (or so it seems to me). Naturally i asked Robin Milner about it and he said "Edinburgh in the '70's". When i recounted this remark to you, you said "Well, he would, wouldn't he"—a parallel between Robin and Mandy Rice Davies that i hadn't forseen.

The world is full of surprises. But never mind. Of course another input into operationalism is Category theory. Is there any operationalist patter in the early Category theory literature?

### **GW replies**

Well, it seems to me that there is a category-theoretic version of operationalism, but that category theory got to it indirectly. What category theory started off as, it seems to me, was an attempt to give a properly mathematical characterisation of “natural” (as opposed to *ad hoc*) constructions. Now the idea of a natural construction is probably deeply rooted in the geometrical tradition, but—and here the history needs filling out—first surfaced as a problem with the beginnings of algebraic topology. So: category theory was invented as an explicit metatheory for the idea of naturality. But it turned out to be unexpectedly powerful and important.

Later on there were more wide-ranging formulations of what category theory does: the important paper is probably Benabou’s [10]. (“Fibred Categories and the Foundation of Naive Category Theory”, JSL 50 (1985)), where Benabou distinguishes between observable and unobservable properties of a category, and says that the observables are precisely those that are identities between morphisms (that’s my formulation, not his). This explains the importance of universal properties: they are (presumably) the properties of objects definable in terms of identities between morphisms (work to be done here, too, and probably only true for certain categories). And it’s a form of operationalism: the essential properties of objects are those definable in terms of morphisms to it or from it.

But . . . this idea of operationalism doesn’t quite get us to the position that operationalist constructions should be entirely in terms of constructors and destructors. It’s very rare, in general, that an object admits either constructors or destructors, and rarer still that it admits both. A systematic way of admitting both was given by Peter Freyd, in his work on compact closed categories: but it’s not entirely clear that he had things pinned down. Strangely enough, this work seems to have had more takeup on the quantum theory/ quantum gravity world than in analysis of data structures.

So, all in all, there’s more work to be done. I think there’s a story to be told, though.

### **tf continues**

Thanks very much for this. The point you make which is novel to me (tho’ i s’pose i should have known it!) is that Category Theory was developed in order to capture the idea of a natural operation.

### **GW replies**

That’s fairly clearly there in the sources, particularly Eilenberg and MacLane’s “General Theory of Natural Equivalences” paper, [29] which is probably the birthplace of category theory.

**tf continues**

Presumably you know Wilfrid's lovely article 'Naturality and Definability'? ([60])

**GW replies**

Yes, I do, though I think he misreads the category-theoretic literature, in that he takes the point to be to characterise natural *isomorphisms*, whereas I think one of the great insights was that these things are much easier if you try to characterise natural morphisms in general.

**tf continues**

However, you haven't brought me closer to the target of my original quest, which is the origin of operationalism in the thinking of the people who started ML and things like that. Should i go and prod Robin again? Or if i prod you will you say something interesting..?

**GW replies**

Well, there's probably a strong current of operationalism in computer science way before ML (after all, ideas like an abstract data type, and data refinement, go a long way back). Probably what happened with ML is that that strand of thought came together with mathematical ideas from category theory, and ML was born. The fit, though, is surprisingly inexact: one problem is that the CS people tended to define abstract data types in terms of rather particular sorts of operations, that is, constructors and destructors, and those turn out to be hard to characterise mathematically. But all these are just feelings, and it would be good to see some documentary work on it.

### 11.8.2 Equivocation and Operationalism

## 11.9 Indeterminacy of translation

A lot of the people who wear baseball caps wear them indoors. I was brought up never to wear a hat indoors. (Not sure why! This interdiction is a recent development in Britain.) Now these people who wear baseball caps indoors... Do they have no objection to people wearing hats indoors? Or is it that baseball caps are not hats in the appropriate sense? Does this question even have an answer?

There is a ... divergence of practice? (I don't want to beg any questions in giving a description) about whether or not the natural numbers start at 0. Number theorists start counting at 1, logicians start counting at 0. Do they actually disagree about any proposition?

### 11.9.1 $\omega$ and $\mathbb{N}$

My Ph.D. student Zachiri writes

$$f : [n]^k \rightarrow \{0, 1\}$$

to mean that  $f$  is a  $\{0, 1\}$ -valued function whose arguments are unordered  $k$ -tuples of natural numbers below  $n$ . If he is using the square-bracket-plus-exponent notation for the set of unordered  $k$ -tuples then—at least if the notation he uses is to mean what he says it means—it must be that  $n$  and  $\{i \in \mathbb{N} : 0 \leq i < n\}$  are one and the same thing. Does he believe that? (There are people who do, or who at least say that they do). Or is it just a clever device of notation, so that ' $[X]^n$ ' denotes the set of unordered  $n$ -tuples of the set  $X$ , at least in circumstances where the letter ' $X$ ' denotes a set; when it doesn't we extract a set somehow from the denotation of the thing between the two square brackets, and context will tell us which set ...?

A pedant like me would have written

$$f : [[1, n]]^k \rightarrow \{0, 1\}$$

where ' $[1, n]$ ' is a notation for the natural numbers between 1 and  $n$  ... and i would probably have lost a few readers in consequence! Perhaps what Zachiri is doing is merely abbreviating ' $f : [[1, n]]^k \rightarrow \{0, 1\}$ ' to ' $f : [1, n]^k \rightarrow \{0, 1\}$ '.

Wilfrid says that the confusion about people saying that (e.g.) ordered pairs of  $x$  and  $y$  are  $\{\{x\}, \{x, y\}\}$  is not a confusion in their heads but a confusion over notation. It's not that they have *false beliefs about* those beliefs; they just *misdescribe* (their beliefs about) their beliefs. Is there a radical translation point to be made here? Not least because if you try to press them about it you won't get an informative answer because they'll think you're being silly and won't pay attention.

Lots of people write ' $\omega$ ' for the set of natural numbers. Do they believe that the first infinite ordinal and the set of natural numbers are the same thing? (They do all know that  $\omega$  is the first infinite ordinal: that much at least is clear). Or is it just a piece of notation for them? Something they picked up when they were students? How can one tell? One could try asking them of course, but i suspect the answer would not be enlightening. Most mathematicians are not interested in notational questions or anything that looks as if it might be a notational question.

And another example.

When i see someone writing

$$\bigcup\{\alpha : \phi(\alpha)\}$$

where ' $\alpha$ ' can be seen to range over ordinals, and it is clear from context that the displayed formula denotes the supremum of a certain set of ordinals, what am i

to infer? Am i to conclude that the symbol ‘ $\cup$ ’ is being used as if it were ‘ $\Sigma$ ’? Or do i infer that the writer really *does* mean set union, and that he is thinking of ordinals in such a way that the sup of a set of ordinals is in fact its sumset? That is to say, for him/her, ordinals are von Neumann ordinals? Quine’s point about indeterminacy of translation is that there is no way of telling—from the fragment of text in front of me—which of these explanations is the correct one. A larger sample of this text might answer the question, but it might not. The writer might genuinely believe (as some people in fact do) that ordinals just are von Neumann ordinals. Or (s)he might not believe any such thing, but might have learnt their set-theoretic notation from people who do, and conformed to their notational practices without acquiring their beliefs.

Again, my point here is that i have actually no way of knowing which of these explanations is correct—beyond asking him. (language shortcuts experiment, after all). But how does an answer from him help? It doesn’t enable me to do anything in the way of understanding-the-notation that i couldn’t do before. In any case, most set theorists, if i were to ask them about this aspect of their praxis, would groan and think “God, we’ve got a nutter here all right”, and i wouldn’t get a sensible answer. In the public space, wherein he is communicating to me some information about a mathematical object, there is simply no fact of the matter.

I want to emphasise that these are genuine live examples from mathematical practice, not perverse unnatural thought-experiments dreamed up by wankers with degrees in philosophy.

### **equivocation**

Fundamental sequences can be thought of sometimes as wellorderings-of-length- $\omega$  and sometimes as functions from  $\mathbb{N}$  to the second number class. This equivocation is mathematically necessary, and this suggests that the formalisation we have is over-detailed and is generating spurious distinctions. Just like the problem we have in conceptualising proof.

Another example: one device in the study of relations between large cardinals and the continuum is a thing (appearing in the definition of  $\kappa$ -Suslin set of reals) which is represented sometimes as a sequence of pairs (of naturals and ordinals below  $\kappa$ ) and sometimes as a pair of sequences. It is important that it can do both jobs. One tends to think of this as a mathematically convenient kind of equivocation, but really it’s not equivocation at all: the datatype that is in play is a thing that supports both unpairing and unsequencing: it just does. I think it is worth pointing out that despite the fact that the community which exploits these objects in the study of sets of reals consists almost entirely of people who profess fairly extreme versions of set-foundationalism (that reals are  $\omega$ -sequences of naturals, that naturals are finite von-Neumann ordinals, that pairs are Wiener-Kuratowski and so on) these Janus-faced data objects are never given an explicit definition anywhere in the literature. I am not complaining that they should be given such definitions: absolutely not. My point is that the sound instinctive mathematical practice of this community overrides the beliefs

they profess to hold. And quite right too. They are misdescribing their own practice.

Another example. Sometimes we have to think of finite trees as posets. Sometimes we have to think of them as digraphs; sometimes we have to think of them as elements of a cute rectype. We need all these things. Again this is not most illuminatingly thought of as equivocation; we have a datatype that just does do all these things.

Contrast this with:

(i) the situation with ordered pairs: where there are lots of pairing functions and it doesn't matter which—we don't need more than one flavour of ordered pair. (This is very striking: why is it the case that we never have two purposes, both of which require ordered pairs, but require us to use ordered pairs of different flavours??)

(ii) the identification of  $\omega_1$  with the second number class. This serves no mathematical purpose whatever—how could it? It serves a *notational* purpose all right: it means that we don't have to have a separate notation for the second number class.

Is there a way of thinking of the von Neumann ordinal caper as a purely notational trick? Zach was writing ' $f[N]^e \rightarrow 2$ ' using ' $N$ ' in the von Neumann sense. This means he doesn't have to write ' $[1, N]$ ', but the type checker will tell us that that is what he really means. Just like the singleton of the unit .... This is almost a case study in the indeterminacy of translation. You would be much more likely to read ' $[N]$ ' as a notational device than an implementational hack if you saw it in a combinatorics article than in, say, a set theory article.

When you see people (usually set theorists) write ' $\omega$ ' to denote the set of natural numbers or the set of finite ordinals, what do you infer? On the face of it, one should be saying that they believe that ordinals are von Neumann ordinals, so that  $\omega$  is identical to  $\mathbb{N}$ . (or—better—that ' $\omega$ ' and ' $\mathbb{N}$ ' denote the same thing.) But it might just be a cultural thing: writing ' $\omega$ ' for ' $\mathbb{N}$ ' might be an example of what i have elsewhere called *lexical choice semantics*: a highly economical way of saying “I'm a set-theorist”<sup>10</sup>. Again, the point is that there is nothing in the language-use that answers this question for us: one has to ask the speaker.

How can i tell that someone who writes ' $\omega_1$ ' for the set of countable ordinals really believes that the ordinal  $\omega_1$  is the same as the second number class? They might just be writing ' $\omega_1$ ' instead of ' $\text{seg}_{<\text{NO}}(\omega_1)$ ' [i *think* that is Rosser's notation from [98]] on the [perfectly sensible] grounds that the correct reading can be recovered by appeal to indigenous strong typing—just as one can recover the correct reading ' $\{\mathbf{1}\}$ ' from the abuse of notation ' $\mathbf{1}$ ' for the trivial subgroup by appealing to indigenous strong typing.

Is this equivocation like the equivocation we have in recursive function theory where we treat a natural number as a single number or as a tuple? Or equivocating between data and program?

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<sup>10</sup>Or perhaps “I would like to be thought of as a set-theorist”.

I think we are going to need a chapter on equivocation!

### 11.9.2 Category Distinctions

Carnap's sentence

This stone is thinking about Vienna (1)

is a good example of a statement that is pretty obviously false. Stones don't do that kind of thing, and to suppose that they might is to commit a *category mistake*.

The Logical positivists' motivation behind this device of the category mistake was to find a wholesale and industrially robust way of eradicating all manifestations of German Idealism wherever they raise their head. Nowadays one does not have to feel the sympathy one undoubtedly does for Carnap and co<sup>y</sup> to see this invention of the category mistake as in any case sensible, because now other reasons for adopting it have become clear. Attention to these category distinctions gives us a very rapid way of detecting at least some falsehoods: falsehoods that can be seen to be such from completely general considerations of their subject matter—considerations that are so banal that they can even be encoded into the syntax. Linguists have a theoretical device of *category* that operates in this kind of way. Many modern programming languages have category distinctions (they are called typing disciplines but it's the same) and programmers who have mastered them laud them for their great power in detecting defective code. They point to the famous brown-out of AT&T's 'phone network in 1990 which happened because of a misplaced semicolon (a command delimiter) in some critical code. C++ has no typing system so it couldn't tell that the code it was treating as an argument to a function was in fact intended to be the next command in the program. A compiler for a typed programming language would have detected the error in the program and rejected it.

Very well: having an animate/inanimate distinction written into our syntax enables us to detect very quickly the falsehood of sentences like (1), and having a number variable/boolean variable distinction in our programming language can enable us to spot very quickly when a command delimiter has been omitted by a hasty programmer. But where do these distinctions come from?

One feature common to the appeal of these category distinctions (where they do have an appeal, that is) is a kind of *operationalism*: thinking of things in terms of what they *do*. If we approach this matter from the perspective of formal logic we can see how the operationalism gives rise to type-distinctions. One starts with a very restricted language for describing what the objects of interest do, and axioms that describe those actions. For example for ordered pairs one has a language containing three function letters: **pair**, **fst** and **snd**. One then adds axioms:

$$\begin{aligned} \text{pair}(x, y) = \text{pair}(x', y') &\rightarrow x = x' \wedge y = y', \\ \text{fst}(\text{pair}(x, y)) &= x \text{ and} \end{aligned}$$

$$\text{snd}(\text{pair}(x, y)) = y.$$

Since every natural number can be expressed uniquely as  $2^x \cdot (2y + 1)$  we have an obvious way of encoding ordered pairs of natural numbers as natural numbers. However this identification of ordered pairs with natural numbers tells us nothing about the nature of ordered pairs. The two realms—of ordered pairs and natural numbers—are formally disjoint.

Then one translates (“implements”) those basic assumptions about the objects of interest (in this case ordered pairs) into whatever ambient language one is using—be it set theory or a programming language of some sort. One then shuts one’s eyes to anything whose meaning is not invariant under choice of implementation. Is it sensible to ask whether or not the set which is the ordered pair  $\langle 3, 5 \rangle$  has the empty set as a member? Clearly not: the only question we can ask about ordered pairs is what their *components* are: they do not have set-theoretic structure, or at least—if they do—that structure is not visible to us. Why is this? because we are thinking of ordered pairs *operationally*. We now introduce a typing system with two types (“categories” if you are from Vienna): **set** and **pair**, and constraints as on page 143.

The formulæ that are well-typed under this scheme are precisely those whose truth-value does not depend on our choice of implementation: operationalism has given rise to a category distinction.

This operationalism is widespread in mathematics. It is generally not acknowledged: the general view among anthropologists is that mathematicians are platonists: they are realists about mathematical entities—even mathematicians themselves think they are realists. However, many mathematicians, if asked whether the real number  $1_{\mathbb{R}}$  is the same as natural number  $1_{\mathbb{N}}$ , will protest that it is a daft question. Why is it daft? Because real numbers measure *length* and natural numbers *count* (members of) sets. *Reals and naturals do different things.*

But it shouldn’t be daft. After all, if the real number  $1_{\mathbb{R}}$  and the natural number  $1_{\mathbb{N}}$  are genuine existent objects it should be possible to pick one up in each hand and compare them to see if they are the same. If the question is daft then perhaps one can’t, and that might be because they aren’t real objects.

### Emergence

This operationalist-driven regimentation appears also with emergent properties. A relatively well-understood example is cardinal arithmetic. (It is a good idea to start with an example we have some hope of understanding before we launch into Life, minds ...) Cardinal numbers emerge from sets. It is a reasonable position to adopt that there are no numbers altho’ there are facts about numbers: facts about numbers are just facts about sets. (Ordered pairs are slightly different: one cannot tell any story that makes them disappear in the way cardinals disappear). However the type-distinctions that arise from things like pairs and from things like cardinals can be given a common logical treatment in the

theory of interpretations: one interprets the theory of sets-plus-cardinals in the theory of sets. If there is a sensible logical account of reductionism this is surely it. These interpretations (at least in a formal context) are mathematical objects and deserve to be examined as such. Indeed there is a logical literature on interpretations (one thinks immediately of the work of Albert Visser) and it cries out to be applied to philosophical concerns. Clearly one cannot deduce an ‘ought’ from an ‘is’ but there is nothing to stop one translating the ‘ought’ language into the ‘is’ language. In my recent book [36] I analyse one particularly simple case of an emergent property: the way in which arithmetic emerges from a theory of sets.

(This is an instance of a special case: the case where the emergent object arise from an equivalence relation. In that setting one can say that (as it were) cardinals are just sets, but equipped with different identity criteria: identical-as-cardinals is not the same relation on sets as identical-as-sets.)

Although ontologically the status of emergent objects like cardinals is (arguably) quite different from the status of things like ordered pairs, the logical treatment is the same. And a typing discipline arises in the same way in the two cases.

Does the Henrard analysis<sup>11</sup> show that equipollence is genuinely a set-theoretic idea? Or is it an implementation of equipollence in set theory? The first sounds wrong: surely Henrard-bijectiveness is not what we have in mind when we say that two sets are in bijection. But the second, too, sounds like an odd thing to say. Equipollence is clearly a binary relation *between sets*. Two sets can stand in this relation without having to be secretly seen as tokens of an implementation of anything. Did we, all along, want to say that equipollence emerges from sets? (thereby finding an illustration of the difference beteeen emergence-from and reduction-to?) If I am confused about this does it mean that the idea of *purely set-theoretic concept* is much less secure than i thought?

There is a similar question about wellordering. Is wellordering a set-theoretic notion? One can say directly that  $X$  is a wellordering of  $Y$  iff it is a collection of subsets of  $Y$  totally ordered by  $\subseteq$  such that distinct members of  $Y$  belong to distinct members of  $X$  and whenever  $Y' \subseteq Y$  is nonempty there is  $a \in Y'$  and  $x \in X$  with  $x \cap Y' = \{a\}$ . I find myself itching to copy the last two paragraphs and replace ‘ $x$  and  $y$  are in bijection’ by ‘ $x$  is a wellordering of  $y$ ’ and make the same points. . . .and of course to contrast both with “ $x$  is a subset of  $y$ ”.

Equipollence and wellordering emerge from sets...

Would it be a sensible requirement on a set theory that it proves the equivalence of these ways of thinking of wellorderings?

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<sup>11</sup>Think in this context of the result of Nathan and mine that every permutation is a product of involutions. One can define **involution** without having to implement ordered pairs. It would be very striking if the truth of AC—which implies that every permutation is a product of two involutions—were to make something that wasn’t a set-theoretic notion into a set-theoretic notion!

### 11.9.3 A Conversation with Ken Manders

Ken is very keen on the idea that much of the conceptualising we do to stick pretheoretical objects into mathematical shape overdetermines the resulting entities. His favoured examples are knots and computable functions. (He says look at Hartley Rogers, particularly ch 4 onwards, and p 182. He thinks this is inevitable: this is the kind of thing that does just happen if you mathematise properly. Typically there won't be just one right way of thinking about any mathematical entity. The error of thinking that there is one he lays at Frege's door.

(Should I perhaps be thinking again about the Indeterminacy of translation?)

This makes me think: might we not be able to avoid this overdetermination by having an operationalist view of mathematical entities?

Operationalism is usually a dirty word in Philosophy of Science and Ken says this is because it results in very impoverished theoretical entities (He mentions Bridgeman in this connection).

So why might it be less problematic in Mathematics? Anything to do with the idea that Mathematics has no subject matter? If you are a scientific realist then operationalism is clearly a bad idea because it won't capture the full throbbing reality of the entities you are looking at. But in *Mathematics*...? If it is true that anything done with sufficient rigour is part of mathematics then we might be all right. Of course the idea that Mathematics has no subject matter is just, in new dress, the old idea that all of mathematics is *a priori* and has no empirical content. Better still, it might be the correct expression of that insight.

Ken thinks there is a parallel between the overdetermination of ordered pairs and the overdetermination of knots but I think these are different. I don't think we are implementing knots, I think we are trying to formalise them. But perhaps it really is the same.

## Chapter 12

# The Coincidence

Set theory can be used as a universal language for mathematics: all of mathematics can be interpreted into it (or so it seems so far). Various programming languages have been designed by humans to serve a similar universalist purpose in everyday problem solving (as long as the problem-to-be-solved is sufficiently mathematical). In both cases we find ourselves making the kind of typing distinction that we consider in chapter 10 above. However in some kinds of set theory—specifically those that descend from the typed set theory of the *Principia Mathematica* of Russell and Whitehead—the typing distinction of the first kind (the kind that was the seek-and-destroy missile for the paradoxes) is still (if not actually in use) at least still available and alive in the syntax. To be specific, the Quine systems contain residual typing distinctions left over from the Russellian ruse that got rid of the paradoxes. Now these set theories naturally also have the type distinctions arising from the considerations of chapter 9.

Russell & Whitehead of course intended PM to be a basis for the whole of mathematics so their type distinction is (at least in principle) in play everywhere. Furthermore, those of us who think that mathematics is strongly typed will see CS-style type-distinctions everywhere. In principle, therefore, there is lots of scope for comparing the lessons taught us by the two kinds of typing (tho' not so much in practice it must be said). What is striking is how often the two approaches give the same answer. Is there perhaps an \*\*Underlying Unity\*\*?!?!

That is the topic of this chapter.

Because there is no type-lowering pair (and similar considerations)  $T$  functions will always intrude when we consider NO. It's easier to make the point about the coincidence in a typed theory like TST or TZT, and then move to the NF setting. Say something about implementing ordinals in TZT; can we implement ordinals by taking a representative from each class. Stuff about DC in my yellow book. Can't just take  $No(R)$  to be that initial segment of the ordinals iso to  $R$ .

Let us think about ordinals as virtual objects in this connection ("there are

no ordinals there are only wellorderings: facts about ordinals are facts about wellorderings.”) In particular there is a resolution of the Burali-Forti paradox that makes the ordinal of the wellordering of the set of all ordinals a *doubly* virtual object and therefore not of the same type as the ordinals in the wellordering it is counting. More generally, for any ordinal  $\alpha$ , the ordinal of the wellordering of the ordinals below  $\alpha$  is (from the point of view of flavour-2 typing) of a different type from  $\alpha$ . We can of course interpret all this in set theory—in particular into a set theory that inherits the flavour-1 type distinctions from Russellian Type Theory. Thus we have a theatre in which both these kinds of type distinction are in play. What is so intriguing and satisfying about this situation is that the flavour-1 typing machinery, too, decides that for any ordinal  $\alpha$ , the ordinal of the wellordering of the ordinals below  $\alpha$  is of a different type from  $\alpha$ .

The type distinction arising from the theory of interpretations tells us that  $\alpha$  and  $T^2\alpha$  are not (*prima facie*) the same thing. This doesn’t mean that they are *in fact* distinct; it means that if they are to be seen to be the same, then a new separate insight will be required.

Very striking that the same distinction is enforced by Russellian-style stratification too.

The Russellian type distinctions compel  $\alpha$  and  $T^2\alpha$  to be of different types because  $\langle x, y \rangle$  cannot be of lower type than  $x$  and  $y$ . Question: why is this? If there were no such object as NO (which is what the ZF-istes would have us believe) then this is pure motiveless coincidence: there would be nothing for it to explain—and we would be at a loss to explain it, in turn. Everything fits together much better if we have the NF picture of ordinals. The ZFiste move of cutting off one’s nose (= eschewing large classes) is usually defended as a way of avoiding the paradoxes. However, the type-distinction that arises from the implementation forstalls paradox, so there is nothing for the ZFiste move to solve.

The fact that the extended axiom of counting seems to be true for small ordinals is another illustration of the *law of small numbers*. When dealing with a small number of small things you encounter many coincidences. (This is a phenomenon known to [some] mathematicians as *The Law of Small [sic] Numbers*) It’s a bit like examining a dataset with respect to too many parameters. One of them will surely turn out to be significant. (A disproportionate number of US presidents have been lefthanded . . . really? How many parameters have been used to classify this dataset? If you examine a data set of 50 points wrt 100 parameters you will get a positive correlation somewhere.) Singly virtual ordinals are as different from doubly virtual ordinals as finite ordinals are from finite cardinals. See Appendix 14.1.

It’s like the fact that finite ordinals coincide with finite cardinals. The fact that finite cardinals coincide with finite ordinals is an interesting fact about finiteness, like the fact that any injection from a finite set  $x$  to a finite set  $y$  the same size as  $x$  is actually a surjection. It’s a fact about finiteness, not

fact about sets. And the fact that finite ordinals are monomorphic (and that countable ordinals are monomorphic, too, if indeed they are) is not a fact about ordinals but a fact about *small* ordinals . . . which is to say *au fond* a fact about smallness.

(This creates an opportunity to have a go at two reactions to the paradoxes. The ZF-istes are cutting off their noses to spite their faces. Dialethism, too, misses the point. Graham Priest said, when i gave a talk about this at Melbourne “why not give in to the instinct to believe the untyped extended axiom of counting and then go paraconsistent?” A: because the strong typing has sound logical roots in the theory of interpretations. The impulse to deny the extended axiom of counting arises from the insight that ordinals were *prima facie* polymorphic. What the Cosmic Coincidence is telling us is that there *is* no contradiction: nature abhors a contradiction, and no matter how you approach the place where you think the contradiction is to be found, your path will be blocked. Dialethism invites you to take this particular contradiction on the chin. But there is no contradiction to take.)

Put it this way: if there were ever going to be an example of a true contradiction, this is where you would find it. If this were a true contradiction, what sort of thing would we see, as we survey the battleground? We certainly wouldn’t expect to see a neat fit. The kind of thing that would make us throw up our hands and concede that some contradictions can indeed be true would be huge conflicts between the findings of the different analyses—yet this is precisely what we do *not* find. “The situation” so the saying goes “is serious but not hopeless.” Dialethism is the—one hates to use the word *contradictory*—view that “The situation is hopeless but it’s not serious”.) Perhaps not so much contradictory as *autological*: the analysis is hopeless and not serious at all.

Of course we are not going to be able to prove by induction on ordinals that they are monomorphic!!

## 12.1 The Burali-Forti Paradox—again

To find a theatre where the Quine-Russell typing (set theoretic in origin) and the endogenous-strong-typing (which arises from the theory of interpretations) are both in play we need a topic in set theory where essential use is made of interpretations. A good paradox will always concentrate the mind and—as far as i can see at time of writing—the only paradox that involves interpreting anything is Burali-Forti, which is a paradox concerning ordinals and sets of ordinals. A thorough analysis of Burali-Forti must concern itself with interpretation of the theory of sets+ordinals into Set theory.

And we had better have an analysis that is not tied to any particular interpretation of ordinals. After all, an analysis that relies on fixing a particular implementation of ordinals is not an analysis of the paradox, but an analysis of one particular implementation of it. This particular barb is directed at those who suppose that once they have decided that ordinals are Von Neumann ordinals, then the problem has been solved. What has been solved—in these circumstances—is one particular interpretation of it. It is not an analysis of the whole problem.

The Burali-Forti paradox concerns the length of the wellordering of the set  $NO$  of all ordinals. Each ordinal counts the sequence of ordinals below it: that is to say, the length of any initial segment  $\mathcal{I}$  of the ordinals is the least ordinal not in  $\mathcal{I}$ . So what if  $\mathcal{I}$  is  $NO$  itself? Not nice. I re-emphasise at this point that we are approaching this riddle from first principles—or perhaps I should say *with an open mind*—which is to say without smuggling in any preconceptions from solutions that have been earlier proposed. In this context this means from the point of view of naïve set theory.

Let us start by identifying two central claims on which the paradox relies:

1. The first is that the collection of all ordinals (whatever they may be)<sup>1</sup> comprise a set.
2. The second is that the length of any initial segment  $\mathcal{I}$  of the ordinals is the least ordinal not in  $\mathcal{I}$ .

The first is the feature usually pounced on, but it is the second that it is in the end most illuminating to analyse. Denying (1) is such a straightforward move and so fruitful—so *immediately* and *enduringly* fruitful—that one is tempted to think that one need not consider (2) at all. But to ignore (2) is to miss all the fun. (It is of course the extended axiom of counting from definition 4.)

(2) has various components, and they can be teased apart. One part that is not going to be controversial is the claim that every set of ordinals (so, in particular, every initial segment of the ordinals) is wellordered by magnitude. (That was theorem 3) The tricky part is the claim that the ordinal of the wellordering of the ordinals below  $\alpha$  is in fact  $\alpha$  itself.

An interpretation of the theory of sets+pairing+ordinals into set theory is a function  $\mathcal{I} : \mathcal{L}(\text{sets+pairing+ordinals}) \rightarrow \mathcal{L}(\text{sets+pairing})$  satisfying various conditions.  $\mathcal{L}(\text{sets+pairing+ordinals})$  will have a binary relation  $Ord(\alpha, \mathcal{A})$  whose intended semantics is that  $\alpha$  is the order type of  $\mathcal{A}$ .  $Th(\text{sets+pairing+ordinals})$  will contain axioms like

$$(\forall\alpha)(\forall\mathcal{A})(\forall\mathcal{B})(Ord(\alpha, \mathcal{A}) \wedge Ord(\alpha, \mathcal{B}) \rightarrow \mathcal{A} \simeq \mathcal{B})$$

Notice we are not going to assume that  $\mathcal{I}$  will send equality to equality. If  $\mathcal{I}$  does send equality to equality then we call it an *implementation*: there will be particular sets that—according to  $\mathcal{I}$ —are ordinals. Two examples that appear in the literature are von Neumann ordinals (of course) and *Scott's trick ordinals*, where the ordinal of a wellordering is the set of all wellorderings that are isomorphic to it and of minimal rank with that property. Interpretations that do not send equality to equality I have called *fakings* or *simulations* ([36]). A simulation of ordinals in a theory of sets+pairing sends variables ranging over ordinals to variables ranging over sets, but does not send equality between

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<sup>1</sup>... of embraces that clasp and that sever  
of blushes that flutter and flee  
round the limbs of Dolores—whoever  
Dolores may be.

Owen Seaman—with apologies to Swinburne.

ordinal-variables to equality between set-variables, but rather to *isomorphism*. One could express this by saying that according to the interpretation ordinals are sets all right, but have different identity criteria from mere sets. Two objects can be identical as ordinals without being identical as sets. This sounds initially odd, but is perfectly all right. However, it does mean that we have to think of the target language as *typed*—and typed in a way that gives ordinal variables a different type from mere-set variables.

The target language is the language of sets+pairing. It is two-sorted, and the variables that range over ordinals get sent to variables of type **set**.<sup>2</sup> However, the variables that are values that  $\mathcal{I}$  gave to variables that were ordinal-variables in the source language have some rather special syntactic features. In no wff that is a value of  $\mathcal{I}$ , for example, is any  $\mathcal{I}$ -of-an-ordinal-variable ever seen to the right of an ‘ $\in$ ’, or either side of a ‘ $=$ ’. However such a variable may well be spotted either side of a ‘ $\simeq$ ’. Visible syntactic features such as these can be made the basis of a typing algorithm.

If you think that logic is that-which-is-invariant-under-permutations then your attention is immediately drawn to the PHF theorem. If mathematics is the theory of the invariant then the PHF theorem ought to tell us that mathematics is stratified. Mathematics certainly seems to be strongly typed. But beware: Euler’s theorem.

fit in somewhere... given that mathematics is stratified it is highly discordant that ZF provides no stratifiable formula that says “ $x$  is an [implemented] natural number”

## 12.2 The existence of sets of size $\aleph_\omega$ or $\beth_\omega$

Russell and Whitehead (see [101] volume III p 173) certainly had the concept of sets of size  $\aleph_\omega$ , and it is clear that they understood that their system provided no method evident to them of proving the existence of such sets.

“Propositions concerning  $\aleph_2$  and  $\omega_2$  and generally  $\aleph_\nu$  and  $\omega_\nu$ , where  $\nu$  is an inductive cardinal, are proved precisely as the above propositions are proved. There is not, however, so far as we know, any proof of the existence of Alephs and Omegas with infinite suffixes, owing to the fact that the type increases with each successive existence-theorem, and that infinite types appear to be meaningless.”

Skolem ([106] p 297) was of the view that sets... this was one of the reasons for him to adopt replacement.

First we have to show (i) that, for every  $n \in \mathbb{N}$ , there are sets of size  $\aleph_n$  (resp.  $\beth_n$ ). Then we have to do something to get from there to the existence of a set of size  $\aleph_\omega$  (resp  $\beth_\omega$ ). In Zermelo set theory we prove the existence of sets of size  $\beth_n$  for all  $n \in \mathbb{N}$  by a straightforward induction—and it really is

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<sup>2</sup>Careful: we don’t really mean that it is two-sorted: it has a typing discipline but not necessarily any sorts.

straightforward. If  $|x| = \beth_n$  then  $|\mathcal{P}(x)| = \beth_{n+1}$ , and that does it. To prove the existence of sets of size  $\aleph_n$  for all  $n \in \mathbb{N}$  we need to appeal to Rosser's axiom of counting.<sup>3</sup>

The idea is to prove by induction on  $n$  that there is a set of size  $\aleph_n$ . The axiom of infinity—in one form or another—takes care of case  $n = 0$ . It is the induction step that requires subtlety.

Recall that  $\aleph_n$  is the cardinal of a set that can be wellordered to length  $\omega_n$ . Now suppose  $X$  is a set of size  $\aleph_n$ . How do we obtain a set of size  $\aleph_{n+1}$ ? If  $|X| = \aleph_n$  then there are wellorderings  $\subseteq X \times X$  of all lengths less than  $\omega_{n+1}$ . Let  $W(X)$  be the set of wellorderings of subsets of  $X$ . The quotient of  $X$  by isomorphism is thus in 1-1 correspondence with the ordinals below  $\omega_{n+1}$  and is thus naturally wellordered to length  $T\omega_{n+1}$  which is  $\omega_{Tn+1}$ . By the axiom of counting  $Tn+1 = n+1$  so the quotient is naturally wellordered to length  $\omega_{n+1}$ , so its cardinality is  $\aleph_{n+1}$ .

The final inference to the existence of sets of size  $\aleph_\omega$  or  $\beth_\omega$  is a straightforward application of replacement. Since the function  $f : n \mapsto \mathcal{P}^n(V_\omega)$  is total, the image  $f``\mathbb{N}$  is a set, and its sumset  $\bigcup f``\mathbb{N}$  will be of size  $\beth_\omega$ . Similarly our construction of sets of size  $\aleph_n$  for each  $n \in \mathbb{N}$  is canonical so we let  $g$  be the function that sends the natural number  $n$  to the canonical set of size  $\aleph_n$  and by replacement the image  $g``\mathbb{N}$  is a set and its sumset  $\bigcup g``\mathbb{N}$  will be of size  $\aleph_\omega$ .

Now consider the situation in NF and its precursor type theories.

How about the induction to prove that, for every  $n \in \mathbb{N}$ , there is a set of size  $\beth_n$ . If  $|x| = \beth_n$  then  $|\mathcal{P}(x)|$  is not  $\beth_{n+1}$  but  $T\beth_{n+1}$ .

[explain this in TT first and then in NF where it is less clear]

[HOLE At some point one has to have a discussion of why the Specker definition of exponentiation is correct!!! see page 111]

[details]

So let's look at why we can't straightforwardly prove the existence of sets of size  $\aleph_\omega$ .

There are two quite separate problems. First we have to prove that for each  $n \in \mathbb{N}$  there are sets of size  $\aleph_n$ . The obvious way to attempt this is by induction on  $n$ . Then—having done that—we have to use replacement or collection (followed by sumset) to obtain a set of size  $\aleph_\omega$ .

This is when one starts thinking that there is some significance to Mathias' formula  $M$ .  
Stuff to fit in here

Let us first consider the programme to prove by induction on  $n$  that there are sets of size  $\aleph_n$ .

Officially we define  $\aleph_{n+1}$  as the cardinality of the smallest wellordered sets whose sizes are bigger than  $\aleph_n$ . Use of the definite description (“the smallest ...”) obliges us to prove existence and uniqueness, and anyone who has had to explain alephs to students learns rapidly that the best way to do that is to explain  $\aleph_{n+1}$  as the number of nonisomorphic ways there are of wellordering a set of size  $\aleph_n$ . In this we appeal to the extended axiom of counting. (Without

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<sup>3</sup>We need to anticipate the objection that if we can prove the existence of sets of size  $\beth_n$  then we can prove the existence of sets of size  $\aleph_n$  by separation. This requires detailed discussion. If for all  $n \in \mathbb{N}$  there is a set of size  $\beth_n$  then for all  $n \in \mathbb{N}$  there is a set of size  $\aleph_n$ .

the extended axiom of counting we would be able to prove only that if there is a set of size  $\aleph_n$  there is a set of size  $\aleph_{Tn+1}$ .)

If we want to prove the existence of sets of size  $\aleph_\omega$  given that there are sets of size  $\aleph_n$  for every  $n$  the obvious thing to do is pick a set of size  $\aleph_n$  for each  $n$  and take their union. If there is a set of size  $\aleph_n$  for each  $n$  then there is in fact a *canonical* set of size  $\aleph_n$ , namely the set of ordinals of wellorderings whose carrier sets are of size  $T^{-2}\aleph_n$ . We don't need choice! And the function that sends  $n$  to the set of ordinals of wellorderings whose carrier sets are of size  $T^{-2}\aleph_n$  is setlike so the image of  $\mathbb{N}$  in it is a set. Curiously there doesn't seem to be a canonical set of size  $\beth_n$  for  $n$  finite. (Why not  $V_{\omega+n}$ ?)

## 12.3 The Paris-Harrington theorem: a case study

The Paris-Harrington theorem is less well typed than Ramsey's theorem. This was remarked on by Harvey Friedman in an FOM post: (Subject: [FOM] PA Incompleteness; Sun, 14 Oct 2007 10:02:58 -0400)

This section needs to be radically cut back

"In "relatively large", an integer is used both as an element of a finite set and as a cardinality (of that same set).

This is sufficiently unlike standard mathematics, that an effort began, at least implicitly, to find PA incompleteness that did not employ this feature, or this kind of feature."

There is a syntactic difference between Finite Ramsey and Paris-Harrington in that the latter—but not the former—has an occurrence of the predicate *relatively large*. This is significant because this predicate is *ill-typed* in both the senses that we consider in this essay. We start by considering how it is ill-typed in the abstract datatype sense.

In languages like ML there is a polymorphic type-constructor `list`. It acts on an arbitrary type  $\alpha$  to give a type  $\alpha\text{-list}$ . In turn we have `length` which will take an object of type  $\alpha\text{-list}$  and output an object of type `num`. Not—the reader will observe—an object of type  $\alpha\text{-num}$ . We could imagine a more strongly typed language in which `length` was instead a polymorphic object of type  $\alpha\text{-list} \rightarrow \alpha\text{-num}$ . A type-checker for such a language would be unable to find an  $\alpha$  to type the concept of *relatively large* since any attempt to do so would be intercepted by an occurs-check and would crash.

It is unstratified also in the Russell-Quine sense, at least potentially. " $x$  is relatively large" will be unstratified for at least some implementations of `natural-number of`. See page ??.

Unlike Harvey Friedman I regard this failure of typing not so much as a bug that makes Paris-Harrington unsatisfactory (tho' admittedly it does render it so from the point of view of Friedman's project to sell logic to the mathematician on the Clapham omnibus) but rather as a feature that makes it an object of interest in the study of typing in live mathematics. I hope to show how this failure of typing is instrumental in making P-H much stronger than Ramsey's theorem.

It's not hard to see how one can prove  $(\forall nmk \in \mathbb{N})(\exists l \in \mathbb{N})(l \rightarrow (k)_m^n)$  directly by careful applications of standard methods for proving the infinite version.

However it is also possible to deduce this finite version of Ramsey's theorem from the infinite version by a compactness argument. There are several reasons why Ramsey didn't do it that way. For one thing, the first appearance of compactness for predicate logic was not until the following year (1930) in a paper—[55]—of Gödel<sup>4</sup>. For another, the compactness proof is highly ineffective in that no bounds can be recovered from it; nobody in their right mind would try to do it that way unless they had an ulterior motive. We do have such an ulterior motive: since the proof of Paris-Harrington (at least the only proof known to me) proceeds by a compactness argument it is very useful to run through the compactness proof of finite Ramsey by way of a rehearsal for it.<sup>5</sup>

So here is the compactness proof of Finite Ramsay,  $(\forall mnk)(\exists p)(p \rightarrow (m)_k^n)$ , from Infinite Ramsay.

*Proof:* Assume Infinite Ramsey, and suppose that Finite Ramsey is false, so that there are  $n, m, k$  in  $\mathbb{N}$  such that for all  $p \in \mathbb{N}$  there is a set  $P$  with  $|P| = p$  and a colouring  $f : [P]^m \rightarrow \{1, 2, \dots, k\}$  such that there is no set  $X \subseteq P$  with  $|X| = n$  and  $|f''[X]^m| = 1$ . Fix  $n, m, k$ , and for each  $p$  let  $Y_p$  be the set

$$\{f : f : [[1, p]]^m \rightarrow [1, k] \wedge \neg(\exists X) \bigwedge \left\{ \begin{array}{l} X \subseteq [1, p] \\ |X| \geq n \\ |f''[X]^m| = 1 \end{array} \right\}\}.$$

of bad  $k$ -colourings of the  $m$ -tuples of the naturals below  $p$ . ( $k$  and  $m$  are fixed.)

(Beware: square brackets are here being used *both* to denote intervals in  $\mathbb{N}$ —as in ‘ $[1, k]$ ’—and to denote the set of  $m$ -sized subsets of things—as in ‘ $[X]^m$ ’.)

For any  $k$ , the set  $F_k$  of all  $k$ -colourings of  $m$ -tuples of initial segments of  $\mathbb{N}$  is countable. (Each initial segment  $[1, p]$  has only a finite set of  $m$ -membered subsets and there are only finitely many ways of colouring the set of those subsets). So we can uniformly wellorder  $F_k$ . Suppose this to be done, somehow. Then, for each  $p$ , we set  $f_p$  to be the first element of  $Y_p$  in the sense of that ordering.

We are now going to define a (bad) partition  $\pi$  of  $[\mathbb{N}]^{m+1}$  into  $k$  pieces. You are given a set  $x \subseteq \mathbb{N}$  of size  $m+1$  and have to decide which piece to put it into. Its last member is  $p+1$  for some natural number  $p$ .  $x \setminus \{p+1\}$  is now a subset of  $[1, p]$  and is therefore a suitable input for  $f_p$ .  $f_p(x \setminus \{p+1\})$  is now a number  $< k$ , and that tells you which piece to put  $x$  into. (Slightly more formally, put  $x$  into the  $f_{(sup(x)-1)}(x \setminus \{sup(x)\})$ th piece.) So  $\pi$  partitions  $[\mathbb{N}]^{m+1}$  into  $k$  pieces. We will show that  $\pi$  is bad.

With a view to obtaining a contradiction suppose  $X$  to be an infinite set monochromatic for  $\pi$ . Let  $p+1$  be a member of  $X$  (and we will want to be

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<sup>4</sup>Theorem X: thanks to the late Torkel Franzén for the citation.

<sup>5</sup>There are other “reverse” compactness arguments to be found in the literature: for example Friedman's proof of FFF.

able to find arbitrarily large such  $p + 1$ ). Consider those  $(m + 1)$ -tuples from  $X \cap [1, p+1]$  whose last element is  $p+1$ . What does  $\pi$  do to them? It sends every such  $(m + 1)$ -tuple  $x$  to  $f_p(\text{butlast}(x))$ , and—because  $X$  is monochromatic—all these  $f_p(\text{butlast}(x))$  are the same, whatever  $x$  we pick up. Now every  $m$ -sized subset of  $X \cap [1, p]$  can be turned into such an  $(m + 1)$ -tuple by the simple expedient of sticking  $p + 1$  on the end, so  $f_p$  sends every  $m$ -tuple from  $X \cap [1, p]$  to the same number  $< k$ . But that is simply to say that  $X \cap [1, p]$  is a subset of  $[1, p]$  that is monochromatic for  $f_p$ . Now  $f_p$  was chosen so that any set monochromatic for it was of size less than  $n$ . So  $X \cap [1, p]$  is of size less than  $n$ . So—no matter how large we pick  $(p + 1) \in X$ —we find that  $X \cap [1, p]$  has at most  $n$  members. So  $|X| \leq n + 1$  and  $X$  was not infinite, contradicting the Infinite Ramsey theorem. ■

**THEOREM 5** (*Paris-Harrington*)

For every  $n, m, k$  in  $\mathbb{N}$ , there is  $p$  so large that whenever  $f : [\{1, 2, \dots, p\}]^m \rightarrow \{1, 2, \dots, k\}$  there is a relatively large  $X \subseteq \{1, 2, \dots, p\}$  such that  $|X| \geq n$  and  $|f''[X]^m| = 1$ .

*Proof:*

We argue by compactness, as before.

Suppose there are  $n, m, k$  in  $\mathbb{N}$  such that for all  $p \in \mathbb{N}$  there is  $f : [\{1, 2, \dots, p\}]^m \rightarrow \{1, 2, \dots, k\}$  such that there is no relatively large  $X \subseteq \{1, 2, \dots, p\}$  such that  $|X| = n$  and  $|f''[X]^m| = 1$ . Fix  $n, m, k$  and  $p$  and let  $Y$  be the set

$$\{f : f : [\{1, 2, \dots, p\}]^m \rightarrow \{1, 2, \dots, k\} \wedge \neg(\exists X) \bigwedge \left\{ \begin{array}{l} X \subseteq \{1, 2, \dots, p\} \\ |X| > \min(X) \\ |X| \geq n \\ |f''[X]^m| = 1 \end{array} \right\}\}.$$

This time let  $Y_p$  be—not the set of

colourings-that-are-bad-in-the-sense-of-lacking-a-monochromatic-set-of-size- $n$

but the set of

colourings-that-are-bad-in-the-sense-of-not-having-any-monochromatic-sets-of-size- $n$ -that-are-relatively-large.

As before, initial segments of the monochromatic set  $X$  will be monochromatic for the colourings  $f_p$ . Now sets that are monochromatic for  $f_p$  are either smaller than  $n$  or are not relatively large. By considering initial segments of  $X$  that are long enough we can take care of the first condition, so the only way they can manage to be monochromatic for  $f_p$  will be by failing to be relatively large. So, for some large  $j$ , consider the initial segment consisting of the first  $j$  elements of  $X$ . We now know that this is not relatively large, so its first element must be bigger than  $j$ . So the first element of  $X$  is at least  $j$ . But  $j$  could have been taken to be arbitrarily large. ■

### 12.3.1 Stratified and Unstratified versions of Paris-Harrington in NF

To get the compactness proof of Paris-Harrington to work we need the particular class abstract— $Y_p$  on page 183—to be a set. This is because we want  $f_p$  to be the first element of  $Y_p$  in the sense of a global wellordering of the union of all the  $Y_p$ s. There is a such a global wellordering all right, and of course every subset of its domain will have a least element. Note the italics! Mere subclasses are not guaranteed to have least elements.

This holds in general, of course. In the NF context  $Y_p$  will be a set if “ $x$  is relatively large” is stratified. It will be a stratified property as long as **natural-number-of** lowers types by 1 (that is to say,  $k = -1$ ) and not otherwise. Therefore if  $k \neq -1$  we cannot run the compactness argument.

We now have a version P-H( $k$ ) of P-H for each  $k$ . Given that it is customary to use only the standard implementation of **natural-number-of**, (that takes  $|x|$  to be  $[x]_\sim$ , the equipollence class of  $x$ ) we should consider what the various P-H( $k$ ) look like once one reverts to usual practice. P-H( $k$ ) is captured by a doctored version of the syntax for P-H where “relatively large( $x$ )” is replaced by “ $|x| \geq T^k(\min(x))$ ”. It remains to be determined whether the various P-H( $k$ ) for  $k \neq -1$  are all equivalent. That may take a little while. For the moment we can at least establish the following, which is a fairly straightforward application of work of Friederike Körner [70]

**THEOREM 6** *For each concrete  $k$ , NF + P-H( $k$ ) is consistent relative to NF.*

*Proof:*

In [70] it is shown that it is consistent relative to NF (in fact relative to any stratified extension of NF) that there should be a *Körner function*, that is to say  $f : \mathbb{N} \rightarrow \mathbb{N}$  such that  $(\forall n \in \mathbb{N})(n \leq f(Tn))$ . Now let  $f$  be such a Körner function and consider the analogue of P-H that we can prove by replacing “relatively large” by ‘ $|x| \geq f(T(\min(x)))$ ’. This allegation is stratified and can be proved by the usual compactness argument. But observe that if  $|x| \geq f(T(\min(x)))$  then  $x$  is relatively large in the old sense (because  $f(T(\min(x))) \geq \min(x)$ ), so we have proved the original unstratified version of P-H.

All right, so all this proves is that the original unstratified version of P-H is consistent relative to NF. What about the other versions, where we claim the existence of monochromatic sets  $x$  which are relatively large in the sense that  $|x| \geq f(T^k(\min(x)))$ ? As it happens, the Körner function exhibited in [70] satisfies  $(\forall n \in \mathbb{N})(n \leq f(T^k n))$  for each concrete  $k > 1$  and thereby takes care of the remaining cases as well, at least where  $k > 0$ . The Körner function achieves this effect because in the relevant model there is  $n_0$  such that  $(\forall n > n_0)(n < Tn)$ . We define the Körner function  $f$  to be  $\lambda n. (n + n_0)$ . If  $k$  is any concrete natural we have

$$\begin{aligned} f(T^k n) &= T^k n + T^k(T^{-k} n_0) \\ &> T^{k-1} n + T^{k-1}(T^{-k} n_0) \\ &\vdots \end{aligned}$$

check the exponent

$$\begin{aligned} &> n + T^{-k}n_0 \\ &> n \end{aligned}$$

The second line follows from the first because the RHS, being bigger than  $n_0$ , is  $< T(\text{RHS})$  and therefore  $> T^{-1}(\text{RHS})$ —which is the RHS of line 2. The third line follows from the second because  $\text{RHS} > T^{-1}(\text{RHS})$  implies  $T^{-1}(\text{RHS}) > T^{-2}(\text{RHS})$  by the usual isomorphism property of  $T$ . And so on.

For negative values of  $k$  we use a Körner function obtained analogously in a model containing  $n_0$  such that  $(\forall n > n_0)(n > Tn)$ ; we define the Körner function  $f$  to be  $\lambda n.(n + n_0)$  as before. ■

Richard Kaye has pointed out to me that if one thinks of Paris-Harrington as an allegation about the existence of monochromatic *tuples* not sets then no stratification problem arises, since tuples of natural numbers are naturally coded in a homogeneous way as natural numbers.

### 12.3.2 Other versions of Paris-Harrington

The literature is full of variants of P-H where “relatively large” is replaced by “ $|x| > g(\min(x))$ ” for various natural functions  $g : \mathbb{N} \rightarrow \mathbb{N}$ . How do such variants fare in the NF context? We would of course expect these  $\text{P-H}^g$  to show the same variety of strengths in the NF context as they do in their native habitat. In the NF context there is the additional complication that any such variant  $\text{P-H}^g$  of Paris-Harrington multifurcates in the same way the original (“P-H=”) did in section 12.3.1. The question of whether or not these are provable in NF or consistent relative to it can be approached by means of Körner functions as in section 12.3.1. It is a simple matter to check that for any definable arithmetic function  $g : \mathbb{N} \rightarrow \mathbb{N}$  there is an Ehrenfeucht-Mostowski permutation model containing a Körner function  $f : \mathbb{N} \rightarrow \mathbb{N}$  satisfying  $(\forall n \in \mathbb{N})(n < f(g(n)))$ .

### 12.3.3 Concluding Random Thoughts

One glaring omission is an exposition of the proof of independence of P-H from PA, and a corresponding discussion of how the independence is connected with the unstratified nature of P-H. I suspect there are some quite enlightening things one could say about that.

There are other details that merit attention. At present it seems to be an open problem whether or not there can be in NF a type-lowering implementation of **cardinal-of** for all sets. The trick used to obtain a type-lowering implementation of **cardinal-of** for finite sets will work also for wellordered sets whose sizes are sufficiently small alephs. If NCI is finite then NC is countable and is therefore the size of a set of singletons <sup>$k$</sup>  for concrete  $k$  as big as you please. So in those circumstances we would have, for each concrete  $k$ , an implementation of **cardinal-of** making the type difference between ‘ $|x|$ ’ and ‘ $x$ ’ precisely equal to  $k$ . In contrast there cannot be a type-lowering implementation of **ordinal-of**—because of Burali-Forti.

## 12.4 Paris-Harrington redux: Ramsey and Paris-Harrington Again

I return to the material in [38] “Paris-Harrington in an NF context”. Various people<sup>6</sup> had commented that the concept of a *relatively large set of natural numbers* is unstratified, and in that essay I mused about whether or not the extra strength of PH over finite Ramsey was to do with this failure of stratification. In the present—self-contained—note I shall show that—somewhat to my annoyance—it is not: Paris-Harrington has a stratified formulation.

I was able to exhibit in [38] formulations of PH and Finite Ramsey which differed only in their quantifier prefix, and this suggested that the difference in strength is located in the difference between the quantifier prefixes, as below, quoted from [38]:

Finite Ramsey:

For all  $n, m, j$  in  $\mathbb{N}$   
 There is  $k$  in  $\mathbb{N}$  so large that  
 For every set  $X$  of size  $k$  and  
 For every  $m$ -colouring  $\chi$  of  $[X]^j$   
 there is an enumeration  $e$  of  $X$  and  
 there is  $X' \subseteq X$  with  $|X'| = n$  and  $X'$  monochromatic wrt  $\chi$  and  
 relatively large wrt  $e$ .

Paris-Harrington:

For all  $n, m, j$  in  $\mathbb{N}$   
 There is  $k$  in  $\mathbb{N}$  so large that  
 For every set  $X$  of size  $k$  and  
 For every  $m$ -colouring  $\chi$  of  $[X]^j$  and  
 For every enumeration  $e$  of  $X$   
 there is  $X' \subseteq X$  with  $|X'| = n$  and  $X'$  monochromatic wrt  $\chi$  and  
 relatively large wrt  $e$ .

The only difference is in the quantifiers in the fifth line. Locating the difference in the quantifier prefix suggests that stratification is not the key to understanding the situation. However, both these formulations make use of the concept of *relatively large subset*, and so are not stratified. I can now exhibit a pair of formulations of finite Ramsey and PH, both stratified, which differ only in the quantifier prefix. This shows that, rather to my disappointment and surprise, stratification plays no role in the extra strength of PH.

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<sup>6</sup>Subject: [FOM] PA Incompleteness; Sun, 14 Oct 2007 10:02:58 -0400): Harvey Friedman wrote

“In “relatively large”, an integer is used both as an element of a finite set and as a cardinality (of that same set). This is sufficiently unlike standard mathematics, that an effort began, at least implicitly, to find PA incompleteness that did not employ this feature, or this kind of feature.”

First, some notation:  $\mathcal{P}_n(x)$  is  $\{y \subseteq x : |y| = n\}$ , and (this is new)  $\mathcal{L}(x)$  is  $\{y : y \cap x \neq \emptyset\}$ . (It's an upside-down ' $\mathcal{P}$ ' reflecting<sup>7</sup> the fact that the operation is dual to power set.) By abuse of notation we will write ' $\mathcal{L}(x)$ ' when  $x \subseteq X$  ( $X$  clear from context) to mean  $\{y \subseteq X : y \cap x \neq \emptyset\}$ .

The new thought is that we should think of PH as saying not so much that there is a monochromatic set with special properties, but rather that (in contrast to Ramsey, which only promises one monochromatic set) the set of  $\chi$ -monochromatic subsets of  $X$  is *large* in the sense that, for every total order  $<$  of  $X$ , it meets  $\mathcal{L}$  of the initial segment containing the first  $n$  elements of  $\langle X, < \rangle$ . That sounds like a quantifier.

Finite Ramsey says

For all  $n, m, j \in \mathbb{N}$   
 There is  $k \in \mathbb{N}$  so large that  
 For every set  $X$  of size  $k$   
 For every  $m$ -colouring  $\chi$  of  $[X]^j$   
 There is  $X' \subset X$  with  $X'$  monochromatic wrt  $\chi$ .

We can rephrase the last line to get

For all  $n, m, j \in \mathbb{N}$   
 There is  $k \in \mathbb{N}$  so large that  
 For every set  $X$  of size  $k$  and  
 For every  $m$ -colouring  $\chi$  of  $[X]^j$   
 The set  $M_n^\chi$  of  $n$ -sized subsets of  $X$  monochromatic for  $\chi$  is nonempty.

Now for the new formulation of PH:

For all  $n, m, j \in \mathbb{N}$   
 There is  $k \in \mathbb{N}$  so large that  
 For every set  $X$  of size  $k$  and  
 For every  $m$ -colouring  $\chi$  of  $[X]^j$   
 The set  $M_n^\chi$  of  $n$ -sized subsets of  $X$  monochromatic for  $\chi$  meets  
 everything in  $\mathcal{L}(\mathcal{P}_n(X))$ .

...and the difference between these two [purely in the fifth line] is that one says that the set of monochromatic subsets of size  $n$  is nonempty, whereas the other says that it meets every member of a fairly large set.

It's probably worth saying a few words about why this version of PH is equivalent to the usual version that asserts the relative largeness of the monochromatic set. A subset of  $\mathbb{N}$  of size  $n$  is relatively large simply if it has a member smaller than  $n$ . But this is simply to say that an  $n$ -sized subset  $X' \subseteq X$  is relatively large wrt an ordering  $<$  iff one of its members is among the first  $n$  members of  $X$  according to  $<$ . For the other direction, if  $Y$  meets some element  $X' \in \mathcal{P}_n(X)$  then it is relatively large wrt any enumeration of  $X$  that counts  $X'$  using only the naturals  $\leq n$ .

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<sup>7</sup>“reflecting” (joke!—geddit??)

This formulation prompts some natural questions. Does this version have a slick compactness proof? Does it give a slick proof of  $\text{Con}(\text{PA})$ ? Does it suggest formulations of analogues of PH for uncountable cardinals?

The new formulation of PH asserts that  $M_\chi^n$  meets every set in the family  $\mathcal{L}$  “ $\mathcal{P}_n(X)$ . Now we know the size of this family (it's  $\binom{k}{n}$ ) and we know the size of all the members of that family (and all these values of  $\mathcal{L}$  are of size  $(2^n - 1) \cdot 2^{k-n}$ ) so we have a lower bound on the size of  $M_\chi^n$  purely in terms of  $k$  and  $n$ . Specifically we can find  $k/n$  pairwise disjoint subsets of  $X$  of size  $n$ , and no monochromatic set of size  $n$  can meet more than  $n$  of them, so  $|M_\chi^n|$  must be at least  $k/n^2$ .

What about the version that says there is a 2-large monochromatic set?

Is there an analogue of Paris-Harrington for the first  $\kappa$  s.t.  $\kappa \rightarrow (\kappa)_k^n$ ?

# Chapter 13

## Miscellaneous Topics

Stuff to fit in:

Have a go at Fictionalism?  
generalised  $T$ -functions.

Can one say that the Russellian device of stratification is really an operationalist analysis? (“sets of level 2 don’t *do* membership of sets of level 4”) I think not: the point is that the Russellian move is coercive to use Charles Pigden’s expression.

### 13.1 Synonymy

One notion that has emerged is that of **synonymous theories**. Synonymy of two theories is a very strong kind of mutual interpretability. Roughly, two theories are synonymous iff they have the same models. A standard example is Boolean rings and Boolean algebras. Recently Kaye-Wong [65] will lead us to synonymy between ZF-minus-infinity-plus-TCo and Peano Arithmetic. Synonymy will crop up here, later.

### 13.2 Emergence and Reduction

The usual reductions of

- (i) integers to equivalence classes of ordered pairs of naturals
- (ii) rationals to equivalence classes of ordered pairs of integers
- (iii) reals to Cauchy sequences/Dedekind Cuts

do seem to be regarded as reductions not emergences. If the difference between reducing reals to Cauchy sequences and to Dedekind cuts were mathematically substantial then the tone of the discussions about it would be quite different from what it is. The choice of reduction seems invariably to be regarded purely as a question of which makes for the most felicitous exposition.

Marcel Crabbé is guilty of a wonderful observation that “emergence is an adjoint (functor) of reduction(ism)”<sup>1</sup>. Does one want to think of what we are about to do as reducing sets+ordinals to sets? Or do we want to think of it as watching ordinals emerge from sets? There may well be no significance to this of course—mathematics just might not be dynamic in a way that could give sense to such talk of a difference—but then again there might. Sets are paradigmatically discrete objects, as are wellorderings and ordinals. It is very natural to think of ordinals as emerging from sets, what with both sorts of chaps being discrete and all that. Can continuous things emerge from discrete things? I can imagine this idea finding a frosty reception. There are plenty of people who think that the continuous and discrete are equal and independent pillars of mathematics, neither more fundamental than the other.

Perhaps it’s a slight imperfection in Crabbé’s formulation that will enable us to resolve the contradiction in my message to fom:

### 13.3 The Discrete and the Continuous

Consider the following assertions, all defensible, each admitting versions held by at least one reasonable person...

1. All of Mathematics can be reduced-to/interpreted-in/etc Set Theory;
2. Set Theory is Discrete;
3. The Discrete and the Continuous are two twin pillars of Mathematics, neither to be explained in terms of the other.

They can’t all be true, so which one, Dear Reader, do you not accept?

The combinatorics of  $\in$  make the discovery of wellordering inevitable. Not so the reals. This is worth noting in view of the huge importance that set-theoretic analysis has had in the development of set theory. Do reals emerge from sets? We can certainly provide interpretations of a higher-order theory of reals into set theory. A banal fact that is not commented on is the fact that the results of set-theoretic analysis are remarkably insensitive to the way in which we think of reals as sets. If we wish to embrace the intuition that the continuous cannot (or at least should not without recourse to perverse [cruel-and-unusual] compulsion) be reduced to the discrete then one might want to say that Real Analysis can be *reduced to* Set Theory, but does not *emerge from* it. In this

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<sup>1</sup>Concerning the idea ...that emergence is an adjunct (functor) of reductionism, I find it extremely appealing indeed to say that in some circumstances “A is reducible to B” amounts to “A emerges from B”. I definitely will prefer to say that arithmetic emerges from logic (or set theory) [rather] than to say that it is reducible to logic.

This I think sheds the right light on Frege’s “Sie sind in der Tat in den Definitionen enthalten, aber wie die Pflanze im Samen, nicht wie der Balken im Hause.” [51] (p 88). “they are indeed contained in the definitions, but rather in the way that plants are contained in seeds, not in the way that timbers are contained in a building.”

Marcel Crabbé, personal communication

I have to be very careful how I state this ...

context it might be worth recalling how tricky it is to prove that  $|\mathbb{R}| = 2^{\aleph_0}$ . (The existence of countably many rationals that have two representations as subsets of  $\mathbb{N}$  by virtue of having a denominator that is a power of 2 means that—along that route at any rate—we have to use Bernstein’s lemma to prove that if you take countably many things from a set of size  $2^{\aleph_0}$  what is left is still of size  $2^{\aleph_0}$ . Perhaps the continued fraction expansion is best). The annoying difficulty of this proof might be a consequence of the unnatural nature of the task of reduction.  $\mathbb{R}$  is a continuous object, and  $\mathcal{P}(\mathbb{N})$  is a discrete object. The more fundamental the cleavage between the Discrete and the Continuous the harder we should expect to find the endeavour of finding a bijection between these two objects.

While we are about it, this cleavage might explain why CH is so intractable.  $\mathbb{R}$  is a continuous object and the second number class is a discrete object. However it doesn’t explain why CH is so much more intractable than showing that  $|\mathbb{R}| = |\mathcal{P}(\mathbb{N})|$ .

## 13.4 Replacement and Lofty Indifference—Again

Zermelo set theory satisfies stratified replacement but then so does NF.

The replacement deniers do not allege that replacement has false consequences, merely that its distinctive consequences (that is: those theorems of ZF that are not already theorems of Z) are not part of ordinary mathematics.

Lofty indifference about ordered pairs will tell us that if we have two pairing functions  $\langle x, y \rangle_1$  and  $\langle x, y \rangle_2$  with associated unpairing operations then we expect that whenever  $\mathfrak{M}$  is a model of whatever-set-theory-it-is-we-favour then the two structures we obtain by expanding  $\mathfrak{M}$  by adding the two kinds of pairing are elementarily equivalent as structures for the typed language. The typed language has two types: **set** and **pair**, and syntactical rules as on page 143.

The point is that in the typed language there is no way of saying that the pair is Wiener-Kuratowski.

We now note that the formula asserting the existence of cartesian products is well-typed in this language. So if there is even one flavour of ordered pair that gives you all cartesian products then all flavours do, and then you get replacement.

Any two setlike pairing/unpairing suites are iso? Depends on whether or not everything is an ordered pair. This question of whether or not everything is a pair seems to matter. Look into it.

It does seem to make a difference whether (i) one thinks of ordered pairs, cardinals, ordinals, reals etc as things that emerge from sets or, rather, (ii) conceives them instead to be authentic autonomous mathematical objects that can—as it happens—be implemented in set theory.

Nobody seriously disputes the possibility (ii) of interpreting all these entities—and everything else—into set theory; after all that is why set theory always appears (albeit sometimes only very briefly) like a Greek chorus at the start of any

Is this really true?

Something wrong here!

mathematical text with even the most cursory foundational brief. A generally unspoken corollary of this possibility is that there may be more than one way of implementing these entities, and—an even more unspoken corollary—that the available variety might matter.

The first view (above) is that all these things emerge from set theory. I suppose there is nothing in the dictionary definition of ‘emergence’ that requires that widgets emerge from gadgets in only one way but the possibility of their emerging in more than one way never seems to be one deemed to merit discussion? Why might you believe that there is essentially one correct way to implement ordinals in set theory? If you think that ordinals emerge from sets. There is clearly no logical compulsion in either direction, but there is a good fit. Perhaps one can say that most modern set theorists *behave as if* they believed that mathematics emerges from set theory.

It’s not clear in the early history of set theory which of these two views underlay the project. However, now 100 years later, the incredulity of set theorists on being reminded that ordered pairs and cardinals are not set-theoretic notions suggests that they regard these objects as emerging from sets.

If we can make clear the difference between “*A* emerges from *B*” and “*A* can be implemented in *B*” I will definitely be of the view that all these entities can be implemented in set theory and not of the view that they emerge from it. It’s plausible that the concept of cardinal emerges from the concept of set. It is far less plausible that the concept of ordered pair arises from the concept of set. What about pairing functions over IN, for heaven’s sake?

My decades-long study of the Quine systems (with their implementations of pairing, cardinal and ordinal so different from the home life of our own dear ZF) has not been wasted: it has ensured that the possibility of multiple implementations is never far from my mind, and has saved me from two widespread and grave misunderstandings. It is standard that Zermelo set theory does not prove that every wellordering is isomorphic to [the  $\in$  relation restricted to] a von Neumann ordinal. People say that this means that ordinals  $\geq \omega + \omega$  cannot be defined. One even sometimes hears students say that if one does not assume AC then cardinals cannot be defined. This, surely, is conclusive evidence that they have been badly taught, and badly taught by people who have not properly appreciated that mathematical entities (in this case cardinals and ordinals) can be implemented in more than one way *and that this matters*

# Chapter 14

## Appendices

### 14.1 The law of small numbers

On 07-Feb-10 17:12:03, Thomas Forster wrote:

You have probably heard this old saw about how a hugely disproportionate number of American presidents have been left-handed.

My reading of this is that the dataset of American presidents is one of great interest (to Yanks at least) and has therefore been pored over by lots and lots of people and been examined wrt more parameters than it has degrees of freedom, so *of course* one of them is going to show up something disproportionate.

Am i correct in thinking that that is all that is going on?

Ted Harding replies:

It is possible, even possibly probable, that you may perhaps be correct in thinking this.

However, lacking definitive data, I have had to consult Wikipedia.

[http://en.wikipedia.org/wiki/Handedness\\_of\\_Presidents\\_of\\_the\\_United\\_States](http://en.wikipedia.org/wiki/Handedness_of_Presidents_of_the_United_States) says that data on handedness of Presidents is hardly available prior to 1929 (though another source gives also the 18th and 20th as left-handed, but says nothing about 19 and 21-30), and gives the following for Presidents 31-44:

- 31 Herbert Hoover 1929-1933 Left-handed
- 32 Franklin D. Roosevelt 1933-1945 Right-handed
- 33 Harry S. Truman 1945-1953 Left-handed (ambidextrous)
- 34 Dwight D. Eisenhower 1953-1961 Right-handed
- 35 John F. Kennedy 1961-1963 Right-handed
- 36 Lyndon B. Johnson 1963-1969 Right-handed
- 37 Richard Nixon 1969-1974 Right-handed
- 38 Gerald Ford 1974-1977 Left-handed
- 39 Jimmy Carter 1977-1981 Right-handed
- 40 Ronald Reagan 1981-1989 Ambidextrous (could also be LH)
- 41 George H. W. Bush 1989-1993 Left-handed

- 42 Bill Clinton 1993-2001 Left-handed  
 43 George W. Bush 2001-2009 Right-handed

So that is 6 (max) out of 12.

According to

<http://en.wikipedia.org/wiki/Left-handedness>

“Left-handedness is relatively uncommon; seven to ten percent of the adult population is left-handed.”

So, giving it its best chance,

```
binom.test(6,12,p=0.10,alternative="greater")
```

Exact binomial test

number of successes = 6, number of trials = 12, p-value = 0.0005412

95 percent confidence interval: (0.2452998, 1.0000000 )

and giving it its worst:

```
binom.test(6,12,p=0.07,alternative="greater")
```

Exact binomial test

number of successes = 6, number of trials = 12, p-value = 7.509e-05

95 percent confidence interval: (0.2452998, 1.0000000 )

Either way, there is highly “significant” evidence ( $P < 0.001$ ) of an excess of lefthandedness amongst the last 12 Presidents (those for whom data are available). And the lower 95% confidence limit ( $0.2452998 \approx 0.25$ ) is well above the 10% upper end of the range (7-10 per cent) that Wikipedia states for the general population. Even the 99.9% lower confidence limit ( $0.1120211$ ) is still above 10 per cent (though not by much).

Then of course there is the issue you raise. Suppose people were scrutinising many traits (one of them handedness), varying independently of each other.

For two traits, the chance that one or the other (or both) would exceed the “ $P = 0.05$ ” threshold would be  $1 - (0.95)^2 = 0.0975$ , so not “significant” evidence at all.

However, the chance that one of the other would be as extreme as “ $P = 0.0005$ ” would, similarly, be  $1 - (0.9995)^2 = 0.00099975$ , so still highly “significant”.

Poring over  $N$  traits, to get “barely significant” (“ $P = 0.05$ ”) result (at least 1 trait out of  $N$  “significant” at “ $P = 0.0005$ ”) would require  $1 - (0.9995)^N = 0.05$ , so  $N = \log(0.95)/\log(0.9995) = 102.5609$ , so  $N = 103$  or greater. So you have to go up to 103 traits before the result is not “significant”.

Now you have to consider what traits are available for general perusal, for people to pore over. I would think that there are not that many physical traits of US Presidents that can be examined by the public for “something disproportionate”. Some, of course, will be known to a few privileged insiders; but I doubt that Marilyn Monroe and Monica Lewinsky *et al.* have shared their knowledge widely.

Executive Summary: It does look as though there is an excess of left-handers amongst the US Presidents (at least amongst the dozen or so for whom the facts are known).

Sorry, I did try not to make this serious!  
Ted.



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## 14.2 stuff to be blended in

Looking again at the notes below, section 14.2.1, years later (september 2017) i think that the principle i wrote on the board must have been “every equivalence relation is effective”.

### 14.2.1 A talk at BEST

The categorists say that an equivalence relation  $\sim$  is **effective** iff there is a function  $f$  such that every  $\sim$ -equivalence class is  $f^{-1}\{\{x\}\}$  for some  $x$ . The assertion that every equivalence relation is effective seems to be the least that one has to add to GB (minus foundation!) to make possible the various constructions (à la Scott’s trick) that set theorists take for granted. It appears to be a useful principle of set (class) theory that is worth isolating for study in its own right

Of course when the equivalence classes are small enuff to be sets, there is no problem. However in a significant number of cases of mathematical interest the equivalence classes are proper classes, but nevertheless still correspond to—or are proxies for—things that are mathematical objects that are of interest in their own right. One thinks of cardinals and ordinals. We know that by use of Global choice or Scott’s trick we can always implement cardinals and ordinals. We also know that there is a way of thinking of cardinals and ordinals *virtually*, so that we interpret a strongly typed theory of cardinals or ordinals into set theory without actually implementing cardinals or ordinals. This strong typing prevents us from asking, in these interpretations, any questions like “Is 3 a member of 5?” If we want to be able to give answers to questions like that it is not enuff to have the natural strongly typed virtual interpretation, but we would have to have an actual implementation as well. There is an old result of GAUNT’T’s to the effect that it is consistent with ZF (without choice or foundation) that there is no such implementation.

The principle (point to board) is precisely what one needs ZF to have in order to deduce that mathematical objects arising from equivalence classes are adequately represented in the set theory as objects.

Now let’s take sets and set pictures.

I won't say too much about what a set picture is. It's something that looks like the  $\in$ -diagram of a transitive set. Wellfounded extensional relations ("BFexts"), APGs, irredundant trees. All these have been used.

There is a relation between set pictures which looks like set membership. Picture-1 E picture-2 iff the set that picture-1 is a picture of is a member of the set that picture-2 is a picture of.

Now, if we have unstratified  $\Sigma_1$ -replacement (as in KP) we can show that any picture corresponds to a set. (This is what the axiom in KP is there for!) This of course works only if we are looking at wellfounded set pictures. If the pictures are illfounded then we need an antifoundation axiom like that of Forti and Honsell.

This gives us a relation on pictures of corresponding-to-the-same-set. This equivalence relation can give us a virtual theory of sets in the same way that equipollence gives us a virtual theory of cardinals. It gives us consistency proofs of theories in the language of set theory.

But there is a third trick up our sleeves. We can use the principle to implement the equivalence classes as sets and then do RB. (Explain)

Discuss its relations to (i) Coret's Axiom, (ii) Von Neumann's axiom, (iii) foundation

Coret's axiom implies the principle for equivalence relations corresponding to stratifiable formulæ. This is worth proving!!

Implied by Global Choice

Implied by foundation

It's a theorem of the Quine systems, and Church wanted it as an axiom for his system.

Say something about inner model of sets hereditarily the same size as a wellfounded set. Clearly it is a model for Coret's axiom.

GB is Gödel-Bernays set theory, without choice or foundation. Let  $\sim$  be an equivalence relation on  $V$ , so that it is a class of the model. If there is a function  $f : V \rightarrow V$  such that  $(\forall xy)(x \sim y \longleftrightarrow f(x) = f(y))$  we say (with the categorists) that  $\sim$  is **effective**. In this note I consider the theory announced in the title.

In the course of my misspent life I have accumulated the following facts, which seem to me to be connected.

1. If the axiom of foundation holds, then there is always such an  $f$ . Simply take  $f(x)$  to be the set of those  $y$  s.t.  $y \sim x$  and  $y$  is of minimal rank. This is **Scott's Trick**. (Worth noting that in this case we typically do NOT have  $x \sim f(x)$ . This may be worth thinking about later, even if not necessarily now.)
2. If Global choice holds, then one can take  $f(x)$  to be the first  $y$  s.t.  $x \sim y$ .

3. If Coret's axiom B holds, which says that every set is the same size as a wellfounded set, then such an  $f$  exists for any  $\sim$  that is definable by a stratifiable formula. Er . . . no! See below.
4. It is known that if choice and foundation fail, then even stratified equivalence relations can fail to have corresponding functions. Gaunt showed in the LA 1967 volume that there can even fail to be a definable function sending two arguments to the same value iff they are the same size.
5. Coret's axiom B has the nice property that both ZF + foundation and ZF + antifoundation are extensions of ZF + B which are conservative for stratifiable formulæ.
6. ZF + foundation and ZF + Antifoundation give equivalent categories of sets. (Not sure what this means exactly but Steve Awodey told me and I believe him!)
7. Takahashi's proof that every  $\Sigma_n^{\mathcal{P}}$  formula is in  $\Sigma_{n+1}^{Levy}$  uses foundation. Does it really need foundation?
8. There are various facts about  $\mathcal{P}$ -embeddings which probably fit in here.

Two things we can do with it:

1. It is precisely what we need if we are to use Rieger-Bernays models to prove the consistency of antifoundation;
2. It's also precisely what we need to concretise ultrapowers!

Actually, the ability to concretise ultrapowers is not as useful as one might think. You care about the ultrapower being wellfounded only if the thing that it's an ultrapower of is wellfounded. But if you are taking an ultrapower of the wellfounded part of the universe then the equivalence classes all consist of wellfounded things anyway, so Scott's trick is available to you.

Mind you, this might not be precisely the right way to approach this idea: an approach through pure set theory might be nicer, but then there is a question about the axiomatisability. Is one trying to axiomatise the class of models  $\mathfrak{M}$  of ZF with the property that whenever the extension of a formula  $\phi$  with two free variables is an equivalence relation  $\subseteq \text{dom}(\mathfrak{M})$  then there is another formula  $\psi$  with two free variables such that  $\mathfrak{M} \models (\forall xy)(\phi(x, y) \longleftrightarrow \psi(x) = \psi(y))$ ? The gnumber of  $\psi$  might not depend on the gnumber of  $\phi$  in any nice way. (There is a similar issue in trying to axiomatise ZF + global choice. My recollection is that the theory of all models of ZF that admit a definable global choice function is just ZFC. Not sure if that is true, but something like it must be.)

I am particularly intrigued by the possibility that this principle might be something to do with foundation. It's obviously implied by foundation but presumably there is no converse. (This surely should not be hard to establish! Why not do a R-B construction using the transposition  $(\emptyset, \{\emptyset\})$ ?)

I claimed above (item 3 in the list on page 205) that Coret's axiom B implies that every homogeneous equivalence relation is effective. Sadly this isn't true.

Let  $x$  be an arbitrary set, and  $n$  a concrete natural. Then  $\bigcup^n x$  is the same size as a wellfounded set, in virtue of a bijection that can be extended to a permutation  $\pi$  of the universe.  $(j^n\pi)(x)$  is now a wellfounded set that is  $n$ -equivalent to  $x$ . So every  $n$ -equivalence class (i.e., every orbit of  $J_n$  for some  $n$ ) contains a wellfounded set. That was the fact which I was misconstruing as the fact that every equivalence class contains a wellfounded set.

Now let  $\sim$  be a homogeneous equivalence relation. It's symmetrical so we can think of it as a class of unordered pairs. Since  $\sim$  is homogeneous this set of pairs is symmetric and must contain a wellfounded pair. However that isn't enough, since we want every  $\sim[x]$  to have a wellfounded member

It seems that what we have to do is establish that, for every  $x$ , there are  $y$  and a permutation  $\pi$  s.t.  $x \sim y$  and  $(j^n\pi)(x) = x$  and  $(j^n\pi)(y)$  is wellfounded. Then  $x \sim (j^n\pi)(y)$  and  $(j^n\pi)(y)$  is wellfounded. The starting point for  $\pi$  is a map between  $\bigcup^n y$  and a wellfounded set. If  $\bigcup^n y$  and  $\bigcup^n x$  are disjoint then we are happy, so we OK as long as  $(\exists y)(x \sim y \wedge \bigcup^n x \cap \bigcup^n y = \emptyset)$ . How might there not be such a  $y$ ? Well, there must be transitive subsets of the symmetrical reflexive relation  $x$  related to  $y$  if  $\bigcup^n x \cap \bigcup^n y \neq \emptyset$ , so we have a problem.

In fact we have a counterexample!! Consider the homogeneous equivalence relation  $x \sim y$  iff  $\bigcup^n x = \bigcup^n y$ . It's just not true that every equivalence class contains a wellfounded set! A even simpler counterexample is the identity relation. Duh! So: what was I mistaking this for?

In this connection we should reflect that there are a number of theorems in ZF + foundation that seem to need foundation. For example

1. The theorem that  $\Sigma_n^P \subseteq \Sigma_{n+1}$ . Here  $\Sigma_{n+1}$  is the class of formulæ that have  $n+1$  **unrestricted** quantifiers, the leading one being existential, and any number of restricted quantifiers in the style  $(\forall x \in y)(\dots \text{ or } (\exists x \in y)(\dots))$ . In  $\Sigma_n^P$  formulæ one counts as restricted even those quantifiers in the style  $(\forall x \subseteq y)(\dots \text{ or } (\exists x \subseteq y)(\dots))$ .
2. Replacement  $\rightarrow$  collection. (Interestingly the result doesn't restrict to stratifiable formulæ, as witness Adrian's *aperçu* about "for every  $n$ , there is a  $n$ -sized set of infinite sets all of different sizes".)
3. Assuming foundation one can prove in ZF that AC follows from the assertion that the power set of any wellordered set is wellordered.

One wonders whether any of these might follow from the Principle above.

Let  $T$  be  $GB$  plus the principle that every equivalence relation is effective.

For what class  $\Gamma$  of formulæ can we prove that  $T$  is conservative over  $GB$  for formulæ in  $\Gamma$ ?

For what class  $\Gamma$  of formulæ can we prove that  $ZF + B$  is conservative over  $T$  for formulæ in  $\Gamma$ ?

There are parallels here with global choice. Slightly complicated by the fact that GC is  $\Sigma_1^2$  but my principle is  $\Pi_2^2$

James sez  $V = HOD$  is the theory of those models of ZF that are the set parts of models of GB with global wellordering.  $OD(X)$  is first-order because of reflection. if  $V = HOD$  everything is definable in a  $V_\beta$  and the Lang is worderable.  $L$  of a cohen real satisfies AC but not  $V = HOD$ .

On Tue, 2 Sep 2003, Thomas Forster wrote:

Am i correct in thinking that the theory of all models of ZFC admitting a definable global choice function is precisely ZFC?

Thomas

Umph. Any model of ZFC is the class of sets of some model of NBG with global choice. The only issue is definability.

Take “definable” to mean “ordinal definable allowing a set of ordinals as parameters”.

Let  $\Phi$  be the sentence “for every set  $S$  of ordinals there is a set  $T$  of ordinals such that  $T$  is not ordinal definable from  $S$ ”.

Then  $\Phi$  is false in any model with a definable global well-ordering. However I should think you would get a model in which  $\Phi$  is true by starting from  $L$  and adding a Cohen subset of every regular initial ordinal using reverse Easton forcing, so that all the “later” subsets are generic over the “earlier” ones.

Adrian

Ian Roberts writes

Dear Thomas

First, I'd actually be rather interested in what you're writing on the philosophy of maths.

Second, I don't know of any language that uses a different counting system altogether depending on what it's counting. An extreme case that one could imagine would be using a vigesimal system in counting inanimate objects and a decimal one in counting animate objects. The nearest thing I can think of is that numbers sometimes show grammatical gender. A really banal example is the word for ”one” in the Romance languages, which varies for masculine or feminine depending on the noun. In Welsh, you have gender distinctions up to four. In some Slavonic languages, I think it goes to five. The Bantu languages have very elaborate so-called gender or noun-class systems which classify objects according to animacy, humanness and shape (round things, long things, etc); I don't know if these classes are marked on numerals but I suspect they are. Many languages, including notably Chinese and Japanese, don't have singular-plural distinctions but treat all nouns as mass nouns (like “milk”, which has no plural unless you talk about types of milk), and obligatorily insert a classifier between a numeral and a noun. The classifier depends on some rather hard-to-define property of a class of objects, again very often their shape. The nearest thing in English is “three head of cattle”. So in Mandarin you have to say “three flat-thing book”. I suspect, though, that none of these wonders is really what you're looking for.

A little anecdote that you may know about: in large parts of the north of England, sheep farmers have special words for counting sheep. These words are very obviously of Celtic origin, being strikingly similar to the Welsh numerals.

Ann Copestake writes

Some languages have different numerals for humans and non-humans - e.g., most of the Formosan languages distinguish. Of course languages like Chinese have a classifier system, which means that the numbers are constant but, in effect, the units change - I'm not sure of the boundaries between this and a morphologically marked distinction in numerals. I may be able to track down a reference for a typology of numerals when I'm in my office but don't know of one offhand. I know people who will know, so let me know if you get stuck and I'll ask.

Cheers,

Ann

PS - do you know Jim Hurford's work? He had a book some years ago on linguistics and numerals that might be relevant.

Dear Thomas

The following two books may be helpful (both are in the library), though I admit I've read neither:

Language and number : the emergence of a cognitive system / by Hurford, James R. 1987.

The linguistic theory of numerals / by Hurford, James R. 1975.

Certainly there are languages where different sets of cardinal numerals have specialized uses.

In Latin, with 'pluralia tantum' such as *castra* 'military camp' (i.e. nouns that are plural in form but singular in meaning) you could not use the ordinary cardinals (*unus*, *duo* etc.) but had to use rather the so-called 'distributive numerals' (*singuli*, *bini* etc.), which mean normally 'two each' or 'in twos' etc. Thus, 'two camps' was not \**duo castra* but *bina castra*.

In Russian, to count human beings, it used to be that you didn't use the ordinary cardinals (*chetyre* 'four' etc.) but had to use so-called 'collective numerals' (*chetvyro* 'foursome' etc.)

In northern England, there are or used to be special numerals (Celtic-derived) for counting livestock (*yan*, *tan*, *tethera*, *methera*, *pimp* etc.).

I think I read somewhere that, in the Philippines, Spanish numerals are still used for counting money whereas native (Tagalog etc.) numerals are used for other purposes.

So there's quite a lot of it about ...

Best regards

Andrew

Andrew Carstairs-McCarthy

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—Original Message—

From: Thomas Forster [mailto:T.Forster@dpmms.cam.ac.uk]  
 Sent: Tue 30/03/2010 23:27  
 To: Andrew Carstairs-McCarthy  
 Subject: Dear Valued Linguist

Or rather, dear \*tame\* linguist....

Sorry to batten on you like this, but this is what happens to you if you know people like me. At the moment i am trying to write a book on philosophy of mathematics (don't worry, i'm not going to ask you to read it) but i have encountered a phenomenon that i suspect has parallels in Natural Language. In all the natural languages known to me (all of them indo-european, admittedly) the number words are what the CS people would call \*monomorphic\*: you use the same numerals to count offspring as you use to count food objects or rivals. It is alleged that in some natural languages the number words are \*polymorphic\*, which is to say that distinct suites of number words are used for counting different types of objects. Can you point me to any literature on this matter..?

I would be quite pathetically grateful!

Thomas

Dear Thomas

There are numerous variations on the Pennine Celtic-derived numbers; a Google search will lead to more info, I'm sure.

Perhaps I should mention too that, in Chinese and some other east Asian languages, most nouns can't be qualified directly by a numeral at all. By that I mean that they have 'classifier systems': you can't say e.g. 'three girls', you have to say 'three person girl'. Likewise, not 'eight plums' but 'eight fruit plum'. Compare English 'three head of cattle'. Usually the classifier to be used with a given noun is lexically specified, but sometimes (I gather) different classifiers are used according to the meaning: thus 'four doors' will have different classifiers according to whether what is meant is 'four doorways' or 'four hinged pieces of wood'.

And then of course there are the languages in which you can't count at all. Some have instead three or four grammatical 'numbers': singular, dual, (trial,) paucal, where 'paucal' means 'more than two' or 'more than three', as appropriate. This system may or may not be supplemented by reference to body parts, e.g. \*hand hand foot toe toe\* to mean 'seventeen'.

Supposedly, Pirahã (Brazil) doesn't have even that. They have words that mean 'approximately one', 'approximately two' and 'more than approximately two'. The linguist Dan Everett tried teaching the Pirahã (at their request) to do arithmetic in Portuguese, but they were hopelessly bad at it. See Dan's very readable book \*Don't Sleep, There Are Snakes\*, as well as the considerable scholarly literature of recent years on Pirahã cognition and language.

Thomas,

The answer is yes. The result is due to a lot of people (including Jensen and myself). Basically one forces to add a generic well-ordering of the universe without adding new sets. Felgner published this in 1971: F. published this. (*Fund. Math.*, 71(1971), pp. 43–62)

I also found the following relevant paper of Gaifman:

Global and local choice functions, *Journal Israel Journal of Mathematics*  
Volume 22, Numbers 3-4 / December, 1975 Pages 257-265

Global and local choice functions

**Abstract** We prove, by an elementary reflection method, without the use of forcing, that ZFGC (ZF with a global choice function) is a conservative extension of ZFC and that every model of ZFC whose ordinals are cofinal (from the outside) with  $\omega$  can be expanded to a model of ZFGC (without adding new members). The results are then generalized to various weaker forms of the axiom of choice which have global versions.

—Bob Solovay

On Mon, Apr 5, 2010 at 1:42 AM, [T.Forster@dpmms.cam.ac.uk](mailto:T.Forster@dpmms.cam.ac.uk) wrote: Is Goedel-Bernays + global choice a conservative extension of ZFC..?

FOM mailing list

[FOM@cs.nyu.edu](mailto:FOM@cs.nyu.edu)

<http://www.cs.nyu.edu/mailman/listinfo/fom>

In his recent posting (April 5, 2010), Thomas Forster asks:

Is Goedel-Bernays + global choice a conservative extension of ZFC?

The answer is a resounding "yes".

The usual way of seeing this is to show, via forcing, that every countable (but not necessarily well-founded) model  $M$  of ZFC can be expanded to a model of GB + global choice. The conservativity result then follows by the completeness theorem of first order logic.

The forcing conditions are (local) choice functions, ordered by inclusion. The generic object  $F$  is easily seen to be global choice function over  $M$ ., but some work is required to verify that  $(M,F)$  satisfies the replacement scheme in the extended language epsilon,  $F$ . Then if  $C$  is the collection of parametrically definable subsets of  $(M,F)$ , then  $(M,C)$  is a model of GBC + global choice.

I recall reading somewhere many people independently discovered the above result (including Cohen, Solovay, Kripke, and Felgner). Felgner's account can be found in the following reference.

U. Felgner, Choice functions on sets and classes. Sets and classes (on the work by Paul Bernays), pp. 217–255. Studies in Logic and the Foundations of Math., Vol. 84, North-Holland, Amsterdam, 1976.

Also note that Gaifman found a new proof of the above conservativity result without forcing, see below:

H. Gaifman, Global and local choice functions. *Israel J. Math.* 22 (1975), no. 3-4, 257–265.

Best regards,

Ali Enayat

Dear Thomas—

Do you remember, years ago in Melbourne, I said I found it interesting that Hartog's Theorem (Hartog 1915 (or is it Hartogs, in which case I should be writing Hartogs's Theorem?)) required the Axiom of Replacement which wasn't stated until Fraenkel 1923 and wasn't that an interesting historical phenomenon. To which you replied that the ORIGINAL version of Hartog's Theorem—Hartog's Hartog's Theorem, so to speak—might have been formulated in a way that didn't make appeal to Replacement necessary. And I failed to follow this up at the time.

Fast forward to 2010. We—by which I mean the University of Alberta philosophers (I took early retirement from Melbourne, and Megan's (=my wife's) mother lives in Edmonton, and nowhere else had stronger claims, and I like cold weather better than hot)—have a History of Logic Reading Group going. Basically going through Van Heijenoort, with occasional deviations: we read the whole of Dedekind's “Was sind...” and not just his letter to Keferstein.

((((Aside: Your ”Reasoning with Abstract Objects” got mentioned and recommended when we were discussing free creation by the human mind.))))

Anyhow... Last night we went over Cantor's letter to Dedekind, with its not-quite-right argument that all infinite cardinals are alephs, and I decided to write up some notes...

(1) is a definition of an ORDERING that struck me as nice because it allows us not to worry about ordered pairs and such-like annoying technicalities. (2) is a statement and proof of... Hartog's Theorem? And then noticed that my argument for it seems to require only the resources of Z, not ZF. Is it something like what you had in mind?

Merry a couple of Christmases and happy a couple of New Year'ses since we last saw each other! I hope you are well: give my regards to Graham Priest if you pass him at an airport or something.

Be well. Allen

Excerpt from notes for Logic Reading Group:

(1) Orders. What is an ORDERING of a set? These days the most familiar definition is that it is a relation (set of ordered pairs) defined over the set, but we have seen an alternative in Burali-Forti: a function mapping the members of the set to subsets of the set. Here's another, close to Burali-Forti's.

An ORDERNEST of a set, S, is a family of subsets of S which

- (i) includes S itself,
- (ii) is NESTED, in the sense that for any two sets in the family, one is a subset of the other, and
- (iii) is such that for any two members of S, there is a set in the family to which one but not the other belongs. It should be obvious that this is equivalent to the standard definition. Given an ordering relation on S, the corresponding ordernest is the family of sets containing S and, for each member of S, the set of members of S that are below it in the ordering. Given an ordernest for S, the corresponding relation is definable:  $x < y$  if and only if there is a set T in the ordernest to which  $x$  belongs but  $y$  doesn't.

Advantage of this way of reifying orders: a structural simplicity which makes it easy to see that the existence of an ordering can be proven by appeal to certain axioms.

(2) Hartog's Theorem. (Published in 1915, so we can forgive Cantor for not citing it in his 1899 letter, but it's a useful lemma in setting out some of the arguments a bit more formally.)

Theorem: For every set  $S$ , there is a well-ordered set  $R$  (so:  $R$  is a set with an ordering which well-orders its members) which is not in one-one correspondence with  $S$  or any subset of  $S$ .

Comment: since the notion of cardinality is defined in terms of the existence of one-one mappings, this amounts to: for every  $S$  there is a well-ordered  $T$  such that  $T$  is neither equinumerous with nor smaller than  $S$ .

Proof: Given  $S$ , consider all the well-orderings of subsets of  $S$ . ( $S$  itself may have a well-ordering, but we are not assuming that: it is at least certain that some of the proper subsets of a nonempty  $S$  will have well-orderings.)

There is a set of these things. ( $S$  is a set, so a couple of appeals to POWERSET, the "axiom" that for every set the multiplicity of its subsets is a set, gives us the set of families of subsets of  $S$ .

Then we appeal to SEPARATION (alias AUSSONDERUNG), the "axiom" that if a multiplicity is a set, so is any "submultiplicity" of it, to get the set of those families of subsets of  $S$  which are ordernests representing well-orderings of subsets of  $S$ .)

So there is a set of the ORDERTYPES of well-orderings of subsets of  $S$ . (O.k., appeals to free creation by the human spirit are no longer fashionable: we want, in this step, to "ontologically reduce" by specifying respectable entities – SETS – that can go proxy for ordertypes. For the purposes of this argument, we don't need proxies for ordertypes in general: it is enough to have a proxy for the ordertype of each well-ordering of a subset of  $S$ . We can use Bertrand Russell's trick: use EQUIVALENCE CLASSES as proxies for ordertypes. That is, we take as the "pseudo-ordertype" of a well-ordering of a subset,  $Q$ , of  $S$  the set of all well-orderings of subsets of  $S$  that are similar (= order-isomorphic) to the given ordering of  $Q$ . ... ... ... At which point, another appeal to POWERSET and SEPARATION assures us that there exists a set of the "ordertypes" of well-orderings of subsets of  $S$ .)

By the usual considerations (comparability of lengths of well-orderings...), these ordertypes are ordered, and indeed well-ordered. The set of ordertypes of well-orderings of subsets of  $S$  is itself a well-ordered set. (Another appeal to POWERSET and AUSSONDERUNG for the existence of the ordernest representing this well-ordering.)

So, there is a LONGER well-ordering: just add one more object at the end! (Exercise: how would you prove that, for any given set  $X$ , there is at least one object which is not a member of it? ... Now prove it again, this time WITHOUT appealing to the fact that the totality of all thinkable objects isn't a set. Hint: use UNION and POWERSET. An object can't be identical with any member of  $X$  if it is a set of greater cardinality than any set which is a member of  $X$ .)

If  $A$  is in one-one correspondence with set  $B$  and  $A$  has a well-ordering,

B has a well-ordering of the same length: define the well-ordering of B by setting one member below another if its corresponding member of A is below the corresponding member of the other.

So, the well-ordering mentioned ten lines ago is not in one-one correspondence with S or any of its subsets. It can't be, because it is LONGER than any well-ordering of a subset of S. QED.

COMMENT: the proof just given makes a lot of appeals to POWERSET and AUSSONDERUNG, but doesn't use the axiom of REPLACEMENT. This is a little bit interesting. The usual proof of Hartog's Theorem in modern textbooks DOES use REPLACEMENT (and the usual statement of the theorem, which talks about (Von Neumann) ordinals instead of well-ordered sets, requires it). Since Hartog's paper was 1915 and the first published statement of REPLACEMENT was, I think, Fraenkel's paper of 1923, I for a long time assumed that Hartog's Theorem must have been proven by a tacit appeal to something like REPLACEMENT before that axiom was explicitly formulated. I mentioned this to Thomas Forster several years ago and he opined that Hartog's original theorem might have been about well-orderings and might not have required REPLACEMENT. I didn't follow this up at the time, but maybe a proof somewhat like this one was what he had in mind.

Dear Thomas—

Quoting "Thomas Forster"

Fast forward to 2010. We— by which I mean the University of Alberta philosophers – have a History of Logic Reading Group going. Basically going through Van Heijenoort, with occasional deviations: we read the whole of Dedekind's "Was sind..." and not just his letter to Keferstein.

Who are these good people? It sounds like a \*really\* good idea!!

Staff members who show up at least part of the time include Bernie (son of Leonard) Linsky, Jeff Pelletier, Adam Morton, and Piotr Rudnicki from computer science— Katalin Bimbo has a heavy teaching load this semester but may come more if we go on next term. Plus about half a dozen graduate students.

(stuff about ordernestings) Yep. Good standard stuff. I wouldn't mind knowing who first spelled out that orderings can be represented in this way.

Don't know. I thought of it myself, but there must have been others who had the same simple idea. I came to it in the context of trying to represent quantification over relations in the "framework" of David Lewis's "Parts of Classes"— see appendix to that monograph.

Notice that the ordering doesn't have to be total for this to work.

Yes! My "Relations in monadic third-order logic" (Journal of Philosophical Logic vol. 26 (1997), pp. 619-628) uses it to represent the partial ordering by RANK of sets in ZF+Foundation. ... All inspired by Lewis: he was worried by the epistemology/philosophy-of-language issue of how we can know about or refer to abstract set-theoretic relations (like membership), and so wanted to give a "structuralist" axiomatization: Ramsey sentence. This requires quantifying

existentially over RELATIONS, but the "logic" (his "framework") he found most respectable gave only the resources of MONADIC 3rd Order Logic. So.... You can handle special cases: symmetric relations (as families of two-element sets) and orderings (by my trick). My proposal, incorporated into the appendix, was to POSTULATE an ordering of the universe

(this HAS to be postulated: my paper includes a proof that the existence of such an ordering can't be proven in Monadic 3rd O L when axiomatized by comprehension and extensionality) and get the effect of quantification over non-symmetric relations by reference to it: a non symmetric relation can be represented by a pair of symmetric ones, whose un-ordered pairs are to be "decoded" into ordered ones by taking them earlier-to-later for the first relation and later-to-earlier for the second. ...

Ironically, you don't NEED a general way of representing quantification over relations to carry out Lewis's program of "Ramsifying" ZF set theory: the special cases suffice.

I have a question about this at the back of my mind which i dust off and think about every now and then... Observe that the notion of \*ordering\* is not a set-theoretic one. (I have a very purist notion of 'set-theoretic'). We can encode ordering using ordered pairs (and ordered pair, God knows, is not a set-theoretic notion). But this \*ordernesting\* (good word - i'd never heard it before) gadget makes it look much more set-theoretic. Is this a trick of the light? perhaps, but perhaps not.

On this tack (tangential to it) is a clever trick of Paul Henrard that explains bijection between sets purely in the language of  $\in$  - without using ordered pairs. (I can't remember the details off the top of my head, but i can easily dig it up for you if you like) Again one has the feeling that a non-set-theoretical notion is being made to look more set-theoretic than it otherwise would. Again, does this actually prove anything? Is it a trick of the light? I don't know...

You can do essentially the Whitehead-Russell version of arithmetic in Monadic 3rd O L. One-one correspondences between disjoint sets can be represented by symmetric relations: families of two-element sets with one element from each of the two disjoint sets. Bijections between general pairs of sets A,B can be handled by having a third set, C, disjoint from both A and B, with one-one correspondences (represented as above) to both. Inf Ax (= there exists a family of sets containing a proper superset of each of its members) proves that, if A and B are finite, there will be a C to do the job. Is this something similar to Henrard's idea?

COMMENT: the proof just given makes a lot of appeals to POWERSET and AUSSONDERUNG, but doesn't use the axiom of REPLACEMENT. ... I mentioned this to Thomas Forster several years ago and he opined that Hartog's original theorem might have been about well-orderings and might not have required REPLACEMENT. I didn't follow this up at the time, but maybe a proof somewhat like this one was what he had in mind.

tf: Bang on the money. That is exactly the proof i had in mind. And it must, surely, be the proof in Hartogs (yes, he is plural, all by himself). As

you say, Hartogs did not know of replacement, so it \*must\* be the same proof. (That is essentially the only proof) It would be an idea to check it, actually. Hartogs 1915 is not in van Heijenoort. I don't suppose you have a copy?

I don't, and don't feel like trying to decipher German this month! But, yes, this ought to be checked. Maybe I can sic a graduate student onto it.

Subtle palates like yours might appreciate the reflection that Hartogs' thm is a fact about sets and wellorderings that seems to be provable only with reference to ordinals (not necc von Neumann ordinals, but ordinals implemented somehow, since the big ordered set is morally a set of ordinals), even tho' ordinals are not mentioned in the result. Thus it looks *prima facie* like a counterexample to the interpolation lemma. It can't be of course, but the trick of the light is... well, something i can go on about for some time. (I \*now\*, finally, think i understand what is going on!!)

And THAT's a comment that should keep me thinking for a while! ... We (= UAlberta Logic Group) actually talked about something similar. Theory of finite sets is a nice theory, can be developed in, say, Monadic 3rd O L. Quantification over natural numbers and arithmetic vocabulary can be introduced by contextual definition. Without Inf Ax, though, you can't prove that every number has a number as its successor. Trick: use (not order types, but) "howmany-ness types," this time not introduced by contextual definition but postulated to be in the range of the individual variables. The appeal to "Frege's Theorem" by the St. Andrews school of neo-logicists is like this.

Be well,

Allen

Here's the course page for the Warwick course that I mentioned:

<http://www2.warwick.ac.uk/fac/sci/dcs/teaching/modules/cs330/>

The historian is Martin Campbell-Kelly:

[http://www.dcs.warwick.ac.uk/people/Martin\\_Campbell-Kelly/](http://www.dcs.warwick.ac.uk/people/Martin_Campbell-Kelly/)

Mike

Dear Professor Forster,

Some ideas jump into the foreground because they are introduced by a single person. Other ideas are lurking in the background for decades. The notion of similarity type belongs to the latter category, in my opinion. As soon as one began thinking about the semantics of first-order logic, the notion of a similarity type was implicitly there. Both Gödel (in his completeness theorem of 1930) and Tarski (in his papers on truth of 1930-35 and his paper on logical consequence of 1936 or 1937) were involved in developing the semantics of first-order languages—they were the first—so they were implicitly involved in talking about similarity types. Also Birkhoff, in his 1935 proof of the HSP theorem and his semantic characterization of equational classes as classes closed under  $H$ ,  $S$  and  $P$  is dealing with similarity types of algebras. Mal'cev's work came later, but he was also concerned with similar subjects. So it is probably hard to say that one person came up with this notion. In fact, the notion is quite secondary to

the work that all four men were doing—they probably treated it as an obvious notion underlying the main task of what they were trying to accomplish. A more focused question—but one that probably has the same answer—is: who first thought about general algebraic structures? The same four names occur: Gödel (here, I would say that his role is rather secondary), Tarski, Birkhoff (for algebras), and Mal'cev; usually, Tarski is recognized today as the main person who developed the notion of a general algebraic structure (admitting relations and operations), but I think one should also include Birkhoff, as least as regards the general notion of an algebra.

Well, that is my take on the subject.

With best regards,

Steve Givant

Subject: Re: FW: [FOM] signature

Dear Professor Givant,

Thanks for this. Why do you think it is a strange questions, by the way? I am trying to trace the history of the idea of \*abstract data type\* which is nowadays so important, and it seems to be that tracking the idea of a signature would be helpful in this enterprise..

Thank you for the info!

On Dec 20 2010, Steven Givant wrote:

Dear Professor Forster,

Hajnal Andreka sent me a letter (see below) in which she asked if I could answer a question that you apparently posed on FOM. She asked that I forward my response directly to you.

Sincerely,

Steve Givant

From: Steven Givant

Sent: Sunday, December 19, 2010 4:41 PM

To: GMAIL

Cc: Steven Givant

Subject: RE: [FOM] signature

It is a strange question. It is of course implicit in early papers of Tarski (On the concept of logical consequence, the truth paper, etc.). More explicit references—though still restricted in scope—occur in the joint papers with Jónsson (Direct Decompositions of finite algebraic systems, Boolean algebras with operators). General formulations appear in “Some notions and methods on the borderline of algebra and metamathematics”, “Contributions to the theory of models”. I don’t think that the term “signature” is due to Tarski, but rather to Mal’cev. I believe that Tarski used the term “similarity type”.

Steve

A message from GG: From 2 papers in prep.

2.4 Parts and moments. A beautiful distinction is made, especially by Edmund Husserl, between parts and moments of a whole, which he called a mani-

fold<sup>1</sup>.[1] Take, for example, a book: parts of it include the cover, and the pages upon which chapter 5 is printed; moments include the colour of the cover, the weight of the book, and the price charged for it on the black market. The difference rests upon independence and dependence: a part can be considered on its own, as an *object*; by contrast, a moment always is of a part or of the whole. The splitting of a whole into parts may be discrete or continuous, the latter being of individuals [Lowe 2009, 50]. Among other contexts, in logic propositions  $P$  and  $Q$  are parts of the compound proposition  $P \vee Q$ , but the conjunction connective is a moment of it; so are the other logical connectives, and also the quantifiers in a predicate calculus. The (untrue) arithmetical proposition  $21 + 3 = 29$  has the equality relation as a moment; further, 21 and 3 are parts of the sum while addition is a moment. Assertion is a pair of moments of the proposition that is asserted. Among other contexts, the distinction between parts and moments applies to sortal predicates; for example, whether or not a part of a whole  $W$  is of the same sort as  $W$  itself (no if  $W$  is a cat, yes if  $W$  is a sheaf of papers), and similarly about a part of a moment  $M$  that can have parts in the first place (no if  $M$  is the colour of the book, yes if  $M$  is its weight).

We turn now to Edmund Husserl, student of Weierstrass, follower of the psychologist Franz Brentano, and junior colleague of Cantor. Husserl was a major early practitioner of phenomenology, a kind of philosophy in which special attention is paid to different manners of perceiving parts and wholes. One of his main interests from the mid 1880s onwards was to examine the foundations of arithmetic; numbers are primitive, but they can be analysed from experiential and psychological points of view.

Husserl distinguished between parts and moments of a whole, which he called a manifold.<sup>2</sup> For example, consider a manifold composed of a plastic bag containing some oranges and some apples. Among its parts are each apple, the bag, and the smallest orange, while moments of the manifold include the weight of the bagful, and the price that I paid for it on the market stall. Parts and moments may themselves have parts and moments; for example, my surprise at the price that I had to pay for the fruit, the skin of that smallest orange, the colour of its skin, and the weight of the bag. Some parts and moments may be empty; the pears in the bag, or the non-existent blueness of the colour of the skin of the oranges. The difference between parts and moments rests upon dependence and independence: I can consider a part on its own, but not any moment. For example, I can take an apple out of the bag in a way that I cannot take away the weight of the bagful from it. Among other contexts, in logic propositions  $P$  and  $Q$  form parts of the compound proposition  $P \wedge Q$ , but the conjunction connective is a moment of it; so are the other logical connectives, and also the

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<sup>1</sup>Husserl described this distinction in various places, especially (if rather ponderously) in the third of his logical investigations [1901, pt. 1, esp. arts. 16-24]. It is elaborated in [Smith 1982].

<sup>2</sup>See especially the third of Husserl's logical investigations [1901, pt. 1, esp. arts. 16-24]; various earlier pertinent texts, including Philosophie der Arithmetik [1891], are available in English in [Husserl 1994, 2003]. [Smith 1982] is an excellent compendium of modern commentaries and some original texts, not by Husserl, on parts and moments.

quantifiers in predicate logic. In mathematics, in an arithmetical proposition such as  $2 + 3 = 5$  the equality is a moment of the proposition; further, 2 and 3 are parts of the sum while addition itself is a moment. In a union  $U$  of multisets each multiset is a part of  $U$  but each union is a moment of it.

[1]