





The Axioms of Set Theory  
Part I: An Introduction to the Axioms of  
Zermelo-Fraenkel Set Theory

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	We allude to KF at places so you'd better define it.	

You can believe that ZF is (part of) the true picture of the world of sets - beco's for example you have embraced the cumulative hierarchy conception of sets - but that doesn't mean you have any special love for the particular axioms with which it is usually presented. You *might* of course, since there are stories to be told about those axioms which might perhaps be quite compelling. In these two slim volumes I am looking to assemble those stories, not because I believe them or because I want my readers to believe them, but because if we hold those stories in our minds it will be easier to remember the axioms and easier to see what they do.

Fit in a joke somewhere about how these axioms are not to be taken any more seriously than the thirty-nine articles.

When mapping new territory there is no *a priori* rule about where you set up your triangulation points. Any such rule would have to rely on information about the territory that *ex hypothesi* you do not yet have. There is a story to be told (and this volume aspires to tell it) about what those triangulation points are, and why our decisions can be seen retrospectively to be wise decisions, but let us not pretend that they were picked out by God.

Is this the kind of book you would even wish your students to read?!

Mervyn Griffith-Jones

## SYNOPSIS

Philosophical prolegomena. The foundationalist project. What are sets? Circularity worries. Cumulative hierarchy, set pictures. List the axioms.

### 0.1 A section on DM Basics

assume the reader knows about transitivity etc. Congruence relations; classifiers

Where do we explain stratification?

How do we divide the material on certificates between the two volumes??

### 0.2 What is set theory *for*?

Foundational role. Unsatisfactory in various pretty obvious ways. Nevertheless it is Generally agreed. Gold standard blah. This foundationalist project underpins, one way or another, much of the justification for the several axioms. How good an argument you feel this is for adopting any given axiom depends, of course, on how attractive you find the foundationalist project, but it does at least help explain how they come to be accepted so readily, and enables one to knit together a story.



# Preface

These two slender volumes – these *Péchées de ma Vieillesse* – are not intended to be an introductory text in set theory: there are plenty of those already. They are designed to do exactly what it says on the tin: to introduce the reader to the axioms of Set Theory. And by ‘Set Theory’ here I mean the axioms of the usual system of Zermelo-Fraenkel set theory, including at least some of the fancy add-ons that do not come as standard.<sup>1</sup> Its intention is to explain what the axioms say, why we might want to adopt them (in the light of the uses to which they can be put) say a bit (but only a bit, for this is not a historical document) on how we came to adopt them, and explain their mutual independence. Among the things it does *not* set out to do is develop set theory axiomatically: such deductions as are here drawn out from the axioms are performed solely in the course of an explanation of why an axiom came to be adopted; it contains no defence of the axiomatic method; nor is it a book on the history of set theory. I am no historian, and the historical details of the debates attending their adoption and who did what and with which and to whom are of concern to me only to the extent that they might help me in the task of explaining to beginners what the axioms say and why one might want to adopt them.

A person picking up a book with a title like ‘An introduction to the axioms of set theory’ is probably not already a set theorist and may well have no plans to become one, but may nevertheless expect set theory to be useful to them – and accordingly is probably willing to be told a story about what meaning the axioms of set theory might have beyond set theory. For some set-theoretic principles at least, it can be argued that their true meaning lies outside set theory. put this para somewhere else..?

Finally I must cover myself by pointing out in my defence that I am not an advocate for any foundational role for set theory: it is a sufficient justification for a little book like this merely that there are a lot of people who *think* that set theory has a foundational rôle: it’s a worthwhile exercise even if they are wrong.

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<sup>1</sup>There are other systems of axioms, like those of Quine’s *New Foundations*, Church’s set theory CUS, and the Positive Set Theory studied by the School around Roland Hinnion at the Université Libre de Bruxelles, but we will discuss them only to the extent that they can shed light on the mainstream material.

Other essays with a brief resembling the one I have given myself here include Mycielski [48], Maddy [42], [43] and Shoenfield [60]. My effort is both more elementary and more general than theirs are.<sup>2</sup>

### But who is the intended audience?

Whom is it for? Various people might be interested. People in Theoretical Computer Science, mathematicians, and the gradually growing band of people in Philosophy who are developing an interest in Philosophy of Mathematics all come to mind. However one result of my attempts to address simultaneously the concerns of these different communities (as I discover from referees' reports) is that every time I put into the cake a silver threepenny bit for one of them to find then the others complain that they have cracked their teeth on it.

This document was prepared in the first instance for my set theory students at Cambridge, so it should come as no surprise that the background it relies on can be found in a home-grown text: [18]. The fact that [18] is an *undergraduate* text should calm the fears of readers concerned that they might not be getting a sufficiently elementary treatment.

I cannot emphasise too firmly that this is not a work of philosophy of mathematics or of history of mathematics. Historians and philosophers are welcome to read it but they are not to complain if it does not address their concerns or does not conform to their practices. My intention is to direct such powers of exposition as i have to those areas where they are most needed. It is not historians or philosophers who need to understand what the axioms of set theory are doing – and in particular what the axiom of choice means and does; it is working mathematicians.

Say something about why  
two volumes

It is a pleasure to be able to thank Ben Garling, Akihiro Kanamori, Adrian Mathias, Robert Black, David Makinson, Douglas Bridges, Imre Leader, Nathan Bowler, Graham White, Allen Hazen (and others, including some anonymous referees) for useful advice, and a pleasure, too, to be able to thank my students for invaluable feedback.

Why do set theory anyway? Where do the axioms come from?

The axioms of ZF are usually presented as arising naturally from the cumulative hierarchy, but a lot of the motivation for them comes from the idea that ZF is to be a foundation for mathematics, so that it must have axioms that enable it to discharge that task. You don't have to share this motivation but if you understand that some people do you will see better where the axioms are coming from.

The LBW law in cricket (or the offside rule in soccer) is whatever humans decide it is, since it is a human construct; however in both those cases there is a conception underlying the game which the rule was intended to make explicit, or to implement<sup>3</sup>. One effect of this is that there can be *good* or *bad* LBW

<sup>2</sup>Despite the promising-sounding title Lemmon [37] is a technical work.

<sup>3</sup>One of my proudest moments was when i wrote to Ritchie Benaud with a suggestion for a revision to the LBW law; he replied that i wasn't going to win that one, but that The Don was of the same view, and argued for the same change.

laws. Something similar happens with set theory: the axioms of set theory are whatever we say that are, but there are shadows that they emerge from. What are these shadows? The debates about these axioms are conducted as if we were agreed what kind of object we are dealing with, but nevertheless just can't agree about what is true of them. But what is actually happening is rather that the objects we are reasoning about are objects belonging to a diverse variety of data types (sets, multisets, lists, tuples ...) that arise during the course of the project of representing all of mathematics inside set theory, all of them more-or-less extensional ...some more, some less. Mostly it doesn't matter which data-type is in play, so we typecheck lazily. However, sometimes it does matter and at such times the unclarity about which-data-type-is-in-play-when can give rise to the – apparent – disagreement. And endless confusion. We will take up the topic of what operations the abstract datatype of sets supports once we have introduced ourselves to ...



# Chapter 1

## The Cumulative Hierarchy

The *axioms of set theory* of the title are the axioms of **Zermelo-Fraenkel** set theory, usually thought of as arising from the endeavour to axiomatise the *cumulative hierarchy* concept of set. There are other conceptions of set, but although they have genuine mathematical interest they are not our concern here. The cumulative hierarchy of sets is built in an arena – which is initially empty – of sets, to which new sets are added by a process (evocatively called *lassoing* by Kripke) of making new sets from collections of old, preëxisting, sets. No set is ever harmed in the process of making new sets from old, so the sets accumulate: hence ‘cumulative’.

Formally we can write

$$V_\alpha =: \bigcup_{\beta < \alpha} \mathcal{P}(V_\beta) \quad (1.1)$$

...where the Greek letters range over ordinals. What this mouthful of a formula says is that the  $\alpha$ th level of the cumulative hierarchy is the power set of the union of all the lower levels: it contains all the subsets of the union of all the lower levels.

$V$  (the universe) is then the union of all the  $V_\beta$ . My only quarrel with this ‘ $V$ ’ notation is that I want to be able to go on using the letter ‘ $V$ ’ to denote the universe of *all* sets (including possibly some – “illfounded” – sets not produced by this process) so I shall sometimes rewrite ‘ $V$ ’ as ‘ $WF$ ’ to connote ‘Well Founded’. This  $WF$  notation is not standard.

This conception of sets is more-or-less explicit in Mirimanoff [46], but it is more usually associated with Von Neumann [69]. He noticed that the cumulative hierarchy gives an **inner model** of set theory.<sup>1</sup> Von Neumann produced the cumulative hierarchy as a possible interpretation of the axioms of set theory (which by then had more-or-less settled down): something of which they might

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<sup>1</sup>For these purposes an inner model is a definable proper class that is a model of ZF such that every subset of it is a subset of a member of it. The expression is being subtly recycled by the votaries of large cardinals even as we speak.

be true. The idea that the cumulative hierarchy might exhaust the universe of sets became the established view gradually and quietly – almost by stealth.

One very important fact about sets in the cumulative hierarchy is that every one has a **rank** – an ordinal sometimes more evocatively described as its **birthday**: the rank of  $x$  is the least  $\alpha$  such that  $x \subseteq V_\alpha$ .

While we are about it, we may as well minute a notation for the cardinalities of these levels of the hierarchy.  $|V_\alpha|$ , the cardinality of  $V_\alpha$ , is defined to be  $\beth_\alpha$ . Since  $V_{\alpha+1}$  is the power set of  $V_\alpha$ , Cantor’s theorem tells that that all the  $V_\alpha$ s are different sizes. In fact  $\beth_0 := \aleph_0$ ; and  $\beth_{\alpha+1} := 2^{\beth_\alpha}$ .

## 1.1 So what do the sets in the cumulative hierarchy actually do

So what operations do sets in the cumulative hierarchy support? This question is motivated by background assumption of ideas of object-oriented programming.

Some abstract datatypes – but not all – support constructors and destructors. For example ordered pairs support pairing and unpairing. Kripke lasso blah. The lasso is part of a constructor kit, tho’ increasingly nowadays people realise that a wand is needed if one is to turn the lasso-contents into an actual set. The destructor is a gadget that, when given a set and a candidate for membership in it, replies ‘yes’ or ‘no’. We shall see later how the axiom of choice is equivalent to the datatype of sets supporting the nondeterministic operation “give me one of your members, or failing that an error message”. We will return to this in volume II

## Chapter 2

# IBE and other Philosophical Odds-and-ends

This is a book (and a *small* book at that) on set theory, not a book on Philosophy of Mathematics; so there will be no long discussions about what it might be for an axiom of set theory to be true (Pontius Pilate’s celebrity was justly earned) nor will we be discussing how one establishes the truth or falsity of any of the candidate axioms. Nevertheless there are a couple of philosophical topics that cannot be evaded altogether and which we will cover briefly here.

read [49] properly to get straight the difference between IBE and abduction

### 2.1 Inference to the best explanation

“There might exist axioms so abundant in their verifiable consequences, shedding so much light upon a whole discipline, and furnishing such powerful methods for solving given problems (and even solving them, as far as possible, in a constructivistic way) that quite irrespective of their intrinsic necessity they would have to be assumed at least in the same sense as any established physical theory.”

[24] This is IBE if ever i saw it.

Although most of the axioms of ZF became part of the modern consensus without any struggle, there are two axioms – namely AC and the axiom (scheme) of replacement – that have been at one time or another under attack. In both cases a defence was mounted, and – although the points made in favour of the defendants in the two cases were of course very different – there was at least one strategy common to the two defences. It was a strategy of demonstrating that the axiom in question gave a single explanation for the truth of an assortment of things already believed to be true. A single explanation for a lot of hitherto apparently unconnected phenomena is *prima facie* more attractive than lots of separate explanations. It’s more *parsimonious*. Unifying-single-explanation

arguments are so common and so natural and so *legitimate* that it is hardly surprising that this method has been identified by philosophers as a sensible way of proceeding – and that there is a nomenclature for it and a literature to boot. It is probable that this is (at least part of) what Peirce had in mind when he coined the word **abduction**; nowadays it is captured by the expression *Inference to the best explanation* “IBE”; see Lipton [40] for an excellent treatment.

The IBE defence was probably more important for Replacement than for Choice. Advocates of the Axiom of Choice (many of them) have stoutly maintained that it is straightforwardly obviously true. (And the IBE case for AC is weak, as we shall see). In contrast, advocates of the axiom scheme of replacement do not claim obviousness for their candidate – even now, after the debate has been won. It is often said to be *plausible*, but even that is pushing it. ‘Believable’ would be more like it: but even ‘believable’ is enough when you can make as strong an IBE case as we will be making below.

## 2.2 Intension and Extension

The intension-extension distinction is a device of mediæval philosophy which was re-imported into the analytic tradition by Frege starting in the late nineteenth century and later Church (see [12] p 2) and Carnap [9] in the middle of the last century, probably under the influence of Brentano. However, in its passage from the mediævals to the moderns it has undergone some changes and it might be felt that the modern distinction and the mediæval idea share little more than a name.

Perhaps the best approach to the intension/extension distinction is by means of illustrations. Typically the syntax for this notation is [wombat]-in-extension contrasted with [wombat]-in-intension, where [wombat] is some suite-or-other of mathematical object. Thus we contrast function-in-extension with function-in-intension. A function-in-extension is a function thought of as a tabulation of arguments-with-values, a lookup table – or a *graph*. The function-in-extension contains no information about how the value comes to be associated with the argument: it merely records the fact that it is so associated. Function-in-intension is harder to characterise, since it is a much more informal notion: something a bit like an algorithm, though perhaps a little coarser: after all one can have two distinct algorithms that compute the same function. The analysts of the eighteenth century – Euler, the Bernoullis and so on – were studying functions from reals to reals, but all the functions they were studying were functions where there was some reason for each input to be associated with a particular output. That is to say, they were studying functions-in-intension. (They were interested in things like polynomials and trigonometrical functions). They did not have the concept of an arbitrary function-in-extension, and would not have considered such things worthy objects of mathematical study.

In its modern guise the intension-extension contrast has proved particularly useful in computer science (specifically in the theory of computable functions, since the distinction between a *program* and the *graph* of a function corre-



sponds neatly to the difference between a function-in-intension and a function-in-extension) but has turned out to be useful in Logic in general. We need it here because the concept of set that the axioms are trying to capture is that of an arbitrary object-in-extension and without that understanding it is not possible to understand why the axioms have the form they do.

“Arbitrary object-in-extension”? This phrase deserves some exegesis, and the exegesis requires a little bit of history.

## 2.3 What is a Mathematical Object?

One of the skills one needs in order to understand how people evolved the positions that they did *vis-à-vis* the various axioms, is an understanding of how people were thinking of sets a century and a half ago, and how it differs from how we see sets now.

Much of that evolution is simply what happens to any concept that becomes swept up into a formal scientific theory. The status of proper mathematical object includes several features, all of them probably inextricably entwined.

- (i) They have transparent identity criteria. Quine [51] had a *bon mot* which captures pithily one reason why it is so important it is to ensure that our concepts should be well-defined: “*No entity without identity*”. For widgets to be legitimate objects it has to be clear – at least in principle – when two widgets are the same widget and when they are distinct widgets. Otherwise there are endless possibilities of fallacies of equivocation. That is not to say that there must be a finite decision procedure; after all, the criterion of identity for sets is that  $x$  and  $y$  are the same set if every member of  $x$  is a member of  $y$  and *vice versa*. If  $x$  and  $y$  are of infinite rank (see chapter 1) then this check can take infinitely long. But it is still a check that can *in principle* be performed – in the sense that there are no *logical* obstacles to its execution. (This is in contrast to the predicament of the hapless Liza who is trying to mend the hole in her bucket. She discovers that the endeavour to mend the hole in her bucket spawns a subtask that required her bucket never to have had a hole in it in the first place. Even infinite time is of no help to her.)
- (ii) If widgets are legitimate well-defined objects one can quantify over them. The literature of philosophical logic contains numerous aftershocks of Quine’s ([50] “On what there is”) observation that “to be is to be the value of a [bound] variable”. This has usually been read as an *aperçu* about the nature of genuinely existent things but it is probably better read as an observation about the nature of mathematical entities.

And if one can quantify over widgets – so that a widget is a value of a variable – then one can then prove things about all widgets by universal generalisation: one can say “Let  $x$  be an arbitrary widget ...” which is to say that one has the concept of an *arbitrary* widget.

- (iii) One final thing – whose importance I might be exaggerating – there is an empty widget. Remember how important was the discovery that 0 is an integer! But perhaps we mean the concept of a *degenerate* widget. My guess is that this will probably turn out to be the same as (ii) but even if it does it is such an important aspect of (ii) that it seems worth while making a separate song and dance about it. Cantor apparently did not accept the empty set, and there are grumblers even now: [64].

Worker hymenoptera do not have a concept of other workers as intensional objects. No *haeccities*! Expand this!

Point (ii) will matter to us because some of the disagreement about the truth of – for example – the axiom of choice arises from a difference of opinion about whether there are arbitrary sets-in-extension. (i) is very important to us because much of the appeal of the  $V_\alpha$  picture of sets (p. 11) derives from the clear account of identity-between-sets that it provides. We will see more of this in chapter 1.

So how can objects acquire this status? Typically they seem to go through a three-step process.

1. At the first stage the objects are not described formally and not reasoned about formally, though we do recognise them as legitimate objects. There are things which are now recognised as mathematical objects which were clearly at this stage until quite recently: knots became mathematical objects only in the nineteenth century.
2. Objects that have reached the second stage can be reasoned about in a formal way, but they are still only mere objects-in-intension; they are not *first-class objects* (as the Computer Scientists say) **and you cannot quantify over them**. Examples: functions  $\mathbb{R} \rightarrow \mathbb{R}$  for the mathematicians of the eighteenth century; proofs and formulæ for the average modern mathematician;<sup>1</sup> chemical elements for chemists even today.<sup>2</sup>
3. Objects at stage three are fully-fledged quantifiable arbitrary entities: they are “First class objects” as the Computer Scientists say.

Further, we do not regard the process as completed unless and until we are satisfied that the concept we have achieved is somehow the “correct” formalisation of the prescientific concept from which it evolved. Or if not *the* correct formalisation then at any rate one correct formalisation among many. There is a concept of *multiset* which has the same roots as the concept of *set* but the (rudimentary) theory of multisets that we have doesn’t prevent our theory of sets from being a respectable mathematical theory.

<sup>1</sup>My *Doktorvater* Adrian Mathias says that a logician is someone who thinks that a formula is a mathematical object.

<sup>2</sup>Sometimes this transformation takes before our eyes. There was a time when Kuiper belt objects were rare and each had a soul – Pluto (plus possibly a soul mate – Charon). Now they are a population of arbitrary objects-in-extension with statistical ensemble properties and soulless nomenclature instead of names. The same happened to comets and asteroids but that was before I was born.

As we noted earlier, it is at this third stage that it becomes possible to believe there are empty ones. One process that is particularly likely to bring empty or degenerate objects to our attention is algebrisation: it directs our attention to units for the relevant operations. We say  $\clubsuit$  is the **unit** for an operation  $*$  if  $(\forall x)(*(x, \clubsuit) = x)$ . For example: 0 is the unit for addition; 1 is the unit for multiplication; the empty string is the unit for concatenation; the identity function is the unit for composition of functions, and so on. By “it becomes possible” what I mean is that until you are considering arbitrary widgets and operations on them then the empty widget is unlikely to attract your attention. How could it, after all? The fact that it’s a unit for various algebraic operations on widgets becomes important only once you are considering operations on widgets and this is more likely once you have arbitrary widgets.<sup>3</sup>

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We need at least some reflection on the difference between prescientific and fully-fledged scientific objects because without it one cannot fully understand the motivation for the axioms; the residual disagreement over some individual axioms (the axiom of choice) too is related to this difference.

## 2.4 The Worries about Circularity

Does this really belong here

Many people come to set theory having been sold a story about its foundational significance; such people are often worried by apparent circularities such as the two following.

- The cumulative hierarchy is defined by recursion on the ordinals but we are told that ordinals are sets!
- Before we even reach set theory we have to have the language of first-order logic. Now the language of first-order logic is an inductively defined set and as such is the  $\subseteq$ -minimal set satisfying certain closure properties, and wasn’t it in order to clarify things like this (among others) that we needed set theory ...? And how can we talk about arities if we don’t already have arithmetic? And weren’t we supposed to get arithmetic from set theory?

<sup>3</sup>An aversion from this view of mathematics is probably what is behind Mordell’s gibe (in a letter to Siegel) about how modern mathematics was turning into the theory of the empty set.

<sup>4</sup>As late as 1963 textbooks were being written in which this point of view was set out with disarming honesty:

“It seems to me that a worthwhile distinction can be drawn between two types of pure mathematics. The first – which unfortunately is somewhat out of style at present – centres attention on particular functions and theorems which are rich in meaning and history, like the gamma function and the prime number theorem, or on juicy individual facts like Euler’s wonderful formula

$$1 + 1/4 + 1/9 + \cdots = \pi^2/6$$

The second is concerned primarily with form and structure.”

[62] p ix. Simmons’ preferred version of Mathematics is Mathematics as the study of interesting intensions. Unfortunately the road to Hell is paved with interesting intensions.

There are various points that need to be made in response to such expressions of concern. One is that we must distinguish two (if not more) distinct foundationalist claims that are made on Set Theory's behalf. The first is that all of Mathematics can be interpreted in set theory. This appears to be true, and it is a very very striking fact, particularly in the light of the very parsimonious nature of the syntax of Set Theory: equality plus one extensional binary relation. This claim does not invite any ripostes about circularity.

Unfortunately it is so striking that one feels that *it must mean something*. Something it could be taken to mean is that set theory is metaphysically prior to the rest of mathematics, or in some other sense provides a foundation for it. This second claim is far from obvious and *does* invite concerns about circularity.

Inevitably claims of this kind were made when set theory was new, and was inspiring high hopes in the way that novelties always do.<sup>5</sup>

It is for claims of this second sort that the above circularities make difficulties. Indeed, the difficulties are such that were it not for the parallel with religion one would be at a loss to explain why the extravagant claims for a foundational rôle for Set Theory should ever have drawn the audience they do. The explanation is that – for people who want to think of foundational issues as resolved – it provides an excuse for them not to think about foundational issues any longer. It's a bit like the rôle of the Church in Mediæval Europe: it keeps a lid on things that really need lids. Let the masses believe in set theory. To misquote Chesterton "If people stop believing in set theory, they won't believe *nothing*, they'll believe *anything*!"

The trouble with the policy of accepting any answer as better than no answer at all is that every now and then thoughtful students turn up who take the answer literally and in consequence get worried by apparent defects in it. In the case of the set-theory-as-foundations one recurrent cause for worry is the circularities involved in it.

I think the way to stop worrying about these circularities is to cease to take seriously the idea that set theory is that branch of Mathematics that is prior to the other branches. It certainly does have a privileged status but that privileged status does not solve all foundational problems for us. If we lower our expectations of finding straightforward foundations for Mathematics it becomes less likely that we will be disappointed and alarmed.

The anxious reader who thinks that Mathematics is in need of foundations and who has been looking to set theory to provide them may well need more than the "chill out" message of the last paragraph to break their attachment to the idea of set-theory-as-foundations. They might find it helpful to reflect on the fact that set theory spectacularly fails to capture certain features that most mathematicians tend to take for granted. There is a widespread intuition that Mathematics is strongly typed. "Is 3 a member of 5?" is a daft question, and it's daft because numbers aren't sets and they don't *do* membership. A thorough-

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<sup>5</sup>Thinking that every problem might be a nail when you have a hammer in your hand is not crazy at all if you have only just acquired the hammer. In those circumstances you may well have a backlog of legacy problems that are unrecognised nails, and it is perfectly sensible to review lingering unsolved problems to see if any of them are, in fact, nails.

going foundationalism about sets (of the kind that says that all mathematical objects should be thought of as sets) fails to accommodate this intuition and seems to offer us no explanation of why this question is daft. This doesn't mean that set theory cannot serve as a foundation for Mathematics, but it is a warning against taking foundationalism too seriously.

Despite these reflections I don't want to be too down on Set Theory's claims to a central rôle in mathematics; the fact that apparently all of Mathematics can be interpreted into the language of set theory means that set theory is available as a theatre in which all mathematical ideas can play. (Perhaps one would be better off trying to argue that Set Theory has a *unifying* rôle rather than a *foundational* rôle.) This fact by itself invests our choice of axioms with a (mathematically) universal significance, and indeed there are set-theoretic assertions with reverberations through the whole of Mathematics: one thinks at once of the Axiom of Choice, but the Axiom Scheme of Replacement has broad general implications too, as we shall see. Set theory as a single currency for mathematics is an easier idea to defend than set theory as a foundation for mathematics.

Since the advent of category theory noises have been made to the effect that we should look instead to category theory for foundations. This does take the heat off the alleged circularities in set theory, but it doesn't deal with the fundamental error of *attachment*. Mathematics doesn't need foundations – at least not of the kind that Set Theory was ever supposed to be providing – and the idea that Set Theory had all along been providing them annoyed a lot of people and did Set Theory much harm politically.



## Chapter 3

# Some History, the Paradoxes, and the Boundaries of Ordinary Mathematics

The Axioms of Set Theory go back to an article by Zermelo [72] of about 100 years ago, and in very nearly their present form. The most significant difference between Zermelo's axiomatisation in [72] and the modern formulations is the absence from the former of the axiom scheme of replacement. Axioms for set theory were being formulated at about the same time as the paradoxes of set theory were becoming evident, so it is natural for later generations to suppose that the first is a response to the second. The currency of the expression “the crisis in foundations” encourages this view. So, too, does this famous and poignant passage from the first volume of Russell's autobiography, in which he describes confronting the paradox that now bears his name.

It seemed unworthy of a grown man to spend his time on such trivialities, but what was I to do? There was something wrong, since such contradictions were unavoidable on ordinary premisses. . . . Every morning I would sit down before a blank sheet of paper. Throughout the day, with a brief interval for lunch, I would stare at the blank sheet. Often when evening came it was still empty.

However – as always – things were more complicated than the narrative we tell. One might think that the paradoxes were clearly a disaster and that the people who lived through those troubled times spent them running around like headless chickens wondering what to do about them, but in fact people at the time – the above passage from Russell notwithstanding – were not particularly perturbed by them, and one can think of at least two good reasons why this

should be so.

One reason is that at the time when the paradoxes started to appear the formalisation of the subject matter had not yet progressed to a stage where malfunctions and glitches were indications that the project was going wrong or was misconceived: it was still at the stage where one expects to see quotidian and transient errors of no significance for the ultimate success of the project.

This is well illustrated by the comparative insouciance which attended the discovery of the Burali-Forti paradox, which was actually the first of the paradoxes to appear, and is by far the nastiest of them. Opinion was divided about what it signified, but it hardly caused a sensation: it was simply put for the time being into the too-hard basket. Mathematicians at that time knew perfectly well that they didn't understand it and couldn't expect to understand it until they had made more progress in making sets into mathematical objects. Not that any of this is conscious! One reason why Burali-Forti is not an obvious *prima facie* problem for an axiomatisation of set theory is that – unlike the paradoxes of Russell and Mirimanoff – it is not a purely set-theoretic puzzle. The time to start worrying is if you have succeeded in formalising set theory but nevertheless *still* have paradoxes!

The other reason is that mathematicians – then as now – had a concept of “ordinary mathematics” in which the paradoxical sets palpably had no rôle. The sets with starring rôles in this *ordinary mathematics* were the naturals, the reals, the set of open sets of reals, the set of all infinitely differentiable functions from  $\mathbb{R}$  to  $\mathbb{R}$  and others of like nature. (The incompleteness theorem of Gödel was a different matter!)<sup>1</sup> Mathematicians would presumably have been perfectly happy with the axioms of naïve set theory had everything gone smoothly but when it didn't they were quite relaxed about it because they'd known perfectly well all along that the big collections were *prima facie* suspect: in any case people weren't interested in them, and shed no tears when told they had to wave them goodbye. Zermelo's axiomatisation wasn't so much an attempt to avoid paradox as an attempt to codify a consensus: to capture this idea of ordinary mathematics. (This idea of *ordinary mathematics* – and with it the idea that set theory has a record of polluting it by dragging in dodgy big pseudosets – is one that will give trouble later). Zermelo's axiomatisation was thus a start on a project of axiomatising those collections/sets/classes that were familiar and could plausibly be assumed not to be harbouring hidden dangers. Quite where lies the boundary between safe collections and dodgy collections is a matter to be ascertained as the project evolves. The intention of the project itself was never a mystery.

And its success was never endangered. The paradoxes should no more cause

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<sup>1</sup>Interestingly the incompleteness theorem was not as shocking to contemporary sensibilities as one might with hindsight have supposed. Clearly this must be in part because it's so much harder to grasp than Russell's paradox, but that cannot be the whole explanation, since there were people around who understood it. Were they shocked? By the time I got round to wondering about the contemporary impact, I knew only one living logician who could remember those days, and that was Quine. He told me he couldn't remember where he learned it or who told him, tho' he could of course remember where he was when he learned of the murder of Jack Kennedy. So even the people who understood it weren't shocked.



us to distrust ordinary mathematics than the occasional hallucination or optical illusion should cause us to distrust our usual perceptions. It is of course agreed that there are situations in which any malfunction will call the whole apparatus into question but – it will be said – this is not one of those situations.

This account – which I owe to Aki Kanamori – is presumably historically accurate. My unworthy feeling that it all sounds a little bit too good to be true. It might be – as Russell says – that concepts of set other than the cumulative hierarchy are “not such as even the cleverest logician would have thought of if he had not known of the contradictions”. One could add that had they not known of the contradictions they perhaps wouldn’t have ever got the idea that the cumulative hierarchy exhausts the universe of sets. For surely it is a safe bet that even (indeed *especially*) the cleverest logicians would have gleefully forged ahead with naïve set theory had there been no contradictions to trip them up. Indeed they would have been failing in their duty had they not done so. It may be of course that even in this dream scenario there would have been people who grumbled about how the large sets were nothing to do with ordinary mathematics – and that therefore we should restrict ourselves to wellfounded sets. They could have argued that wellfounded sets are conceptually more secure because we have a secure recursive concept of identity for them.<sup>2</sup> But they would not have been able to point to the paradoxes as an apparently compelling reason for their position. In any case set theorists have heard grumbles like this before and know what to think of them. Here we will deal with these grumbles in chapter 6.

### 3.1 What are sets anyway?

There is a way of thinking about sets which is perhaps very much a logician’s way: sets as minimalist mathematical structures. What do we mean by this? The rationals form an ordered field. Throw away the ordering, then the rationals are a field. Throw away the multiplicative structure then they are an abelian group. What are you left with once you have thrown away everything you can? Do we have a name for the relict? Yes: it’s a *set*. That’s what sets are: mathematical structures stripped of all the gadgetry.

The idea that a mathematical structure can be stripped bare by her bachelors in this way is more novel and radical than it may appear to a modern reader. It would have looked very odd to Euler and the Bernoullis. Be prepared to think of it as a strange idea.

The sets that naturally arise in this fashion are special in two ways. For one thing they are not arbitrary sets, but always specific motivated sets-in-intension. But there is a second point that concerns us more at the moment: they are not typically sets *of sets*. This approach motivates the *set of* rationals, but it does not give us a way as thinking of each individual rational *as a set*. From the set’s point of view the rationals seem to be structureless atoms. They may have

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<sup>2</sup>This point is very rarely made. This isn’t because it is a weak argument, but because the idea that the cumulative hierarchy exhausts the universe is not under concerted attack, and no defence is called for.

We need to think very hard about how to divide this material between the two volumes

This could be the right place to explain expansions and reductions.

internal structure but that structure is not set-theoretic. Back in the early days of set theory, before we had methods of finding – for every mathematical object under the sun – *simulacra* of those objects within the world of sets, people were more attracted than they are now to the idea that set theory should accommodate things that aren't sets. It is a sign of a later stage in the mathematician's love affair with sets that the idea arose that it would be nice if somehow one could think of the rationals too (to persist with our example) as sets, rather than merely as atoms, and indeed to somehow coerce all things too into being sets.

Even now there are some versions of set theory that explicitly leave the door open to structureless atoms. These atoms come in two flavours. First there are empty atoms: sets which have no members but which are nevertheless distinct from each other. These are often called by the German word *urelement* (plural *urelemente*). The other style of atom is the *Quine atom*. A Quine atom is a set  $x = \{x\}$ . The reader will perhaps not be surprised to learn that there are synonymy results that tell us that it doesn't much matter which variety of atom you plump for<sup>3</sup>. Of slightly more importance is the circumstance that your decision about whether you want atoms or not (whichever sort of atoms it is) doesn't seem to affect which other axioms of set theory you are disposed to adopt. Although flavour 1 atoms (but not flavour 2 atoms) contradict extensionality and flavour 2 atoms (but not flavour 1 atoms) contradict foundation, uses can nevertheless be found for these objects from time to time. The imperialist endeavour of Set Theory – to express the whole of Mathematics in Set Theory – is nowadays played out by implementing all the various primitive mathematical entities of interest (reals, rationals, complexes, lines, planes *etc.*) as sets in various ways, and there are now industry standards about how this is to be done. (Ordinals are Von Neumann ordinals, natural numbers are finite Von Neumann ordinals, integers are equivalence classes of ordered pairs of naturals and so on). However – in most cases – there is no deep mathematical reason for preferring any one successful implementation of these entities to any other. That is because – for most implementations – the internal set theoretic structure of the reals-as-sets or the complexes-as-sets has no meaning in terms of the arithmetic of reals or complexes. This being the case one might make a point of it by implementing them as sets with no internal structure at all: that is to say, as atoms of one of these two flavours.<sup>4</sup>

However the consensus view nowadays among set theorists is that we should eschew atoms and think of sets (“pure sets”) as built up from the empty set iteratively.

## Ordinals

It has probably by now struck the astute reader that the usual way of narrating the cumulative hierarchy (as in section 1) makes essential use of ordinals. Can

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<sup>3</sup>At least if you working in a standard theory of wellfounded sets such as we are considering in this volume, and not something like Quine's NF.

<sup>4</sup>See Menzel [45] where he implements ordinals as atoms, and even arranges to have a set of atoms – by weakening replacement

this be avoided? Realistically, no. Admittedly, one way of thinking of the cumulative hierarchy is as the  $\subseteq$ -smallest collection that contains all its subsets, and that seems not to involve ordinals. However if one thinks of  $V$  as the smallest collection that contains all its subsets one has let out of the bag the possibility of there being other collections that contain all their subsets. And that sits ill with the assumption underlying the axioms of ZF, namely that  $V$  is the only such connection. It is a matter of record that – everywhere in the literature – the cumulative hierarchy is presented as being constructed by a recursion over the ordinals. Does this matter? Again, no. There are two ideas that we must keep separate. One should not allow the (halfway sensible) idea that set theory can be a foundation for mathematics to bounce one into thinking that one has to start entirely inside Set Theory and pull oneself up into Mathematics by one's bootstraps. That is not sensible. (see the discussion on page 18.) On the contrary: it is perfectly reasonable – indeed almost *essential* – to approach the construction of the cumulative hierarchy armed with the primitive idea of *ordinal*. What is an ordinal anyway?

Ordinals are the kind of number that measure length of (possibly transfinite) processes. More specifically: transfinite *monotone* processes. The reason why one insists on the 'monotone' is that the iteration of non-monotone processes does not make sense transfinitely.

The class of (monotone) processes has a kind of addition: "Do this **and then** do that". It also has a kind of scalar multiplication: "Do this  $\alpha$  times". Monotone processes – by supporting these two operations of addition and scalar multiplication – seem to form a kind of module, and a module over a new sort of number at that. What sort of number is this  $\alpha$ ? It's an *ordinal*. This gives us an operational definition of ordinal: that's the sort of thing ordinals are: that's what they do. This tells us that 0 is an ordinal (the command: "Do nothing for the moment!") is the same as the command: "Do this 0 times"); it also tells us that the sum of two ordinals is an ordinal; ("Do this  $\alpha$  times and then do it  $\beta$  times"). It even tells us that ordinals have a multiplication:  $\beta \cdot \alpha$  is the number of times you have performed  $X$  if you have performed  $\alpha$  times the task of doing- $X$   $\beta$ -times.

In fact these properties of ordinals all follow from the three assumptions that

- (i) 0 is an ordinal; and
- (ii) if  $\alpha$  is an ordinal, so is  $\alpha + 1$ ; and
- (iii) if everything in  $A$  is an ordinal, then  $\sup(A)$  is an ordinal too.

This last is because if I have performed some task at least  $\alpha$  times for every  $\alpha$  in  $A$ , then I have done it  $\sup(A)$  times.

This definition is in some sense constitutive of ordinals, and tells us everything we need to know about them as mathematical objects. For example it follows from this recursive definition that the class of ordinals is wellordered by the engendering relation (see page 64). This is by no means obvious, and not everybody will want to work through the proof. (Those who do can see the discussion in [18].) Readers from a theoretical computer science background will

Describe Cantor on derived sets; we will say more about this in vol II

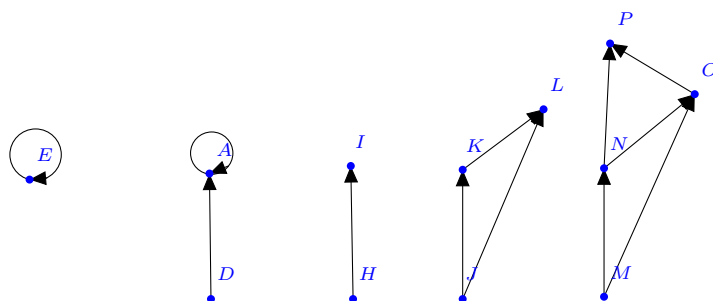
Also: nothing else is an ordinal

This reference has got lost. Sort out this word 'engendering'

be happy with this as an example of a recursive datatype declaration. Others less blessed might find the discussion at the end of part Zero of [13] calming.

However, none of this gives us any clue about how to think of ordinals as *sets*. I shall not here explain how to do that, since it is one of the things that is explained in every book on set theory written in the last 80 years, so the reader is guaranteed to learn it anyway. In contrast *this* is quite possibly the last time the reader will have made to them the point that one does not need to know how ordinals are implemented as *sets* to understand that they are legitimate mathematical objects and to understand how to reason about them. This point is generally overlooked by set theory textbooks in their headlong rush into developing ordinal arithmetic inside set theory. Textbook after textbook will tell the reader that an ordinal is a transitive set wellordered by  $\in$ . Ordinals are not transitive sets wellordered by  $\in$ : they are not sets at all. And it's just as well that they aren't, since if they were one would not be able to sensibly declare the recursive datatype of the cumulative hierarchy in the way we have just done in formula 1 p. 11 above, and the circularity worries discussed around page 18 would come back with a vengeance.

### 3.1.1 Set Pictures



According to this view, sets are the things represented by accessible pointed digraphs, or APGs.<sup>5</sup> An APG is a digraph with a designated vertex  $v$  such that every vertex has a directed path reaching  $v$ . The idea is that the APG is a picture of a set, specifically the set corresponding somehow to the designated vertex. The other vertices correspond to sets in the transitive closure of the depicted set. For example, in the pictures displayed above,  $E$  is a Quine atom, a thing identical to its own singleton;  $D$ ,  $H$ ,  $J$  and  $M$  are all  $\emptyset$ .  $I$ ,  $K$  and  $N$  are  $\{\emptyset\}$ ;  $L$  and  $O$  are  $\{\emptyset, \{\emptyset\}\}$ . By now the reader has probably worked out that  $P$  is  $\{\{\emptyset\}, \{\emptyset, \{\emptyset\}\}\}$ .  $A$  is an example of a pathological object for which we don't have a common noun.

<sup>5</sup>So they should really be APDs, but the notation is now standard.

How do we get from these things to sets? One could say that sets are APGs but are APGs equipped with different identity criteria. Two APGs that are isomorphic are identical-as-sets. Or one could identify sets somehow with isomorphism classes of APGs, or (since it looks rather circular to say that sets are isomorphism classes of ASPGs) identify them instead with entities abstracted somehow from the plural objects we are thinking of instead of isomorphism classes. This gives rise to some fun mathematics, and readers who express an interest in it are usually directed to Aczel [1] – though the seminal paper is the hard-to-get [27] and an equally good place to start is the eminently readable [3].

The APG story about what sets are is popularly connected in people’s minds with an antifoundation axiom, and this antifoundation axiom comes to mind naturally if we think about how APGs correspond to sets. It is possible to *decorate* an APG with sets in the following sense: a decoration of an APG is a function that labels every vertex of the APG with a set in such a way that the decoration of a vertex  $v$  is the set of all the decorations of the vertices joined to  $v$  by a directed edge. The sets-as-APGs picture leads one to speculate that every APG must have a decoration, so that the set corresponding to the APG is the label at the designated vertex. If this is to be a good story about what sets are, then every APG had better have a decoration. Better still, every APG should have a *unique* decoration. This is the axiom of antifoundation from [27]:

Every APG has a unique decoration. (APG 1)

Why ‘Antifoundation’? Well, consider the APG that has only one vertex, and that vertex pointing to itself. We see that any decoration of it will be a Quine atom. This contradicts the axiom of foundation. If we do not want to postulate the existence of Quine atoms – or indeed of any other sets that would not be wellfounded – then we could weaken the axiom to

Every wellfounded APG has a unique decoration. (APG 2)

(A wellfounded APG is of course one whose digraph relation is wellfounded).

The two conceptions of sets given us by (APG 1) and (APG 2) will differ in that the conception given by (APG 1) includes sets that are not wellfounded, but that given by (APG 2) does not. One very striking fact about these two APG ways of conceiving sets is that that is the *only* difference between them: all the other axioms suggested by one conception are also suggested by the other. Equally striking is the fact (making the same exception) that the axioms arising from the two APG conceptions are the same as the axioms that arise from the cumulative hierarchy conception. (The axioms of the second bundle (see below) correspond to straightforward operations on APGs.) Indeed Marco Forti has made the point that it is probably a pure historical accident that set theory came down on the side of the axiom of foundation rather than the side of the axiom of antifoundation. It is striking how little would change if set theory were to change horses in midstream and use the antifoundation axiom instead. See the discussion of Coret’s axiom on page 41.



## Chapter 4

# Stating the Axioms

In this chapter we state all the axioms of ZF, but do not explain their roles in any great detail. Some of the axioms deserve to have chapters dedicated to an explanation of their meaning.

The axioms of Set Theory can be divided into four – or perhaps five – natural bundles. The first bundle tells us what sort of thing sets are; the second bundle tells us which operations the universe of sets is closed under; the third bundle tells us that the second bundle at least has something to work on. The fourth is a result of bundling the remaining axioms into a ... bundle.

### 4.1 First Bundle: The Axiom of Extensionality

The axiom of extensionality tells us what sort of things sets *are*. It arises immediately from the conception of sets as minimalist mathematical objects, as at the start of section 3.1. Why does this give us extensionality? One direction is easy. Clearly sets with distinct members must be distinct sets, by the indiscernibility of identicals. For the other direction: if we discard all the gadgetry from our structures, and for each structure retain only its members, then clearly it is only the members that remain to enable us to tell them apart. This is precisely the content of the axiom of extensionality: *distinct sets have distinct members*. If  $x \neq y$  are two sets then there is something that belongs to one but not the other.

The name is no accident. The axiom arises from the concept of sets as arbitrary objects-in-extension. Every suite of objects-in-extension has a kind of extensionality principle. Two ordered pairs with the same first component and the same second component are the same ordered pair. Two lists with the same members in the same order are the same list. Two functions-in-extension that contain the same ordered pairs are the same function-in-extension. The axiom of extensionality for sets that we have just seen is merely the version of this principle for sets.

## 4.2 Second Bundle: The Closure Axioms

Next we list the axioms that tell us what operations the universe is closed under. As well as stating each axiom (scheme) we will try to find something enlightening to say about why the axiom appears in our line of sight, and what it tries to do. Mostly this can be done in-line, but in some cases this explanation is long enough to justify a dedicated chapter (for replacement) or even – God help us – a separate volume (for the axiom of choice).

Better prepare the audience for this!

We haven't mentioned levels yet

In each subsection in this section one wants to find something to say not only about what the axiom in question means/does, but also why one might want (perhaps, sometimes) to do without it. If you drop Infinity it's beco's you are studying  $V_\omega$ , which is to say  $\mathbf{N}$ - beco's of Kaye-Wong [26]. If you drop Pairing it's beco's you are interested in many-sorted set theory; If you drop power set it's beco's you are interested in a world where everything is countable; If you drop all of Infinity, Pairing and Power set it is beco's you are interested in Level Theory, in studying levels for their own sake.

This second bundle of axioms contains:

### 4.2.1 Pairing

For any two sets  $x$  and  $y$  the pair set  $\{x, y\}$  exists.

$$(\forall x)(\forall y)(\exists z)(\forall w)(w \in z \longleftrightarrow (w \in x \vee w \in y)).$$

The axiom of pairing is such an apparently innocent demand – what harm could there possibly be in  $\{x, y\}$  always existing? – that it doesn't really invite discussion. Also it is so basic that it is quite difficult to imagine what life would be like without it. Without pairing we cannot construct Wiener-Kuratowski ordered pairs. Maybe there is a different way of implementing ordered pairs that doesn't need *unordered* pairs, but putting it like that doesn't make it sound hopeful. And if we haven't got ordered pairs then we can't think of binary relations as sets of ordered pairs. Functions – and relations of higher degree – are presumably in the same boat. It sounds as tho' pairing is something so basic that there is no sense to made of what life without it would be like, and no further argument for its adoption is needed. Interestingly this is not so: the axiom of pairing does have a meaning: it represents a choice between two ways of doing set theory – typed vs untyped, and there is a sensible alternative<sup>1</sup>. It just so happens that that alternative is not one that we wish to pursue, but it is worth outlining nevertheless, if only to give a contrastive explanation of the role of the axiom.

Consider the binary relation  $R(x, y)$  that says  $(\exists z)(x \in z \wedge y \in z)$ . Sets related by this relation are in some sense *cohabitable*; they can cohabit in a set. Recall Alcuin's puzzle about getting the fox, the goose and the bag of beans across the river. The fox and the bag of beans are cohabitable, but neither of them are cohabitable with the goose!

<sup>1</sup>There is a comment to this effect in [?].



The relation  $R$  is evidently symmetric. And – as long as every set belongs to *something* – it will be reflexive. Now the axiom of power set tells us that every set belongs to its power set so  $R$  is indeed reflexive. It is transitive? This is slightly tricky: if we have decided that it is reflexive on the grounds that we have embraced the axiom of Power set then if we decide it is to be transitive it will actually be the universal relation. This is because every power set contains the empty set. So every set is related to the empty set, so the relation is the universal relation – which is what the axiom of pairing would be telling us if we adopt it. If we want the relation to be transitive without being the universal relation then we will have to do some clever footwork with empty sets, atoms, that sort of thing. We won't go into that here (but see [33] if you wish to pursue this). Suffice is to say that one sane alternative to the axiom of pairing (which tells us that the relation is the universal relation) is an axiom to say that the relation is an equivalence relation.

It will be as long as we have – for example – an axiom to say that  $x \cup y$  exists as long as  $x$  and  $y$  have nonempty intersection.

There are two natural questions to ask about this equivalence relation:

1. Is it the universal relation?
2. Are the equivalence classes sets?

(This looks more like one choice than two co's if both answers are 'yes' you get a universal set and that is fraught with danger)

The axiom of pairing is quite simply the assertion that this relation  $R$  is the universal relation.

If the answer to (1) is 'no' and the answer to (2) is 'yes' then the equivalence classes are little microuniverses inhabited by sets that are pairwise cohabitable. *Microuniverses?*

Yes, microuniverses. Let us suppose that we have retained at least some of the other axioms of ZF – specifically power set and sumset – despite dropping pairing. The existence of  $\mathcal{P}(x)$  means that all subsets of  $x$  are equivalent (any two of them cohabit in  $\mathcal{P}(x)$ , after all and similarly the existence of  $\bigcup x$  means that any two members-of-members of  $x$  are equivalent. Indeed, for any  $x$  and, for any concrete  $n$ , any two members-of <sup>$n$</sup>   $x$  are equivalent. These equivalence classes are starting to look a bit like the *levels* of models of typed set theory as in things like [56]. If we want to go in that direction we can add an axiom to say that all equivalence classes are sets. If we do, quite a lot of typed set theory follows spontaneously. For example, no equivalence class can have more than one equivalence class as a member (because of the axiom of sumset).

Before we leave this topic it might be worth noting that one can set up a strongly typed theory of sets in the language  $\mathcal{L}(\in, =)$  of set theory, with no extra type predicates. This is discussed in [33]

That is pleasing and potentially useful, but it is not really our concern here – beyond making the point that the adoption of the axiom of pairing represents opting for a one-sorted theory of sets instead of a many-sorted theory of sets.

Of course in dropping pairing in order to go for a many-sorted theory of sets we do not in fact discard the idea of pairing *altogether*. It is perfectly sensible to adopt for typed/many-sorted set theory an axiom that says that one can form the pair  $\{a, b\}$ ... as long as  $a$  and  $b$  are of the same sort. That way one gets all the gadgetry of ordered pairs, relations, functions etc etc *locally*.

## A Digression on Level Theory

One natural setting in which we do not assume the axiom of pairing is the *Level Theory* LT of Button and Potter, which uses ideas going back to Scott [57]. It is presented to the public as an alternative way of describing the cumulative hierarchy. Generally set theorists have not embraced it. It's worth mentioning briefly in this connection because a *level* is an initial segment of the cumulative hierarchy in which *inter alia* pairing does not hold.

This is the first point at which we mention categoricity. It appears in vol II as well. Should be have a glossary entry for it?

The point of departure for LT is the *quasi-categoricity* of ZF. ZF isn't *literally* second-order categorical in the sense of having precisely one second-order model. However it is the case that the second-order models of ZF are precisely suitably closed initial segments  $V_\alpha$  of the cumulative hierarchy, specifically those  $V_\alpha$  where  $\alpha$  is weakly inaccessible. The thought is that for any sensible condition  $\mathcal{C}$  on initial segments of the cumulative hierarchy there could be a set theory whose second-order models are precisely the initial segments of the cumulative hierarchy satisfying  $\mathcal{C}$ .

Since Zermelo is a fragment of ZF and a typical model of Zermelo could be an initial segment of the cumulative hierarchy – a  $V_\alpha$  – one might hope that the second-order models of Zermelo are those  $V_\alpha$  satisfying some weaker condition ...  $\alpha$  limit, perhaps. It turns out that this is not the case; it transpires that if one wants a weakening of ZF the class of whose second-order models is precisely the class of initial segments  $V_\alpha$  of the cumulative hierarchy satisfying some weaker condition on  $\alpha$  one needs to identify and axiomatise the idea of a *level of the cumulative hierarchy*. This will lead us to LT.

So: how do we say that every  $V_\alpha$  exists? It turns out that one can give a good definition of a *level* **without** using ordinals, as follows. We need the operation that sends a set to its closure under  $\subseteq$ . Button uses the symbol ' $\P$ ' for this:  $\P(X) = \{y : (\exists x \in X)(y \subseteq x)\}$ . Being a closure operation  $\P$  is idempotent and distributes over  $\bigcup$  and  $\bigcap$ .

We say  $h$  is a *history* iff  $(\forall x \in h)(x = \P(x \cap h))$ .

We then say that  $X$  is a *level* if  $X$  is  $\P(h)$  for some history  $h$ .

It is not hard to show that, thus defined, every level is transitive.

Button proves the rather striking result that  $\in$  restricted to the class of levels is wellfounded. (This is an improvement on a result of Hurkens [34]). We supply the proof since it is pleasing and unexpected, and deserves to be better known.

We actually prove something slightly more general. For the moment let a *closed* set be a set closed under  $\subseteq$  – a value of  $\P$ . We will show that  $\in$  among closed sets is wellfounded. Let  $W$  be a set of closed sets with no  $\in$ -minimal member. We will show that if  $W$  is nonempty then every wellfounded

set belongs to  $\bigcap W$ . Suppose every member of  $X$  belongs to everything in  $W$ , and let  $w$  be an arbitrary member of  $W$ . Then  $X \subseteq$  every member of  $w$  and – since  $w$  is closed under  $\subseteq$  – we have  $X \in w$ . But  $w$  was an arbitrary member of  $W$ . So  $x \in \bigcap W$  as desired. So  $\bigcap W$  is a proper class, which is impossible since it is included in each of the sets in  $W$ . So  $W$  must have been empty. Thus  $\in$  restricted to the class of closed sets is wellfounded. So *a fortiori*  $\in$  restricted to the class of levels is wellfounded.

This is something to do with the fact that  $\P$  is morally a second-order notion.

LT is now the theory that has separation plus the assertion that every set is included in a level. It is not hard to show that this extra assertion implies that  $\in$  is wellfounded on all sets, not just on levels. LT now has the pleasing property that its second-order models are precisely the  $V_\alpha$ , with  $\alpha \geq \omega$ .

Notice that LT does not assume pairing or power set or infinity!

## H I A T U S

Another way of motivating dropping power set and pairing in the context of levels ...

We wrote of *microuniverses* earlier [...]. Distant cousins of microunivwerses are *levels*. The parallel is well-motivated by the discussion in [?]. On this picture the set-theoretic universe is assembled from levels, and one describes it by a process of stacking the levels on top of each other. To obtain the cumulative hierarchy as the result of this process one wants to be able first to describe initial segments (namely levels/towers-of-levels) – and for this we do not need power set or pairing – and then one wants to say that there is no last level. That, of course, is where the axiom of power set come in. Idea: *power set is what you add to Level Theory to get Set Theory*.

### 4.2.2 Sumset

$$(\forall x)(\exists y)(\forall z)(z \in y \longleftrightarrow (\exists w)(z \in w \wedge w \in x)).$$

The  $y$  whose existence is alleged is customarily notated ' $\bigcup x$ '.

One could express this more [...] by saying that it tells us that the structure  $\langle V, \subseteq \rangle$  is *complete* in the sense that every subset of it has a least upper bound. More broadly this tells us that that if we are trying to construct an object by recursion over the ordinals, where we add something at each stage, so our stages are ordered by  $\subseteq$ , and the stages in the construction are indexed by ordinals... then we don't automatically crash at limit ordinals. This is beco's the existence of the objects we desire at limit stages is assured by applying the axiom of sumset to the set of earlier stages.

### 4.2.3 Aussonderung

also known as **separation**. This axiom scheme is

$$(\forall x)(\forall \vec{w})(\exists y)(\forall z)(z \in y \longleftrightarrow (z \in x \wedge \phi(z, \vec{w}))).$$

Any subcollection of a set is a set. This axiom appeals to a limitation of size principle which we shall discuss in more detail below, around p. 50. If safety is to be found in smallness, then any subset of a safe thing is also safe. But there is also an idea of *definiteness* in the air: if  $x$  is a thing definite enough to be a set, and  $\phi$  is a property definite enough to be written down, then the fragment of  $x$  containing those things that are  $\phi$  is also definite enough to be a set.

*Definiteness* loomed large in the early literature – albeit as an undefined (dare one say *indefinite*) notion. Nowadays ‘definite’ has morphed into ‘captured by a first-order formula of  $\mathcal{L}(\in, =)$ ’ and the modern reader can probably safely regard this evolution as having concluded.

### 4.2.4 Power Set

$$(\forall x)(\exists y)(\forall z)(z \in y \longleftrightarrow z \subseteq x).$$

I think we are going to have to assume that the reader knows Cantor’s theorem!

The power set (here “ $\mathcal{P}(x)$ ” – the use of a capital ‘P’ of some font or other is standard notation for this construct) of  $x$  is bigger than  $x$  (that’s Cantor’s theorem) but not *dangerously* bigger. Again this is an axiom that could arise only once one had the idea of sets as objects-in-extension. With ideas of definiteness still ringing in our ears one might doubt that one could assemble all the subsets of a given set into one object if one doesn’t know what they are: the idea of a collection of all the subsets of a given set is deeply suspect to those who conceive sets as intensional objects. To collect all the subsets suggests that there is an idea of *arbitrary subset* – and of course that way of thinking is part of the object-in-extension package.

Perhaps this is the stage at which we need to say a bit more about why we adopt it.

Felt to be impredicative and suspect.

There are people who are wary of this axiom beco’s of its impredicative nature. This wariness is akin to the wariness that many people feel around second-order logic – tho’ that second wariness is much more widespread. I once asked my then office-mate Oren Kolmen what one should say to people who heedlessly embrace second-order logic. His reply was “Ask them what they think their quantifiers mean” ... the point being that you don’t know what all the subsets of an [infinite] set-in-extension  $X$  are. If you don’t know what the subsets of  $X$  are, a quantifier over *all* of them is mysterious and unaccountable, and you don’t know what it is doing. And, by the same token, you don’t know what  $\mathcal{P}(X)$  is either. So you’d better not ask for it to exist.

Nevertheless, a received view (and one that informs this book) is that second-order logic is The Work Of The Devil but that the axiom of power set is all right.

### 4.2.5 Axiom Scheme of Replacement

The **Axiom Scheme of Replacement** is the scheme that says that the image of a set in a function is a set. Formally:

$$(\forall x)(\exists! y)(\phi(x, y)) \rightarrow (\forall X)(\exists Y)(\forall z)(z \in Y \longleftrightarrow (\exists w \in X)(\phi(w, z)))$$

This is a scheme rather than a single axiom because we have one instance for each formula  $\phi$  that captures a function.

The discussion of the axiom scheme of replacement will take us a long time, because it gets its tentacles into many other areas and we will have to get into each of them far enough to explain why it gets involved: it will have an entire chapter to itself (chapter 6).

## 4.3 Third Bundle: The Axioms of Infinity

On reflecting upon the axioms of the second bundle we notice an annoying fact: if there are no sets at all then vacuously all the axioms in the first and second bundles are true! We need an axiom to start the ball rolling: something to say that the universe is nonempty. Since (by putting a self-contradiction for  $\phi$  in the axiom scheme of separation above) we can show that if there are any sets at all there is an empty set, then the weakest assertion that will start the ball rolling for us is the assumption that there is an empty set:<sup>2</sup>

- **Empty Set**  $(\exists x)(\forall y)(y \notin x)$ .

However, just as the empty universe is a model for all the axioms up to (but not including) the axiom of empty set, we find that a universe in which every set is finite can be a model for all those axioms *and* the axiom of empty set. This means that we haven't yet got all the axioms we want, since there are at least some sets that are indubitably infinite:  $\mathbb{N}$  and  $\mathbb{R}$  for example. If we are to find any simulacra for them in the world of sets we will have to adopt an axiom that says that there is an infinite set.

- **Axiom of Infinity**: There is an infinite set.

We will leave unspecified for the moment the precise form that this axiom will take.

Thus one can think of the axiom of empty set and the axiom of infinity as being two messages of the same kind: “The Universe is nonempty!”; “The Universe is really *really* nonempty!”.

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<sup>2</sup>There is a literature (see for example [64]) whose burden is that it is possible to believe in the existence of sets while not believing in the empty set. Some people even repudiate singletons. I shall ignore whatever merits there may be in this point of view, on the same grounds that I here ignore NF and positive set theory: it's not part of the mainstream. In any case, as I argued on p. 16, once one accepts arbitrary widgets-in-extension one has accepted null widgets.

Once one thinks of these two axioms as bearing two messages of the same kind, one starts wondering if there might perhaps be other messages of the same kind, and perhaps even a reliable and systematic way of finding bottles containing them – perhaps even a whole cellar. It turns out that there are. The way in is to think about ways of iterating the step that took us from the axiom of empty set to the axiom of infinity.

We needed the axiom of empty set because we noticed that without it the universe might not contain anything. We then noticed that we needed the axiom of infinity because if we assumed only the axiom of empty set then there might not be any infinite sets and no way of representing infinite objects like  $\mathbb{N}$ ,  $\mathbb{Q}$  and  $\mathbb{R}$ .

How are these two moves to be seen as moves of the same kind? In both these cases there is a property  $\phi$  such that the axioms-so-far do not prove  $(\exists x)(\phi(x))$ , and the new axiom-to-be asserts  $(\exists x)(\phi(x))$ .

## BAD JOIN

The axioms of set theory say that  $V$  is the result of closing  $\{\emptyset\}$  under certain operations. This closure defines an operation from sets to sets. A new axiom will say that the universe is closed under this new operation.

setlike defined at dfn 2

And so on! Connect with *setlike*. Let's do this properly. Let  $f$  be some operation. We will have an axiom to say that  $V$  is closed under  $f$ . Writing ' $\text{clos}(A, f)$ ' for the closure of  $A$  under  $f$  we might spice this up to say that  $(\forall x)(\text{clos}(\{x\}, f) \text{ exists})$ . If we write ' $\text{clos}(\{x\}, f)$ ' as ' $F(x)$ ' then our axiom will say that  $V$  is closed under  $F$ .

## BAD JOIN

This suggests a strategy for developing an endless stream of axioms of infinity. At each stage one devises the next axiom of infinity by thinking of a natural property  $\phi$  such that the axioms-so-far do not prove  $(\exists x)(\phi(x))$ , so we take  $(\exists x)(\phi(x))$  to be our new axiom.

But what is this  $\phi$  to be? We need a sensible *systematic* way of dreaming up such a  $\phi$ . There are of course lots of ways, some more natural than others. In fact the axiom of infinity itself illustrates one sensible way. From the perspective of ZF-with-empty-set-but-not-yet-infinity we think that the universe might consist of  $V_\omega$ , the collection of hereditarily finite sets. It is true that every set in  $V_\omega$  (and therefore every set in what the universe might be) is finite, so “being not-finite” is certainly a candidate for  $\phi$ . However we can say more than that:  $V_\omega$  itself is not finite: infinitude is not only a property possessed by none of the things we have axiomatised so far but is also a property of the collection of them. So, in general, one way to get the *next* axiom is to think of an initial segment  $V_\gamma$  of the wellfounded universe that is a model of the axioms we have so far, and find a  $\phi$  that is true of  $V_\gamma$  but not of any of its members.

There are various ways of turning this strategy of developing a sequence of axioms of infinity into something a bit more formal. Some of them can be quite recondite, and this is not the place for a treatment of material of such

sophistication. Suffice is to say that any suitably systematic and formal strategy for developing new axioms of infinity will itself start to look like a principle that says that the universe is closed under certain operations – in other words to look like an axiom (or axiom scheme) of the second bundle.

### 4.3.1 The particular form of the axiom of infinity

Make the point somewhere that although the morally correct way to state Infinity is just to say there is an infinite set, and this gives us an implementation of arithmetic, this gives us immediately only a “local” implementation. If we want a global implementation (immediately) then go the grubby route.

Perhaps this should be moved: it duplicates material around p. ??

Axiom of Infinity:  $(\exists x)(\emptyset \in x \wedge (\forall y)(y \in x \rightarrow y \cup \{y\} \in x))$

Quite why it should take this form has something to do with the implementation of ordinals, to which we now turn.

The von Neumann implementation of ordinals (the industry standard) implements the natural number zero as  $\emptyset$  and implements the successor function as  $x \mapsto x \cup \{x\}$ .

The version of the axiom of infinity displayed above is precisely configured to give us a set containing 0 and closed under successor. Such a set is sometimes called a *successor set*.  $\mathbb{N}$  is of course the  $\subseteq$ -least successor set and, given any successor set  $A$ , we can obtain  $\mathbb{N}$  from it by separation, as

$$\{x : x \in A \wedge (\forall y)(\emptyset \in y \wedge (\forall w)(w \in y \rightarrow w \cup \{w\} \in y) \rightarrow x \in y)\}$$

which is a set by separation.

## 4.4 Fourth Bundle

So far we have axioms of three kinds

- (i) extensionality;
- (ii) the closure axioms (pairing, power set, sumset, separation: all the axioms that tell you how to make new sets from old);
- (iii) axioms of infinity (which some authors regard as closure axioms).  
Then there are
- (iv) axioms like the axiom of choice and the axiom of foundation.

These are different from the other sporadic axioms in that they are almost universally regarded as core axioms. The other sporadic axioms are not always pairwise consistent: they include Gödel’s axiom  $V = L$  (which we will discuss briefly) and Martin’s Axiom and the Axiom of Determinacy (which we won’t).

Of these axioms, the axiom of foundation deserves a chapter to itself, and the axiom of choice merits a whole book.





## Chapter 5

# The Axiom of Foundation

Somewhere we have to make the point that the Axiom (scheme) of foundation implies in GB – via Scott’s trick – that every equivalence relation has a classifier. Actually it’s slightly more nuanced than that. I think you need the axiom of levels, so that the classifier sends each object to the intersection of its equivalence class with the lowest level it meets. Mere foundation by itself is not enough (Can’t do it in Zermelo)

If it is so obviously true why is it so easy to prove its independence?

Scott’s trick isn’t explained until later.

Say something about Saveli-eff..?

The axiom of foundation is the assertion that every set belongs to the cumulative hierarchy. Standard textbooks explain how this is equivalent to the principle of  $\in$ -induction and also to the assertion that  $\in$  is wellfounded, so there is no need to go over that here.

What is its status? This question can be read in two ways: first as an enquiry about politics, second as an enquiry about mathematics. Politically the answer is clear: practically everyone who studies Set Theory adopts it, and 99% of time when someone uses the expression ‘Set Theory’ to mean an axiomatic set theory (rather than – more generally – the study of the combinatorics of the  $\in$  relation) then the theory that have in mind will be ZFC, and foundation is one of its axioms. Mathematically the situation is more complex. It’s a sensible axiom to adopt, but it has sensible alternatives too. We will spend some time on them, but not much: after all, my brief is “An Introduction to the Axioms of Set Theory” and ‘Set Theory’ for these purposes means ZFC,

Set theorists (most of them) would have you believe that the axiom of foundation is a fundamental fact about the nature of God’s creation, so that the whole of Mathematics is perched on the back of a gigantic Empty Set, and Always has Been. But it’s really nothing of the sort. Fraenkel rather let the cat out of the bag when he said, quite early on, that the axiom of foundation was a kind of restrictive axiom which one can adopt *pro tem* and could be discarded later when it turns out to be an obstacle to progress. Meanwhile it has become entrenched: temporary expedients have a way of becoming permanent fixtures. Boolos [6] has the grace to admit “There does not seem to be any argument

tho’ I could be talked into providing such an explanation.

Need a citation

We probably need to say a little bit about *Beschränktheitsaxiome* – otherwise known as *ring-barking axioms* i’ve tried to get an anagram but it doesn’t quite work.

find this reference Blah sunset clauses blah Britain goes metric yeah right.

that is guaranteed to persuade someone who really does not see the peculiarity of a set's belonging to itself, or to one of its members etc., that these states of affairs are peculiar". Indeed there isn't. What there is is a tendency in human affairs for people to find *post hoc* narratives and provenances for things that could have been otherwise. Whig history.

Altho' the idea that the axiom is *true* is pretty crazy, a policy of ignoring counterexamples – of pretending that all sets are wellfounded – is actually quite defensible. Why might this be so? If you think that a large part (perhaps all) of the motivation for set theory is to provide a foundation – or at least a public space – for mathematics, then you evaluate competing conceptions of sets according to how well those different flavours of sets provide that foundation or space. Interestingly, several of the competing conceptions result in ways of doing set theory (and therefore of implementing mathematics) that turn out to be pretty much the same. And *pretty much the same* in ways that can be made precise.

The axiom of foundation is almost universally adopted by people studying set theory. There are several things going on here. It is certainly the case that some of the people who adopt it do so because they simply believe it to be true. They have an iterative conception of set from which the axiom of foundation follows inescapably.<sup>1</sup> There are others who, while having a more inclusive view of what sets are or might be, nevertheless feel that there is nothing to be gained by remaining receptive to the possibility of extra sets violating the axiom of foundation, simply because the illfounded sets bring us no new Mathematics. This is a much less straightforward position, but of course also much less contentious, and much more interesting. The idea that illfounded sets bring us no new Mathematics is an important one, and merits some explanation. There are two relevant results here. To capture them both we need Coret's axiom: *Every set is the same size as a wellfounded set*. See Forster [21] and Coret [14].

first what?

The first is the folklore observation that the two categories of wellfounded sets and sets-arising-from-AFA are equivalent. In fact all that is needed is that both foundation and antifoundation imply that every set is the same size as a wellfounded set, so both categories are equivalent to the category of sets-according-to-Coret's-axiom.<sup>2</sup>

For the second we need to reflect on the idea that Mathematics is strongly typed: reals are not sets of natural numbers, the real number 1 is not the same as the natural number 1 and so on. If we take this idea seriously we should expect that if all of Mathematics can be interpreted into set theory then it should be possible to interpret it into a set theory in such a way that all the interpretations are strongly typed in some set-theoretic sense of 'strongly typed'; the obvious candidate for this kind of strong typing is Quine's notion of stratification, which has venerable roots in Russell-and-Whitehead [56].

Then one might be receptive to the result that the two extensions

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<sup>1</sup>Perhaps not *inescapably* (see Forster [20]) but certainly *plausibly*.

<sup>2</sup>Thanks to Peter Johnstone for reassurance on this point.

- (i) ZF + foundation and
- (ii) ZF + AFA

of ZF + Coret's axiom are both conservative for stratifiable formulæ. That is to say, if all of Mathematics is stratified, ZF + foundation and ZF + AFA capture the same mathematics. So there really is nothing to be gained by considering illfounded sets.

Against that one can set the observation that among the alternative conceptions of sets are several that tell us that there will be illfounded sets. The most important of these are:

- (i) the Antifoundation view of Forti and Honsell [27];
- (ii) Church's Universal Set theory [11];
- (iii) The NFU conception of illfounded set;
- (iv) The positive set theory of Hinnion, Malitz etc.

All these theories can be interpreted into ZF (or natural enhancements of ZF). This creates an opening for the rhetorical move that says: all these things can be interpreted into ZF so they can be seen as mere *epiphenomena*. The difficulty for people who wish to adopt this point of view is that there are interpretations in the other direction as well: ZF can be interpreted in all these theories (or natural enhancements of them as before). So which conception is primary? One is reminded of what philosophers call the "paradox of analysis"<sup>3</sup>

Do we *really* need to talk about GPC?

Say something about this

So one has all these various competing theories which are mutually interpretable. People who wish to stick with the axiom of foundation can always invoke the opportunity cost consideration: the other conceptions of illfounded set are things one simply doesn't want to explore: our lifetimes are finite, there are infinitely many things one might study, to live is to make choices, and to make choices is to abandon certain projects the better to concentrate on others that we judge to have better prospects. Let's stick with the devil we know!

However there is an extra reason for adopting the axiom of foundation, which is a purely pragmatic one. It enables one to exploit a useful device known as *Scott's trick*, which I will now explain.

Many mathematical objects arise from equivalence classes of things. For example cardinal numbers arise from the relation of equipollence:  $x$  and  $y$  are equipollent iff there is a bijection between them. Two sets have the same cardinal iff they are equipollent. If one wants to implement *as sets* mathematical objects that arise from an equivalence relation  $\sim$  in this way then one is looking for a function  $f$  from the universe of sets to itself which satisfies

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<sup>3</sup>The phrase first appears in [36] p 323: "Let us call what is to be analyzed the *analysandum*, and let us call that which does the analyzing the *analysans*. The analysis then states an appropriate relation of equivalence between the *analysandum* and the *analysans*. And the paradox of analysis is to the effect that, if the verbal expression representing the *analysandum* has the same meaning as the verbal expression representing the *analysans*, the analysis states a bare identity and is trivial; but if the two verbal expressions do not have the same meaning, the analysis is incorrect." but the literature goes back through G. E. Moore all the way to Plato's *Meno*.

$$x \sim y \longleftrightarrow f(x) = f(y) \tag{5.1}$$

“classifier”?

Such a function  $f$  is an *implementation* (such as we will consider in section 6.6). What could be more natural than to take  $f(x)$  to be  $[x]_{\sim}$ , the equivalence class of  $x$  under  $\sim$ , so that – for example – we think of the number 5 as the set of all sets with five members? Natural it may be, but if we have the other axioms of ZF to play with, we get contradiction fairly promptly. If 5 is the set containing all five-membered sets, then  $\bigcup 5$  is the universe, and if the universe is a set, so is the Russell class, by separation.

This prevents us from thinking of cardinals as equivalence classes – despite the fact that that is how they *arise*. There is no special significance to the equivalence relation of equipollence here: the same bad thing happens with any other natural equivalence relation of this kind. In ZF mathematical objects that arise naturally in this way from equivalence relations cannot be thought of as equivalence classes.

The axiom of foundation offers us a way out. In general, we want to implement a mathematical object as the set of all its instances, the things we are trying to abstract away from. The collection of such instances might not be a set, as we have just seen in the case of the number 5. However, there is nothing to stop us implementing the mathematical object as the *set of all its instances of minimal set-theoretic rank*. The object answering to the italicised description is a set by the axioms of ZF<sup>4</sup>, since it can be obtained by separation from the set  $V_{\alpha+1}$ , where  $\alpha$  is the minimal rank of an instance.

It is true that some of the entities we want to implement as sets can be implemented by special *ad hoc* tricks without assuming foundation. For example, the implementation of ordinals as von Neumann ordinals does not exploit Scott’s trick: to prove that every wellordering is isomorphic to a von Neumann ordinal one does not need foundation, one needs only replacement (specifically the consequence of it called Mostowski’s Collapse lemma of section 6.5.8). Nevertheless, the smooth and uniform way in which Scott’s trick enables us to implement arbitrary mathematical objects (at least those arising from equivalence relations on sets, or on things already implemented as sets) enables us to make a case for adopting the axiom of foundation that will be very powerful to people who just want set theory sorted so they can get on with doing their mathematics.

It has to be conceded that if one is interested in Scott’s trick then Foundation is overkill. The (for example) cardinal of  $x$  is the set of things of minimal rank that are equinumerous with  $x$  that are of minimal rank. Of course any wellfounded relation will do. To get Scott’s trick to work it is enough to have a wellfounded relation  $R$  on the universe with the property that the collection of things that have rank  $\alpha$  is a set. (Remember that any wellfounded relation supports a rank function). There is nothing to say that  $R$  has to be  $\in$ . Savelieff [?] has considered generalisations where  $R$  is not  $\in$ .

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<sup>4</sup>And we do really mean ZF here, not Zermelo set theory. It seems that replacement is needed to get the set-theoretic rank function to behave properly. Tho’ Scott’s trick works in Level Theory.

Finally we should return briefly to the axiom of choice in this connection – specifically in connection with equipollence and the implementation of cardinals. The axiom of choice implies that every set can be wellordered. One consequence of this is that every set is equinumerous with a von Neumann ordinal,[at least if we have replacement!!!!] This means that we can take the cardinal of a set to be the least ordinal with which it is equinumerous. This implementation works very well. In fact it works so well that there are people who think it is the *only* implementation (so that they think that cardinals just *are* special kinds of von Neumann ordinals) and believe that if one does not assume AC then one cannot implement cardinals in set theory at all! This is not the case, since as long as we have foundation there is always Scott’s trick<sup>5</sup>. This example serves to underline the importance of Scott’s trick. Gauntt [29] showed that if we assume neither the axiom of foundation nor the axiom of choice then we can find a model which has no implementation of cardinals.

We do need to Say something  
about the axiom of restric-  
tion

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<sup>5</sup>Tho’ we do need replacement as well to get the minimal-rank thing



## Chapter 6

# The Axiom Scheme of Replacement

[summary: Statement of the axiom, and collection. Spurious arguments in favour (Randall's slides) spurious arguments against.

LOS, literally and figuratively.

Uses in set theory: In Zermelo can't define rank or Scott's trick.  $\aleph_\omega$ . Existence of transitive closures, Mostowski collapse; existence of inductively defined families, Borel determinacy. equivalence of various forms of infinity. Reflection. Collection and normal form theorems. Essential for forcing and constructibility.

Must say something about replacement and second-order categoricity

Significance outside set theory implementation-invariance.

Should Say something about how we need replacement to do infinite limits and colimits. Or IO! ]

### 6.1 Stuff to be put in the right place in this chapter

Why collection is stronger than Replacement.

Both replacement and collection are conditionals; the antecedents are similar, but the antecedent of an instance of replacement has an additional uniqueness condition, and this renders the conditional weaker. If one has global choice then one can always satisfy a uniqueness condition by using the global choice function. One can use other ruses such as foundation, or the axiom **50** of Saveli-eff. How does that work? Suppose everybody has some widgets; we want a bundle of widgets such that everyone has a widget in that bundle. If we are able to choose one widget for each person, then we can use replacement to build a collection of widgets answering to the conclusion of the appropriate instance of the axiom scheme of collection. (That's how we exploit global choice). But we can build such a collection even if we are unable to decide on a particular widget

for each person, as long as we can find a **set**  $f(p)$  of widgets for each person  $p$  ...for then we can use replacement to form the set  $\{f(p) : p \text{ is a person}\}$  and then its sumset  $\bigcup\{f(p) : p \text{ is a person}\}$  is the set that the axiom of collection is supposed to deliver. How are we to find such a **set** of widgets? This is where we need foundation or **5Φ**.

The IBE arguments for replacement were probably not much articulated at the time (in the sense that the expression was not in use) tho' they were the driver. The utility of replacement/collection in proving normal form theorems for restricted quantifiers and implementation-insensitivity are subsequent discoveries that vindicate the original IBE explanation.

Need to declare adjunction and subcison

This is where we first use the notation ‘  $f$ “ $x$  ’

One point definitely worth making is that replacement and collection for finite sets follow by induction using nothing more than adjunction. We prove that, for all (total) functions  $f$  and any finite set  $a$ ,  $f$ “ $a$  is a set.  $f$ “ $\emptyset = \emptyset$ ; if  $f$ “ $X$  exists then, for any  $a$ ,  $f$ “ $X \cup \{f(a)\}$  exists by adjunction, and this object is precisely  $f$ “(  $X \cup \{a\}$  ).

[The proof for collection is analogous]

Notice how notations like  $\{f(x) : x \in A\}$  presuppose replacement ...

Much the same goes for AC of course

Scylla and Charybdis

People who deny it (because they haven't understood it and think it isn't needed). Typically people in this camp know very little set theory.

## 6.2 Statement of the scheme

Recap from section 4.2.5

The **Axiom Scheme of Replacement** is the scheme that says that the image of a set in a function is a set. Formally:

$$(\forall x)(\exists!y)(\phi(x, y)) \rightarrow (\forall X)(\exists Y)(\forall z)(z \in Y \longleftrightarrow (\exists w \in X)(\phi(w, z)))$$

This is a scheme rather than a single axiom because we have one instance for each formula  $\phi$ .

One can think of Replacement as a kind of generalisation of pairing: Pairing (+ sumset) is the economical (finite) axiomatisation of the scheme that says that any finite collection is a set. This scheme is certainly a consequence of the idea that any surjective image of a set is a set – at least once we have an infinite set! The name ‘replacement’ comes from the imagery of an agent taking a set and *replacing* each element in it by a novel element – namely the value given to that element by a function that the human has in mind.

The **Axiom Scheme of Collection** states:

$$(\forall x \in X)(\exists y)(\psi(x, y)) \rightarrow (\exists Y)(\forall x \in X)(\exists y \in Y)(\psi(x, y)),$$

where  $\psi$  is any formula, with or without parameters.



How can we motivate ‘collection’ as a name for the axiom scheme of that name?

Every French Impressionist painter painted some paintings. A representative French Impressionist **collection** contains at least one painting by each French impressionist painter. The axiom scheme of collection now says that if the multitude of French impressionist painters constitute a set, then there is a collection of their paintings that is also a set, thus: if for every  $x$  [secretly a painter] in  $F$  [secretly the set of French impressionist painters] there is a  $y$  related to  $x$  [secretly a painting painted by  $x$ ] then there is a set  $C$  that contains at least one painting from each French impressionist painter. There may be lots of such  $C$  of course.

Weaker versions of collection (e.g., for  $\psi$  with only one unrestricted quantifier) are often used in fragments of *ZFC* engineered for studying particular quantifier classes? phenomena.

In general, for most natural classes  $\Gamma$  of formulæ, the two schemes of replacement-for-formulæ-in- $\Gamma$  and collection-for-formulæ-in- $\Gamma$  cannot be relied upon to be equivalent. There are interesting subtleties in this connection that we have no space for here. At any rate, in the full version of these schemes as in *ZF*,  $\Gamma$  is the set of all formulæ and we’ve established that these two *unrestricted* schemes are equivalent. So now we can consider the proposal to adopt them as axioms.

**THEOREM 1.**  $WF \models \text{Collection and Replacement are equivalent.}$

*Proof:*

Replacement easily follows from Collection and Separation.

To show that replacement implies collection, assume replacement and the antecedent of collection, and derive the conclusion. Thus

$$(\forall x \in X)(\exists y)(\psi(x, y)).$$

Let  $\phi(x, y)$  say that  $y$  is the set of all  $z$  such that  $\psi(x, z)$  and  $z$  is of minimal rank. Clearly  $\phi$  is single-valued, so we can invoke replacement. The  $Y$  we want as witness to the “ $\exists Y$ ” in collection is the sumset of the  $Y$  given us by replacement. ■

This proof is very much in the spirit of Scott’s trick, with its exploitation of the idea of sets of minimal rank. Something like the axiom of foundation really seems to be necessary for the equivalence of collection and replacement – the existence of a universal set implies the axiom scheme of collection since a universal set collects everything we might want to collect! In contrast the existence of the universal set is incompatible with replacement because replacement implies separation, and universal set plus separation gives Russell’s paradox. And there are some absurdly weak systems of set theory with a universal set.

In general, for most natural classes  $\Gamma$  of formulæ, the two schemes of replacement-for-formulæ-in- $\Gamma$  and collection-for-formulæ-in- $\Gamma$  cannot be relied upon to be equivalent. There are interesting subtleties in this connection that we have no

space for here. At any rate, in the full version of these schemes as in ZF,  $\Gamma$  is the set of all formulæ and we've established that these two *unrestricted* schemes are equivalent.

### 6.3 Bad Reasons for and against

A point to make: not one in a hundred of the people who say that you can do Ordinary Mathematics without using Replacement have any idea how to. They may think they're not using replacement but much of the time they are, and anyone who doesn't notice when they are using replacement is hamstrung when confronted with the task of removing all applications of it. Most of them are not weirdo foundationalists but ordinary mathematicians who just want to get on with their mathematics. Time is short and they have better things to spend it on than worrying about replacement; that is not a crime. However, if you are such a person, and you propose to pick a fight with those people who annoy you by saying that Replacement really is indispensable, then the time-is-short argument does not let you off the hook. You have to show how you can do your mathematics without replacement. *Hic Rhodos hic salta*.

[Anyone who uses a notation like  $\{f(a_i) : i \in I\}$  is covertly using replacement, or at least an instance of it. And they are trusting replacement to deliver it even if they think replacement is not to be trusted – at least that is what their notation is saying. Do you think the set of squares of zeroes of the Riemann  $\zeta$ -function is a set? If you do it's probably because you think you can obtain it by replacement. You *can* get it by separation, it's true, but is that why you think it's a set?]

There are people who mutter<sup>1</sup> that the axiom scheme of replacement is

1. not needed for *ordinary mathematics*, and that
2. the only people who take any interest in it are sad misfits who self-identify as set-theorists.

Both of these assertions are wrong. Replacement is in fact used in “ordinary mathematics” which is why it is not just sad marginal people like set theorists who use it. (But perhaps it's only sad marginal set theorists who actually *take an interest*). More to the point tho', is that what mathematicians actually *do* will change from time to time, so the answer to a question of whether or not some topic belongs to “ordinary mathematics” will be determined by the date at which the question is asked, and not by the nature of the topic it is being asked about. Mathematics (may she live for ever) is time-invariant, so objections on the basis that something is not part of ordinary mathematics are simply not to be taken seriously. If it isn't part of ordinary mathematics today, who is to say that it won't be part of the ordinary mathematics of tomorrow?

People who doubt the applicability of replacement to ordinary mathematics should perhaps consider the following example sheet question:

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<sup>1</sup>This is not a typo for ‘nutter’.

Let  $G$  be a graph where, for each vertex  $v$ , the collection  $N(v)$  of neighbours of  $v$  is a set<sup>2</sup>.

- (i) Give an example to show that  $G$  might be a proper class.
- (ii) Now suppose  $G$  is connected. Prove that it is a set.

Answer in the appendix of answers to exercises.

*HOLE* Make somewhere the point that if we don't have replacement it is possible to have a classifier for equinumerosity that makes the collection of cardinals into a set. It is also possible (at least if one also drops foundation) to have a set whose transitive closure is the entire universe. Even if we don't drop foundation one can have a set whose transitive closure is a proper class.

We deal with (1) in section 6.5 below.

### 6.3.1 Is Replacement perhaps true in the Cumulative hierarchy?

There are also some bad arguments in favour.

It is often said that the axiom scheme of replacement is obviously true in the cumulative hierarchy, or at least that the cumulative hierarchy picture motivates the axiom scheme. The argument seems to be something like: every set-indexed process can be completed. Given your set  $I$ , for each  $i \in I$  you have to find some thing that is related to it by  $R$ . You are told that you can do this for each  $i \in I$ , so the composite process is a set-indexed composite of things we know we can do.

I am indebted to Robert Black<sup>3</sup> for a heroic attempt at making this plausible:

*“We want to show that the image of a set  $W$  under a functional relation is a set. Since  $W$  exists, there is a stage at which it is formed, and thus a stage at which every  $x$  in  $w$  is already formed. Further, for each of the  $f(x)$  there is a stage at which it is formed. We need a stage at which all such  $f(x)$  have been formed. In other words we must use the existence of  $W$  to argue from the premise that for every  $f(x)$  there is a stage at which it is formed to the stronger conclusion that there is a stage by which every  $f(x)$  is formed. Now imagine going through the sequence of stages necessary for the formation of  $W$ . At each stage, if any members of  $W$  have been formed at that stage, then pause to go through the stages necessary to form the corresponding  $f(x)$  before continuing with the creation of  $W$ . By the time  $W$  itself is formed, all the  $f(x)$  will have been formed, so they can also be collected. In effect we are assuming a strong*

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<sup>2</sup>Here our graphs have undirected edges, no labels on the edges and no multiple edges.  $v'$  is a neighbour of  $v$  iff there is an edge between  $v$  and  $v'$ .

<sup>3</sup>Personal Communication.

*form of Cantor's idea that an infinite process can be regarded as completed. We are assuming that if a process can be completed, and if for every stage of the process there is a further process which can be completed, then the maxi-process consisting of all these further processes can be completed, even if all the processes concerned are infinite."*

A similar argument is to be found on p 239 of Shoenfield [61] and also Shoenfield [60] on page 324 of the Barwise handbook [2]. The assumption underlying this sounds to me rather like replacement under another name ... a bit circular.

I suspect that all these attempts are in fact exercises in bad faith, and that no mathematician has ever been persuaded of the rightness of the axiom scheme of replacement by thinking about the cumulative hierarchy, since the real reason why set theorists adopt the scheme of replacement is that it enables them to do the things they want to do. IBE in fact. "Man is a moralising animal" wrote Philip Toynbee, and for many of us it is not enough merely to have our own way, we feel we have to be *right* as well. Thus they feel that some further justification beyond has to be provided, and one such justification would be a claim that replacement is true in the cumulative hierarchy.

Randall Holmes [32] has considered this question, and argues that the cumulative hierarchy conception cannot be used to underpin the full axiom scheme of replacement, but only those instances that are  $\Sigma^2$ .

## 6.4 Limitation of Size

[ $V$  is a finite object. An integer is an infinite set but a finite object. Do we want to talk about the entropy of a natural?]

The "Limitation of Size" principle says that

**Anything that isn't too big is a set**

LOS-1

This version lends immediate plausibility to the axiom scheme of separation, which says that any subclass of a set is a set. This is really just a footnote for us, since this axiom scheme is the characteristic axiom scheme of the system  $Z$  of *Zermelo set theory*, which lacks the axiom scheme of replacement – from which separation follows easily. We have bigger fish to fry. For our present purposes the significance of LOS-1 – and LOS-2 below – is that they seem to underpin Replacement. All replacement says is that the surjective image of a set is a set, so it appears not to give us big sets from small. It certainly seems to be in the spirit of a principle of limitation of size, and to the extent that limitation-of-size is a Good Idea, Replacement seems to have a Good Argument going for it.

However, we shall see later that replacement does in fact give us existence proofs for large sets (larger than one can prove without it) so replacement flies in the face of LOS by delivering large sets in industrial quantities.

However, I shall be arguing that to the extent that limitation-of-size is a good principle it enjoins us to consider sets not of limited *size* but of limited amount of *information*.

Sometimes LOS can be spotted in the form:

**Anything the same size as a set is a set**

LOS-2

These two versions are not quite the same, as we shall see. They arise from the insight that the dodgy big sets that give rise to the paradoxes all have in common the feature that they are much bigger than anything that arises in ordinary mathematics. Naturally enough one then explores the possibility that it is this difference in size that is the key to the difference between the safe sets of ordinary mathematics and the outsize sets of the paradoxes.

However, LOS is not well-regarded nowadays, and for a number of good reasons, which we will now consider.

Where does LOS come from anyway? When set theory was started a policy was needed about what sort of objects it was supposed to be capturing. Well, it was clearly supposed to capture stuff like  $\mathbb{N}$ ,  $\mathbb{Q}$  and  $\mathbb{R}$ , and it was clearly not supposed to capture nasty things like the Russell Class. OK, so what salient feature distinguishes  $\mathbb{N}$  etc on the one hand from the Russell class on the other? There are probably plenty, but one feature is that it is possible to think that one can in some sense *complete* the construction of  $\mathbb{N}$ ,  $\mathbb{Q}$  and  $\mathbb{R}$  – particularly if one has the cumulative hierarchy in mind – whereas one “keeps on getting” new sets that are not members of themselves, so the Russell class never gets properly launched. This is not a crazy idea, but it takes a lot of ingenuity to take it beyond mere hand-waving. Fortunately there are other salient features that distinguish  $\mathbb{N}$ ,  $\mathbb{Q}$  and  $\mathbb{R}$  from the Russell class, one of them being *size*. And that’s much less hand-wavy.  $\mathbb{N}$ ,  $\mathbb{Q}$  and  $\mathbb{R}$  are much smaller than the Russell class, and that’s why they are more acceptable than it. (They are smaller than the (nonexistent!) Russell class in the sense that we can find sets of non-selfmembered sets (= subsets of the Russell class) as big as  $\mathbb{N}$ , or  $\mathbb{Q}$ ,  $\mathbb{R}$  ... or bigger). Evidently size is, if not a *sufficient* criterion for acceptability, at least a halfway-decent proxy for it.

How good a proxy? To answer this question we need a bit of mathematical slang: *finite object* Blah integers Blah Church.

Church in a underregarded but fertile paper [11] makes the point that the feature common to the paradoxical sets is not outlandish *size*. He emphasises that it is possible to find consistent theories in which the universal set is a set. One should never forget that the nonexistence of the universal set requires set-theoretic axioms (specifically separation) whereas the nonexistence of the Russell class is a theorem of first-order logic – *constructive* first order logic indeed. Church presumably had the set theory NFU in mind (he certainly knew about Jensen’s consistency proof for it when writing [11]) but he helpfully sketches a simple consistency proof for a theory that (unlike NFU) has full extensionality. Church’s Universal Set Theory (CUS) also has (*inter alia*) complementation (every set has a complement), replacement<sup>4</sup> for wellfounded sets (every surjective image of a wellfounded set is a set) and axioms to say that the wellfounded sets are a model of ZF(C). And he sketches how to obtain a model of this theory

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<sup>4</sup>Since it has a universal set it also has full collection.

from a model of  $\text{ZF}(\text{C})$ . Roughly, one exploits the fact that any model of  $\text{ZF}(\text{C})$  admits a definable bijection  $V \longleftrightarrow V \times \{0, 1\}$  to make two copies of every set of the model one starts with. The two copies become a complementary pair in the new dispensation – a set and its complement. For example, the two copies of the empty set become  $\emptyset$  and  $V$ .

Say something about this

Recent work (by Tim Button, [7]) has shown that this construction is in some sense reversible.....

### 6.4.1 LOS and some proofs

If we think about how the paradoxical sets come to be paradoxical (that is to say, we examine the proofs of the paradoxes) we find in every case that – as Church emphasised – the size of the paradoxical set is not a contributory factor. In every case the cause of the trouble turns out to be a logical feature of the definition of the set.

The worry is not just that the set concerned is paradoxical, the problem is that the proof of the contradiction is itself pathological: it is a pathological *proof*. For example, the natural deduction proof of the Russell paradox in naïve set theory has what is known in the trade as a *maximal formula*. This doesn't mean that it isn't a proof: it's a proof all right, but it does have some features that one would rather it didn't have. The significance of this for a discussion about the limitation of size principle is that there are perfectly respectable theorems about uncontroversially ordinary sets whose proofs have pathological features that echo (perhaps a better word is *encyst*) the pathologies in the proof of the paradoxes. For example there is a proof using only *aussonderung* (and this was known already to Zermelo) that for any  $x$  there is a  $y$  not in  $x$ . In particular we can take  $y$  to be  $\{z \in x : z \notin z\}$ . It seems very hard to develop a proof system for set theory in which we can give a proof of  $\{z \in x : z \notin z\} \notin x$  that is not in some sense pathological. Size is clearly not the problem in this case.

Display a proof

### Foundation and Replacement

Unless we assume the axiom of foundation *ab initio* it is perfectly clear that not everything the same size as a well-founded set is well-founded. If  $x = \{x\}$ , this  $x$  is the same size as any other singleton, but it is not well-founded. Thus a whole-hearted embrace of LOS is liable to contradict foundation. The obvious way to deal with this is to modify LOS to:

Any wellfounded collection that is not too big is a set.

### 6.4.2 Replacement not consistent with limitation of size?

Clearly Replacement is – on the face of it – very much in the *spirit* of the limitation of size principle (at least if we read that principle in a particularly literal-minded way): it says only that a surjective image of a set is a set. However, despite this promising start, it turns out that Replacement actually has

consequences that seem to violate the *letter* of LOS, as we shall now illustrate.

Consider the function  $f$  that sends  $n$  to  $\mathcal{P}^n(\mathbb{N})$ . Because of Cantor's theorem duplication? See p 50 (which tells us that  $|X| < |\mathcal{P}(X)|$ ) we know that  $f$  has no largest value. Now consider the image of  $\mathbb{N}$  in  $f$ , namely

$$\{\mathcal{P}^n(\mathbb{N}) : n \in \mathbb{N}\}.$$

Replacement tells us this object will be a set. Therefore its sumset

$$\bigcup \{\mathcal{P}^n(\mathbb{N}) : n \in \mathbb{N}\}$$

will be a set too<sup>5</sup>. The trouble now is that this is a set bigger than any of the  $\mathcal{P}^n(\mathbb{N})$ . Of course this doesn't actually *contradict* LOS but it does sit ill with it and make it *not* look like a sensible fundamental principle. A sensible fundamental principle should, one feels, be formalisable in such a way as to not have consequences that are untrue to its spirit. Of course the problem might be that Limitation Of Size is a sensible fundamental principle but that replacement is not a formalisation of it, but nobody seems to draw this moral. The moral usually drawn is that replacement is a good idea and that limitation of size is not.

### 6.4.3 Limitation of *Size*, or of *information content*?

We need the concept of a *finite object*.

Natural numbers, and Ordinals below  $\epsilon_0$  – or for that matter ordinals below  $\Gamma_0$  – are – in a fairly precise sense – finite objects. The ordinals mentioned (tho' not the natural numbers) are – according to the usual ways of representing these objects in the world of sets – infinite sets. They are (intensionally) finite in the sense that we can set up a system of notation for e.g. natural numbers in such a way that every one is the denotation of a finite string of characters and can be recovered from that string. Ordinals below  $\epsilon_0$  and ordinals below  $\Gamma_0$  similarly. These things are all *finite objects*. Finite objects are no problem; they are easy to reason about. Arbitrary reals in general are not finite objects (tho'  $e$  and  $\pi$  are) and even some functions  $\mathbb{R} \rightarrow \mathbb{R}$  are. It's not entirely crazy to think of the Hilbert Programme as a project to refashion mathematics wherever possible as the study of finite objects (tho' such a formulation would have probably have raised Hilbert's eyebrows).

Armed with this concept we might feel tempted to modify the limitation of size idea to an idea of *limitation of metaphorical size*. The universal set is the complement of the empty set, and that's as good a finite description as you could want. The universal set has huge (literal) size but very small *metaphorical* size. We could modify LOS so that we refrain from postulating the existence of things whose metaphorical size is too great. In this spirit one might embrace the axiom system of Church mentioned above 51 which says that the complement of a set

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<sup>5</sup>It has to be admitted that we here make essential use of the axiom of sumset, but *that* axiom, at least, is not controversial.

is a set. If  $x$  is a finite object then clearly so is its complement. Limitation of size thus reconceived certainly invites us to adopt an axiom of complementation: the complement of a set is a set. This is one of the central axioms of Church. [11]

I don't want to overstate the message of the last paragraph. Limitation Of Size is generally felt to be misguided and, although it can be made to look more sensible by reinterpreting it in the way i have just done, the new axioms the reinterpretation guides us to are not part of our project here.

Fit this material in here somewhere or discard it

We have now reached a point at which we need to invoke the distinction between *large* sets (as in *large* cardinals) and *big* sets which are collections that in most jurisdictions are not sets at all but proper classes. Within the big sets we might want to distinguish a subspecies of **intermediate** classes. Church [11] makes a distinction between **low** sets (a low set is a set the same size as a wellfounded set) and **co-low**<sup>6</sup> All other sets he calls **intermediate**. Our *big* sets include his co-low (high) and intermediate sets. Examples of intermediate classes are things like the Russell class and its congeners<sup>7</sup>, the collection of wellfounded sets, and the collection of all Von Neumann ordinals.

Church points out that none of the known paradoxical sets are co-low: they are all intermediate. This is a point worth emphasising, since there are many people who have not yet taken it, and who believe in particular that the universal set is a paradoxical object. This belief can be shown to be false by the exhibition of any of the consistent set theories with a universal set: NFU, Church's set theory CUS, Generalised positive comprehension are all salutary examples.<sup>8</sup> These theories themselves do not fall within our remit here, but the moral we have just drawn from their consistency most certainly does. Another way of making the same point is to observe that the paradoxical nature of the Russell class can be seen clearly even without adding non-logical axioms to first-order logic: one can prove the nonexistence of the Russell class entirely in first-order logic, without using any set-theoretic axioms at all. There is nothing analogous concerning the universal set.

Church's wider point – that not only is  $V$  not paradoxical but that only intermediate sets can be paradoxical – seems to be true, and he seems to have chosen his definition of **high** with some care. Had he said that a high set is one that is manifestly the same size as the universe, then Randall Holmes' observation that  $x \mapsto \{\emptyset, \{x\}\}$  injects  $V$  into the Russell class would show that the Russell class would be high, rather than intermediate.

(There is yet a third candidate for a characterisation of the intermediate classes: they are those proper classes whose complements are also proper classes. (Notice that the complement of  $V$  is a set; whether or not the complement of the Russell class is a set will depend in part on whether or not the axiom of foundation is true). The assertion that all big classes (the intermediate classes included) are nevertheless the same size as  $V$  has highly nontrivial consequences. Von Neumann took up this assertion as an axiom: see 8.0.2 in chapter 8.)

Church's system is entirely compatible with the idea of limitation of metaphorical size. Every actually small set is metaphorically small, and the complement of a metaphorically small set is small. Sets that are neither small nor cosmall (so metaphorically small) are intermediate and all

<sup>6</sup>Church calls them **high** but this seems to me to be profligate of notation.

<sup>7</sup> $\{x : x \notin^2 x\}$ ,  $\{x : x \notin^3 x\}$  and so on.

<sup>8</sup>Appreciation of the nonparadoxical nature of the Universal set antedates explicit consistency proofs. See, for example, [16] and [17] .



the paradoxical sets seem to be intermediate. Church's set theory doesn't imply the existence of intermediate sets. Everything fits.

There are people who look askance at such theories, since they don't think they are theories of *sets* but of something else, so that what they show the consistency of is not a universal set, but a universal something-else. This is a spat over which community is to own the word 'set', and it is a *turf* war not a dispute over mathematical substance.

## 6.5 Essential Applications of Replacement in Set Theory

There are various things that set-theorists want to do for which replacement really is necessary. If you are not a set-theorist (and not planning to become one) then you might not be convinced that Set Theorists need to be getting up to these things anyway (whatever they are) and that it's no concern of yours even if they should. You might be waiting impatiently for a story about why Replacement is important for people – like you – in *other* areas of mathematics. But Replacement is an axiom (scheme) of Set Theory and this is a book about axioms of set theory so you should give those things a hearing. But your patience will be rewarded, for we will later (sections 6.6 and 6.5.1) be saying something about the implications of Replacement outside Set Theory and why it matters.

Say something about Borel Determinacy

### 6.5.1 Existence of Inductively defined Sets

The standard example of an inductively defined set is  $\mathbb{N}$ , the natural numbers. However, this is such a soothing and familiar object that not all expositors sell it as an inductively defined set: chaps are just supposed to know what it is. When it is presented as an inductively defined set, it is in the following style:

*0 is a natural number*

*The successor of a natural number is a natural number*

*Nothing else is a natural number.*

We think of 0 as the *founder* (in general there may be more than one) and the successor operation ( $x \mapsto x + 1$ ) as a *constructor*. The third clause is in some sense *negative* and we have known since Aristotle that attempts to define things negatively do not end well. However there is a way of capturing this negativity in a legitimate formal way as follows, which says

$n$  is a natural number if  $(\forall y)((0 \in y \wedge (\forall m \in y)(m + 1 \in y)) \rightarrow n \in y)$

or, if we agree not to quibble about set existence principles:

$$\mathbb{N} = \bigcap \{Y : 0 \in Y \wedge S^{\omega} 0 \subseteq Y\}$$

(where  $S$  is the successor operation.)

There is a lot to be said about inductively defined sets ...how an inductively defined family supports an induction principle, and supports definition by recursion, how it is the least fixed point for a monotone operation, and so on. However, *for the moment* the only thing that matters is that an inductively defined set can be thought of in two ways: *from above* and *from below*.

The inductively defined set that best illustrates this feature (because there are fewer peripheral worries to distract us) is the construct that Frege and Russell called the *ancestral* of a relation but which nowadays tends to be called the *transitive closure*. We need the concept of the *n*th power of a relation. Two objects  $x$  and  $y$  in the domain of a relation  $R$  are related by the square  $R^2$  of  $R$  if there is  $z$  such that  $R(x, z) \wedge R(z, y)$ . Higher powers are defined analogously. The union of all these powers of  $R$  (we are thinking of  $R$  as a set or ordered pairs) is the *ancestral*  $R^*$  of  $R$ . Why ‘ancestral’? Well, if  $R$  is parent-of, then  $R^*$  is ancestor-of. Thus the ancestral  $R^*$  of  $R$  is obtained as the union  $\bigcup_{n \in \mathbb{N}} R^n$ . We can think of this as obtaining  $R^*$  “from below” (by *iteration*).

However we can also obtain  $R^*$  “from above” as the intersection of all sets of ordered pairs  $S$  s.t.  $R \subseteq S$  and  $S^2 \subseteq S$ . It is a not-entirely trivial exercise (very good for the souls of first-year students) to prove that these two definitions are equivalent. It may look obvious, but it’s the kind of thing that everyone should have done properly at least once.

say a bit more about this

These two ways of characterising an inductively defined set are always available to us. Propositional language, Borel sets...

Conway’s principle.

An important part of the significance of the axiom scheme of replacement is that it is a kind of omnibus existence theorem for recursive datatypes. (This is emphasised by some writers in a modern computer science tradition – see e.g. [66]). As emphasised above, an inductively defined family can be thought of either “from above” or “from below”. In ZF(C) the “from above” way of thinking is not available, since there is generally no way of showing directly that there is a set that contains the founders and is closed under the constructors (unlike the situation with set theories with a universal set, where there is always such a set, namely  $V$ ). If one is to demonstrate the existence of an inductively defined set it has to be done from below. We obtain the desired object by iteration. We use Hartogs’ lemma to reassure ourselves that there are enough ordinals available to us to ensure that we construct all the approximants we need, we then use replacement to obtain the set of the approximants, then finally sumset to stick them all together.

Here is an illustration of how it works. Let  $X$  be a set and  $f$  a  $k$ -ary operation on sets. We want the closure of  $X$  under  $f$  to be a set. We define a sequence of sets by

$$\begin{aligned} X_0 &= X; \\ X_{n+1} &= X_n \cup f^{\langle k \rangle}(X_n^k). \end{aligned}$$

(That is to say,  $X_n$  contains those things which can be made from things in  $X$  by at most  $n$  applications of  $f$ ).

Then

$$\{X_i : i \in \mathbb{N}\}$$

is a set by replacement, since it is the result of replacing each  $i$  in  $\mathbb{N}$  by  $X_i$ .

Then

$$\bigcup \{X_i : i \in \mathbb{N}\}$$

is a set by the axiom of sumset and it is the closure of  $X$  under  $f$  that we desired.

This reassures us that any collection that is defined as the closure of a set under a finitary operation will be a set. That was a simple case, where the length of the construction is  $\omega$  (because the constructors are finitary). What about closure under infinitary operations? Then of course the length of the construction will be an ordinal  $> \omega$ . There are standard (if perhaps recondite) examples of inductively defined families where the construction is of transfinite length because the operations under which we are closing are not finitary. The best-known example is the collection of Borel subsets of  $\mathbb{R}$ :

- Every open set is Borel;
- A complement of a Borel set is Borel;
- A union of countably many Borel sets is Borel.

(And of course nothing else is a Borel set). Actually with this example there is no problem in proving that the inductively defined collection is a set, since it is a subset of  $\mathcal{P}^2(\mathbb{R})$ . However if we *do* want to construct it from below by iteration there is a question about long we have to keep iterating. As it happens,  $\omega_1$  steps suffice, but that is far from obvious. We won't prove that here, because the current topic is the need for replacement.

One example where we need replacement on wellorderings of uncountable length is the construction of the collection  $H_\kappa$  of sets hereditarily of size less than  $\kappa$ . This set<sup>9</sup> can be obtained “from below” by defining a sequence of approximants indexed by an initial segment of the ordinals as follows. First we define  $\mathcal{P}_\kappa(x)$  to be  $\{y \subseteq x : |y| < \kappa\}$ . Then

$$\begin{aligned} X_0 &=: \emptyset \\ X_\alpha &=: \bigcup_{\beta < \alpha} \mathcal{P}_\kappa(X_\beta) \end{aligned}$$

Armed with replacement we can be confident that, for any ordinal  $\alpha$ , the collection  $\{X_\beta : \beta < \alpha\}$  is a set. Then  $\bigcup \{X_\beta : \beta < \alpha\}$  is a set by the axiom of sumset. Do we ever reach an  $\alpha$  such that  $\bigcup \{X_\beta : \beta < \alpha\} = H_\kappa$ ? Let us consider the simplest nontrivial case, where  $\kappa = \aleph_1$ . (This case is actually typical). We seek  $\alpha$  such that any countable subset of  $\bigcup_{\beta < \alpha} X_\beta$  is already a subset of one of the  $X_\beta$ . Since the cofinality of  $\omega_1$  is  $\omega_1$  then  $\omega_1$  is such an  $\alpha$ .

glossary entry for cofinality

We really do need replacement for this sort of thing:  $H_{\aleph_1}$ , the class of all hereditarily countable sets, cannot be proved to be a set in Zermelo set theory

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<sup>9</sup>There are several things one might mean by “ $x$  is hereditarily of size less than  $\kappa$ ”; here we mean that everything in  $TC(\{x\})$  is of size less than  $\kappa$ .

$Z$ , even though it is only of size  $2^{\aleph_0}$  and  $Z$  proves the existence of much bigger sets than that. See page 84.

## The Argument from Categoricity

Categoricity is an idea from model theory: a theory is *categorical* if it has precisely one model (up to isomorphism). Nowadays people tend to consider only first-order theories in this connection, the model theory of higher-order theories being a conceptual nightmare<sup>10</sup>. There are second-order theories that can be seen to be in some sense categorical. The theory of complete ordered fields, for example, has only one model, namely the reals. The second-order theory of the naturals is categorical (see below). This example is more important for us than the example of the reals, for it is a special case of a general phenomenon. A **recursive datatype** is a family of sets built up from some founder objects by means of the application of constructors. The natural numbers is the simplest example of such a family, being built up from the single founder object 0 by means of the single unary operation  $S$  (successor, addition of 1). The intuition to which all the second-order-talk appeals is this. Suppose I construct a recursive datatype (as it might be  $\mathbb{N}$ ) starting with my founder objects and applying the constructors (which are after all entirely deterministic). Suppose when I have finished I wipe my blackboard clean and go away and have a cup of tea. Then, when I come back and repeat the performance, *I must obtain the same result as I did the first time*. Indeed, to sharpen the point, let us consider the *gedankenexperiment* of you and I simultaneously constructing this datatype (whichever it was) at two separate blackboards<sup>11</sup>. The moves available to you are the same as the moves available to me, so as we proceed with our two constructions we build an isomorphism between them.<sup>12</sup>

This is why people think that the second order theory of arithmetic is categorical. However our concern here is with the recursive datatype of wellfounded sets in the cumulative hierarchy. The second-order theory of this structure is going to be categorical for the same reasons as before.

That is to say, if we think of the axioms of ZF as second-order and include the axiom of foundation (so that we know that what we are trying to axiomatise is the cumulative hierarchy) we should find that the theory generated by the axioms is second-order categorical, and describes precisely the cumulative hierarchy of wellfounded sets.

<sup>10</sup>Open Question number 24 (the last in the list) at the end of Appendix B of [10] (p 514) is “Develop the model theory of second- and higher-order logic”!

<sup>11</sup>No timelike line between the two blackboards of course.

<sup>12</sup>Second-order arithmetic has an obvious model, the *standard* model that axiomatic arithmetic was intended, all along, to describe. Let us call this model  $\mathfrak{M}$ . Second-order arithmetic includes as one of its axioms the following:

$$(\forall F)(F(0) \wedge (\forall n)(F(n) \rightarrow F(n+1)) \rightarrow (\forall n)(F(n)))$$

This axiom enables us to prove that every natural number is standard – simply take ‘ $F(n)$ ’ to be ‘ $n$  is a standard natural number’ – which is to say “ $x$  is in  $\mathfrak{M}$ ”. Therefore  $\mathfrak{M}$  is the only model of second-order arithmetic.

It's not *quite* as straightforward as that, since there are various axioms of infinity that tell us how long the construction of the cumulative hierarchy has to be pursued, so the idea is that a model of second-order ZF is uniquely determined by values of two parameters:

- (i) the number of urelements and
- (ii) the height of the model.

(This is in Zermelo [74].)

So whenever  $\mathfrak{M}$  and  $\mathfrak{M}'$  are two models that agree on these two parameters there is an isomorphism between them. The obvious way to construct this bijection is by transfinite recursion.

Suppose we have two models  $\mathfrak{M}$  and  $\mathfrak{N}$  of second-order ZF. The idea is to use replacement to define an isomorphism between them by recursion on the membership relation of one of them. Either will do. (They're isomorphic, after all). Let  $i$  be the isomorphism we are building:  $i : \mathfrak{M} \rightarrow \mathfrak{N}$ . Suppose we have defined  $i(y)$  for every  $y \in x$ . What is  $i(x)$  going to be? The theory in which we are discussing this construction enjoys replacement – by assumption. So it knows that  $i''x$  exists. This  $i''x$  is a subset – indeed a *bounded* subset – of (the carrier set of)  $\mathfrak{N}$ . But  $\mathfrak{N}$  has second-order separation, so every bounded subset of a member is a member. So  $i''x \in \mathfrak{N}$  and is the desired destination for  $i$  applied to  $x$ .

The point is: **You need replacement to construct the bijection.**

This is really just another illustration of the way in which replacement is the omnibus existence axiom for recursively defined sets. Except here the thing we are defining (the bijection) is a proper class, and replacement is being used to prove that all its initial segments are sets.

[*HOLE The point rather is that we need replacement to show that the recursive datatype declarations are unique definitions. If the retype is going to be a set can we usually get away without replacement, using nothing more than separation, but if we want to prove uniqueness for retypes that are proper classes we will need replacement.*]

Collate everything to do with categoricity

Gödel contrasts  $L$  with  $V$  (which is the real universe) and by then it's clear that  $V$  is *WF*.  $L$  is just a model: no suggestion that  $V = L$  is *true*.

Aki sez: Cumulative hierarchy in Mirimanoff. Von Neumann in the late 1920's and Zermelo [?] were interested in 2nd order categoricity.

## Existence of Transitive Closures

Inconveniently, the expression 'transitive closure' has two meanings, and we must not confuse them. (See the glossary p. 105). On the one hand the **transitive closure** of a *relation*  $R$  is the least transitive relation extending  $R$ . Since the transitive closure of the *parent* relation is the *ancestor* relation Russell and Whitehead called this operation the **ancestral** and some writers (Quine, for example) perpetuated this usage. Despite the catchy mnemonic (and

We have already used this idea in the section on  $\Delta_0$  formulae

blend this with the material on p 56.

pædagogically useful) character of this terminology it has almost completely passed out of use and everybody nowadays writes of *transitive closures* instead.

On the other hand the transitive closure  $TC(x)$  of a set  $x$  is the set of all those things that are members of  $x$ , or members of members of  $x$ , or members of members of members of  $x$  and so on. The two uses of ‘transitive closure’ are neatly tied together by a *bon mot* of Allen Hazen: ‘*is the transitive closure of*’ *is the transitive closure of* ‘*is the transitive closure of*’.

Since in order to give you a set I have to give you all its members (and all their members and so on) the transitive closure of  $x$  contains all those things that I have to give you when giving you  $x$ . It’s very tempting to think that this means that  $TC(x)$  contains all the sets that are **ontologically prior** to  $x$ , particularly if – like most set theorists – you think that the cumulative hierarchy exhausts the universe of sets. In the cumulative hierarchy setting it is clear that  $\in$  (or rather its transitive closure – in the *other* (ancestral) sense!) is the relation of ontological priority between sets. Clearly, for any object  $x$  whatever, the collection of things on-which- $x$ -relies-for-its-existence is a natural collection to consider (Zermelo would surely have considered it *definite*) so it is not at all unreasonable to desire an axiom that tells us that it is always a set.  $TC(x)$  is certainly a set for some  $x$ . If there is to be a special underclass of sets  $x$  for which  $TC(x)$  does not exist it would be nice to have an explanation of who its members are and why. Deciding that  $TC(x)$  is always a set spares us the need to dream up such an explanation.

That is a rather philosophical reason for being attracted to the axiom that  $TC(x)$  is always a set. There are also technical reasons which we have no need to go into here, beyond saying that if one wishes to infer the axiom scheme of  $\in$ -induction from an axiom of foundation (in any of its forms) one will need the existence of transitive closures. I refer the reader to [18] for the details, since this text is about the *axioms* of set theory, not the *theorems*.

The significance of this for us is that the axiom scheme of replacement gives us an easy proof of the existence of  $TC(x)$  for all  $x$ . The existence of  $TC(x)$  for all  $x$  is an assumption much weaker than full replacement and it is sometimes adopted as an axiom in settings where people don’t want to make assumptions as strong as full replacement.

Let’s deduce the existence of transitive closures from replacement. Let  $x$  be any set, and consider the recursively defined function  $f$  that sends 0 to  $x$ , and sends  $n + 1$  to  $\bigcup(f(n))$ . This is defined on everything in  $\mathbb{N}$ . By replacement its range is a set. We then use the axiom of sumset to get the sumset of the range, which is of course  $TC(x)$ .

Finally the set picture view of sets compels us to take seriously the idea of the transitive closure of a set: for any set  $x$  the APG picture of  $x$  has a vertex for every element of  $TC(x)$ . Further, the APG of the transitive closure of  $x$  can be obtained by a fairly trivial modification of the APG of  $x$ . Again, the edge set for the APG of the transitive closure of  $x$  is the (graph of) the transitive closure of the edge relation of the APG of  $x$ .

No, that’s not quite right. . .

we need the existence of  $TC(x)$  to deduce  $\in$ -induction from the axiom of

regularity.

### 6.5.2 Existence of sets of size $\beth_\omega$ and beyond

Between Zermelo's axiomatisation and the advent of the axiom scheme of replacement there was a minor irritant in the form of a question about the existence of sets of size  $\beth_\omega$  and beyond. Russell and Whitehead ([56] volume III p 173) certainly had the concept of sets of that size, and it is clear that they understood that their system provided no method evident to them of proving the existence of such sets. They wrote:

“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.”

The same problem occurs in connection with Zermelo set theory: altho' people of those times did not have independence methods available in the way we do now, they did have a pretty shrewd idea that Zermelo's axioms did not prove the existence of such sets. Was this a Good Thing or a Bad Thing? Skolem ([63] p 297) was of the view that sets of this kind should be accommodated, and used the fact that replacement proved their existence as an argument for adopting it. Cantor ([8] page 495) claims that there are sets of size  $\aleph_\omega$  but gives no explanation for this claim. We will take this up in section 6.5.3.

### 6.5.3 Facts about $V_{\omega+\omega}$

It is becoming ever clearer with the passing of the years that Skolem was right: the large sets like  $V_{\omega+\omega}$  did indeed have a shining future awaiting them in Mathematics, so the fact that we seem to need the axiom scheme of replacement if we are to prove their existence is an IBE point in favour of the scheme.

How did we discover that Skolem was right? Well, it is an obscure consequence of the second incompleteness theorem of Gödel that we keep getting new theorems of arithmetic as we move higher up the cumulative hierarchy. There are theorems about  $\mathbb{N}$  which can be proved only by reasoning about sets of naturals; there are theorems about  $\mathbb{N}$  which can be proved only by reasoning about sets of sets of natural numbers; indeed there are theorems about  $\mathbb{N}$  which can be proved only by reasoning about sets of transfinite rank (sets that are beyond being sets<sup>n</sup>-of-natural-numbers for any finite  $n$ ). Although Gödel's theorem predicts the eventual appearance of such theorems, it doesn't supply any natural examples, and none turned up until the 1970's. Now we know lots. Let us remember that the sets in  $V_{\omega+\omega}$  are accepted by all parties to the debate. They coincide roughly with the naturals and the reals and the sets<sup>n</sup> of sets of reals. If we are to wholeheartedly accept sets in  $V_{\omega+\omega}$  then we will have to be

similarly welcoming to any sets about which we have to reason if we are to prove facts about sets in  $V_{\omega+\omega}$ . The chain of reasoning now is:

- (i) We accept the sets in  $V_{\omega+\omega}$ ;
- (ii) There are facts about these sets that can be proved only by reasoning about sets of higher rank;
- (iii) We need replacement to prove the existence of these sets of higher rank.

Item (iii) is standard (see section 7.1); (i) is agreed. So a natural example of facts about sets in  $V_{\omega+\omega}$  that can be proved only by reasoning about large sets beyond  $V_{\omega+\omega}$  would be a clincher. Probably the best-known natural example (and one of the earliest) is Borel Determinacy. In recent years Harvey Friedman has produced many more.<sup>13</sup>

There are people who deny the significance of these theorems, but it is hard to find good grounds for doing so. The need for sets of rank  $> \omega + \omega$  in the proof of things like Borel Determinacy is presumably not in dispute. The only option left is to deny that Borel determinacy (and the Friedmanesque combinatorics) belong to ordinary mathematics, and we dealt with that argument above (p 48).

### 6.5.4 Gödel's Argument

In the naïve, hand-wavy picture of the genesis of the cumulative hierarchy as in chapter 1 one tends to describe the  $V_\alpha$ s as being defined by a recursion over the ordinals. One doesn't enquire too closely where the ordinals came from, and – as I argued in section 3.1 – one shouldn't so enquire: after all, ordinals are numbers not sets, so a creation-myth for sets is not to be accused of inconsistency or absurdity merely on the grounds that it presupposes ordinals.

There is another reason for not worrying about ordinals here: any sequence that can be constructed by recursion over one wellordered sequence can equally be constructed by recursion over any other. Once we take seriously the idea that the cumulative hierarchy is constructed by recursion, and that wellorderings and ordered pairs *etc.* can be implemented in set theory – and therefore within the cumulative hierarchy – one then notices that one can describe the construction of the cumulative hierarchy within itself.

Let us take a specific example.  $V_{\omega+\omega}$  is an initial segment of the cumulative hierarchy. It contains, for example, a wellordering of length  $\omega + \omega + \omega$ . This sounds as if we ought to be able to describe inside  $V_{\omega+\omega}$  the construction of  $V_\alpha$ s with  $\alpha < \omega + \omega + \omega$ . Of course we can't, because  $V_{\omega+\omega+3}$  cannot be a member of  $V_{\omega+\omega}$  (wellfoundedness of the cumulative hierarchy forbids it). This means that, were we to perform the thought-experiment of pretending that  $V_{\omega+\omega}$  were the whole universe, we would find that the universe contains wellorderings of lengths  $\alpha$  (e.g.,  $\omega + \omega + \omega$ ) but does not contain the  $V_\alpha$ s that we should be able to construct by recursion over those wellorderings. If set theory is to

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<sup>13</sup>A good place to start looking is the foundations of mathematics mailing list run by Martin Davis.



be a satisfactory foundation for all our mathematical activity, then we ought to be able to describe within it the mathematical activity of constructing the cumulative hierarchy. That is, whenever we find a wellordering of length  $\alpha$ , then we want to be able to construct all  $V_\beta$  with  $\beta < \alpha$ . The argument that the universe should be closed under  $\alpha \mapsto V_\alpha$  is in [74].

Clearly what we are demanding here is that if the universe contains a wellordering  $\langle X, <_X \rangle$  then it also contain the image of  $X$  in the function that sends the minimal element of  $X$  to  $\emptyset$ , sends the  $<_X$ -successor of  $x$  to  $\mathcal{P}(Y)$  whenever it sends  $x$  to  $Y$  and is  $\subseteq$ -continuous at limit points. And this of course is an instance of the axiom scheme of replacement.

### Set Pictures again

The same point can be made by reference to set pictures. For example, at stage  $\omega + 1$  we can produce an APG which is a picture of the Von Neumann ordinal  $\omega + \omega$ . This of course does not come into existence until level  $\omega + \omega$ . Recall now the two conceptions APG 1 and APG 2 from page 27.

Every set picture is a picture of a set; (APG 1)

Every wellfounded set picture is a picture of a wellfounded set. (APG 2)

The first of these is contentious; after all, not everybody believes Forti-Honsell antifoundation. In contrast APG 2 is much more widely accepted. What axiom does it give rise to? If every (wellfounded) set picture is to correspond to an actual set we need something like Mostowski's collapse lemma (section 6.5.8) to prove it, and for that we will need the axiom scheme of replacement.

### 6.5.5 The Normal Form Theorem for Restricted Quantifiers

Another reason for adopting replacement (Forster [18]) is that it enables us to prove a normal form theorem for restricted quantification. This is actually an argument for the axiom scheme of *collection* but – as we saw at the start of this chapter – in the presence of the axiom of foundation the two are equivalent.

In [18] we encountered restricted quantifiers in set theory (see pages 127, 169, 184-5, 188) and we saw a hierarchy of classes of formulæ, which we will now review.

A  $\Delta_0$ -formula in the language of set theory is a formula built up from atomics by means of boolean connectives and restricted quantifiers.

A restricted quantifier in the language of set theory is  $'(\forall x)(x \in y \rightarrow \dots)'$  or  $'(\exists x)(x \in y \wedge \dots)'$ .

Thereafter a  $\Sigma_{n+1}$  (respectively  $\Pi_{n+1}$ ) formula is the result of binding variables in a  $\Pi_n$  (respectively  $\Sigma_n$ ) formula with existential (respectively universal) quantifiers.

We immediately extend the  $\Sigma_n$  and  $\Pi_n$  classes by closing them under interdeducibility-in-a-theory- $T$ , and signal this by having ‘ $T$ ’ as a superscript, so our classes are  $\Sigma_n^T$  and  $\Pi_n^T$ .

This linear hierarchy of complexity for formulæ will be very useful to us in understanding  $T$  as long as we can be sure that every formula belongs to one of these classes<sup>14</sup>: it is standard that we can give a  $\Pi^{n+1}$  truth-definition for  $\Sigma_n$  formulæ. That is to say, we desire a *normal form theorem* for  $T$ .

It is easy to check that if  $T$  is not ludicrously weak we can show that both  $\Pi_n^T$  and  $\Sigma_n^T$  are closed under conjunction and disjunction. To complete the proof of the normal form theorem we would need to show that these classes are closed under restricted quantification. After all, if  $\phi$  is a  $\Pi_n^T$  formula what kind of a formula is  $(\exists x \in y)\phi$ ? Life would be very simple if it, too, were  $\Pi_n^T$ . It’s plausible that it should be  $\Pi_n^T$  (it has the same number of blocks of unrestricted quantifiers after all) but that is not at all obvious. Nevertheless there are sound philosophical reasons why we might expect it to be – at least if  $V = WF$ . The point is that  $WF$  is a recursive datatype, and recursive datatypes always have a sensible notion of restricted quantifier, and typically one can prove results of this kind for the notion of restricted quantifier that is in play. Any recursive datatype has what one might call an *engendering* relation between its members: it is the relation that holds between a member  $x$  of the datatype and the members of the datatype that went into the making of  $x$ . (For example, with the recursive datatype  $\mathbb{N}$  the appropriate notion of restricted quantifier is  $(\forall x < n)(\dots)$ .) In general, when dealing with a recursive datatype, we can define  $\Delta_0$  formulæ – as above – as those with no unrestricted quantifiers, where we take restricted quantifiers to be ‘ $(\exists x)(R(x, y) \wedge \dots)$ ’ and ‘ $(\forall x)(R(x, y) \rightarrow \dots)$ ’, and  $R$  is the engendering relation. We find that  $\Delta_0$  formulæ behave in many ways as if they contained no quantifiers at all – as follows. An unrestricted quantifier is an injunction to scour the whole universe in a search for a witness or a counterexample; a restricted quantifier invites us only to scour that part of the universe that lies in some sense “inside” something already given. The search is therefore “local” and should behave quite differently: that is to say, restricted universal quantification ought to behave like a finite conjunction and ought to distribute over disjunction in the approved de Morgan way. (And restricted existential quantification too, of course).

One effect of this is that  $\Delta_0$  predicates are **absolute** between transitive models. This merits a short discussion. If  $\phi(x)$  is a formula with one free variable and no quantifiers, and  $\mathfrak{M}$  believes there is an  $x$  such that  $\phi(x)$ , then any  $\mathfrak{M}' \supseteq \mathfrak{M}$  will believe the same. This much is obvious. The dual of this is similarly obvious: If  $\phi(x)$  is a formula with one free variable and no quantifiers, and  $\mathfrak{M}$  believes that  $\phi(x)$  holds for every  $x$ , then any  $\mathfrak{M}' \subseteq \mathfrak{M}$  will believe the same. We say that existential formulæ **generalise upwards** and universal formulæ **generalise downwards**. Something analogous holds for  $\Sigma_1$  formulæ and  $\Pi_1$  formulæ. They generalise upwards and downwards in the same way as long as  $\mathfrak{M}$  and  $\mathfrak{M}'$  are both transitive models.  $\Delta_0$  formulæ of course generalise

End-extension?

<sup>14</sup>well, *lots* of these classes: after all if  $\phi$  is in  $\Sigma_n^T$  it is also in  $\Pi_{n+1}^T$ .

both upward and downward and are therefore – as we say – **absolute**.

The study of the various naturally occurring recursive datatypes of interest have evolved in their own ways, and sometimes the binary relation in the restricted quantifier isn't *literally* the engendering relation. It is in the case of arithmetic of  $\mathbb{N}$  – the quantifiers are  $(\forall n < m)$  and  $(\exists m < n)$  – but not in set theory where the relation is membership rather than the transitive closure  $\in^*$  of membership, but the effect is the same.

There is a **hierarchy theorem** about this collection, and it has several parts. One part claims that every formula belongs to one of the classes  $\Sigma_n$  and  $\Pi_n$ , and the second part claims that the classes are all distinct. The second part is in severe danger if  $V \neq WF$ : when there is a universal set, any formula  $\psi$  is equivalent to both  $(\exists x)(\forall y)(y \in x \wedge \psi^x)$  and to  $\forall x \exists y(y \notin x \vee \psi^x)$ . This is a crude fact, but the question of whether or not  $V = WF$  has some subtle implications for the first part too.

What we will now see is that, if we have the axiom scheme of collection, then we can prove an analogue of the prenex normal form theorem:

**THEOREM 2.** *Given a theory  $T$ , which proves collection, for every expression  $\phi$  of the language of set theory there is an expression  $\phi'$  s.t.  $T \vdash \phi \longleftrightarrow \phi'$  and every restricted quantifier and every atomic formula occurs within the scope of all the unrestricted quantifiers.*

*Proof:*

It is simple to check that  $(\forall x)(\forall y \in z)\phi$  is the same as  $(\forall y \in z)(\forall x)\phi$  (and similarly  $\exists$ ), so the only hard work involved in the proof is in showing that

$$(\forall y \in z)(\exists x)\phi$$

is equivalent to something that has its existential quantifier out at the front. (This case is known in logicians' slang as “quantifier pushing”.) By collection we now infer

$$(\exists X)(\forall y \in z)(\exists x \in X)\phi,$$

and the implication in the other direction is immediate.

This shows that  $\Sigma_n$  is closed under restricted universal quantification. Dually we infer that  $\Pi_n$  is closed under restricted existential quantification. It is of course immediate that  $\Sigma_n$  is closed under restricted existential quantification and that  $\Pi_n$  is closed under restricted universal quantification.

Now have the analogue of the prenex normal form theorem we can complete the proof that every formula belongs to one of the classes  $\Pi_n^T$  or  $\Sigma_n^T$ .

So one argument for replacement is that it enables us to prove the Prenex Normal Form theorem for the theory of well-founded sets (with restricted quantifiers) which ought to be provable, and which we do not seem to be able to prove otherwise.

### 6.5.6 Reflection

If  $\Phi$  is an expression and  $\mathfrak{M}$  a structure (with domain  $M$ ), and  $\mathcal{I}$  is a map from the predicate and function letters of the language of  $\Phi$  that sends an  $n$ -place predicate to a subset of  $M^n$  (and function letters similarly) then  $\Phi^{\mathfrak{M}}$  (the *interpretation of  $\Phi$  in  $\mathfrak{M}$* ) is the formula we get from  $\Phi$  by applying the following rules recursively to  $\Phi$ :

if  $\psi$  is an atomic formula  $R(x_1 \dots x_n)$  then  $\psi^{\mathfrak{M}}$  is  $\psi$ ;

$(\psi \wedge \theta)^{\mathfrak{M}}$  is  $(\psi^{\mathfrak{M}}) \wedge (\theta^{\mathfrak{M}})$ ; ( $\rightarrow$ ,  $\vee$  similarly);

$(\exists x \psi)^{\mathfrak{M}}$  is  $\exists x (x \in M \wedge (\psi^{\mathfrak{M}}))$ ;

$(\forall x \psi)^{\mathfrak{M}}$  is  $\forall x (x \in M \rightarrow (\psi^{\mathfrak{M}}))$ .

Subject to some small print (concerning cases where the language of  $\mathfrak{M}$  is not the same as the language of which  $\Phi$  is part)  $\Phi^{\mathfrak{M}}$  is supposed to be the same as  $\mathfrak{M} \models \Phi$ . If  $\Phi^{\mathfrak{M}}$  is true, we say that  $\mathfrak{M}$  is a *model of  $\Phi$* .

If  $\phi \longleftrightarrow (\phi^{V_\gamma})$ , we say  $\gamma$  **reflects**  $\phi$ .

Unless  $\phi$  is  $\Delta_0$ , there is no reason to expect that there are any  $\gamma$  that reflect  $\phi$ . The **reflection principle** says that there is nevertheless always such a  $\gamma$ . In fact one can prove the following.

**THEOREM 3.** *For every  $\phi$ , ZF proves*

$$\phi \longleftrightarrow (\exists \text{ a closed unbounded class of } \alpha) \phi^{V_\alpha}.$$

Proof. See Lévy [38], [39].

The principle of reflection tells us that if the universe satisfies  $(\forall x)(\exists y)\phi$  (so that the universe is, so to speak, closed under  $\phi$ ) then there is a  $V_\alpha$  that is closed under  $\phi$ . Roughly this tells us that (modulo a certain amount of small print) the closure of any set under any suite of operations is a set. Reflection is an omnibus existence theorem for recursive datatypes. See [66].

**COROLLARY 1.** *ZF is not finitely axiomatisable.*

*Proof:*

If ZF were finitely axiomatisable, then by reflection there would be an ordinal  $\alpha$  such that  $\langle V_\alpha, \in \rangle$  were a model of ZF. This  $V_\alpha$  is a set. This is important because, once we have a Gödel numbering of formulæ, the assertion that every formula in some semidecidable set  $\Sigma$  of formulæ is true in  $\langle V_\omega, \in \rangle$  is an expression in the language of set theory, and we can set about proving that all logical consequences of  $\phi$  are also true in  $\langle V_\alpha, \in \rangle$ . We do this by structural induction on proofs. Then we will have established that the set of logical consequences of  $\Sigma$  has a model and is free of contradiction. We know because of Gödel's incompleteness theorem that no recursively axiomatisable theory extending PA can prove its own consistency, so no initial segment  $\langle V_\alpha, \in \rangle$  can provably be a model of ZF. Reflection tells us that if ZF were finitely axiomatisable, we would be able to find such an initial segment. So ZF is not finitely axiomatisable. ■

$\Delta_0$  not defined yet ...

In fact, we can show something slightly stronger than corollary 1:  $ZF$  proves the consistency of any of its finitely axiomatisable subsystems. If  $\phi$  is the conjunction of all the axioms of a finite fragment of  $ZF$ , we have  $ZF \vdash \phi$ , so for some  $\beta$ ,  $V_\beta \models \phi$ .

Perhaps we can omit this

Indeed this even shows (Montague) that no consistent extension of  $ZF$  can be finitely axiomatisable.

### 6.5.7 Versions of the Axiom of Infinity

There are various expressions that can serve the rôle of an axiom of infinity. Here are three that we can usefully consider:

Perhaps this should be moved: it duplicates material around p. ??

- (i) There is a Dedekind-infinite set;
- (ii)  $V_\omega$  exists;
- (iii)  $(\exists x)(\emptyset \in x \wedge (\forall y \in x)(y \cup \{y\} \in x))$ .

The first two formulæ are perfectly intelligible, given the discussion around page 35. It is the third that needs some explanation. It says that there is a set that contains the empty set and is closed under the operation  $y \mapsto y \cup \{y\}$ . It's pretty clear that any  $x$  satisfying (iii) will be Dedekind-infinite, but why all the extra information?

The significance of the extra information is that, of the two clauses of (iii), the first is related to the fact that, in the Von Neumann implementation of  $\mathbb{N}$ , 0 is implemented as the empty set; the second is related to the fact that the function concerned –  $y \mapsto y \cup \{y\}$  – is the successor function on natural numbers in the Von Neumann implementation. What (iii) is trying to tell us is that there is a set that contains (among possibly other things) all Von Neumann naturals. The set of Von Neumann naturals itself is the  $\subseteq$ -least set witnessing (iii), and will be a set if we have separation.

Are (i)–(iii) all equivalent? Not unless one has replacement! If one has the axiom scheme of separation then as long as  $V_\omega$  exists one can obtain from it the set of all Von Neumann naturals. (Every Von Neumann natural is a member of  $V_\omega$ ). So (ii)  $\rightarrow$  (iii). Evidently (iii)  $\rightarrow$  (i) since the Von Neumann  $\mathbb{N}$  is manifestly Dedekind-infinite. It's the direction (i)  $\rightarrow$  (ii) that is problematic.

What can we do? It is standard that if there is a Dedekind-infinite set  $X$  then the quotient of  $\mathcal{P}(X)$  under equinumerosity contains (an implementation of)  $\mathbb{N}$ . This is because every Dedekind-infinite set has subsets of all inductively finite sizes. We take our implementation of  $\mathbb{N}$  to be the intersection of all infinite initial segments of the quotient of  $\mathcal{P}(X)$  under equinumerosity. How is one to obtain  $V_\omega$  or the Von Neumann  $\mathbb{N}$  from this? The obvious way to obtain  $V_\omega$  is to take the sumset of the collection  $\{V_n : n \in \mathbb{N}\}$  which of course one obtains by replacement in a way familiar from section 6.5.1. Interestingly it turns out that this use of replacement is necessary: there are models of Zermelo set theory in which (iii) is true but (ii) is not. See Mathias [44]. (Also Boffa [5]; and [15] p 178; and [68] p. 296.)

Thus by adopting the axiom scheme of replacement we erase all need for concern about which form of the axiom of infinity we should adopt. Finally –

Actually there is multifurcation in the absence of AC, and we should say something about that too

in situations where extreme rigour is called for – there is the consideration that (iii) cannot even be *stated* unless one has already established the existence and uniqueness of the empty set, since (iii) contains a defined term that denotes it. This will matter if one wishes to claim that the axiom of empty set follows from the axiom of infinity.

### 6.5.8 Mostowski

One of the purposes served by Set theory, as we have noted, is a framework within which one can do Mathematics. In particular one wants sets that will be simulacra of cardinals and ordinals. The industry standard nowadays for these are von Neumann ordinals for ordinals, and initial von Neumann ordinals for cardinals. Von Neumann ordinals serve as ordinals, and initial ordinals will serve as cardinals as long as one has the axiom of choice,

What do we mean by “von Neumann ordinals serve as ordinals”? We mean that every wellordering is isomorphic to (the membership relation on) a von Neumann ordinal.

It’s widely understood that this cannot be proved in mere Zermelo set theory, where there is no axiom scheme of replacement:  $V_{\omega+\omega}$  is a model of Zermelo set theory containing wellorderings not isomorphic to any von Neumann ordinal in  $V_{\omega+\omega}$ . Conversely it is standard that if we have the axiom scheme of replacement we can prove the lemma of Mostowski that tells us that every wellfounded extensional structure is isomorphic to the membership relation on a transitive set. In other words: every wellfounded extensional structure has an  $\in$ -copy.

That sounds recondite, but it matters. If we are to use the Von Neumann implementation of ordinals – which everyone in fact does, despite the availability in ZFC of Scott’s trick – then we need to know that the function that sends wellorderings to their ordinals is well-defined and total. This requires us to prove that every wellordering is isomorphic to a Von Neumann ordinal. We cannot prove this without at least some use of replacement.

In ZF we can also use Scott’s trick ordinals should we so wish. If we cannot use von Neumann ordinals in Zermelo set theory, can we at least use Scott’s trick ordinals? Annoyingly, it turns out that we cannot do so without additional assumptions: there are models of Zermelo that lack Scott’s trick ordinals. This is one of the reasons why Zermelo set theory is unsatisfactory. A much more widely-used system – by those who (perhaps because of reservations about replacement) want something weaker than ZF – is the system KP of Kripke-Platek, which has replacement for some  $\Pi_1$  formulæ only. KP is strong enough to prove Mostowski’s lemma.

So can we in fact implement cardinals and ordinals inside Zermelo set theory *tout court*? Yes, but an elementary document such as this is not the correct forum to demonstrate such an implementation. And in any case such a demonstration – being part of a case that replacement is not necessary – should be made by the replacement-deniers themselves.

Suppose  $\langle X, <_X \rangle$  is a wellordering. We want to show that it is isomorphic to  $\langle \alpha, \in \rangle$  for some von Neumann ordinal  $\alpha$ . This  $\alpha$  will be the range of the

recursively defined function  $\pi$  where  $\pi(x) =: \pi\{\{x' : x' <_X x\}\}$ . So “ $y = \pi(x)$ ” will be

$$(\forall \sigma)((\langle \min(X), 0 \rangle \in \sigma) \wedge (\forall x \in X)(\sigma \upharpoonright \{x' : x' <_X x\} \text{ is total}) \\ \rightarrow \langle x, \sigma\{\{x' : x' <_X x\}\} \rangle \in \sigma) \rightarrow \langle x, y \rangle \in \sigma).$$

Indeed Suppose  $\langle X, R \rangle$  is a wellfounded binary structure. We want show that there is a homomorphism  $\pi$  to  $\langle Y, \in \rangle$ . The homomorphism  $\pi$  will satisfy  $x R x' \rightarrow \pi(x) \in \pi(x')$ . Clearly we want  $\pi(x) =: \{\pi(x') : x' R x\}$ . So “ $y = \pi(x)$ ” will be

$$(\forall \sigma)(\text{ if } \sigma \text{ contains } \langle x, \emptyset \rangle \text{ for all } R\text{-minimal members } x \text{ of } X \text{ and} \\ \text{ if, whenever } \sigma \text{ restricted to } \{x' : x' R x\} \text{ is total, } \sigma \text{ also contains} \\ \langle x, \{\sigma(x') : x' R x\} \rangle, \text{ then } \langle x, y \rangle \text{ is in } \sigma).$$

This isn't quite right

$$(\exists \sigma)(\sigma \text{ contains } \langle x, \emptyset \rangle \text{ for all } R\text{-minimal members } x \text{ of } X; \text{ and if,} \\ \text{ whenever } \sigma \text{ restricted to } \{x' : x' R x\} \text{ is total and } \sigma \text{ is defined at } x, \\ \text{ then } \sigma(x) =: \{\sigma(x') : x' R x\}, \text{ and } \langle x, y \rangle \text{ is in } \sigma).$$

## 6.6 Implementation-invariance

Let's get some definitions out of the way.

**DEFINITION 1.** *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  $(j(f))(x) = f\text{“}x$ .  
 And  $f\text{“}x$  is of course  $\{f(y) : y \in x\}$

From NF studies we have the concept of a *setlike* function.<sup>15</sup>

**DEFINITION 2.** *A (unary) function  $f$  is*

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

*An  $n$ -ary function  $f$  is 1-setlike if  $f\text{“}(X_1 \times \dots \times X_n)$  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 the composition of two  $n$ -setlike relations is  $n$ -setlike.

Perhaps readers will forgive me for starting with a self-quotation (from [22]).

Perhaps a title here?

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<sup>15</sup>The idea (tho' not the terminology) goes back to [14]. 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.

“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. If we wish to tell a story in which everything is a set then we have to have ways of implementing these mathematical objects from outside set theory as *sets*. 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.

The importance of this distinction is surely one of the morals one can draw from [4]. Benacerraf’s point about the Zermelo naturals and the von Neumann naturals is that they can’t both be a correct account of what natural numbers are; what he doesn’t say – but what thoughtful readers can work out for themselves – is that they *can* nevertheless *both* be acceptable *implementations* of natural numbers. There is a story one could tell about the emergence of the concept of *implementation* (nowadays folklore among computer scientists), about its roots in mathematics, and about how it can feed back into Philosophy of Mathematics and into mathematical praxis; it should be told.”

Do a string search for other occurrences of ‘implementation’.

Once one has appreciated that what one is doing is *implementation* rather than *definition* one is well-placed to appreciate the importance of what one might call *implementation-insensitivity* or perhaps *implementation-invariance*. Evidently there are many ways of implementing real numbers as sets, but this fact is of no interest to people who study real arithmetic, and they don’t want to have to think about it. Why not? No mathematician supposes that the Riemann hypothesis might turn out to be true if we think of reals as Dedekind cuts but false if we think of them as equivalence classes of Cauchy sequences.

Possible duplication of cauchy; see pp 72 and 71.

This is a possibility that most mathematicians would never even consider, it’s so obviously absurd. But the Blindingly Obvious can sometimes have consequences that one can overlook. This insensitivity to the difference between Dedekind cuts and Cauchy Reals has huge consequences for set theory: if set theory is to capture mathematics in the way that its advocates want, it will have to reproduce this feature of invariance-under-implementation that we wish to take for granted. Suppose I prove (in my system  $T$  of set theory, whatever it is) a theorem about [an implementation of] some suite of mathematical entities, as it might be the reals – the Riemann Hypothesis, say. How do i know that what I have proved is a theorem about the reals and not merely a theorem about some special sets that happen to implement reals in the mock-up of the real line that I am tinkering with in my shed??<sup>16</sup> Well, I’d be in real trouble if  $T$  proved, for some *other* implementation of  $\mathbb{R}$ , that those other “reals” did *not* obey the Riemann Hypothesis. That had better not happen: we’d better not be able to prove that the reals as implemented by the Pink Real Company<sup>©</sup> and the reals

Duplication in these two paragraphs

<sup>16</sup>Frege apparently worried about this



as implemented by the **Blue Real Company**<sup>©</sup> are non-isomorphic. The best way to do that is to adopt axioms that ensure that [in all situations like this] they *are* isomorphic. This isomorphism that bijects the set of **Pink Real**<sup>©</sup>s with the set of **Blue Real**<sup>©</sup>s is going to have to be something that is visible to the set theory as a genuine object, in other words, a set: **implementation-insensitivity mandates a set existence principle!** And that set-existence principle turns out to be replacement.

[Here is a challenge. The Dedekind reals are of course Dedekind cuts in  $\mathbb{Q}$ , the *Cauchy* reals are equivalence classes of Cauchy sequences in  $\mathbb{Q}$ . We want them to be complete (“every bounded sequence has a least upper bound”) It is obvious that the Dedekind reals are complete. Prove that the Cauchy reals are complete – *and* without using AC, if you please. If it needed AC that would be a very powerful inference-to-the-best-explanation argument for AC.] This is treated in [22] but the cutest illustration of the rôle played by replacement here is to be found in an *aperçu* of Mathias’ in connection with cartesian products.

Possible duplication of  
cauchy; see pp 72 and 70.

A *pairing* function is a dyadic function **pair** equipped with two *unpairing* functions **fst** and **snd** such that

$$\begin{aligned}\mathbf{pair}(x, y) &= \mathbf{pair}(x', y') \rightarrow x = x' \wedge y = y'; \\ \mathbf{fst}(\mathbf{pair}(x, y)) &= x; \text{ and} \\ \mathbf{snd}(\mathbf{pair}(x, y)) &= y.\end{aligned}$$

Clearly we need pairing and unpairing functions if we are to code relations and functions as sets, since their graphs are sets of ordered pairs: the binary relation  $R$  will be coded as  $\{\mathbf{pair}(x, y) : R(x, y)\}$  and functions similarly. Equally clearly there is no *prima facie* reason for preferring one kit of pairing-with-unpairing functions to any other. There may conceivably be technical difficulties if the pairing or unpairing functions are sufficiently perverse and the set theory we are using is sufficiently weak but there are no *mathematical* reasons to prefer any one suite of pairing-and-unpairing functions to any other. How could there be?

One thing in particular that we are certainly going to want is that, whatever pairing-and-unpairing kit we choose,  $X \times Y$  should be a set, for all  $X$  and  $Y$ . If we use Wiener-Kuratowski ordered pairs, then it is possible to show – using only the axioms of power set, pairing and separation – that  $X \times Y$  does indeed exist for all  $X$  and  $Y$ . However this demonstration relies on particular features of the Wiener-Kuratowski ordered pair and does not work in general. If we want a proof that doesn’t depend on any particular features of the pairing-and-unpairing kit we use but is completely general then we have to use replacement. To obtain  $X \times Y$ , procede as follows. For each  $y \in Y$  consider the function  $I_y : x \mapsto \langle x, y \rangle$ . By replacement the set  $I_y “X$  is a set<sup>17</sup> for each  $y \in Y$ . So consider the function  $I_X : y \mapsto I_y “X$ .  $\bigcup(I_X “Y)$  is now  $X \times Y$ .

Interestingly (and this is Mathias’ point) we really do need replacement for this: replacement follows from the assumption that  $x \times y$  exists for all  $x$  and  $y$  and every implementation of ordered pair. Here is his proof:

Let  $F$  be any function class and consider the pairing function

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<sup>17</sup>  $f “x$  is  $\{f(y) : y \in x\}$ . We introduce this notation on p 46.

$$x, y \mapsto \langle F(x), \langle x, y \rangle \rangle$$

where the angle brackets denote (say) the Wiener-Kuratowski ordered pair. This is clearly an ordered pair function.

Then if  $Y = X \times \{\emptyset\}$  exists for this new kind of ordered pair we can recover  $F^{\text{“}}X$ , since it is the set of things that are the first component of a Wiener-Kuratowski ordered pair in  $Y$ , and that set-of-first-components can be defined using only separation and no replacement.

This seems to be treated earlier...

This is a very arresting wee result, and is educationally valuable; however not everybody is convinced. Randall Holmes says<sup>18</sup> that the specification for the abstract data type of ordered pairing, if understood correctly, already requires that cartesian products always exist. So all that Mathias has done is to point out that if you don't write out the spec for pairing-and-unpairing properly then – in order to show that every implementation that obeys that (improper, incomplete) spec also behaves properly – you will need replacement. Well yes; so what!<sup>19</sup> Holmes may be right, but some writers assume that the existence of cartesian products follows without any need to assume replacement. In Godement [30] there is an axiomatisation of set theory which does not include the axiom scheme of replacement. However the author insists nevertheless that a choice of pairing function does not matter, and that the only thing that matters is that one should be able to form pairs *ad lib* and recover the components. Either way Mathias' illustration is still a good way to get people to think about these matters.

Possible duplication of cauchy; see pp 71 and 70.

I now have to come clean and admit that i have slightly exaggerated the claim for the sake of emphasis. We can in fact prove in Zermelo set theory (i.e., *without* using replacement) that the Dedekind-cut reals are isomorphic to the equivalence classes of Cauchy-sequences reals. The proof works because both these collections are sets. (The existence of cartesian product for all pairing-unpairing functions implies replacement, but then  $V \times V$  is a proper class.) If we are to insist on implementation-insensitivity for all implementations of absolutely everything then we obtain replacement as a corollary, and we will prove this.

#### REMARK 1.

*Suppose that whenever  $b$  and  $p$  are two classifiers for an equivalence relation  $\sim$  on a proper 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  $X \twoheadrightarrow A$ . Call it  $p$ . Then let  $b$  be  $h \cdot p$ . Then  $\pi$  is just  $h$ , and

<sup>18</sup>Personal communication

<sup>19</sup>But this is simply to say that the pairing and unpairing functions must be 1-setlike. (See definition 2.

In fact, if Holmes' analysis is the right way to go, then one probably wants pairing/unpairing functions to be fully setlike.

is 1-setlike by the assumption. So  $h$  (which was arbitrary) is 1-setlike. This is replacement. ■

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 of Typed Set Theory, Natural model? TST?

$$X, \mathcal{P}(X), \mathcal{P}^2(X) \dots \mathcal{P}^n(X)$$

and

$$f^{\ast}X, \mathcal{P}(f^{\ast}X), \mathcal{P}^2(f^{\ast}X) \dots \mathcal{P}^n(f^{\ast}X)$$

are isomorphic.

This last observation 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

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  $f^{\ast}x$  is a set if  $x$  is;  $x \times f^{\ast}x$  is a set since our pairing function is setlike, and  $f \upharpoonright x$  is now a subclass of  $x \times f^{\ast}x$ , and it will be a set because we have  $\Delta_0$  separation. ■

They don't know what Mac Lane set theory is

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.

**REMARK 4.** (*Mac Lane Set Theory*) (*Coret [14]*)

*Every 1-setlike function is setlike.*

*Proof:*

We first prove that if  $f$  is 1-setlike then  $j(f)$  is 1-setlike.

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.

If  $f$  is 1-setlike then, by the preceding paragraph,  $j(f)$  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 [or 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 4.** (*Zermelo, KF?*)

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

*Proof:*

First we find the bijection between  $b^{\text{“}}X$  and  $p^{\text{“}}X$ . To what element of  $p^{\text{“}}X$  should we send  $b(x) \in b^{\text{“}}X$ ? 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  $p^{\text{“}}X \rightarrow b^{\text{“}}X$ . 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 remarks 3 and 4 it will suffice to show that  $\pi$  is 1-setlike. Suppose  $P \subset p^{\text{“}}X$ ; we want  $\pi^{\text{“}}P$  to be a set. Now  $\pi^{\text{“}}P$  is a subcollection of the set  $b^{\text{“}}X$ , so we can aspire to use separation to prove it a set. And indeed we can: it is  $\{y \in b^{\text{“}}X : (\exists x \in P)(y = b(x))\}$ . ■

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  $p^{\text{“}}X$  and  $b^{\text{“}}X$  are both sets?

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

So if  $b^{\text{“}}X$  and  $p^{\text{“}}X$  are proper classes there is some work to do. Is it sufficient that  $b$  and  $p$  be setlike? Consider the relation  $(\exists X')(P' = p^{\text{“}}X \wedge B' = b^{\text{“}}X)$  (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  $\mathcal{P}(p^{\text{“}}X)$  and  $\mathcal{P}(b^{\text{“}}X)$ ? Given an arbitrary  $P' \subseteq p^{\text{“}}X$  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  $f^{\text{“}}a = b$  ... and this is precisely collection!

Thus everything is all right if we have replacement: replacement is sufficient. In fact we can prove that replacement is not only sufficient but is *necessary*.

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.,  $\text{On}$ ) 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.

Get the primes in the correct place

(\*)

[We'd better prove this! Hmm...we seem to need an extra assumption that there is no unbounded set of ordinals!!

Let  $\alpha_p$  be a **pink** ordinal. There is a wellordering  $\mathcal{A} = \langle A, <_A \rangle$  whose **pink** ordinal is  $\alpha_p$ . The collection of all initial segments of  $\mathcal{A}$  is a set by power set and separation, and the set of **pink** ordinals of members of this set is a set by setlikeness of  $p$ . We would now like the function  $\alpha_p \mapsto$  ordinals below  $\alpha_p$  to be not only total but setlike. Why should it be?]

Fix a wellordering  $\mathcal{A} = \langle A, <_A \rangle$  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.

Sort this out and remove message-to-self

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.

Explain monomorphic

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 formulæ are invariant. So we should be aiming to prove that stratified replacement suffices to prove invariance of the strongly typed formulæ but that full replacement is required to prove implementation-insensitivity of the less strongly typed formulæ.

'invariant'?

There is the "obvious" proof that the strongly typed sentences are invariant, namely by appeal to the fact that the occurrences of the  $b$ s and  $p$ s 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

(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!

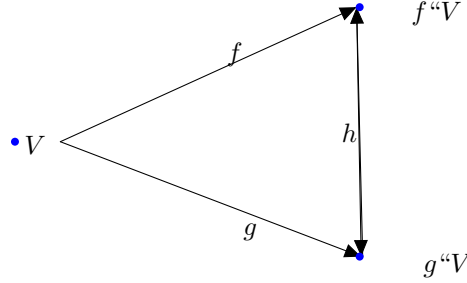
What set-theoretic axioms do we need, and what conditions on classifiers, to ensure that Rosser’s Axiom of Counting holds?

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  $\bigcup_{i < \omega} H_{\beth_i}$ .



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  $(\forall xy)(x \sim y \longleftrightarrow g(x) = g(y))$ . Suppose further that  $f$  and  $g$  are 1-setlike. We seek a function  $h$  making the triangle 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)  $NC$  (the collection of all cardinals) 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.

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 1.** (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  $f \restriction x' \subseteq f$  so  $f \restriction x'$  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  $Tn =_{df} |\{m : m < n\}|$ .

## Envoi

I think we should close these discussions with a reflection that puts it all in perspective. Whatever the philosophical points that philosophers of Mathematics make about the axiom scheme of replacement, it remains the case that for the mathematician who actually studies the cumulative hierarchy, the question of whether or not the axiom scheme of replacement is true in the cumulative hierarchy has long since got lost in the dust visible in the rear-view mirror: for better or worse, the debate is over.

Do we need to set this up in lights?

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

Do we really need this next lemma?



## Chapter 7

# Independence Proofs

The independence of the various axioms of set theory from their comrades is a matter of rather more moment than one might expect. Typically, in the construction of the model that demonstrates the independence of a particular axiom, one exploits that very axiom. Thus in the very act of demonstrating the independence of an assertion one provides an IBE argument that one should adopt that very assertion as an axiom! Also the ease with which one can find set models of – for example – ZF-minus-power-set is an argument for the power set axiom. “After all,” one can say to oneself, “if it weren’t true, one would be able to pretend that everything was hereditarily countable, and that is clearly not true”.

The various systems of axiomatic set theory available to us nowadays have evolved in accordance with a principle one might call *Graceful downward compatibility*. Each axiomatic set theory is geared to a particular aspect of Mathematics, and one axiomatises it in terms of the principles it is trying to capture, rather than in terms of the incremental differences between it and the others. Naturally this non-incremental way of devising axiomatic systems makes for a great deal of redundancy. For example we retain the axiom of pairing as one of the axioms of ZF (even though it follows from replacement and power set) because we want to be able to say that Zermelo set theory is ZF minus replacement. By the time one reaches strong set theories one has accumulated in this way quite a stock of what one might call *legacy* axioms.

However, although it is clear that some instances of the axiom schemes of separation and replacement can be derived from others, it is standard that the remaining axioms of ZF are independent from each other. For all other axioms  $A$  we can show that  $A$  cannot be deduced from  $ZF$ -minus- $A$ . And for the scheme of replacement we can show that  $ZF$ -minus-replacement does not imply all instances of replacement, though it does prove some.

An independence proof is of course just a kind of consistency proof:  $A$  is independent of  $T$  if  $T + \neg A$  is consistent<sup>1</sup>. Our consistency proofs below will

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<sup>1</sup>Some writers prefer to say that  $A$  is independent of  $T$  only if  $T + \neg A$  and  $T + A$  are both

be of two kinds. The first is generally called a *relative* consistency proof. If  $T$  is consistent then so too is  $T + \neg A$ . (“ $T + \neg A$  is consistent *relative to*  $T$ ”.) Typically in these cases – and certainly in all the cases below – the inference from the consistency of  $T$  to the consistency of  $T + \neg A$  is proved in a very very weak system indeed.

One hesitates to call the other *absolute* but one has to call it something to contrast it with ‘relative’. Suppose one wishes to prove the independence of axiom  $A$  from a theory  $T$ . If  $T + A$  proves the consistency of  $T$  outright then we know that  $T$  cannot prove  $A$ , for then  $T + A$  would prove its own consistency, contradicting Gödel’s incompleteness theorem.

### Hereditarily this and that

A device that turns up in many of these independence proofs is the idea of the set of things that are hereditarily  $\phi$ , where  $\phi$  is a one-place predicate. The intuition is that  $x$  is hereditarily  $\phi$  if everything in  $TC(x)$  is  $\phi$ . (The reader may be familiar with this adverb ‘hereditarily’ from Topology: a space is *hereditarily Lindelöf* [for example] iff all its subspaces are Lindelöf. This is not the same usage!)

Annoyingly there are *three* ways of defining  $H_\phi$ , the class of things that are hereditarily  $\phi$ , and it is easy for the beginner to become confused. I am going to start with my favourite definition:

**DEFINITION 3.**

$$\begin{aligned} \mathcal{P}_\kappa(x) &:= \{y \subseteq x : |y| < \kappa\}; & H_\kappa &:= \bigcap \{y : \mathcal{P}_\kappa(y) \subseteq y\}; \\ \mathcal{P}_\phi(x) &:= \{y \subseteq x : \phi(y)\}; & H_\phi &:= \bigcap \{y : \mathcal{P}_\phi(y) \subseteq y\}. \end{aligned}$$

In this I am following the notation of Boffa [5].

The first thing to notice with this definition is that everything inside  $H_\phi$  under this definition will be wellfounded. This is because  $H_\phi$  is a recursive datatype and comes equipped with a principle of induction. We can use this induction to argue that every set in  $H_\phi$  is wellfounded. Another way of seeing it is by noting that the  $H$ s are least fixed points.

A word is in order on the definition and the notation involved. The two uses of the set-forming brackets in the *definiens* of ‘ $H_\kappa$ ’ and ‘ $H_\phi$ ’ are naughty: in general there is no reason to suppose that the collection of all  $y$  such that  $\mathcal{P}_\phi(y) \subseteq y$  is a set. If there is even one  $x$  such that  $\mathcal{P}_\phi(x) \subseteq x$ , then  $\{y \subseteq x : \mathcal{P}_\phi(y) \subseteq y\}$  will have the same intersection as  $\{y : \mathcal{P}_\phi(y) \subseteq y\}$ , and so no harm is done. But this relies on there being such an  $x$ .

One could write:

$$H_\kappa := \{x : (\forall y)(\mathcal{P}_\kappa(y) \subseteq y) \rightarrow x \in y\}$$

and

$$H_\phi := \{x : (\forall y)(\mathcal{P}_\phi(y) \subseteq y) \rightarrow x \in y\}$$

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consistent.

obscure example

Tidy this up

Of course  $H_\phi$  genuinely might not be a set, in which case we shouldn't be trying to prove that it is. For example  $H_{x=x}$  is just  $WF$  (or  $V$  if you prefer): the universe of wellfounded sets. In those circumstances one cannot define  $H_\phi$  as the intersection of all sets  $x$  such that  $\mathcal{P}_\phi(x) \subseteq x$ , since there are no such sets; the intersection of the empty set is  $V$ , and that isn't what we want. In those circumstances one wants the second definition, to which we now turn.

The second way of defining  $H_\phi$  is as the collection of those  $x$  such that  $\phi(y)$  for all  $y \in TC(x)$ . It would be nice if this were to give the same result as the first definition in cases where both deliver a set not a proper class, but this is not reliably true. Quine atoms are hereditarily finite under the second definition, even though their failure of wellfoundedness prevents them from being hereditarily finite under the first definition. However it is fairly straightforward to check that if one is assuming the axiom of foundation then the two definitions are equivalent. Since – most of the time – we will be working with the axiom of foundation, the difference between these two definitions is not significant.

There is another tradition that regards the set of things that are hereditarily  $\phi$  as the set of things  $x$  s.t.  $TC(x)$  is  $\phi$ . This is a bad notation for various reasons. For one thing it makes sense only when  $\phi$  is a property which is preserved under subsets (like being smaller than  $\kappa$ ) and it prevents us from making sense of expressions like “The collection of hereditarily transitive sets”. For another, even in cases where it does make sense, it can result in subtle confusions. Let us consider two cases – both of them sets we will need later in our independence proofs – the first of which is unproblematic and the second not. (And we will assume foundation to keep things simple.)

If we consider ‘ $H_{\aleph_1}$ ’ – the notation for the set of hereditarily countable sets – we get the same collection under both readings (as long as we assume the axiom of countable choice). If  $TC(x)$  is countable then clearly all its subsets are, and so all its members (which are all subsets) will be countable too. (We need a union of countably many countable sets to be countable to secure the converse).

However if we consider ‘ $H_{\beth_\omega}$ ’ then we find that  $\{V_{\omega+n} : n \in \mathbb{N}\}$  belongs to the denotation of this expression under one reading but not under the other. Every set in the transitive closure of  $\{V_{\omega+n} : n \in \mathbb{N}\}$  is of size less than  $\beth_\omega$ , so  $\{V_{\omega+n} : n \in \mathbb{N}\}$  belongs to  $H_{\beth_\omega}$  according to our definition. However  $TC(\{V_{\omega+n} : n \in \mathbb{N}\})$  is not of size less than  $\beth_\omega$ ; it is in fact of size *precisely*  $\beth_\omega$  and therefore  $\{V_{\omega+n} : n \in \mathbb{N}\}$  does not belong to  $H_{\beth_\omega}$  according to the other definition.

The moral is, when reading an article that exploits sets that are hereditarily something-or-other, look very carefully at the definition being used.

Probably worth noting that even when  $H_\phi$  is a set we tend to need replacement to prove it's existence. For example in Zermelo, where we do not have replacement, we cannot prove the existence of  $H_{\aleph_1}$ .

## 7.1 Replacement

$V_{\omega+\omega}$  is a model for all the axioms except replacement. It contains well-orderings of length  $\omega$  but cannot contain  $\{V_{\omega+n} : n \in \mathbb{N}\}$  because we can use the axiom of sumset (and  $V_{\omega+\omega}$  is clearly a model for the axiom of sumset!) to obtain  $V_{\omega+\omega}$  from  $\{V_{\omega+n} : n \in \mathbb{N}\}$ . Therefore it refutes that instance of the axiom scheme of replacement that says that the image of  $\mathbb{N}$  in  $n \mapsto V_{\omega+n}$  is a set<sup>2</sup>

Readers are encouraged to check the details for themselves to gain familiarity with the techniques involved.

## 7.2 Power set

$H_{\aleph_1}$  is a model of all the axioms of ZFC except power set.

The obvious way of proving that  $H_{\aleph_1}$  is a set is to use transfinite iteration of the function  $x \mapsto \mathcal{P}_{\aleph_1}(x)$ , taking unions at limits, so that (as on page 57) we define:

$$\begin{aligned} X_0 &=: \emptyset \\ X_\alpha &=: \bigcup_{\beta < \alpha} \mathcal{P}_{\aleph_1}(X_\beta) \end{aligned}$$

This function –  $x \mapsto \mathcal{P}_{\aleph_1}(x)$  – is not  $\omega$ -continuous, since new countable subsets might appear at  $\omega$ -limits:  $X_\omega$  could have a countable subset that is not included in any  $X_n$  with  $n$  finite. This means we will have to iterate the construction of the  $X_\beta$  up to a stage  $\alpha$  such that any countable subset that is present at stage  $\alpha$  was created at some earlier stage. By use of countable choice we can show that the first such  $\alpha$  is  $\omega_1$ . So we iterate  $\omega_1$  times and then use replacement to conclude that  $X_{\omega_1}$  is a set.

$H_{\aleph_1}$  gives us a model of ZF minus the power set axiom. The axiom of infinity will hold because there are genuinely infinite sets in  $H_{\aleph_1}$ . This is not sufficient by itself since “is infinite” is not  $\Delta_0$ , but whenever  $X$  is such a set there will be a bijection from  $X$  onto a proper subset of itself, and this bijection (at least if our ordered pairs are Wiener-Kuratowski) will be a hereditarily countable set. So any actually infinite member of  $H_{\aleph_1}$  will be believed by  $H_{\aleph_1}$  to be actually infinite. We have been assuming the axiom of choice, so the union of countable many elements of  $H_{\aleph_1}$  is also an element of  $H_{\aleph_1}$ , so it is a model of the axiom of sumset.

Everything in  $H_{\aleph_1}$  is countable and therefore well-ordered, and, under most implementations of pairing functions – in particular the Wiener-Kuratowski pairing function which is the one most commonly used – the well-orderings will be in  $H_{\aleph_1}$ , too, so  $H_{\aleph_1}$  is a model of AC, even if AC was not true in the model in which we start.

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<sup>2</sup>If you are worried about how to represent the function  $n \mapsto V_{\omega+n}$  in the language of set theory you are right to worry. It is not straightforward, and you should seek advice; it is too technical for here. Or is it...? Ask the publisher's reader!

This last paragraph might arouse in the breasts of suspicious readers memories of section ?? where much is made of the different available implementations. AC follows here not from an implementation of ordered pairs as Wiener-Kuratowski but from the mere *possibility* of implementing ordered pairs as Wiener-Kuratowski.

what is this reference?

### 7.3 Infinity

$H_{\aleph_0}$  provides a model for all the axioms of ZF except infinity and thereby proves the independence of the axiom of infinity.

The status of AC in  $H_{\aleph_0}$  is like its status in  $H_{\aleph_1}$ . Everything in  $H_{\aleph_0}$  is finite and therefore well-ordered, and under most implementations of pairing functions the well-orderings will be in  $H_{\aleph_0}$  too, so  $H_{\aleph_0}$  is a model of AC, even if AC was not true in the model in which we start. This is in contrast to the situation obtaining with the countermodels to sumset and foundation: the truth-value of AC in those models is the same as its truth-value in the model in which we start.

### 7.4 Sumset

Recall the definition of beth numbers from chapter 1. Recall from section 6.5.1 how we can use replacement to prove the existence of inductively defined sets such as  $H_{\beth_\omega}$ . Then  $H_{\beth_\omega}$  proves the independence of the axiom of sumset.

We should check quickly that it verifies the other axioms. It's not hard to check infinity, pairing and power set. A surjective image of a set of size strictly less than  $\beth_\omega$  is also of size strictly less than  $\beth_\omega$ . This ensures that  $H_{\beth_\omega}$  is a model of replacement. Next we notice that there are well-orderings of length  $\omega + \omega$  inside  $H_{\beth_\omega}$ , and that every  $V_{\omega+n}$  is in  $H_{\beth_\omega}$ . Therefore by replacement  $\{V_\alpha : \alpha < \omega + \omega\}$  is a set. Indeed it is hereditarily of size less than  $\beth_\omega$ . However, its sumset  $\bigcup \{V_\alpha : \alpha < \omega + \omega\}$  is  $V_{\omega+\omega}$  which is of course of size  $\beth_\omega$  and is not in  $H_{\beth_\omega}$ .

### 7.5 Foundation

For the independence of the axiom of foundation and the axiom of choice we need **Rieger-Bernays models**.

If  $\langle V, R \rangle$  is a structure for the language of set theory, and  $\pi$  is any permutation of  $V$ , then we say  $x R_\pi y$  iff  $x R \pi(y)$ .  $\langle V, R_\pi \rangle$  is a *permutation model* of  $\langle V, R \rangle$ . We call it  $V^\pi$ . Alternatively, we could define  $\Phi^\pi$  as the result of replacing every atomic wff  $x \in y$  in  $\Phi$  by  $x \in \pi(y)$ . We do not rewrite equations in this operation:  $=$  is a logical constant, not a predicate letter. The result of our definitions is that  $\langle V, R \rangle \models \Phi^\pi$  iff  $\langle V, R_\pi \rangle \models \Phi$ . Although it is possible to give a more general treatment, we will keep things simple by using only permutations whose graphs are sets.

It turns out that if  $\Phi$  is a stratifiable formula then  $\langle V, R \rangle \models \Phi$  iff  $\langle V, R_\pi \rangle \models \Phi$ . Not all the axioms are stratifiable, but it is quite easy to verify the unstratifiable instances of replacement, and the first version of the axiom of infinity on page 67 is stratifiable. Foundation fortunately is not stratifiable, and therefore might not be conserved. The  $\pi$  we need to demonstrate its independence is the transposition  $(\emptyset, \{\emptyset\})$ . In  $\mathfrak{M}^\pi$  the old empty set has become a Quine atom: an object identical to its own singleton:  $x \in_\pi \emptyset \iff x \in \pi(\emptyset) = \{\emptyset\}$ . So  $x \in_\pi \emptyset \iff x = \emptyset$ . So  $\mathfrak{M}^\pi$  is a model for all the axioms of ZF except foundation.

### 7.5.1 Antifoundation

There is another way of proving the independence of the axiom of foundation and that is to prove the consistency of an axiom of *antifoundation*. To this end let us return to the ideas of section 3.1.1. If we work in ZF with foundation then we can use Scott's trick to implement abstract APGs. There is a binary relation between these abstract APGs which corresponds to the membership relation between the sets corresponding to the APGs. We now have a model of ZF + Antifoundation: the elements of the model are the abstract APGs given us by Scott's trick, and the membership relation is the binary relation just alluded to.

The best-known exposition of this material is the eminently readable Aczel [1]. I shall not treat it further here, since—although attractive—it is recondite, and the proof of independence of foundation that it gives does not naturally give rise to a proof of the independence of the axiom of choice. This is in contrast to the previous independence proof for foundation, which will naturally give rise to the proof of the independence of choice which we will see in section 7.7.

In connection with representation theorems make the point that Forti-Honsell's antifoundation says that every set picture is a picture of an actual set.

rewrite, and allude to the section early where set pictures are introduced

## 7.6 Extensionality

First, some slang. If  $T$  is a name for a system of axiomatic set theory (with extensionality of course), then  $TU$  is the name for the result of weakening extensionality to the assertion that *nonempty* sets with the same elements are identical. 'U' is for 'Urelemente' – German for 'atoms' (see p. 24).<sup>3</sup>

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

<sup>3</sup>A point-scoring opportunity here for syntax buffs: the letter ' $T$ ' is of course not being used as a name for a theory but as a letter ranging over such names . . .



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

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

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

What is true in  $\langle V, \in_f \rangle$ ? Try pairing, for example: what is the pair of  $x$  and  $y$  in the sense of  $\in_f$ ? A moment's reflection shows that it must be  $f\{x, y\}$ : if you are a member of  $f\{x, y\}$  in the sense of  $\in_f$  then you are a member of  $f^{-1} \cdot f\{x, y\}$ , so you are obviously  $x$  or  $y$ . The other sporadic axioms yield individually to hand-calculations of this kind. Replacement yields to an analysis like that on page 86.

### 7.6.1 More about Extensionality

In the light of this result and the discussion on page 24 the reader might reasonably suppose that atoms are the sort of things one can take or leave: it shouldn't make any difference whether we allow them or not. We have just proved the independence of extensionality from the other axioms, and we can prove its consistency too: just consider the class of sets that are hereditarily atom-free.

The relative strength of extensionality and its negation is quite sensitive to other considerations too. Does the language contain an abstraction operator? See Scott [58] where he shows that a version of ZF without extensionality can be interpreted in Zermelo set theory! See also [28].

Mention Benedikt's synonymy result here

Amplify this. Synonymy of single quine atom and countably many empty atoms?

## 7.7 Choice

Proving the independence of the axiom of choice from ZF is hard work, and was finally cracked by Cohen in 1964 ([?]) with the advent of *forcing*. Forcing is too demanding for a text like this, but there are other ideas that go into the independence proof, and some of them can be profitably covered here.

One useful thought is that the axiom of choice says that the universe contains some highly asymmetrical objects. After all, any wellordering is rigid. If we can arrange matters so that everything in the universe has some symmetries then we will break AC.

We start with a model of ZF with urelemente. In the original treatment these urelemente are taken to be empty. For technical reasons it's easier to take them to be Quine atoms. The effect is that one drops foundation rather than extensionality, but the two constructions have the same *feel*.

We start with a model of ZF + foundation, and use Rieger-Bernays model methods to obtain a permutation model with a countable set  $A$  of Quine atoms. The permutation we use to achieve this is the product of all transpositions

$(n, \{n\})$  for  $n \in \mathbb{N}^+$ .  $A$  will be a **basis** for the illfounded sets in the sense that any class  $X$  lacking an  $\in$ -minimal element contains a member of  $A$ . Since the elements of  $A$  are Quine atoms every permutation of  $A$  is an  $\in$ -automorphism of  $A$ , and since they form a basis we can extend any permutation  $\sigma$  of  $A$  to a unique  $\in$ -automorphism of  $V$  in the obvious way: declare  $\sigma(x) := \sigma``x$ . Notice that the collection of sets that this definition does not reach has no  $\in$ -minimal member if nonempty, and so it must contain a Quine atom. But  $\sigma$  by hypothesis is defined on Quine atoms.  $(a, b)$  is of course the transposition swapping  $a$  and  $b$ , and we will write  $\tau_{(a,b)}$  also for the unique automorphism to which the transposition  $(a, b)$  extends. Every set  $x$  gives rise to an equivalence relation on atoms. Say  $a \sim_x b$  if  $(a, b)$  fixes  $x$ . We say  $x$  is of (or has) **finite support** if  $\sim_x$  has a cofinite equivalence class. (At most one equivalence class can be cofinite)

The union of the (finitely many) remaining (finite) equivalence classes is the **support** of  $x$ . Does that mean that  $x$  is of finite support iff the transitive closure  $TC(x)$  contains finitely many atoms? Well, if  $TC(x)$  contains only finitely many atoms then  $x$  is of finite support ( $x$  clearly can't tell apart the cofinitely many atoms not in  $TC(x)$ ) but the converse is not true:  $x$  can be of finite support if  $TC(x)$  contains cofinitely many atoms. (Though that isn't a sufficient condition for  $x$  to be of finite support!!)<sup>4</sup>

It would be nice if the class of sets of finite support gave us a model of something sensible, but extensionality fails: if  $X$  is of finite support then  $\mathcal{P}(X)$  and the set  $\{Y \subseteq X : Y \text{ is of finite support}\}$  are both of finite support and have the same members with finite support. We have to consider the class of elements hereditarily of finite support. Let's call it  $HF$ . This time we *do* get a model of ZF.

**LEMMA 2.** *The class of sets of finite support is closed under all the definable operations that the universe is closed under.*

*Proof:*

When  $x$  is of finite support let us write ' $A(x)$ ' for the cofinite equivalence class of atoms under  $\sim_x$ . For any two atoms  $a$  and  $b$  the transposition  $(a, b)$  induces an  $\in$ -automorphism which for the moment we will write  $\tau_{(a,b)}$ .

Now suppose that  $x_1 \dots x_n$  are all of finite support, and that  $f$  is a definable function of  $n$  arguments.  $x_1 \dots x_n$  are of finite support, and any intersection of finitely many cofinite sets is cofinite, so the intersection  $A(x_1) \cap \dots A(x_n)$  is cofinite. For any  $a, b$  we have

$$\tau_{(a,b)}(f(x_1 \dots x_n)) = f(\tau_{(a,b)}(x_1) \dots \tau_{(a,b)}(x_n))$$

since  $\tau_{(a,b)}$  is an automorphism. In particular, if  $a, b \in A(x_1) \cap \dots A(x_n)$  we know in addition that  $\tau_{(a,b)}$  fixes all the  $x_1 \dots x_n$  so

$$\tau_{(a,b)}(f(x_1 \dots x_n)) = f(x_1 \dots x_n).$$

---

<sup>4</sup>A counterexample: wellorder cofinitely many atoms. The graph of the wellorder has cofinitely many atoms in its transitive closure, but they are all inequivalent.

So the equivalence relation  $\sim_{f(x_1 \dots x_n)}$  induced on atoms by  $f(x_1 \dots x_n)$  has an equivalence class which is a superset of the intersection  $A(x_1) \cap \dots \cap A(x_n)$ , which is cofinite, so  $f(x_1 \dots x_n)$  is of finite support. ■

This takes care of the axioms of empty set, pairing, sumset and power set. To verify the axiom scheme of replacement we have to check that the image of a set hereditarily of finite support in a definable function (with parameters among the sets hereditarily of finite support and all its internal variables restricted to sets hereditarily of finite support) is hereditarily of finite support too. The operation of translating a set under a definable function (with parameters among the sets hereditarily of finite support and all its internal variables restricted to sets hereditarily of finite support) is definable and will (by lemma 2) take sets of finite support to sets of finite support.

So if  $X$  is in  $HF$  and  $f$  is a definable operation as above,  $f''X$  is of finite support. And since we are interpreting this in  $HF$ , all members of  $f''X$  are in  $HF$ , so  $f''X$  is in  $HF$  too, as desired.

To verify the axiom of infinity we reason as follows. Every wellfounded set  $x$  is fixed under all automorphisms, and is therefore of finite support. Since all members of  $x$  are wellfounded they will all be of finite support as well, so  $x$  is hereditarily of finite support. So  $HF$  will contain all wellfounded sets that were present in the model we started with. In particular it will contain the von Neumann  $\omega$ .

It remains only to show that AC fails in  $HF$ . Consider the set of (unordered) pairs of atoms. This set is in  $HF$ . However no selection function for it can be. Suppose  $f$  is a selection function. It picks  $a$  (say) from  $\{a, b\}$ . Then  $f$  is not fixed by  $\tau_{(a,b)}$ . Since  $f$  picks one element from every pair  $\{a, b\}$  of atoms, it must be able to tell all atoms apart; so the equivalence classes of  $\sim_f$  are going to be singletons,  $\sim_f$  is going to be of infinite index, and  $f$  is not of finite support.

So the axiom of choice for countable sets of pairs fails. Since this axiom is about the weakest version of AC known to man, this is pretty good. The slight drawback is that we have had to drop foundation to achieve it. On the other hand the failure of foundation is not terribly grave: the only illfounded sets are those with a Quine atom in their transitive closures, so there are no sets that are gratuitously illfounded: there is a basis of countably many Quine atoms. On the other hand it is only the illfounded sets that violate choice!

## 7.8 Pairing

Pairing is not independent of the other other axioms of ZFC, since (as we saw in section 4.2) it follows from the axioms of empty set, power set and replacement. Nevertheless it is independent of the other axioms of Zermelo Set Theory. Let us say that a set-theoretic structure  $\mathfrak{M}$  is **supertransitive** if it is transitive and every subset of a member of  $\mathfrak{M}$  is also in  $\mathfrak{M}$ . Let  $\mathfrak{M}$  and  $\mathfrak{N}$  be two supertransitive models of Zermelo Set Theory such that neither is a subset of the other. Then  $\mathfrak{M} \cup \mathfrak{N}$  is a supertransitive model of all of Zermelo Set Theory except pairing. See section 13 of Mathias [44] for details. The way in which the derivability

of pairing from the other axioms relies on the presence of the axiom scheme of replacement reminds us of the way in which replacement can be thought of as a generalisation of pairing. see p 46.

blend this in

In section 4.2.1 we floated the idea of a world in which pairing fails, and the sets in that world are strongly typed. This is an alternative view, but it doesn't give a proof of independence of pairing from the other axioms because replacement and separation are not literally true in those models – because of the typing restrictions.

More to be said here

This is another example of *graceful downward compatibility*: we retain the axiom of pairing in ZF (despite its derivability from empty set, power set and replacement).

## Chapter 8

# ZF with Classes

The idea is that you can form an extension to correspond to any intension. But whenever you form one you have to hold fire at the last minute and not let it loose on the world until you're sure it's safe to do so. It's not a good idea to loose upon the world the extension corresponding to **not-self-membered**. The final step, after dreaming up the extension, is saying abracadabra and bringing it into existence. Then it becomes a set. Until then it doesn't really exist. That is to say, it's not one of the things that you sweep up when you are assembling an extension to correspond to an intension.

Sets are constructed using a lasso. Nothing is safe from Kripke's lasso. If the extension that contains all sets that are not members of themselves is not to cause trouble then we are not to be allowed to tap it with our wand and turn it into a set, an actual thing that can be caught in a lasso later. It has to remain in a twilight state. In most treatments of ZF (or ZF-with-classes) everything is a "class" but some things – those that were lasso-contents that got tapped by the wand – become sets and can get snaffled by the lasso later and put into a set. *Proper* classes are lasso-contents that never get tapped and turned into sets and therefore can never become members of anything.

H I A T U S

Nowadays set theorists get by without having axioms for proper classes: none of the modern strong axioms need variables ranging over classes. So a chapter on axioms for proper classes is a bit of a side-show and is included really only for the sake of completeness.

But what are classes anyway? It is sometimes convenient to accord a kind of shadowy existence to collections that are not sets, particularly if there are obvious intensions of which they would be the extensions were they to exist. One thinks of the collection of all singletons, or the collection of all things that are equal to themselves (the corresponding intensions are pretty straightforward after all!). We call these things **classes** or (since some people want to call all collections "classes" – so that sets are a kind of class) **proper classes**. In

the earliest set-theoretical literature (at least that part of it that is in English) collections were always routinely called *classes*, and the use of the word ‘set’ to denote particularly well-behaved classes in this way is a later development.

If we allow classes, we can reformulate *ZF* as follows. Add to the language of set theory a suite of uppercase Roman variables to range over classes as well as sets. Lowercase variables will continue to range solely over sets, as before. Since sets are defined to be classes that are members of something we can express “*X* is a set” in this language as ‘ $(\exists Y)(X \in Y)$ ’ and we do not need a new predicate letter to capture sethood.

Next we add an axiom scheme of class existence: for any expression  $\phi(x, \vec{y})$  whatever, we have a class of all  $x$  such that  $\phi(x, \vec{y})$ :

$$(\forall X_1 \dots X_n)(\exists Y)(\forall z)(z \in Y \longleftrightarrow \phi(z, X_1 \dots X_n)) \quad (8.1)$$

We rewrite all the axioms of *ZF* except replacement and separation by restricting all quantifiers to range over sets and not classes. We can now reduce these two schemes to single axioms that say “the image of a set in a class is a set” and “the intersection of a set and a class is a set”. Does this make for a finite set of axioms? This depends on whether the axiom scheme of class existence can be deduced from finitely many instances of itself. The version of this scheme asserted in the last paragraph cannot be reduced to finitely many instances. This system is commonly known as Morse-Kelley set theory.<sup>1</sup> However, if we restrict the scheme 8.1 to those instances where  $\phi$  does not contain any bound class variables, then it can be reduced to finitely many axioms, and this system is usually known as ‘GB’ (Gödel-Bernays). GB is exactly as strong as *ZF*, in the sense that – for some sensible proof systems at least – there is an algorithm that transforms GB proofs of assertions about sets into *ZF* proofs of those same assertions. Indeed, for a suitable Gödel numbering of proofs, the transformation is primitive recursive. See [59].

So GB is finitely axiomatised even though *ZF* isn’t. One might think that having finitely many axioms instead of infinitely many axioms should make life easier for the poor logician struggling to reason about the axiom system, but in fact it makes no difference at all. Unless the axiom system has a set of axioms that is *semidecidable* (so that one can recognise an axiom when one sees one) nothing sensible can be done anyway. If one has a finite procedure that correctly detects axioms and rejects non-axioms (we say of such a system that it has a *decidable* set of axioms)<sup>2</sup> then the axiom system in some sense has finite character and it will be fully as tractable as a system that is genuinely finitely axiomatisable. It is a theorem of Craig’s that any first-order theory with a semidecidable set of axioms has a set of axioms that is decidable. (Notice that the famous incompleteness theorem of Gödel applies to systems of arithmetic with semidecidable sets of axioms and not merely to those with finitely many

<sup>1</sup>It was actually first spelled out by Wang [71], who called the system ‘NQ’.

<sup>2</sup>The old terminology – still very much alive in this area – speaks of *recursively axiomatisable theories*.

axioms.) It is also true that it will turn out to be mutually interpretable with a finitely axiomatisable theory that can be obtained from it in a fairly straightforward way. Indeed GB arises from ZF in precisely this manner. However nothing is gained thereby. It is because of this that set theorists now tend to work with ZF rather than GB.

Morse-Kelley is actually stronger than GB, and although the details are hard, it is not hard to see why this might be so. Since a set is a class that is a member of something we can represent variables over sets as variables over classes and ensure that the version of the scheme 8.1 where all variables must range over sets only is a subscheme of 8.1. This means that the more inclusive version of the scheme proves the existence of more classes, and therefore – through the rôle the class existence scheme plays in the set existence axioms of separation and replacement – proves the existence of more sets. And, given the rôle played by set existence axioms in proving induction, it means we can prove more inductions, as follows. Mathematical induction follows from the definition of  $\mathbb{N}$  as the intersection of all sets containing 0 and closed under successor. If  $T$  is a theory that proves that a definable set  $X$  contains 0 and is closed under successor, it proves that all natural number are in  $X$ . That is to say, it proves an instance of mathematical induction. But if  $T$  doesn't know that  $X$  exists then it doesn't know that it is one of the things that contain 0 and is closed under successor, so it doesn't know that every natural number is in it. The more sets a theory  $T$  proves to exist, the more mathematical inductions it can prove. And the more mathematical inductions it can prove, the more consistency results it can prove (“For no  $n$  is  $n$  a proof of  $\neg\text{Con}(T')$ ”).

### 8.0.1 Global Choice

One version of the axiom of choice says that every set can be wellordered. If this can be done sufficiently uniformly then there might be a wellordering of the entire universe, a *global* wellordering. This of course is an axiom asserting the existence of a particular kind of *class* and so is not an axiom of set theory. A strong form of Global choice, which we will see below, states that there is a proper class that wellorders the universe in such a way that every proper initial segment is a set.

### 8.0.2 Von Neumann's axiom

The introduction of the device of proper classes into Set Theory is usually credited to Von Neumann [69]. One of the axioms to be found there is:

**A class is a set iff it is not the same size as  $V$ .**

**THEOREM 5.** *Von Neumann's axiom is equivalent to the conjunction of Coré's axiom, the axiom scheme of replacement, and the strong form of Global choice that we have just mentioned.*

*Proof:*

(We will use separation and power set)

We assume von Neumann's axiom and infer the conjunction of the others.

The collection of Von Neumann ordinals has a wellordering of a rather special kind: every initial segment of the graph is a set. Since this collection is a proper class this axiom tells us that it must be the same size as  $V$ . So  $V$  has a wellordering of this special kind too. This is the strong form of Global Choice that was promised

Armed now with AC, we can infer the axiom scheme of replacement: if  $X$  is a surjective image of a set  $Y$ , then there is an injection  $X \hookrightarrow Y$  by AC. Now if  $X$  were the same size as  $V$  there would be an injection  $V \hookrightarrow Y$  and therefore an injection  $\mathcal{P}(Y) \hookrightarrow V \hookrightarrow Y$  and the graph of this injection would be a set by separation, contradicting Cantor's theorem. So  $X$  is not the same size as  $V$ ; so it is a set.

Finally we infer Coret's axiom. The collection  $WF$  of wellfounded sets is a paradoxical object (this was Mirimanoff's paradox) and is therefore a proper class, and is accordingly the same size as  $V$ , by means of a class bijection which we will write  $\pi$ . So every subset  $x$  of  $V$  is the same size as a subset  $\pi"x$  of  $WF$ , which is a set by replacement. But  $\pi"x$ , being a set of wellfounded sets, is wellfounded itself, so  $x$  is the same size as a wellfounded set.

$R \rightarrow L$

By Coret's axiom every set is the same size as a wellfounded set so every isomorphism class of wellorderings contains a wellfounded set. Therefore we can use Scott's trick<sup>3</sup>, and we can define the proper class  $On$  of (Scott's trick) ordinals.

$On$  has a wellordering every proper initial segment of which is a set. By the assumption of strong Global choice, so does  $V$ . Now we build a bijection between  $V$  and  $On$  by recursion in the obvious ("zip it up!") way. The map we construct will be a bijection because (i) were it to map an initial segment of  $V$  onto  $On$  then  $On$  would be a set by replacement and (ii) were it to map an initial segment of  $On$  onto  $V$  then  $V$  would be a set by replacement.

Now let  $X$  be a proper class. Then for any set  $x$  there is  $y \in (X \setminus x)$ , and by AC there is a function  $f$  that to each set  $x$  assigns such a  $y$ . Define  $F : On \hookrightarrow X$  by setting  $F(\alpha) = f(\{F(\beta) : \beta < \alpha\})$ . This injects  $On$  into  $X$ . In the last paragraph we injected  $V$  into  $On$  so  $X$  is as large as  $V$ . ■

One might think that von Neumann's axiom implies some form of antifoundation: after all any Quine atom is strictly smaller than the universe, and therefore ought to be a set. However there is a missing step: the axiom shows that any class  $x$  such that  $x = \{x\}$  is a set; it doesn't tell us that there is such a class! But of course a Quine atom is a member of itself and is therefore a set anyway.

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<sup>3</sup>Coret's axiom implies that if  $\sim$  is an equivalence relation defined by a stratifiable formula then every  $\sim$ -equivalence class contains a wellfounded set.



Worth remarking that Von Neumann's axiom doesn't apparently imply foundation, tho' one might expect it to. Mirimanoff's paradox tells us that  $WF$  cannot be a set, so von Neumann's axiom implies that it must be the same size as  $V$ . But that doesn't seem to be enough to prove that it must be identical to  $V$ . However it does imply Coret's axiom that every set is the same size as a wellfounded set. But we got that from the fact that the von Neumann  $On$  is a proper class, which ensures that every set is the same size as a von Neumann ordinal.



## Chapter 9

# Set-theoretic principles that have meaning outside set theory

Choice, Replacement and IO. And possibly pairing!

Well *five* actually, beco's the real meaning of Hartogs' lemma is that you never run out of ordinals.

We need the possibility of making disjoint copies if we are to do sums of cardinals or of ordinals.

It's worth making the point that some set-theoretic principles (or theorems) are important not because of what they tell us about Set Theory but because they have meaning outside set theory. Some of these principles are axioms or axiom schemes and we commented on their meanings at the state when they were introduced. We saw how an important part of the mathematical meaning of the scheme of replacement is implementation-insensitivity. The axiom of choice tells us (or will in vol II) that all transfinite deterministic monotone processes can be executed. It will also tell us (but, again, not until vol II) that every element of a recursive data type has a certificate. The axiom of pairing tells us that we are doing set theory not type theory.

However there are some propositions in need of explanation which did not get coralled as axioms. A piece of set theory doesn't have to be elevated to an axiom before you pay attention to it and perhaps notice that it derives some of its meaning from something outside set theory.

Hartogs' lemma tells us that if a transfinite deterministic monotone process fails to complete it's not because you have run out of ordinals.

Representation theorems tend to be underpinned by set existence principles.

I want to mention here a principle that was identified in the Quine set theories that has a meaning outside set theory. Nobody noticed it for ages because it is so trivially provable in wellfounded set theories. It is the principle

(known in NF circles as IO) that says that every set is the same size as a set of singletons.

## 9.1 Representation Theorems

There is a story to be told about how representation theorems line up with set existence principles, or at least set-theoretic principles.

Stone's representation theorem is equivalent to BPI;

Forti-Honsell Antifoundation is a representation theorem: every set picture is a picture of a (unique) set.

"Every poset is iso to an ordernesting" is equivalent to IO;

IO implies the existence of limits, colimits and coequalisers

IO is also equivalent to the principle that you can make as many disjoint copies of a structure as you want..

"Every group is iso to a group of permutations" is straight-out provable.

What other representation theorems are there?

## 9.2 Certificates

This difference between counted sets and countable sets is part of a general point about *certificates*. A counted set is simply a set that happens to be countable and is equipped with a certificate to that effect. AC is generally the assertion that for any recursive data type `indiscrete-category` there is a casting function `indiscrete category → indiscrete-category × indiscrete-category-certificate`. (One should smuggle in here the word *paper-trail*.)

This is surely not true ... not *indiscrete* categories..

There is a concept of *certificate* that does a lot of work in complexity theory. It seems to me to work along the following lines.

A certificate that a natural number  $n$  is composite is a pair of natural numbers  $n_1$  and  $n_2$  s.t.  $n_1 \cdot n_2 = n$ . A certificate that an object  $x$  is in a recursive datatype is a record of how the object was produced from its immediate subobjects accompanied by certificates for those subobjects. (This is slightly more general than the illustration we started with, co's the set of composite numbers is not in any obvious (or – here – useful) way a recursive datatype.) Examples:

- (i) A certificate that  $n$  is a natural number would presumably be the set  $[0, n]$  decorated with a bit of extra structure which we won't quibble about;
- (ii) A certificate for a wff in a language is presumably a parse tree for that wff;
- (iii) a certificate that a wff is a theorem of a theory  $T$  is presumably a proof of that wff in some proof system for  $T$ . Presumably, for each recursive datatype the family of certificates-of-membership-in-it also constitute a recursive datatype.

Apparently now that the set of primes is known to be in  $P$  every prime has a polynomial certificate of primality.

The idea seems to be that for various flavours of object that if you are an object of that flavour then there will be a certificate to that effect. What do these flavours have in common? Presumably they have to be  $\Sigma_1$  in some suitable language, but can one be more specific?

The idea also seems to be that a certificate in these cases is information-suitably-packed. How do we *unpack* it? For example, if a certificate of compositeness of a natural number  $n$  is to be a pair of factors of  $n$ , the audience have to be able to multiply them together to recover  $n$  without any help from you. (If they couldn't then the certificate would have to contain a representation of the multiplication.) What sort of calculation is the audience supposed to be capable of? What is the system used to recover the full details from the certificate? Or does it vary? Horses for courses?

I'm interested in extending this to recursive data types of infinite character, such as the countable ordinals. Consider for example the infinitary recursive data type  $\mathcal{C}$  generated by the set of countable sets closed under countable unions. A  $\mathcal{C}$ -certificate for  $x$  is either a counting of  $x$  or a counted set of  $\mathcal{C}$ -certificates for a family  $F$  with  $\bigcup F = x$ . The trouble is, certificates of this kind aren't available without AC! This has the makings of a real pain. It seems pretty obvious that if  $x \in \mathcal{C}$  and  $|y| = |x|$  then  $y \in \mathcal{C}$ . The idea is simply to take a certificate of  $\mathcal{C}$ -ness for  $x$ , copy it over by the bijection between  $x$  and  $y$  to obtain a certificate for  $y$ . But can we do it without certificates? I think induction on rank works.

### 9.2.1 Jech's theorem about $HC$

My point of departure is the idea of a recursive datatype or 'recursive data type' for short. A recursive data type has *founders* and is built up by constructors. The typical examples are **free** in the sense that each object in the recursive data type is denoted by a unique word in the constructors. Examples are the natural numbers, lists and trees in the ML style. Such recursive data types are presumably initial objects in a suitable category. There is a natural tendency for computer scientists to be interested only in recursive data types of **finite character**: finitely many founders and finitely many constructors each of finite arity. There is an obvious reason for this. However there is no mathematical reason not to consider recursive data types of infinite character, and the cumulative hierarchy of sets is one. It has no founders at all, and has one constructor – **set-of** – of unbounded arity. This is a free recursive data type and is well-behaved.

So, thus far, we have two parameters with which we classify recursive data types. They may be of finite character vs infinite character, and they may be free vs not-free.

Free

not-free

finite character		-----	
		The naturals	
		lists, trees,	
		a la ML	
-----		-----	
infinite character		The cumulative	
		hierachy of sets	The ordinals

I can't think of an obvious example for the top right but you probably get the idea anyway.

Next i need the idea of a *certificate* or *proof*. If you are a member of a recursive data type there is always a good reason for you so to be, and a certificate or proof is that reason. If the recursive data type is free (so it's an initial object in a suitable category) every object has a unique certificate. If the recursive data type is not free there may be a multiplicity of certificates. Notice that even if the recursive data type  $R$  is not free, the recursive data type of certificates-for- $R$  is free. Perhaps I should be a bit more explicit about what a certificate is to be. A certificate that  $x$  belongs to the recursive data type is a record of the constructor used in the last step in the construction of  $x$ , together with a list of arguments to that constructor, with certificates for each of those arguments. So a certificate is a word in the constructors and founders.

Now we need a slightly finer distinction, within the family of recursive data types of infinite character. Specifically i shall be interested in the following recursive data types.

1. The collection of wellfounded hereditarily countable sets. The single constructor is countable-set-of. This collection is often called  $HC$ ;
2. The recursive data type whose founder is the ordinal number 0, with constructors **successor** and **sup-of-omega-sequence-of**. This is a substructure of the ordinals;
3. The recursive data type whose founders are all the countable sets, and whose constructor is union-of-countable-set-of;
4. The recursive data type whose founders are all the  $\omega$ -sequences and whose constructor is  $\omega$ -sequence of;
5. the cumulative hierarchy of sets.

(1) and (4) are free. (2) and (3) are not.

Now any recursive data type admits a canonical rank function, which is a map to the ordinals, whereby the rank of any object in the recursive data type

is the least ordinal bigger than the ranks of all the things in the recursive data type that go into the construction of that object. In the case of (5) the recursive data type rank is literally the same as the set-theoretic rank.

Now let's think about free recursive data types of infinite character, but *bounded* character, so their constructors have bounded arities.

Jech has a wonderful theorem that says that every set in  $HC$  has rank less than  $\omega_2$ . It's a very important fact that the proof of this is purely combinatorial and does not use AC at all. It exploits the fact that the recursive data type  $HC$  is free: each object has a unique certificate. I think that in general Jech's theorem shows that in any free recursive data type of countable character every object must have rank  $< \omega_2$ .

The freeness is important here. It is a theorem of Gitik [23] that the recursive datatype 2 (in the list above) can contain all ordinals

It is standard that if AC holds, then  $|HC| = 2^{\aleph_0}$ . In a sense this isn't really the theorem; the theorem that underlies it goes like this:

Each of these recursive data types is the least fixed point for a suitably chosen operation. So if you can find another fixed point ("pick a fixed point, any fixed point"!) you should be able to embed the recursive datatype in it and thereby bound its size. Consider not  $HC$  but the recursive datatype (3). The reals is the same size as the set of  $\omega$ -sequences of reals. That means that we can define by recursion on the recursive datatype (3) an injection into the reals. We need the freeness of (3) to ensure that the map we are defining is an *injection*.

Moral: every free recursive datatype of *bounded* character is a set. and by Jech's argument we have tight control of the ranks of the ordinals used.

But what about the non-free recursive data types? One thing that this has brought home to me is that unless we assume AC we have no reason to suppose that a recursive data type of infinite character is a surjective image (in the obvious way) of its recursive data type of certificates. For example, in the model of Gitik's where every limit ordinal has cofinality  $\omega$  the recursive data type (2) generated from 0 by `succ` and  $\omega$ -sups contains all ordinals, and the recursive data type of certificates for it is a free recursive data type of countable character, so every certificate has rank  $< \omega_2$ . In those circumstances there is a point in the recursive data type after which every object lacks a certificate.

Free recursive data types of infinite bounded character are well-behaved, but we need AC to show that every infinite recursive data type is a surjective image of a free one. So with non-free recursive data types there is a nontrivial task of proving their sethood in the absence of AC. For example in NF we do not know if the recursive data type (2) is the universe<sup>1</sup>. And this despite the fact that we know that not every set can be a projection of a member of recursive data type (3), that recursive data type being bounded.

Presumably AC is equivalent to the assertion that every recursive data type is a surjective image of its recursive data type of certificates.

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<sup>1</sup>Tho' we do know that it is a set.





# Answers to Exercises

Let  $G$  be a graph where, for each vertex  $v$ , the collection  $N(v)$  of neighbours of  $v$  is a set<sup>2</sup>.

- (i) Give an example to show that  $G$  might be a proper class.
- (ii) Now suppose  $G$  is connected. Prove that it is a set.

Answer:

(i)

Turn the universe into a graph by putting an edge between any two sets of the same rank. Every  $N(v)$  is a set. But the graph is a proper class.

Or: The example we want is the graph whose vertex set  $V$  is the whole universe and whose edge set  $E$  is empty.

(ii)

Fix a vertex  $v$ . Let  $V_n$  be the set of vertices of distance  $n$  from  $v$ . We prove by induction on  $n$  that every  $V_n$  is a set. (using replacement! We'd better explain!!) So the union of all of them is a set. (again, using replacement, this time on  $\mathbb{N}$ ). But  $G$  was connected.

Suppose  $G$  is connected. By assumption  $N(v)$  is a set for all vertices  $v$ . For any vertex  $v$  let  $N_n(v)$  be the collection of  $n$ -neighbours of  $v$ , the vertices whose shortest path to  $v$  is of length at most  $n$ . We prove, by induction on  $n$ , that: for all  $v$ ,  $N_n(v)$  is a set. Evidently true for  $n = 1$ , by assumption. For the induction, reflect that – for any vertex  $v$  –  $N_{n+1}(v)$  is  $N_n(v) \cup \bigcup \{N_1(u) : u \in N_n(v)\}$ .  $\{N_1(u) : u \in N_n(v)\}$  is a set by replacement, so its sumset is a set and then we use binary union. So, for any  $v$ , the function  $n \mapsto N_n(v)$  is well defined, and its range is a set, and the sumset of the range is also a set. This sumset is the entire edge set.

Without replacement the second part no longer holds. Work in  $V_{\omega+\omega}$ . Let  $G = V$  as before. Join every set of finite rank to  $V_\omega$ . For sets  $x, y$  of infinite rank, if  $x \in y$  and  $\text{rank}(y) = \text{rank}(x) + 1$  then put an edge between  $x$  and  $y$  – and not otherwise. Then every neighbourhood  $N_1(v)$  is a set, and the graph is connected – but it is not a set. Neither the vertex set nor the edge set is a set.

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<sup>2</sup>Here our graphs have undirected edges, no labels on the edges and no multiple edges.  $v'$  is a neighbour of  $v$  iff there is an edge between  $v$  and  $v'$ .



# Glossary

## Banach-Tarski Paradox

Assuming the axiom of choice we can partition a solid sphere into several pieces, which can be reassembled to make *two* spheres the same size as the original sphere. As well as Wikipædia, consult Wagon [70].

## Borel Determinacy

For  $A \subseteq \mathbb{R}$ , Players I and II play the game  $G_A$  by everlastingly alternately picking natural numbers, and thereby build an  $\omega$ -sequence of naturals, which is to say a real. If this real is in  $A$  then I wins, otherwise II wins. Borel Determinacy is the assertion that if  $A$  is a Borel set of reals, then one of the two players has a winning strategy.

## Burali-Forti Paradox

Rosser's **Axiom of Counting** asserts that there are  $n$  natural numbers less than  $n$ . The generalisation to ordinals asserts that the set of ordinals below  $\alpha$  is naturally a wellordering of length  $\alpha$ . (This was known to Cantor). So the length of any initial segment  $X$  of the ordinals is the least ordinal not in  $X$ . So what is the length of the set of all ordinals?

## Categoricity

### Digraph

A digraph is a set  $V$  equipped with a binary relation, usually written ' $E$ '. The ' $V$ ' connotes 'vertex' and the ' $E$ ' connotes 'edge'. If the ordered pair  $\langle x, y \rangle$  is in  $E$  we say there is an edge from  $x$  to  $y$ .

### Dedekind-infinite

A set  $X$  is Dedekind-infinite iff there is a bijection between  $X$  and some proper subset of itself. Equivalently  $X$  is Dedekind-infinite iff it has a subset the same size as  $\mathbb{N}$ , the set of natural numbers.

### Maximal formula

A maximal formula in a proof is one that is both the output of an introduction rule and an input to an elimination rule for the same connective. For example:

$$\begin{array}{c} [A] \\ \vdots \\ \frac{B}{A \rightarrow B} \rightarrow\text{-int} \quad A \rightarrow\text{-elim} \\ B \end{array} \quad (9.1)$$

where the ‘ $A \rightarrow B$ ’ is the result of an  $\rightarrow$ -introduction and at the same time the major premiss of a  $\rightarrow$ -elimination.

Another example:

$$\frac{\frac{A \quad B}{A \wedge B} \wedge\text{-int}}{A} \wedge\text{-elim} \quad (9.2)$$

Here the ‘ $A \wedge B$ ’ is the conclusion of an  $\wedge$ -introduction and the premiss of a  $\wedge$ -elimination.

One feels that the first proof should simplify to

$$\begin{array}{c} A \\ \vdots \\ B \end{array} \quad (9.3)$$

and the second to

$$A$$

### Mirimanoff’s paradox

This is the paradox of the set of all wellfounded sets. Every set of wellfounded sets is wellfounded (see definition below) so the collection of all wellfounded sets is wellfounded, and therefore a member of itself – so it isn’t wellfounded. But that makes it a set all of whose members are wellfounded that is nevertheless not wellfounded itself. This is a contradiction.

### Module

A field is a set with two constants 0 and 1, two operations  $+$  and  $\times$ , and axioms to say  $0 \neq 1$ ,  $x \times (y + z) = x \times y + x \times z$ ,  $x + (y + z) = (x + y) + z$ ,  $x \times (y \times z) = (x \times y) \times z$ ,  $x + y = y + x$ ,  $x \times y = y \times x$ , and that every element has an additive inverse, and that every element other than 0 has a multiplicative inverse. If we drop this last condition then we do not have a field but merely a *ring*.

A vector space consists of vectors, which admit a commutative addition; associated with the family of vectors is a field (whose elements are called **scalars**)

there is an associative operation of **scalar multiplication** of vectors by scalars, giving vectors. It distributes over vector addition.

### **Prenex Normal Form Theorem**

Every formula of first-order logic is logically equivalent to a formula with all its quantifiers at the front and all connectives within the scope of all quantifiers. Such a formula is said to be in Prenex Normal Form.

### **Primitive Recursive**

The primitive recursive functions are a family of particularly simple computable functions. They take tuples of natural numbers as inputs and give individual natural numbers as outputs. The successor function  $n \mapsto n + 1$  is primitive recursive, as is the zero function  $n \mapsto 0$ . The result of composing two primitive recursive functions is primitive recursive, and if  $f$  and  $g$  are primitive recursive so is the function  $h$  defined as follows:

$$\begin{aligned} h(0, x_1 \dots x_n) &=: f(x_1 \dots x_n); \\ h(y + 1, x_1 \dots x_n) &=: g(h(y, x_1 \dots x_n), y, x_1 \dots x_n) \end{aligned}$$

### **Quine atom**

A Quine atom is a set identical to its own singleton:  $x = \{x\}$ .

### **Stratifiable Formula**

A formula in the language of set theory is stratifiable if every variable in it can be given an integer label in such a way that in every subformula ' $x \in y$ ' the label given to ' $x$ ' is one lower than the label given to ' $y$ ' and in any subformula ' $x = y$ ' the two variables receive the same label.

### **Transitive Set**

A set  $x$  is transitive if  $x \subseteq \mathcal{P}(x)$  ( $x$  is included in the power set of  $x$ ) or equivalently if  $\bigcup x \subseteq x$  (the sumset of  $x$  is included in  $x$ ). Notice that these two formulæ that say that  $x$  is transitive are not *stratifiable* in the sense of the last paragraph.

### **Transitive Closure**

This expression has two distinct but related meanings.

In Set Theory  $TC(x)$ , the transitive closure of the set  $x$ , is the  $\subseteq$ -least transitive set  $y$  such that  $x \subseteq y$ . Another way to think of it is as the collection of those things that are members of  $x$ , or members of members of  $x$ , or members of members of members of  $x$  and so on.

The other meaning is related. If  $R$  is a (binary) relation, the transitive closure of  $R$  is the  $\subseteq$ -least transitive relation  $S$  such that  $R \subseteq S$ . It is often written ' $R^*$ '. Russell and Whitehead referred to  $R^*$  as the *ancestral* of  $R$ , since the transitive closure of the parent-of relation is the ancestor-of relation.

### Wellfounded

A binary relation  $R$  is wellfounded iff there is no  $\omega$ -sequence  $\langle x_n : n \in \mathbb{N} \rangle$  with  $R(x_{n+1}, x_n)$  for all  $n \in \mathbb{N}$ .

A set  $x$  is wellfounded iff the restriction of  $\in$ , the membership relation, to  $TC(x)$  is wellfounded. That is to say, there is no  $\omega$ -sequence  $\langle x_n : n \in \mathbb{N} \rangle$  with  $x_0 = x$  and  $x_{n+1} \in x_n$  for all  $n \in \mathbb{N}$ .

(These definitions are not strictly correct, but are equivalent to the correct

Must define countable choice definitions as long as countable choice holds.)

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## Further Reading

A quick glance at the bibliography will show that there are several volumes alluded to more than once. The Van Heijenoort collection [68] is essential for anyone interested in the history of Logic; the Barwise volume [2] contains a lot of useful material too. The volume in which the Gauntt article appeared is full of treasures. The fullest historical treatment of the Axiom of Choice that is readily available is the Moore volume [47]. Although the book [31] by Hallett and the book [65] by Tiles are not alluded to in the body of the text, they are still definitely worth a read. The Väänänen article [67] could be profitably consulted by those interested in pursuing second-order categoricity. Quine’s *Set theory and its Logic* [52] is eccentric but valuable. Although modern readers will find Quine’s notation an obstacle – and they may well not share his interest

in set theories with a universal set – they will probably still find the book useful. Quine was an instinctive scholar as well as a working logician and the book is well-supplied with references that will enable the reader to trace the emergence of the ideas he describes. Quine was born in 1909 and lived through much of this evolution and his account of it has the vividness and authority of an eyewitness report.

The Axioms of Set Theory  
Part II: How to Understand The Axiom of Choice

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# Chapter 1

## Introduction

I know this book is supposed to be vol 2 of a brace of volumes about the axioms of set theory, but it has evolved to be an object of a different kind from its companion volume. It was inevitable that that there had to be two volumes, because AC is of such a different character from the other axioms and so much more problematic than them. But even when i realised that, i didn't appreciate how different its volume would be from the other. This second volume is inevitably more philosophical and ...

There is plenty of literature on the axiom of choice. However the bulk of it is designed primarily for sophisticates – people who already understand the axiom of choice and are interested in *minutiæ*: mainly questions of which versions imply which other versions, or how much set theory one needs to prove two versions equivalent ... perhaps some history. However, most people who seek information about the axiom of choice are not really interested in which mathematical assertions it is equivalent to, or which mathematical assertions need it for their proofs; their concerns are of a much more basic sort: what does AC do? And when am I using it? What in God's name is going on?

This means that the entirely excellent (in its own terms) [17] is precisely the kind of thing I am trying *not* to write. Books like that (and the invaluable classic [23]) explain various equivalents of the axiom, and weak versions, but they do not set out to banish the bafflement that beginners experience when first trying simply to understand what on earth is going on. The endeavour to understand when you are using the axiom of choice and when you aren't is not at all the endeavour of learning which things are equivalent to AC and which are weak versions of it. Explaining the technical details of these equivalences and independences is an important exercise, but it is not what the troubled mathematician-in-the-street is looking for. The mathematicians in the street do not want to know what the equivalents of AC are; they want to know when they are using it – and when they should be using it – and that is what the stuff in books like Rubin-and-Rubin [23] doesn't help with.

My intended audience is the working mathematician who has heard stories

expand this

about the obscure but important rôle played by this annoying thing called the *Axiom of Choice*, and who thinks it might be an idea to find out what on earth is going on. It assumes that its readers know enough mathematics to have kinds of concerns that the author blah. Probably not for every mathematics undergraduate, but perhaps at least every mathematics undergraduate who is moved to pick up a book with a title like this one has. The collection of such people make up what the philosophers of Science call a *Natural kind*. In this it is intended to be the companion volume to [11] However there is one bolus of material here which is emphatically not of the kind routinely mastered by the mathematician on the Clapham omnibus, and that is the proof theory touched on in chapter 4.

It's not technical, and yet it's not foundational/philosophical either

When i say that most mathematicians have essentially no understanding of AC I am not having a go at them. Most mathematicians don't need to understand it, and choosing not to master it is not a dereliction of duty.

In a nutshell: I am offering not a tutorial on the mathematics of AC, (e.g. i'm not going to say anything about the rôle of countable choice in Analysis) but rather a commentary about how it enters into our mathematical practice. To that extent my project is exactly parallel to my project in volume I *vis à vis* the other axioms of ZFC. I am addressing myself to people who have reached a stage where they can state the axiom – and perhaps know a few equivalents of it – but don't really know what it means and are unsure about when they are making use of it, and who want to know what all the fuss is about. I think my target audience – in the first instance at least – was third-year students at Cambridge who are attending the lecture course on *Set Theory and Logic*. At all events I am addressing working mathematicians who are trying to understand the meaning of their praxis. I am not going to get involved in discussions of whether or not the axiom of choice might be true (whatever that might mean).

In some ways this document is going to be a bit like the sex lessons you had at school. People dishing them out are at pains to reassure you that they are not advocating any particular course of conduct, but are merely trying to put you in possession of certain facts. I don't want my readers to have to learn about this stuff behind the bike sheds the way I had to. The parallel may be better than I know: as with the sex lessons there will be reactionaries who will complain that young people will be encouraged to experiment and . . . . You can think of reading this book as like having a vaccination against human papilloma virus. It gives you a certain freedom; how you make use of that freedom is a decision for *you*.

So who am I to lift my head above the parapet and write the book that no-one else does? What is my excuse? The peculiar history of my mathematical education resulted in my being a dedicated student of a distinctly odd set theory, namely Quine's *New Foundations*. One of its oddities is that it refutes the axiom of choice, so that anybody who wishes to explore the world it describes has to be

prepared to eschew the axiom of choice altogether, and if you are to be able to do that you have to be able to detect when you are using it. Subsequently hanging out with theoretical computer scientists (initially as a postdoc) inevitably made me more sympathetic to constructivists than an unreconstructed Quinean would be, and compounded my scepticism of AC. It also made me think in terms of datatypes, and (as I hope to show in what follows, starting in [9]) arriving at a proper understanding of AC relies on first unpicking fallacies of equivocation about datatypes. Finally a logician in a mathematics department is forced to think about AC and explain it to their students, even if only because no-one else will.

The Axiom of Choice has – arguably – been the cause of more anxiety and of more ill-informed and unproductive disputation than any other proposition of pure mathematics. Its only rivals for that title are to be found in the disputes about infinitesimals and about the status of complex numbers, both now mercifully a distant memory. What is distinctive about the debate around the axiom of choice is that there seems to be no agreement about what is at stake. There is an old joke that the axiom of choice is obviously true, the wellordering principle obviously false and that the jury is still out on Zorn’s lemma. This joke – like so many really good jokes – circles around an uncomfortable truth: in this case the uncomfortable fact that *we don’t know what the axiom itself means*.

The situation is not that

- (i) it’s evident that AC and ZL are the same proposition but we don’t know whether we believe it or not;
- it’s the much more alarming
- (ii) they appear to be distinct even tho’ we know they’re equivalent.

This is alarming beco’s of “No entity without identity.” We haven’t even reached the stage of individuating this proposition properly, let alone a stage where we can tell whether or not it’s true. It actually isn’t a joke at all.

This uncomfortable truth is a daily nightmare of pure mathematics: there is probably a majority of pure mathematicians who profess to believe it, but despite that it’s only a minority – even of the believers – who can state it correctly. And – remarkably – even among those who can state it correctly there are plenty who do cannot tell when they are using it (or not using it) ... which reminds us (again) that they – and we – can’t have understood it.

The first warning one has to give to the reader, the reader I am addressing myself to, who “just wants to know what in God’s name is going on” is that altho’ the Axiom of Choice is not in any obvious sense a logical principle, the techniques one needs to employ if one is to flush it out tend to be familiar to logicians rather than to Mathematicians at large. That is absolutely not to say that only a logician can understand what is going on, but if you are to break into the problem you do need some *aperçus* that do have a rather logical flavour. It is no accident that this little book is being written by a logician. But do not be discouraged!



## Chapter 2

# Understanding the Axiom

We shall start with a form of it that is particularly simple, to make it easier for the reader to see what is being claimed, and perhaps see whether or not they want to believe it. This first form that we consider is the axiom that Russell [25] called the *multiplicative axiom*. We will see later (p. 28) why it bears this name.

One pleasing feature of this version of the axiom is that it is purely set-theoretical and needs no notation beyond ‘=’ and ‘∈’, and no concepts beyond *set*. (No pairing, no functions ...)

Let  $X$  be a nonempty family of pairwise disjoint nonempty sets, so that  $(\forall y, z \in X)(y \cap z = \emptyset)$  and  $(\forall x \in X)(\exists y)(y \in x)$ .

Then there is a set  $Y$  such that, for all  $y \in X$ ,  $Y \cap y$  is a singleton. (M)

$Y$  is said to be a *transversal set*<sup>1</sup>.

I can remember thinking – when I first encountered this axiom – that it must be a consequence of the axiom scheme of separation, which says that any subcollection of a set is a set. The  $Y$  that we are after (once we are given  $X$ ) is obviously a subcollection of  $\bigcup X$ , and *that’s* a set all right. There are people who say that every subclass of a set *however specified or conceived* is a set, and on this view the axiom of choice, in the above formulation, is a straightforward consequence of comprehension. We will return to this later (p 88), but for the moment let us assume we have only first-order axioms of set existence. If we were to deduce the axiom of choice from them we would have to be able to present  $Y$  as the set of those things in  $\bigcup X$  satisfying some property or other. Unfortunately there is no obvious way of finding a property  $\phi$  so that  $Y$  is  $\{w \in \bigcup X : \phi(w)\}$ . Contrast with the existence of a bijection between  $A \times B$  and  $B \times A$ : we can specify such a bijection *without knowing anything about  $A$  and  $B$*  – just flip the ordered pairs round. To find such a  $Y$ , given  $X$ , it seems that we need to be given a lot of information specific to  $X$ . For an arbitrary  $X$

---

<sup>1</sup>However one should bear in mind that ‘transversal set’ has additionally other meanings/uses.

we do not have that kind of information; accordingly we cannot prove (M) above for arbitrary<sup>2</sup>  $X$ ; this leads us to the conclusion that if we want to incorporate  $M$  and its logical consequences in our theory then we will have to adopt it as an axiom.

The problem seems to be that in order to obtain  $Y$  we have to select an element from every member of  $X$  and we need information about  $X$  (and its members) to guide us in making our choices.

At this point I shall revert to a more usual version of the axiom:

**Every family of nonempty sets has a choice function, a function that picks one member from each element of the family.**

I come to this version of the axiom with some reluctance, since it involves a new mathematical notion, that of *function*; the reader might quite reasonably suspect that we need to sort out the correct way to conceptualise functions before we can understand how they play their rôle in the axiom of choice. Actually we don't, and I hope the reader will consent to read on while leaving 'function' undefined.

Equally simple might be: every surjection has a right inverse

Is this the right place to be making this point?

One fact we must hang onto, for it is important (even tho' we won't prove it here!) is that the axiom of choice does not follow from the other axioms of set theory. It genuinely is a principle about sets distinct from the other principles of set existence. In insisting on this fact I am not taking a position on whether or not the axiom of choice is *true*. As far as I am concerned you are welcome to believe that the axiom of choice is true, and even that it is obvious (tho' I shall argue that if you do that then you are probably in the grip of a radical misunderstanding)<sup>3</sup>; what you are not at liberty to believe is that the axiom follows from the other axioms of set theory.

## 2.1 Misunderstanding the axiom

The failure to grasp AC properly can result in two mistakes. One mistake is thinking that you aren't using it when you are; the other is thinking that you need it when you don't. Thus each mistake tends to crowd out the other, but – interestingly – these two very divergent lifestyle choices both (tend to) lead to the one outcome: you end up believing it, albeit for very different reasons. Let me explain.

If you are making the first mistake, you don't know what all the fuss is about. So, these sad weirdos called *logicians* keep buttonholing you and telling you that you are using the axiom of choice (to do things that are obviously OK)

<sup>2</sup>The sudden appearance of the word 'arbitrary' at this juncture is an indication that the stage at which we need to make the axiom of choice explicit at precisely the stage where we acquire the concept of an arbitrary set...in-extension! NOT THE PLACE FOR THIS REMARK

<sup>3</sup>To misquote Maynard Keynes: mathematicians who believe AC are usually the slave of some hopelessly naïve philosophical theory. "Practical men who believe themselves to be quite exempt from any intellectual influence, are usually the slaves of some defunct economist."

so suppose for the sake of argument that they are right (they're weirdos rather than idiots, after all) then – of course – that tells you that the axiom of choice is OK.

If, on the other hand, you are making the second mistake, of always thinking that you need it, then you naturally think you have to assume it from Day One, since otherwise you would never get anything done.

In broad terms, the thrust of this little essay is to help sufferers recover from whichever of these two errors is afflicting them. I start by attempting to explain how the two forms of this inability arise. I shall address the other issues later. I freely admit that the explanation I offer is conjectural and pretty vague, but my aim is pædagogical not philosophical, so I will be happy even if all it achieves is helping some readers get their thoughts in order. Acquiring a correct understanding of what is going on with the Axiom of Choice does more for us than tidying up a skeleton in a remote cupboard in the west wing, since in the process one will come to a clearer understanding of how one does one's mathematics.

Chapters 4 and 3 are inspired by these two errors.

Not sure which chapter  
should come first





## Chapter 3

# Getting the right datatype, and the fallacy of equivocation

Job of set theory primarily is concretisation. But why do we think that we concretise with *sets*? Beco's sets are the most basic datatype, the one with least structure.

This chapter is designed in the first instance for people who make the second mistake: that of thinking that they don't need the axiom of choice when in fact they do. At the risk of medicalising their errors one can here make good use of the medical slang expression *presents*<sup>1</sup>: usually the first sign that someone is in the grip of this error comes when you hear them assert blithely that a union of countably many countable sets is countable, *and that no special assumptions are required to show it*. That is the point when you realise that something has gone wrong.

There are many instances of this second mistake being made out there all the time. In this chapter I discuss four examples.

### 3.1 Four Examples

- (i) A union of countably many countable sets is countable;
- (ii) Russell's example of the countable family of pairs of shoes;
- (iii) Every perfect binary tree has an infinite path;
- (iv) Lagrange's theorem that the order of a subgroup of a group divides the order of the group.

Actually perhaps a good fifth example might be that every infinite set has a countable subset. What is true is that every infinite subset of  $\mathbb{R}$

---

<sup>1</sup>The stress is on the second syllable; this isn't something you find under a Christmas tree.

### 3.1.1 A Countable Union of Countable Sets is Countable

Draw the  $X_i$  out in a doubly infinite array, and then count their union by zigzagging, as in the picture below. Let  $x_{i,j}$  be the  $j$ th member of  $X_i$ . Put the members of  $X_i$  in order in row  $i$ , so that  $x_{i,j}$  is the  $j$ th thing in the  $i$ th row. The *zigzag construction* uses a bijection  $f : \mathbb{N} \longleftrightarrow \mathbb{N} \times \mathbb{N}$ . Indeed we can even exhibit a definable bijection. On being given  $n \in \mathbb{N}$ , we want to decode it as the pair  $\langle x, y \rangle$  we recover the largest triangular number  $\binom{k}{2} \leq n$ . Think about the increment  $y$  that we have to add to  $\binom{k}{2}$  to obtain  $n$ . Evidently  $\binom{k+1}{2} = \binom{k}{2} + (k+1)$  so we infer  $0 \leq y \leq k$ . If we now rewrite  $k$  as  $x+y$  we have

$$n = \binom{x+y}{2} + y.$$

Copy stuff about Indiscrete Categories from logicrave to here...?

5	15	$\dots$	$\vdots$	$\dots$						
4	10	$\searrow$	16	$\dots$	$\vdots$	$\dots$				
3	6	$\searrow$	11	$\searrow$	17	$\dots$				
2	3	$\searrow$	7	$\searrow$	12	$\searrow$	$\vdots$	18	$\dots$	$\dots$
1	1	$\searrow$	4	$\searrow$	8	$\searrow$	$\searrow$	13	$\searrow$	$\vdots$
0	0	$\searrow$	2	$\searrow$	5	$\searrow$	$\searrow$	9	$\searrow$	$\vdots$
	0	1	2	3	4	5	6	7	8	9

### 3.1.2 Socks

In [25] (p 126) we find the *sutra* of the millionaire whose wardrobe contains a countable infinity of pairs of shoes and a countable infinity of pairs of socks. OK, countably many *pairs* of shoes; how many *shoes*? Obviously  $\aleph_0$ . Again, countably many *pairs* of socks; how many *socks*?  $\aleph_0$  again? However, classroom experience has taught me that many students baulk at being asked to actually *prove* that there are precisely  $\aleph_0$  shoes. The task seems so trivial to them that they experience any challenge to actually *execute* it as an affront to their status as adults. After all, isn't it a sign of grown-up-ness that you are allowed to say that things are trivial? You may feel the same, Dear Reader. However you should stick with it and provide a proof ... and one that doesn't involve any movement of the upper limbs if you please. If you want a hint, think about why the puzzle contrasts shoes with *socks*, rather than with (say) *gloves*. Then you will see why there really are  $\aleph_0$  shoes, not just  $\aleph_0$  *pairs* of shoes.

Should have an Erewhonian joke somewhere about having the socks

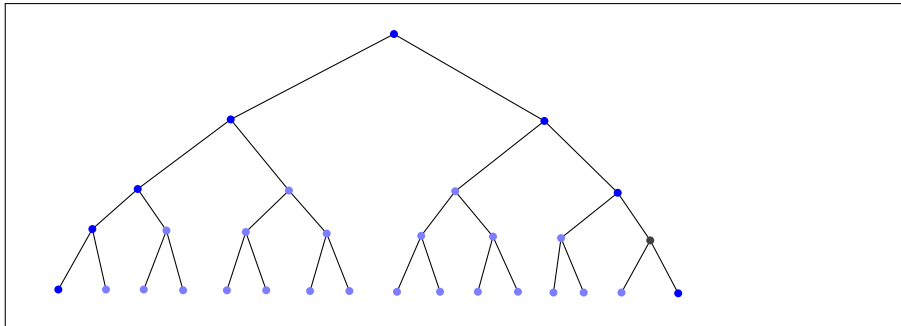
Very well: you have proved that there are countably many shoes; *how many socks*?

### 3.1.3 Every Perfect Binary Tree has an Infinite Path

A perfect binary tree is a tree with one root wherein every node has precisely two children. Probably best thought of as a connected digraph wherein every vertex is of outdegree 2 and every vertex but one is of indegree 1, with the solitary exception being of indegree 0. There are no decorations on the graph, neither on the edges nor the vertices.

The standard example of a perfect binary tree is obtained from the set  $\{0, 1\}^{<\omega}$  of finite sequences of 0s and 1s by discarding the labels at each node..

If one has a blackboard to hand when telling this story, one is tempted to start off drawing a perfect binary tree on it:



... which makes it obvious that this perfect binary tree at any rate (the one you have started drawing) has an infinite path. Start at the top and always choose the leftmost branch. What could be easier!?

### 3.1.4 Lagrange’s Theorem

There is a theorem of Lagrange that says that if  $H$  is a subgroup of  $G$ , then  $|H|$  (the “order” of, the number of elements in,  $H$ ) divides the order of  $G$ .

We all know the proof. The cosets of  $H$  partition (the carrier set of)  $G$ . And they’re all the same size, and that size is  $|H|$ . So clearly  $|H|$  divides  $|G|$ .

[Is this the place to exhibit the proof that they are all the same size, and to point out that they are not uniformly the same size?]

## 3.2 The Fallacy of Equivocation

There are plenty of other examples, but those four are sufficiently familiar and exhibit enough of the features common to all the examples to serve as generic examples. If you think that the proof-sketches above are sketches of actual proofs – that need only to be written out neatly to be satisfactory – then you are mistaken; as it happens, none of these proofs used the axiom of choice, whereas it is known that these results cannot be proved without some use of AC. *Ergo: these things are not proofs.* In trying to describe the sin of omission that is being committed here, the question before one is whether one is to reach for the word *fallacy* or the word *ellipsis*; the analysis I offer here is that the likeliest cause of your mistake is the commission of a *fallacy of equivocation*.

In a *fallacy of equivocation* an unauthorised conclusion is drawn by reading one of the terms in the argument in two different ways. One of my students supplied me with the following excellent illustration.

Bronze is a metal; all metals are elements  


---

 Bronze is an element.

This argument (as we say) “*equivocates on*” the word ‘metal’.

However in the three cases above we are equivocating on something less tangible than ‘metal’.

So: what fallacy of equivocation is being committed by the miscreants here? What are they equivocating between...? They are equivocating between datatypes. Datatypes?

## 3.3 Datatypes: an informal Introduction

We invoke ADTs as an explanatory device when there is a syncretistic error of thinking that two mathematical objects are of the same flavour when they aren’t; in those circumstances we want to identify the difference so that we can characterise the error as a fallacy of equivocation. The device of the ADT is the theoretical construct that enables this analysis. There may be other ways of doing it, but ADTs are fashionable and they do the job.

An Abstract Data Type (hereafter ADT) is a Way Of Being In The World. The different Ways of Being are distinguished from each other by what the Beings Who Have Chosen Those Ways can do. A **list** can, on request, give you its **head** and its **tail** (which is another **list**). It can also be constructed by the **cons** constructor from an object and another **list** (stick the object on the front of the list to obtain a new **list** of which it is the **head** and of which the old list has become the **tail**). **Ordered Pairs** will, when asked politely, reveal their two components, and can be constructed from two given objects by a **pairing** constructor. The two functions that take the pair and return the two components from which it was constructed are sometimes called the *destructors*. Generally an ADT is characterised by the operations it “supports”, and sometimes (but not always) by the ways in which its members can be constructed and taken apart. This talk of operations supported by an ADT was one of the motivations for developing the conceptual apparatus of ADT in the first place.

A lot of these ADTs are parametrised (“polymorphism”) by other ADTs. A **list** is typically a **wombat-list** (a **list** of **wombats**) where **wombat** is another ADT. **Ordered pairs** similarly, of course.

The idea of an Abstract Data Type is an example of what linguists call a *semantically open category*: we can invent new ones as our need for explanations and theorising changes over time. (Adjectives are a semantically open category, prepositions are semantically *closed*: you can’t invent new ones). In the course of discussing (below) whether or not every perfect binary tree has an infinite path we will be moved to conceptualise the ADT of **naked tree** and the ADT of **naked tree decorated with a map into  $\mathbb{R}^2$** .

The idea goes back at least as far as Tarski’s idea of a *similarity type*, and surely has roots in the informal notions the Viennese positivists employed to give an explanation of the oddity of “This stone is thinking about Vienna” (one of their examples) which is of course false, but false for reasons to do with misuse of abstract datatypes, and thereby furnished an example of what they called a ‘category mistake’. The idea became richer and more explicit in Edinburgh in the 1970s. As an explanatory/analytical device it is much more part of the currency of Theoretical Computer Science than of Mathematics. Datatypes crop up at this juncture because they are a nice way of explaining the four conundra we introduced in this chapter. However, this is a book about Set Theory, and the ADT of greatest importance to us will be that of **set**, so we may as well get a discussion of that out of the way now.

### 3.3.1 The Constructor(s) of the Abstract Data Type **set**

A throwaway remark of Kripke’s (quoted by Boolos [1]) has vivified an ageing narrative<sup>2</sup> of an *iterative* datatype of sets. You throw a lasso *and then* you tap with a wand the contents you have just captured to obtain thereby a **set** . . . which you then throw back into the cosmic abyss in which you are fishing with

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<sup>2</sup>From the German. . . *Narr*: a fool.

your lasso. Even if we do not separate the two actions of ensnaring the lasso-contents and of turning them into a set (and Kripke/Boolos does not separate them) there is still the thought that the members of the set-being-created are in some sense prior to that set being created. This construction is iterative, because you do it repeatedly.

So the constructor is the kit consisting of lasso-plus-wand. There doesn't seem to be a destructor ... which is a pity, beco's it would be nice to put the word *dismember* to good use.

There are three things that it is helpful to keep separate in our minds.

1. The question of what constructors (and perhaps destructors) **set** has;
2. The question about what operations the ADT **set** supports; and finally
3. questions about which axioms the world of **sets** obeys.

Although the distinction between them is not hard-and-fast the attempt to maintain it will be a helpful discipline.

We want to keep (1) and (2) separate beco's one-wand concepts of set and two-wand concepts both give rise to the one package of operations supported by **set**. However both (1) and (2) seem intimately connected to (3). (1) and (3) are connected beco's one- and two-wand conceptions of sets support the same operations but give rise to different axioms; (2) and (3) are connected beco's the question of whether or not **set** supports "give me an element or an error message if you can't" is obviously connected to AC.

There are three fundamental queries that any ADT of **set** must support:

- "*!Is x empty?*"
- "*!Is x a member of y?*"
- "*!Is x equal to y?*"

The third bullet isn't specific to sets: after all, There Is No Entity Without Identity. In the case of set theory we ensure this last by Extensionality. To check whether or not  $x = y$  we ask of each and every  $z$  whether or not it is in  $x$  and whether or not it is in  $y$ .

Not necessarily an argument for the axiom of foundation, since a second wand that creates complements (as in Church, Button, Forster) still gives us an ADT of **set** that supports  $!x = y?$  and  $!x \in y?$  and the test for emptiness. If we augment the wizard-kit with further constructors we have to be sure that the resulting ADT supports the three fundamental queries.

The lasso-plus-wand construction always succeeds; every time you throw out your lasso you get something, and every time you *ping!* that something with a wand you get something – a set. If you have more than one wand there is a danger that you may get the same set in more than one way, which is a feature one could do without (since it can greatly complicate the task of verifying extensionality) and that is a good reason for being careful in one's choice of wands.

It's tempting to try to think of proper classes as failed-sets, somehow. However, the best way of exploiting this imagery has it that the lasso-plus-wand construction always succeeds, so there is no way of thinking of proper classes as the result of the failure of a lasso to grip or of a wand to *ping!* properly. Instead, proper classes are what happens *after* the iterative construction. The iterative construction is never going to give you the Russell class, no matter how many or how kinky your wands, so if you want some sort of access to the Russell class (and not everybody does) you have to fit it in *after* the iterative construction. Thus proper classes are never available to be ensnared by lassos and never become members of anything. You might find that a proper class turns out to be coextensive with a set created earlier by your iterative process, and in those cases there doesn't seem to be any harm in simply identifying the proper class with the set with which it is coextensive; this means one can get away with thinking of sets as a special kind of class.

Two points to make about Kripke's lasso.

He missed out the wand beco's the only set he was interested in making from a lasso-contents was the set with the same extension as the lasso-contents. Once you try to make more than one set from a given lasso-contents (e.g. a set and its complement) you then realise that you have to actually *do* something, so you need a wand – in fact (in that case) two wands. But this tells us that Kripke needed a wand in the original case all along.

The second point is that Kripke was going to use his (unacknowledged) wand only in a setting where he was creating wellfounded sets, and starting from nothing. However the apparatus can be used more generally. See eg [?] and [?].

The lasso picture tells us that **set** supports “ $\iota x \in y?$ ” and “ $\iota x = y?$ ” but we mustn't jump to the conclusion that it supports

“Give me a member, or an error message if you are empty”.

There's nothing to say that it *doesn't* support this third operation, but support for the third operation doesn't come from the lasso story.

There is an obvious connection between this operation and the Axiom of Choice. The ability to demand a member of a set you meet on the Clapham omnibus is clearly a version of the axiom of choice. Can we make simultaneous demands of this nature of all sets simultaneously? If demanding a member of  $x$  and demanding a member of  $y$  are actions that are not *entangled* then clearly if we can demand a member of any set we meet then every family of sets has a choice function. If they are entangled then the endeavour to wellorder the universe will be a supertask, and we may have reservations about supertasks. But enough has been said to make it clear that the third operation should be regarded with suspicion. You may like the axiom of choice, or you may not, but even if you do you should not pretend that it emerges from the lasso picture.

what datatypes did mathematicians have before they invented sets?

Is the constructive ADT **set** different from the classical ADT **set**?

What operations does the ADT **situated set** support?

Always good to ask what operations a datatype supports. There is a partial order of complexity on ADTs – e.g.,

`set < multiset < list`

or

`group < ring < ordered ring < ordered field < complete ordered field`

determined by the operations the ADTs support, or (equivalently, one hopes) the amount of structure they have. Thus for ADTs  $\mathcal{A}$  and  $\mathcal{B}$  we say  $\mathcal{A} \leq \mathcal{B}$  if every  $\mathcal{B}$  is an expansion of an  $\mathcal{A}$  and every  $\mathcal{A}$  is a reduct of a  $\mathcal{B}$ .  
expansion is defined on p 22.

So what operations does the ADT `set` support?

We observed above that there is no entity without identity, so every ADT has to support “Are you equal to  $x$ ?”. We have a choice between just stipulating that the ADT `set` supports testing for equality, and somehow deducing it from prior principles. If we are squeamish we could try to derive a test-for-equality from foundation or determinacy of the games  $G(x = y)$ . Why is it better to derive it than to stipulate it? Theft over honest toil.

If `set` supports “are you equal to  $y$ ?” then it will also support “Is  $y$  a member of you?” since one simply asks “Are you equal to  $y$ ?” of every member  $y$  of  $X$ . That is, you play  $G(y = x)$  for every  $x \in X$ . Every game concludes, but there might be no finite bound on the lengths of the games. Perhaps it’s simplest just to build in membership-testing *ab initio*.]  
Have we defined this game??

The ADT `set` does not support “Give me a random element”. That is to say, the collection of all `sets` does not come equipped with a global nondeterministic choice function, something that picks a member from any `set` but without any guarantee that if you ask a `set` for a random member it always gives you the same member. To see this, consider the naïve way in which one uses AC to wellorder any set  $x$ . You keep on picking random members from  $x$  (without replacement) in a process indexed by the ordinals until you run out of members. Hartogs’ lemma tells you you will succeed. Since you never ask any subset of  $x$  for a member more than once the fact that your choice function is nondeterministic makes no difference. Therefore the assumption that `set` supports “Give me a random member or, failing that, an error message if you are empty.” is equivalent to AC.  
duplication

To properly deploy the understanding of the concept of datatype in the struggle to understand AC you have to free yourself from the idea that – for example – there is this object which is  $\mathbb{R}$  and can be thought of indifferently as a total order, a field, an ordered ring, a real-closed field, a complete ordered field etc. There is no one object in that sense (or at least, it isn’t helpful to think as if there is); instead there are all these objects:  $\mathbb{R}$ -as-an-ordered-set,  $\mathbb{R}$ -as-a-real-closed-field . . . etc, all obtained by clothing the `naked-set` of reals with the various appropriate gadgets.

Distinguishing between these various manifestations of the reals seems a very unnatural move to most working pure mathematicians; they are not interested in thinking of the reals (or for that matter anything else) as a set, but rather as a



rich and complex structure with more aspects than you can shake a stick at, and certainly more than you can be bothered to count<sup>3</sup> Indeed, the very idea that mathematical structures are sets-with-knobs-on couldn't even get started until mathematicians acquired/invented the concept of set less than two centuries ago. If you think of the entire history of human mathematics as compressed into a single day, Set Theory appears a few minutes before midnight, just after the extinction of the trilobites.

For them the natural point of departure is the reals themselves. The reals can do, and act, many things – “one man in his time plays many parts” after all – so one can think of the reals as ... a field, as an ordered set, as a topological space, as a vector space ... but the tendency is to think that the reals remains the same though all these retellings, and to think that there is no significance – no *cost* – attached to the decision to change one's viewpoint. This is an error of attachment.

To use another bit of logical (well, *philosophical*) jargon, our picture of the reals is rather *intensional* ... we think of the reals as an entity that has a soul, and a soul in which all the other manifestations of  $\mathbb{R}$  – complete ordered field, vector space over  $\mathbb{Q}$ , etc etc – somehow partake. How else are we to explain the common mathematical parlance of [for example] “Consider the reals as an additive abelian group”? I am not saying that there is anything *wrong* with this way of thinking; what i am saying is that there are times when one has to stand back from it.

Of course  $\mathbb{R}$  is only one example of a structure that has lots of reducts and natural expansions, all of which one naturally wants to think of as being the same thing; there are other examples. And it is in (some of) these other settings that the intensional way of thinking can obscure uses of the axiom of choice and lead us into error.

These distinctions that standard mathematical practice tends to blur are actually in themselves legitimate objects of mathematical study, and we often use the word “(Abstract) Data Type” (or *ADT*) to describe the kinds of structure that conventional practice equivocates between: *Group*, *ring*, *vector space*, *list*, *tree*, ... and there has been – since the 1960's – an interesting and growing literature on the subject, mainly generated by (theoretical) Computer Scientists.

Let us now consider what this identification-of-different-manifestations looks like from the point of view that considers all these manifestations to be distinct mathematical objects, mathematical objects distinguished by their datatypes. Any attempt to reason in a way that identifies these (now officially distinct) entities commits ... a *fallacy of equivocation*.

If, Dear Reader, you are an ordinary working mathematician, you probably

We need to move this following paragraph to somewhere earlier in this chapter

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<sup>3</sup>This reluctance to think of the reals as a **naked-set** – or even to contemplate the **naked-set** that you obtain from their conception of reals by throwing away all the extra (fun!) structure – is exemplified by the striking lack of interest that most mathematicians show in the properties that the reals has *qua set*. It is remarkable how many people with degrees in mathematics from reputable institutions do not know that the cardinality of the reals so much as has a *name* let alone that that name is ‘ $2^{\aleph_0}$ ’ rather than – say – ‘ $\aleph_1$ ’. Ask around, you'll see what I mean. And we should not be surprised by this: most of the interesting questions about the set-with-knobs-on that is  $\mathbb{R}$  concern the knobs not the naked set.

think of the distinction-between-datatypes to which I am pointing as a mere contrivance, a pointless and annoying artefact of post-nineteenth-century mathematics. I hope to persuade you that it is quite useful, and that it is really quite straightforward: no structure is the same as any of its proper reducts – and it is only by grasping this fact that you can come to understand the independence results concerning AC.

### 3.4 Expansion and Reduction

The rationals as an ordered set are an ordered set; the rationals as an abelian group are a group. They have the same underlying set but are distinct structures. Many mathematicians grumble about being leant on to observe the distinction between a set and a set-with-knobs-on, but the distinction does make a lot of things clear. A group is not the same as the set of its elements. An ordered set is not the same as a naked set. Model theory has the wonderful word **reduct** which is very useful here. For example, the rationals as an ordered set are a reduct of the rationals as an ordered group. The converse operation is **expansion**, and the rationals as an ordered group are an expansion of the rationals as an ordered set. The rationals as an ordered set are a *reduct* of the rationals as an ordered group because one obtains the first object from the second by “*throwing away*” some structure; the rationals as an ordered group are an *expansion* of the rationals as an ordered set because one obtains the former from the latter by *adding* some structure. A key observation is that a structure and an expansion of it remain distinct even if one can be turned into the other in only one way. The abelian group of natural numbers less than  $p$  ( $p$  prime) with addition mod  $p$  can be turned into a field in only one way, but – even so – that field is not the same thing as the abelian group, let alone the set  $[0, p]$  of natural numbers less than  $p$ .

Compare the way in which Kripke/Boolos conceals the wand; mathematics is the art of concealment  
This isn't quite right

An ADT can be a subtype of another type: **naked tree decorated with a map into  $\mathbb{R}^2$**  is a subtype of **naked tree**. If **A** is a subtype of **B** then often things of type **A** can be turned into things of type **B** by decorating them with stuff; this is called *expansion*; conversely things of type **B** can be turned into things of type **A** by discarding decorations; this is called *reduction*. The rationals as a field are an expansion of the rationals as an abelian group – which of course are a reduction of the rationals as a field. A two-coloured graph is an expansion of a two-colourable graph.

rewrite this properly

### 3.5 Back to our four examples

Thus armed, we can return to the four examples we considered at the beginning of this chapter.

#### 3.5.1 The Perfect Binary Tree

The fact that is obvious is not the fact that

A perfect binary tree has an infinite path; (i)

but the fact that

A perfect binary tree *with an injection into the plane* has an infinite path; (ii)

since we cannot follow the rule “take the leftmost child in each case” unless we can tell what the leftmost child is, and this information is provided for us not by the tree itself but by its injection into the plane. Let us coordinatise the plane: equip it with an origin and two axes. Then the two children of any one node have two distinct addresses that are ordered pairs of reals. When constructing an infinite path we extend it from a given bud node by proceeding to the child node whose address is the lexicographically first of the two addresses of the two children.

Are not (i) and (ii) the same? They certainly will be if any two perfect binary trees are isomorphic, since if even one of them is equipped with a bijection into the plane that one will have an infinite path and you can then use the isomorphisms to find copies of that infinite path in other perfect binary trees. And aren’t any two perfect binary trees isomorphic? Isn’t that obvious?

No, it isn’t: what is obvious is *not*

Any two perfect binary trees are isomorphic; (iii)

but

Any two perfect binary trees *equipped with injections into the plane* are isomorphic; (iv)

and (iii) and (iv) are not the same. A perfect binary tree is not the same thing as a perfect binary tree equipped with an injection into the plane.

Perhaps  
about  
Lemma

Say something  
König’s Infinity

### 3.5.2 The Countable Union of Countable Sets

We need to be alert to the difference between *countable set* and *counted set*. A countable set is a naked **set** that just happens to be countable – there is in the universe somewhere a bijection between it and  $\mathbb{N}$ , but the whereabouts and nature of this bijection have not been revealed to us; we are like the hero in the mediæval romance who knows there is somewhere in the universe a magic sword to cut the head off the dragon that guards the ring, but he has not been told where it is nor what it looks like. A **counted set** is not a mere naked **set** at all, but is a structure consisting of a **set** actually equipped with a bijection with  $\mathbb{N}$ . According to Classical Logic **knowing of a naked set that it can be counted is not the same as being in possession of a designated counting of it; your knowledge might have been obtained from a nonconstructive proof**. The set and the set-equipped-with-a-counting are two different kinds of object, different ADTs.

What operations does the ADT **counted-set** support? Presumably all the operations supported by **set** (whatever they are) but in addition we can ask it for its  $n$ th member. Is it a random access device or a serial access device? (i.e., do we obtain the  $n$ th member from it by walking along a trail consisting of the first  $n - 1$  members?) For our purposes it won't matter.

(Brief remark: we are overloading “union” below to mean all four operations: countable/counted sets of countable/counted sets.)

Given that we can now distinguish between countable **sets** and **counted sets**, our original claim *A countable union of countable sets is countable* multifurcates into:

- (Ci): A counted union of counted sets is counted;
- (Cii): A countable union of counted sets is countable;
- (Ciii): A counted union of countable sets is countable.
- (Civ): A countable union of countable sets is countable.

and of course (Civ) is the formulation with which we started<sup>4</sup>. The contrast is between (Ci)–(Cii) – which are both provable from first principles – and the last two, which aren't.

These four assertions sound so similar that it is easy for the incautious to confuse them. Fortunately we are now in a position to disentangle them. The zigzag construction from page 14 will be essential.

Let start with (Ci). It sounds snappy but unfortunately it's not literally true as stated. It's a snappy shorthand for the – perfectly correct – observation that there is a canonical way of obtaining a counting of the sumset of a counted family of counted sets: the zigzag algorithm. I shall follow general practice in using this shorthand.

Now let's think about (Cii). The zigzag algorithm gives us a way of taking a counted bundle of countings and returning a counting of the union of the sets counted. Reflect that the zigzag construction wants its input to be a counted set of counted sets. Let  $\{A_i : i \in \mathbb{N}\}$  be a counted set of counted sets. At stage  $n = \binom{x+y+1}{2} + x$  the algorithm says to  $A_x$ : “Give me your  $y$ th element”. The zigzag algorithm is the function  $f$  in the syllogism. ‘ $A(x)$ ’ says that  $x$  is a counted set of countings, and ‘ $B(y)$ ’ says that  $y$  is a counting of the union of the sets counted. So (Cii) is straightforwardly provable from first principles, and we have made no use of the axiom of choice.

For each  $n$ , do the following. Decode  $n$  as  $\binom{x+y+1}{2} + x$ , obtaining  $x$  and  $y$ . Ask  $A_x$  for its  $y$ th member. All these actions can be performed simultaneously, since they do not interfere with each other.

Observe that the execution of the zigzag algorithm is not what we will, later – in section 6.4.2 – come to call a *supertask*: a supertask is a sequence of tasks of transfinite length, done in sequence not in parallel. All the actions in a run of the zigzag algorithm can be done simultaneously since, for any  $n$ , we can ask a counted set for its  $n$ th member without first having asked it for its  $k$ th member,

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<sup>4</sup>I learnt this formulation from Conway – hence the ‘C’.

with  $k < n$ . (This is true even if **counted set** is a serial access device). Just send the number  $\binom{x+y+1}{2} + x$  to the  $x$ th member of the  $y$ th counted set.

Those drawn to realizability semantics for constructive logic might like to think of the zigzag algorithm as a *realizer* of the universally quantified conditional (Cii). A realizer<sup>5</sup> of a conditional  $A \rightarrow B$  is a function from the set of realizers of  $A$  to the set of realizers of  $B$ .

Now consider (Ciii). We have a countable family of countable sets. OK, let's count the family, so we can think of it as  $\{A_i : i \in \mathbb{N}\}$ . We can do that with a single choice, so we are not using AC.

Reflect that the zigzag construction wants its input to be a counted set of counted sets, as it was in case (Cii), where everything was tickety-boo. In contrast, here, faced with a counted set  $\{A_i : i \in \mathbb{N}\}$  of merely countable sets it won't run. Let's think a bit about this.

This is why we have to have the datatype section after the one-choice-is-allowed section

**ZIGZAG ALGORITHM** (to **XSA**<sub>1</sub>)      “Give me your first element”.

**A**<sub>1</sub> (*bursting into tears*)      “I’m a *set* not a *wellordering* –  
I don’t *do* “first” elements; I don’t  
need this; I want my mummy!!”

In computer science terms what happens is that we here encounter a **typecheck\_error**, and we would get a message from the operating system saying something along the lines:

I expected a counted-set, but I found a naked-set.

The reader may think that that fact – that  $A_1$  is a **set** not a list – is a mere detail, that you can turn  $A_1$  into a **list** (and then ask for its first member) at run-time. But you can't. You have to *expand* it into a list – at *compile-time* as it were. Expanding all the  $A_i$  to **lists** simultaneously at compile-time is where you use the axiom of choice.

This next paragraph doesn't have to be tied to this example

The reader may by now be thinking: “OK, so what are these ADTs? I've had quite a lot dangled in front of me: **set**, **counted-set**, **binary-tree**, **binary-tree-with-an-injection** .... Can we have a shopping list please?” 'Fraid not. We make up these ADTs as we go along, invoking them locally to provide contrastive explanations. As we remarked on p 17, ADT is an open category.

This is probably the correct juncture to make the point that the only reason why so many people are confused about this is because they were lied to about the zigzag construction. Some such explanation is required, because it is not natural to be confused about things that are this easy. And the lie is a salient candidate for the correct explanation.

Incidentally this is the first place where the *lie* is mentioned.

<sup>5</sup>The 'z' in this word is not a violation of the spelling rules of the British English in which I am writing this essay: 'realizer' (as in constructive logic) is an American loan word and we retain the original spelling in order to flag its distinctive use.

**OK, so how *do* we use AC?**

Let us take AC in the form we first encountered it here: every set of pairwise disjoint nonempty sets has a transversal. If we have to invoke AC here, the question is, which family of pairwise disjoint nonempty sets is it that we desire a transversal for?

A moment's reflection will persuade the reader that we can assign a counting to each  $A_i$  by applying AC to the (countable) family of sets-of-countings; for each  $A_i$  there are plenty of countings –  $2^{\aleph_0}$  of them to be precise – and we need to decorate each  $A_i$  with one of them in order to expand them into counted sets suitable for the zigzag construction.

In effect we have used AC to get us back into situation (Cii); we use AC to expand every member of a (counted) family of objects of type **naked-set** into a counted family of objects of type **counted-set**.

In fact this use of AC is necessary: it cannot be proved in pure set theory that a union of countably many countable sets is countable, but demonstrating this unprovability is a nontrivial task. We discuss below (section 6.5.3) what can be said in the absence of AC.

Do we Say something about independence of AC in vol 1?

Somewhere here emphasise the elementary point that for all  $n \in \mathbb{N}$  a union of  $n$  countable sets is countable.

Put this in the right place

The fallacy of equivocation here is between (Cii) and (Ciii).

**3.5.3 Lagrange**

People who are interested in complexity of computations and time costs etc pay a great deal of attention to the way in which a data object is presented. Such people will be receptive to explanations of why it matters whether you specify a coset as  $C$  or as  $gH$ .

We want to say that  $|H|$  divides  $|G|$ , of course. That is to say that there is a set  $C$  such that  $|C| \cdot |H| = |G|$ . That – in turn – is to say there is a bijection between  $C \times H$  and  $G$ , since that is how multiplication of cardinals is defined ... so every element of  $G$  can be represented by an ordered pair  $\langle h, c \rangle$  with  $h \in H$  and  $c \in C$ .

*But what is this set  $C$ ?*

If we cannot find such a  $C$  then all we can say is that  $G$  is the union of a certain number –  $c$ , say – of things all the same size  $h$ . But in the absence of AC the expression “the cardinality of a union of  $c$ -many things each of size  $h$ ” cannot be relied upon to denote the cardinal  $c \cdot h$ . The union of countably many pairs (e.g. of socks, yes) cannot be assumed to be of size  $2 \cdot \aleph_0$  (which of course is  $\aleph_0$ ) in the absence of AC.

How do we find such a set  $C$ ? This is an instance of a general problem, but in this case it's clear what we have to do. Every left coset of  $H$  is a bijective copy of  $H$ , so for each such coset  $H'$  we pick  $g \in G$  such that  $gH = H'$  and call it  $g_{H'}$ . Then the set  $C$  we want is  $\{g_{H'} : H' \text{ is a left-coset of } H\}$ . Then every  $g \in G$  really does correspond to a unique pair  $\langle h, g \rangle$  with  $h \in H$  and  $g \in C$ .

What a lot of faff! The average pure mathematician revolts at the thought. Why do they revolt? Because they have been happily equivocating between two data types, and are now being told not to. Indeed even being forced to listen to talk like ‘data type’ is an unwelcome distraction. In this case the two data types are

- (i) the data type of (naked) **set**, which is a set of group elements, and the other
- (ii) is that of **decorated set** which is a coset  $C \subseteq G$  decorated with one of the  $gs$  such that  $C = \{gh : h \in H\}$ .

It’s rather like the difference between countable **set** and **counted-set**. In fact in one sense it is *exactly the same distinction*: it’s the distinction between two datatypes.

That is why I was right to use the letter ‘ $H$ ’ for the coset, rather than write ‘ $gH$ ’. ‘ $gH$ ’ is not really a natural notation for denoting a coset, but it is a natural notation for denoting a *decorated coset*.

Of course if there is such a  $C$  you can take it to be the set of  $H$ -cosets.

### Change the Definition of Multiplication . . . ?

The thoughtful and suspicious reader might look at this explanation and say that the difficulty to which the axiom of choice purports to be the answer arises only beco’s of the manner in which we have defined multiplication. We have been saying of three cardinals  $\mathfrak{a}$ ,  $\mathfrak{b}$  and  $\mathfrak{c}$  that  $\mathfrak{a} = \mathfrak{b} \cdot \mathfrak{c}$  if and only if there are sets  $A$ ,  $B$  and  $C$  such that  $\mathfrak{b} = |B|$ ,  $\mathfrak{c} = |C|$  and  $\mathfrak{a} = |B \times C|$ . Perhaps we should instead say that  $\mathfrak{a} = \mathfrak{b} \cdot \mathfrak{c}$  if and only if a set of size  $\mathfrak{a}$  can be expressed as a union of  $\mathfrak{b}$  things each of size  $\mathfrak{c}$ . On the face of it, this sounds sensible. However, for this definition to succeed we would have to show that:

whenever  $A_1$  and  $A_2$  are two sets both of which can be written  
as a union of  $\mathfrak{b}$  things each of size  $\mathfrak{c}$  then  $|A_1| = |A_2|$ . (Eqn)

The point is that if you define multiplication in this – wrong – way then the product of the two cardinals  $|A|$  and  $|B|$  isn’t well defined, since it doesn’t just depend on the two cardinals  $|A|$  and  $|B|$  but depends on  $A$  and  $B$  as well. Proper (cardinal) multiplication corresponds to an operation on sets (cartesian product) for which equipollence is a congruence relation. This new operation on cardinals does not correspond to any operation on sets for which equipollence is a congruence relation.

The reader has probably guessed by now that proving equation (Eqn) requires the axiom of choice.

The reader might also think (as I did, for a while) that the fact that the axiom of choice enables us to prove that these two definitions of cardinal multiplication are equivalent is what lies behind the name “The Multiplicative Axiom”. That is not so. The significance of AC in this context is that it enables us to prove that *infinite* products (for cardinals) are defined: a product of nonempty sets is nonempty. If we want to show

It might be an idea to supply  
a discussion

that a product  $\prod_{i \in I} \mathfrak{a}_i$  of a family  $\langle \mathfrak{a}_i : i \in I \rangle$  of cardinals is well defined, we use AC to pick a representative set  $A_i$  from each  $\mathfrak{a}_i$  and then use AC *again* to show that the direct product  $\prod_{i \in I} A_i$  is nonempty. Of course the product of the family  $\langle \mathfrak{a}_i : i \in I \rangle$  of cardinals is  $|\prod_{i \in I} A_i|$ . In fact we need AC even to define infinite *sums* of cardinals, because we need it to pick a representative from each of the cardinals being summed. But that's all we need it for: we don't need it (in the way we have just used it for infinite products) to show that the answer is non-degenerate.

### 3.5.4 Socks

How many shoes does the squillionaire have if he has countably many pairs of shoes? You want to say “countably many” and you are right, but why are you right? Let's go back to basics. What is a countable set? One that is in bijection with  $\mathbb{N}$ —one that has a *counting*. So, given that we want to infer that the set of shoes is countable from the information that the set of pairs of shoes is countable, what we should be doing is arguing from the existence of a counting for the set of *pairs* to a counting for the set of *shoes*. The appropriate response to a challenge to do  $X$  is to do  $X$ ; classroom experience teaches me, however, that that is not what most mathematicians actually do when confronted with this challenge. They tend to say things like “it's obvious” or “I can count them”; “just pick one, and then another ...”. It's not clear to me (and I suspect not to them either) whether they think (i) that they have risen to the challenge, or have instead (ii) justified their decision not to. A certain amount of tact is required on the part of those who insist (as I do in these circumstances) that if you claim that a set is countable then you have issued an IOU that commits you to paying the bearer on demand with a counting of it. Merely *outlining* an answer is not the same as actually *supplying* an answer. At some point you have to either exhibit an enumeration or admit that you don't know how to. No stonewalling!

**ME**           What does it mean to say that a set is countable?  
**VICTIM**    It means you can count it.  
**ME**           The set of shoes is countable?  
**VICTIM**    Yes.  
**ME**           So it can be counted?  
**VICTIM**    Yes!  
**ME**           I challenge you to count it.  
**VICTIM**    [*flannel and bluster*]  
**ME**           OK! If you know how to count it (but don't want to show me) at least tell me which shoe is the first shoe according to your scheme. While we're about it, which is the 15th?  
**VICTIM**    [*silence*]  
**ME**           I don't believe you can count it.  
**VICTIM**    [*more flannel and bluster, plus indignation*]  
**ME**           Prove me wrong in front of witnesses!  
               [*to the audience*]



He can't count the shoes! He has a Ph.D. in Mathematics and he can't count shoes?<sup>6</sup> He probably can't even tie the laces on them! Can anyone here show him how to do it?

The point is easily missed. It is of course true that it doesn't matter in the least (for the rube's purposes or mine) which counting you use. But it does matter a very great deal that there is a counting, so it matters that you should be able to produce one on demand – even tho' it doesn't matter which one you produce. People who are affronted by the demand that they produce a counting are confusing two things, one of which matters and the other of which doesn't. It's another fallacy of equivocation.

*It doesn't matter which bijection I use*

is not the same as

*It doesn't matter whether I can produce a bijection or not.*

The first is true and the second is not.

Sophisticates might discern here a connection of ideas with the constructive critique of classical mathematics. Constructivists never admit that  $(\exists x)F(x)$  has been proved until something that is  $F$  has been produced. My insistence here, that people who say the set of shoes is countable should be willing to exhibit a counting, isn't really based on these (what a student of mine used to call) *exhibitionist* scruples. I'd be perfectly happy with a nonconstructive proof that there is a counting of the shoes; it's just that prompting the rube to produce a constructive proof is more effective polemically and pædagogically; in any case all the obvious proofs that the set of shoes is countable are in fact straightforwardly constructive so there is no additional cost to the victim in demanding of them that the proof they come up with should be constructive.

A combination of cajolerie and threats of public humiliation will – eventually – persuade most mathematicians to get off their high horse and condescend to say, out loud:

“Yes, the left shoe from the  $n$ th pair can be sent to  $2n$  and the right shoe from the  $n$ th pair can be sent to  $2n + 1$ .”

(Tho' this is often done with bad grace, as much as to say that any insistence on an actual bijection is the height of unreason). This does, indeed, show that there are  $\aleph_0$  shoes. Good! Blood from a stone is better than no blood at all (in fact it's the best blood there is). The point of insisting – at gunpoint – that the rube actually come up with a way of counting the shoes is essential for what lies ahead, for it is only once they have done that that they will appreciate the significance of their inability to do the same for the socks when the time comes...

... which it does now. How many socks? One wants to say “countably many” of course. This invites the same challenge: “Count them” and, with any luck, the victim will provide (or at least initially reach for) the same answer as they

eventually reached with the shoes: send the left sock from the  $n$ th pair to  $2n$  etc. Of course, for that to work you have to have always a readily identifiable *left* sock and a readily identifiable *right* sock, and of course only mathematicians have odd socks. Old jokes are the best.

With the shoes you have an algorithm that you can hand on to unskilled unsupervised labour so that you can bunk off for a leisurely pint at the restaurant at the end of the universe where they will join you at the End of Time when they've finished. With the socks there is no such algorithm.

Part of the attraction of this parable (for the preacher) is that, at first blush, the two cases – socks and shoes – look essentially equivalent, and this renders all the more striking the revelation that they are not equivalent after all. How can they look so similar when they are so different? The answer is that the very physical nature of the setting of the parable has smuggled in a lot of useful information. It cues us to set up mental pictures of infinitely many shoes (and socks) scattered through space. The shoes and socks – all of them – are (or can with only a minimal amount of abstraction be thought of as) extended regions of space and – as such – they all have nonempty interior. Every nonempty open set in  $E^3$  contains a rational point (a triple all of whose entries are rational), and the set of rational points has a standard wellordering. This degree of asymmetry is enough to enable us to choose one sock from each pair, as follows. In any pair of socks, the two socks have disjoint interiors<sup>7</sup> and both those interiors contain rational points. Consider, for each sock, that rational point in its interior  $S$  which is the first in some standard wellorder of the rational points, fixed in advance. For example there will be a least natural number that appears as the denominator of a coordinate of a rational point in  $S$ , and only finitely many rational points decorated with that minimal natural, and it's not hard to develop this idea into a choice function on open subsets of 3-space. This will distinguish between the socks, since one will have been given a rational point earlier in the canonical ordering than the rational point given to the other. The physical intuitions underlying this last argument make it very clear to us that we can pick one sock from each pair – as indeed we can. Space is *just sufficiently* asymmetrical for us to be able to explicitly enumerate the socks in countably many pairs scattered through it.

There is another proof, at least if we can shrink each sock to a point.  
Every set of isolated points in a Polish space is countable, even without choice

So we have another example of a fallacy of equivocation, this time between:

*Every countable set of pairs has a choice function* (P)

and

---

<sup>7</sup>All right! The two socks in your pair of socks might be folded into each other the way your mother used to do it, so their interiors are not disjoint. However even in these circumstances their interiors  $S_1$  and  $S_2$  are at least *distinct*. The first rational point in the symmetric difference  $S_1 \text{ XOR } S_2$  will belong to one of the two socks, and we can pick that sock!

Every countable set of pairs of open subsets of  $E^3$   
has a choice function ( $P'$ )

( $P'$ ) does not need the axiom of choice, and it ought to be obvious. However I suspect it's worth banging the drum for a proof, as we have just done. In contrast ( $P$ ) does need the axiom of choice, but it looks obvious if you confuse it with ( $P'$ ).

... should say something about how  $P'$  is obvious to us because of our physical intuitions rather than for any mathematical reasons. This might matter.

In summary:

*Typically, when someone believes of a consequence of the axiom of choice that it is obvious, it's because they have committed a fallacy of equivocation.*

This is true *typically* rather than *invariably* but the reader is encouraged to take it personally anyway:

**If you find yourself thinking of some familiar assertion that it looks obviously true without recourse to the axiom of choice, while nevertheless having at the same time access to expert testimony to the effect that AC is needed for it, then you should go looking for a fallacy of equivocation.**

It may be helpful to think of many of these fallacies of equivocation as failures to attend to the question of which datatype one is using. That is arguably the situation with the four cases we have considered.

- In the case of the socks we are failing to distinguish between `naked-set` [of socks] and `set-equipped-with-injections-into-space` [of socks embedded in space];
- In the case of the perfect binary trees we are failing to distinguish between things of datatype `tree` and things of datatype `trees-equipped-with-an-injection-into-the-plane`.
- In the case of the countable union of counted sets we are failing to distinguish between `naked sets` and `counted sets`.
- In the case of Lagrange's theorem it's a failure to distinguish between cosets as `naked-sets` and cosets as `decorated-sets`.

## 3.6 How the fallacy gets committed

Whatever else it is, mathematics is at least a social activity, and a part of becoming a mathematician is learning how to talk like a mathematician. People

Not mentioned yet  
certificates

learning mathematics who have learnt to say things like “let  $gH$  be a (left-)coset of  $H$  in  $G$ ” think they are merely learning how mathematicians talk, but they are actually taking on board a great deal more than that. Several philosophical communities have the expression ‘analytic truth’ which means [roughly!] something whose truth is ascertainable merely by analysing it (rather than by checking the way the world is). Ever since Quine made the point a lifetime ago it has been a commonplace among philosophers in his tradition that your decisions about which propositions are to be analytic are made as soon as you choose a language. For those who want a way in to this literature <https://plato.stanford.edu/entries/analytic-synthetic/> is as good a place to start as any. They are learning a language all right, but it is a language that has induced its users to assume the axiom of choice by artfully bundling it into the machinery they use.  $gH$  is not a mere left-coset of  $H$  in  $G$  – a **naked-set**; it is a left-coset decorated with a certificate. That is no more the same thing as a mere left-coset any more than the rationals as an ordered field are the same thing as the rationals as an abelian group. Such people are not merely learning a language, they are unwittingly adopting unacknowledged assumptions. Sometimes when you eat a mushroom you are taking in more than just butter and garlic.

### OK, so how *does* it get committed?

Mathematics is not revealed, anew and afresh, to each generation. It is transmitted by toiling professionals – whose expositions may be trapped in local optima – to bright young minds that cannot be expected to absorb in a handful of lectures all the facts and all the ill-articulated pitfall-avoidance skills that their elders have somehow accumulated. It is never possible, when handing on the torch to the next generation, to tell them everything at once. You paint a broad picture, leave some details out, tell a few jokes to make them comfortable and – above all – you *don’t frighten them*. Respecting the need for omissions sometimes results in the telling of outright lies, and the way in which first-year students are told that a union of countably many countable sets is countable is a case in point. N.B. the lie is not “a union of countably many countable sets is countable”; the lie is the claim that the usual story is a *proof*. Reasonable people can disagree about whether the axiom of choice ought to be embraced; reasonable people can disagree about whether a policy of lying to children is defensible in this case; what reasonable people are *not* free to disagree about is whether or not the story is a lie. I can imagine that some readers will baulk at this claim, but they shouldn’t. We know that some invocation of a choice principle is needed to prove that a countable union of countable sets is countable, so any purported proof that doesn’t invoke any such principle is defective. Nowadays we have objective criteria for whether or not a proof is correct, in the form of computer-verification of proofs. And we all know what the verdict would be.

Hidden Curriculum!

Say something about how one feels sympathy with people who confront the expository problem.

[If you want to defend current practice (and it may well be entirely defensible) you have to do it on the basis that it is one of those cases where it is all right to lie to children. Part of that will be an argument that there is no other way of doing it. I will argue that there is a way of doing it without lying, and that is to explain datatypes and casting.]

Even if the mathematics lecturer is not him/herself in the grip of the fallacy of equivocation, the elisions and glossings-over caused by the pædagogical need to press on to the mainstream subject matter of the course have the effect that the students are led to commit the fallacy on their own account. This is because committing the fallacy is the simplest way of exhibiting the behaviour – namely not worrying about AC – that is being exhibited to them by their lecturer.

This is the primrose path. You start off by eliding uses of AC from the proof, on the grounds that the details it would involve are not germane to your immediate purposes: the lecturer can be forgiven for feeling that the distinction between **counted-set** and a countable **naked-set** is one that serves no *immediate* purpose for the first year student, and that accordingly it is entirely proper to gloss over it for the moment, and to leave to lecturers of subsequent years the task of explaining what is going on.

Thus it happens that the first-year students are told that a countable union of countable sets is countable, and they are shown the zigzag picture ... and then the caravan moves on without further explanation. They subsequently learn on the grapevine that there are these sad weirdos called *logicians* who insist that this – by now familiar – **fact** (namely that a countable union of countable sets is countable) apparently needs this axiom called “*the axiom of choice*”(!!) Mostly they aren’t told why, so – rather than make use of the countable/counted distinction (which in any case they haven’t been told about) – they innocently and uncomprehendingly embrace the axiom by means of an inference-to-the-best-explanation.

So what happens in their second year, when someone should be dotting the *i*’s and crossing the *t*’s on the dodgy first-year proof that a countable union of countable sets is counted? They are then lectured by someone *who has been through the same process, and made the same mistake*. (A policy of *putting off the explanation until some suitable later date* is always likely to run into trouble if there is no-one whose responsibility it is to ensure that the explanation is ultimately provided.) Never telling the student these things is a sort of tail-event; at any stage there is the possibility of providing an explanation, so it can always be put off. It is only at the end of time that one knows one has failed, and by then it is too late.

‘Lying’ ... Isn’t that a bit harsh? Perhaps nobody has been actually *lied to*, not literally, not *strictly*, but they have been victims of a policy of telling judiciously selected half-truths – albeit with the best intentions. Such policies do not reliably achieve their ends. I remember being told the story of the child that was told that if it sucked its thumb it would swell up and burst. The child takes this on board, as children always do. Next day the child sees on the bus a woman who is 7-months-pregnant and says to her reproachfully “Anybody can

see what you’ve been doing”.

There are people who brazenly defend this policy. One of my colleagues (Imre Leader) says that one doesn’t routinely flag uses of the axiom of pairing when giving a proof, so why should one have to flag AC? The answer is as follows. The only reason for wanting to flag all uses of the axiom of pairing is if one is trying to reconstruct mathematics inside set theory and is concerned to keep track of which bits of set theory one is doing, in order to prove a point. That is a very special situation to find oneself in, and most of us don’t find ourselves in it, so we don’t flag uses of the axiom of pairing nor – usually – any other axioms of set theory. The reason why we flag uses of AC is not that AC is a set-theoretic axiom and we want to flag all uses of the axioms of set theory, rather it’s because AC is an important distinct mathematical principle: if you’re carrying it, you’d better declare it.

See the chapter in vol 1.  
Rewrite this

## Chapter 4

# Finitely many Choices ... but what is a Choice?

### 4.1 Thinking you need the axiom when you don't

If nobody brought you up to recognise the axiom of choice when you see it, but you learnt behind the bikesheds that this kind of thing goes on, you might startle at shadows. For example you might think that you need AC in the proof of the familiar fact that

$$\binom{n+1}{k+1} = \binom{n}{k} + \binom{n}{k+1} \quad (\text{A})$$

Plenty of people do!

If you were one of the people who are spooked by this equation then this chapter is for you.

Cast your thoughts back to the old proof of this fact that you have stored somewhere in the back of your mind. If we want to select  $k+1$  things from a set  $X$  of  $n+1$  things then we arbitrarily pick one of  $n+1$  things, as a sort of pivot – call it ‘ $p$ ’ for pivot – and we then either

- (i) select a further  $k$  things from  $X \setminus \{p\}$ ; or
- (ii) select  $k+1$  things from  $X \setminus \{p\}$ .

Process (i) gives us  $\binom{n}{k}$  things (the first summand). Process (ii) gives us  $\binom{n}{k+1}$  things (the second summand).

Process (i) gives us all the unordered  $(k+1)$ -tuples that contain the pivot  $p$ , and process (ii) gives us all the unordered  $k+1$ -tuples that do *not* contain  $p$ . So we add the two terms to sweep up all the  $(k+1)$ -tuples we need. But how do we decide which thing to choose for a pivot? It is true that the aggregate number comes out the same whichever pivot we *in fact* choose, but

- (a) We still have to choose some element, don't we ...? and
- (b) Aren't we going to need the axiom of choice to tell us that we can in fact choose some element?

Of course the answer to (a) is yes – as expected – but the answer to (b) is *no*. This 'no' answer to (b) requires some explanation.

Before we go to a dissection of this mistake let's have another example. This one is a bit more sophisticated, but it has connection to a proof we will see soon of a special case of AC, and it will be good to keep it in view.

#### CHALLENGE 1

*If  $f : X \rightarrow \bigcup X$  is a choice function, so that  $f(x) \in x$  for all  $x \in X$ , and  $A \notin X$  is a nonempty set, then  $f$  can be extended to a choice function for  $X \cup \{A\}$ .*

You have to pick a member of  $A$ , don't you!... don't you<sup>1</sup>??

The key to understanding why you are not using the axiom of choice on those occasions when you mistakenly think you do – like equation (A) above – lies in some very basic logic. (Indeed the mild disdain with which many mathematicians regard formal logic probably has a significant rôle in perpetuating this misunderstanding)

In elementary Logic classes students are invited to take sentences of ordinary language and regiment them into the language of first-order logic. The aim of the exercise is of course to introduce the student to the idea of formalisation: bringing-out and dealing-rigorously-with the features of interest, while concealing everything else.

- (1) If there is a Messiah then we are saved.
- (2) If there is even one person in this room with COVID then we are in trouble.
- (3) If there is even one righteous man in the Cities Of The Plain then God will not fry the city.

Consider (1). Using an obvious lexicon such as ' $M(x)$ ' for ' $x$  is a Messiah' and ' $s$ ' a propositional constant for 'We are saved' we get

$$(\exists x)(M(x)) \rightarrow s \quad \text{or perhaps} \quad (\forall x)(M(x) \rightarrow s)$$

Or, looking at (2), writing ' $C(x)$ ' for ' $x$  is a person in this room with COVID' and  $p$  for 'we are in trouble' we get

$$(\exists x)(C(x)) \rightarrow p \quad \text{or perhaps} \quad (\forall x)(C(x) \rightarrow p)$$

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<sup>1</sup>It is possible to worry about excluded middle here .... We do assume that every tuple either does or does not contain the pivot  $p$ .



The fact that in each of these examples there are two apparently quite different formulations is a reflection of the fact that the two following formulæ are logically equivalent:

$$((\exists x)\phi(x)) \rightarrow A \quad (\forall x)(\phi(x) \rightarrow A) \quad (1)$$

By ‘logically equivalent’ we mean that once we have determined what  $\phi$  and  $A$  are then the two results have the same truth value.

Attend closely to where the brackets open and close: the first formula is of the form  $A \rightarrow B$  (top level connective is an if-then); the second formula is of the form  $(\forall x)$  stuff ... (the top level connective is a universal quantifier.)

The fact that the two formulæ in (1) are equivalent means that the following inference is good, whatever  $\phi$  and  $A$  are.

$$\frac{(\exists x)\phi(x) \quad (\forall x)(\phi(x) \rightarrow A)}{A} \quad (S)$$

The letter ‘S’ here is intended to suggest ‘syllogism’. (It’s not a proper syllogism in the classical Greek sense, but never mind<sup>2</sup>). This inference is the crucial one to bear in mind when considering situations that look like applications of the axiom of choice that aren’t, and the reader is advised to stare at it for a good long while. It’s telling you that if, for any  $x$ , the  $\phi$ -ness of  $x$  is sufficient for  $A$  to be the case, then any  $x$  will do to ensure  $A$  ... *you don’t need to know which one is  $\phi$  and you don’t need to nominate one to do the job – its mere existence is sufficient!*

We can use this syllogism to shed some light on situations where some people think we need the axiom of choice. Let us return to one of our earlier examples.

Let’s deal with equation (A) first. We are given a set  $X$  of  $n + 1$  things, and we want to prove that the number of  $k + 1$ -sized subsets of  $X$  is  $\binom{n}{k} + \binom{n}{k+1}$ . We notice that, for any  $x \in X$ , there is a partition of the set of  $k + 1$ -sized subsets of  $X$  into two pieces (when wondering which piece to put a subset into, ask whether or not the pivot  $p$  is a member of the subset in question) of sizes  $\binom{n}{k}$  and  $\binom{n}{k+1}$ . Notice that ‘ $p$ ’ does not appear in these formulæ for the sizes of the two pieces, so we get the same answer whichever pivot we use. This fact gives us the equality we desire – always assuming that there is such a  $p$ . But of course there is such a  $p$  – in fact there are  $n + 1$  of them.

Let us recall challenge 1:

If  $f : X \rightarrow \bigcup X$  is a choice function, and  $A \notin X$  is a nonempty set then  $f$  can be extended to a choice function for  $X \cup \{A\}$

Consider now the assertion:

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<sup>2</sup>You could obtain something like a syllogism:  
 There are men  
 All men are mortal  
 There are mortals

If  $f : X \rightarrow \bigcup X$  is a choice function, and  $a \in A \notin X$ , then  $f \cup \{\langle A, a \rangle\}$  is a choice function for  $X \cup \{A\}$ .

This is pretty straightforwardly true.

Using quantifier-speak it becomes

$$(\forall X)(\forall \text{ choice functions } f : X \rightarrow \bigcup X)(\forall A \notin X)(\forall a \in A)(\exists x)(x \text{ is a choice function for } X \cup \{A\})$$

which (assuming we are allowed to mix our languages for the sake of telling a story) is unexceptionable. The  $x$  in question is of course  $f \cup \{\langle A, a \rangle\}$ . However when we do the above manipulation to the  $\forall a$  quantifier we get

$$(\forall X)(\forall \text{ choice functions } f : X \rightarrow \bigcup X)(\forall A \notin X)((\exists a)(a \in A) \rightarrow (\exists x)(x \text{ is a choice function for } X \cup \{A\}))$$

which rewrites to

$$(\forall X)(\forall \text{ choice functions } f : X \rightarrow \bigcup X)(\forall A \notin X)(A \neq \emptyset \rightarrow (\exists x)(x \text{ is a choice function for } X \cup \{A\}))$$

which might suggest that we have smuggled in a choice of a member of  $A$ . In a sense we have, but what this shows is that *one* choice is all right!

(It might help to consolidate this in your mind by reminding yourself that, in example (2) above, if there is even one person in this room with COVID then we are in trouble ... *even if we don't know who that person is.*)

So the moral to be drawn is that in order to prove things like (A) at the start of this chapter then, yes, you do have to make a choice, but that the act of making that choice is authorised – by first-order logic if you want to think of it that way. We can certainly formalise a proof of (A) in first-order arithmetic.

This means we can upgrade Challenge 1 to an actual lemma!

**LEMMA 1**  $(\forall X)(\forall \text{ choice functions } f : X \rightarrow \bigcup X)(\forall A \notin X)(A \neq \emptyset \rightarrow (\exists g)(g \text{ is a choice function for } X \cup \{A\}))$ .

We are now in a position to prove

**THEOREM 1** (*The Finite Axiom of Choice*)

*Every finite set has a choice function. For every  $n \in \mathbb{N}$ , if  $X$  is a set of nonempty sets with  $|X| = n$  then  $X$  has a choice function.*

*Proof:*

The proof is by induction on  $\mathbb{N}$ .

The base case is  $n = 0$ . The empty function is a choice function for the empty set of nonempty sets.

If you are not happy about the empty function (and you might not be) then start instead with the case  $n = 1$ . In this case  $X$  is  $\{x\}$  for some nonempty  $x$ . But then, for any  $y \in x$ , the singleton  $\{\langle x, y \rangle\}$  is [the graph of] a selection function for  $X$ .

For the induction step we use lemma 1.

■

Knowledge of this fact goes back quite a long way. There is a proof in Russell and Whitehead [26] volume 2, as theorem \*120.63.

We note for future reference that this proof is acceptable to intuitionists/constructivists; it uses no logic principles they would find suspect.

## 4.2 What is a choice? How do we individuate Choices?

The fact revealed by theorem 1 is often expressed by some formulation like the *aperçu* :

“We can always make finitely many choices:  
to make infinitely many choices we need AC” (F)

(F) is extremely arm-wavy, but the thing it is waving towards is important and true. It would be nice to know what a choice is, what choices are, and how we count them. There is a famous remark of Quine’s: “No entity without identity” which is very much to the point here. If we haven’t got identity criteria for widgets, so that we don’t know how to tell whether or not two widgets are the same widget, then we don’t really know what a widget is, and that means that we can’t use our concept of widget to explain anything; an explanation that makes essential use of the vague concept of widget will itself be too vague to be of any use as an explanation.

If we are to make sense of (F) then we’d better have a way of individuating choices. What is a choice? One pointer towards answers to these challenges comes from reflecting on the fact that theorem 1 can be proved without using AC, and therefore – if the *aperçu* is correct – while making only finitely many choices. So what is a choice? On the face of it we make a choice every time we go through the induction loop. How are we to reconcile this with the desire to think of this proof as making only finitely many choices? Well, one could try saying that that for each actually finite set one needs only that many actual choices to obtain a choice function. I don’t like the sound of this because it has the feeling of conflating the single assertion that every finite set has a choice function with the scheme of assertions that every set of concrete finite size has a choice function. Fortunately there is another approach.

Places where we seem to be making choices are typically places where we invoke the syllogism, so I am going to risk doing enough logic to analyse the syllogism. It would be very good for the reader’s soul to learn some basic Natural Deduction, but of all the multitude of rules it harbours (two for each connective or quantifier, for starters) the one that will concern us most here is the rule of  $\exists$ -elimination, which is the rule that tells us how to exploit – when building a proof – the information wrapped up in an assertion like  $\exists x F(x)$ .

## A bit of Proof Theory

### 4.2.1 The Rule of $\exists$ -elimination

The picture below is a hugely simplified picture of a proof of an expression  $p$  using  $\exists$ -elimination. The rule of  $\exists$ -elimination is famously intimidating to beginners so I am hoping that my readers will not feel that their intelligence is being insulted if I offer a few words of patter. The rule is telling us that

**if**

- (i) we can deduce  $p$  from the news that  $x$  has property  $F$  – and that the security of the deduction doesn’t depend on  $x$ , that any  $x$  will do – *and*
- (ii) that we know (somehow) that there is something that is  $F$ ,

**then**

we can deduce  $p$ .

The calligraphic ‘ $\mathcal{D}$ ’ names a *Derivation* of  $p$  from the assumption  $F(t)$ , represented by the vertical dots that it stands next to. The square brackets round the ‘ $F(t)$ ’ mean that that assumption is “used up” by the  $\exists$ -elimination that is the last line of the proof. That is to say, although  $F(x)$  was an assumption in the proof  $\mathcal{D}$  (of  $p$ ) that eventually got processed into our proof of  $p$ , it is no longer an assumption of that final proof, of which  $\mathcal{D}$  is a proper part.  $\exists x F(x)$  is an assumption in our (displayed) proof of  $p$ , but  $F(x)$  isn’t. The ‘(1)’ connects the application of the rule to the premiss being discharged (there may be lots of other  $\exists$ -eliminations in the stuff  $\mathcal{D}$  abbreviated by the vertical dots.)

$$\frac{\begin{array}{c} [F(t)]^{(1)} \\ \vdots \\ \vdots \\ \vdots \\ p \end{array} \quad \mathcal{D}}{\exists x F(x) \quad p} \quad \exists\text{-elim (1)}$$

### 4.2.2 The Rule of $\forall$ -introduction

There is also the rule of  $\forall$ -introduction:

$$\frac{\begin{array}{c} \vdots \\ A(t) \end{array}}{(\forall x)(A(x))} \quad \forall\text{-int}$$

To prove that everything has property  $A$ , reason as follows.

Let  $t$  be an object about which we know nothing; we reason about it for a bit and deduce that  $t$  has  $A$ ; remark that no assumptions were made about  $t$ ; conclude therefore: *all*  $x$ s must therefore have property  $A$ .

But it is important that  $x$  should be an object about which we know nothing, otherwise we won't have proved that *every*  $x$  has  $A$ , merely that  $A$  holds of all those  $x$ 's that satisfy the conditions  $x$  satisfied and which we exploited in proving that  $x$  had  $A$ . The rule of  $\forall$ -introduction therefore has the important side condition: ' $t$ ' **not free in the premisses**. The idea is that if we have proved that  $A$  holds of an object  $x$  *selected arbitrarily*, then we have actually proved that it holds for *all*  $x$ .

explain free variable

The rule of  $\forall$ -introduction is often called **Universal Generalisation** or **UG** for short; readers may know it under that name. It is a common strategy and deserves a short snappy name. It even deserves a joke.<sup>3</sup>

**REMARK 1** (*Universal Anarchist theorem*). Every government is unjust.

*Proof:*

Let  $G$  be an arbitrary government. Since  $G$  is arbitrary, it is certainly unjust. Hence, by universal generalization, every government is unjust. ■

This is of course also a fallacy of equivocation.

### 4.2.3 Some remarks about $\exists$ -elim and $\forall$ -int

There are some remarks about  $\exists$ -elim and  $\forall$ -int which are commonplace in Natural deduction circles.

- (i) They are "dual";
- (ii) They have the same *side conditions*; and
- (iii)  $\forall$ -int is easier to understand (or perhaps I mean *accept*) than  $\exists$ -elim.

#### (i) In what sense are they dual?

$\exists$ -elim says that if you can deduce  $p$  from  $F(a)$ , then you can deduce  $p$  from  $\exists xF(x)$ . (Modulo side conditions) In the following proof we use the rule of *classical negation* which says that if we can derive a contradiction from  $\neg p$  then we may deduce  $p$ .

$$\begin{array}{c}
 [\neg p]^1 \\
 \vdots \\
 \neg F(a) \\
 \hline
 (\forall x)\neg F(x) \quad \forall\text{-int} \quad \neg(\forall x)(\neg F(x)) \\
 \hline
 \frac{\perp}{p} \text{ classical negation (1)} \quad \rightarrow\text{-elim}
 \end{array} \tag{4.1}$$

<sup>3</sup>Thanks to the late Aldo Antonelli [https://en.wikipedia.org/wiki/G.\\_Aldo\\_Antonelli](https://en.wikipedia.org/wiki/G._Aldo_Antonelli).

and compare it with the proof by  $\exists$ -elimination on p 40. That proof contained a deduction of  $p$  from  $F(a)$ . But if there is such a deduction then, by contraposition, we can easily obtain from it a deduction of  $\neg F(a)$  from  $\neg p$ .

The proof displayed above shows how, if you can deduce  $p$  from  $F(a)$ , then you can deduce  $p$  from  $\exists xF(x)$  (Modulo the same side conditions) using  $\forall$ -int (instead of  $\exists$ -elim) as long as you have a rule of classical negation and accept that  $\exists xF(x)$  is the same as  $\neg(\forall x)(\neg F(x))$ . (You might, Dear Reader, but not everybody does.)

Moral: if you are happy with  $\forall$ -introduction, you should be happy with  $\exists$ -elimination.

**(ii) They have the same side-conditions**

$t$  not free in premisses!!!

( $\forall$ -elim and  $\exists$ -int have no side-conditions.)

Do we mention them o/w?  
Make a joke about how the wags call them *off-side* conditions, co's nobody understands them.

**(iii)**

The significance of the duality lies partly in point (ii). If you are happier with  $\forall$ -int than  $\exists$ -elim then you might find the demonstration in the proof on p. 40 above helpful in making  $\exists$ -elim acceptable.

#### 4.2.4 Back to The Syllogism

What has all this got to do with The Syllogism? Answer: if we try to corall an argument that uses The Syllogism into anything like Natural Deduction form then we find occurrences of the rule of  $\exists$ -elimination appearing wherever we needed The Syllogism. Here is a proof of The Syllogism in Natural Deduction style.

$$\frac{\frac{\frac{(\forall x)(F(x) \rightarrow p)}{F(a) \rightarrow p} \forall \text{ elim}}{p} \quad \frac{[F(a)]^1}{\rightarrow\text{-elim}} \quad \frac{(\exists x)(F(x))}{\exists\text{-elim}(1)}}{p} \quad (4.2)$$

Suppose we have a proof of a proposition  $p$  and at some point in the proof we need there to be a thing which is  $F$ . Specifically, if  $a$  is such a thing then the deduction  $\mathcal{D}_1$  will lead us from  $F(a)$  to the conclusion  $p$  as desired. We don't care which thing is  $F$  (and we may not even know) but we do at least know there is one. This is the assumption ' $\exists xF(x)$ '.

There will be an instance of the rule of  $\exists$ -elimination at any stage in the proof where our construction needs a thing that is  $F$  and we know there are some but we haven't identified any. We will be applying  $\exists$ -elim to the formula ' $\exists xF(x)$ '. On our analysis, this is where we make a (single) choice.

With this in mind let us look closely at the proof of theorem 1.

### 4.2.5 Another look at the proof of theorem 1

It is a proof by induction on ‘ $n$ ’ that every unordered  $n$ -tuple of nonempty sets has a choice function. The base case is clear enough, so let us consider the induction step.

Our induction hypothesis is that every set  $\mathcal{X}$  of nonempty sets with  $|\mathcal{X}| = n$  has a choice function. We wish to deduce that every set  $\mathcal{X}$  of nonempty sets with  $|\mathcal{X}| = n + 1$  has a choice function.

Let  $\mathcal{X}$  be a set of nonempty sets with  $|\mathcal{X}| = n + 1$ . Since  $|\mathcal{X}| = n + 1$ , there is an  $X \in \mathcal{X}$  with  $|\mathcal{X} \setminus \{X\}| = n$ . Choose one such  $X$ . (Clearly we are going to be doing an  $\exists$ -elimination using this  $X$ ). Since  $|\mathcal{X} \setminus \{X\}| = n$  we apply the induction hypothesis to  $\mathcal{X} \setminus \{X\}$  to infer that it has a choice function. (This conclusion is going to be the premiss of another  $\exists$ -elimination)

Now suppose  $f$  is a choice function for  $\mathcal{X} \setminus \{X\}$ . By assumption,  $X$  is a nonempty set. Then, whenever  $x \in X$ , we have that  $f \cup \{\langle x, X \rangle\}$  is a choice function for  $\mathcal{X} \cup \{X\}$ .  $X$  is nonempty by assumption, so there is, in fact, such an  $x$  so (by  $\exists$ -elimination – again!) there is a choice function for  $\mathcal{X}$ . But  $\mathcal{X}$  was an arbitrary  $n + 1$ -sized set so, by UG, every  $n + 1$ -sized set has a choice function.

Thus the inference from “Every set with  $n$  nonempty members has a choice function” to “Every set with  $n + 1$  nonempty members has a choice function” makes three uses of  $\exists$ -elimination. Thus, if  $n$  is a concrete natural number we can prove that every set with  $n$  nonempty members has a choice function using  $3n$  uses of  $\exists$ -elimination. And we can prove it in some very very minimal set theory without even any arithmetic. The proof of theorem 1 uses only three instances of  $\exists$ -elim but they sit inside an induction loop and the proof wherein that loop resides is in a stronger system with at least some arithmetic.

Thus the fact that you can make *one* choice is a gift of first-order logic (*constructive* first-order logic indeed – we don’t need excluded middle or anything even remotely suspect like that); the fact that you can make *any concrete finite number of* choices, too, is a gift of first-order logic (yes – *constructive* first-order logic, again); the fact that you can make any arbitrary finite number of choices is a theorem of (a suitably spiced up) arithmetic. You prove by induction on  $n$  that, for every  $n$ , you can make  $n$  choices. But this is not a theorem of pure logic (it can’t be: pure logic does not know the concept of arbitrary natural number). What about infinitely many choices?

### 4.2.6 Infinitely many Choices

Taking up the idea that

- (i) there is a correspondence between constructions (“recipes” à la Euclid) and proofs, and that
  - (ii) making a choice corresponds to performing a  $\exists$ -elimination,
- what sort of proof might correspond to a construction that makes infinitely many choices? Well, it’s going to have to be an infinite proof, since one has to

somehow fit in infinitely many uses of  $\exists$ -elimination. In this frame of mind let us look at the proof that a union of countably many countable sets is countable.

The application of infinite conjunction is subsumed by the rule for zigzag. should Say something about ...

It will use the rule of infinite conjunction and have a  $\exists$ -e in each branch. It would look a bit like this:

$$\begin{array}{c}
 \left[ \begin{array}{l} c_1 \text{ is an enum-} \\ \text{eration of } x_1 \end{array} \right]^{1..} \quad \left[ \begin{array}{l} c_i \text{ is an enum-} \\ \text{eration of } x_i \end{array} \right]^{i..} \\
 \vdots \qquad \qquad \qquad \vdots \\
 \vdots \qquad \qquad \qquad \vdots \\
 \text{zigzag}\{c_i : i \in \mathbb{N}\} \text{ is an enumeration of } \bigcup_{i \in \mathbb{N}} x_i \\
 \hline
 (\exists e)(e \text{ is an enumeration of } \bigcup_{i \in \mathbb{N}} x_i) \qquad E_1, E_2, \dots E_i \dots \quad \exists\text{-i} \\
 \hline
 (\exists e)(e \text{ is an enumeration of } \bigcup_{i \in \mathbb{N}} x_i) \quad \exists\text{-elim}
 \end{array}$$

where ‘ $E_i$ ’ is short for ‘ $(\exists e)(e \text{ is an enumeration of } x_i)$ ’.

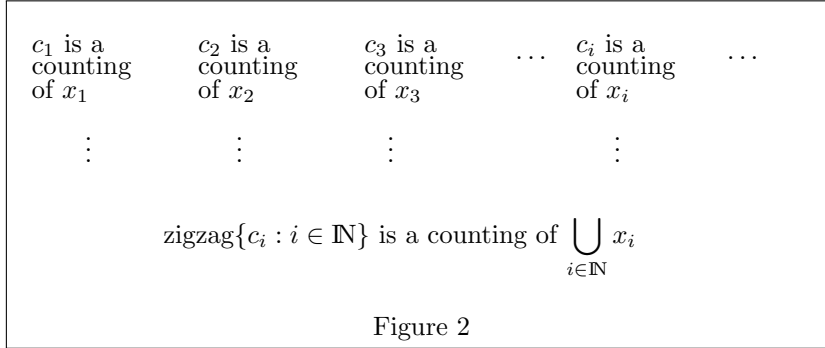
Figure 1

The single occurrence of  $\exists$ -elimination discharges simultaneously all the infinitely many assumptions “ $c_i$  is an enumeration of  $x_c$ ”.

Return to the topic of countable unions of countable sets, and the proof in Figure 1 above. If you have an  $f$  such that  $\forall i \in \mathbb{N}$ ,  $f(i)$  is a counting of  $x_i$  then the corresponding construction simply zigzags through the  $c_i$  in the way you always thought you were supposed to, and allowed to.

If, for each  $i \in \mathbb{N}$ , we can actually supply a counting  $c_i$ , then we can simplify that proof to:





and the infinitary occurrence of  $\exists$ -elim has disappeared. The proof is still infinitary, but it makes no choices. All the  $x_i$  are now *counted sets* not mere *countable sets*. And a counted union of counted sets is, as they say, counted.

#### 4.2.7 Maximal Formulæ

In a proof a formula is said to be a *maximal* formula iff it is both the conclusion of an introduction rule and the major premiss of the corresponding elimination rule. There are some technical terms in there for possible future reference; for the moment what matters is that these technicalities *direct our attention to formulæ that can be got rid of*. In this proof

$$\begin{array}{c}
 [p]^1 \\
 \vdots \\
 \frac{q}{p \rightarrow q} \rightarrow\text{-int (1)} \quad p \\
 \hline
 q \quad \rightarrow\text{-elim}
 \end{array} \tag{4.3}$$

the formula ' $p \rightarrow q$ ' is maximal within the meaning of the act (since it is the conclusion of a  $\rightarrow$ -introduction rule and the major premiss of a  $\rightarrow$ -elimination rule. And there is an obvious manipulation that will turn the proof into

$$\begin{array}{c}
 p \\
 \vdots \\
 q
 \end{array} \tag{4.4}$$

...which has the same premisses and the same conclusion but less stuff in the middle; the *maximal formula* has gone. That maximal formula was the conclusion of an  $\rightarrow$ -introduction and the premiss of an  $\rightarrow$ -elimination. Of more concern to us is the following example of a maximal formula that is the conclusion of an  $\exists$ -introduction and the premiss of an  $\exists$ -elimination.

$$\begin{array}{c}
 \begin{array}{c} \vdots \\ \vdots \\ \vdots \\ F(a) \\ \hline \exists x F(x) \end{array} \quad \mathcal{D}_2 \quad \exists\text{-int} \quad \begin{array}{c} [F(a)]^{(1)} \\ \vdots \\ \vdots \\ \vdots \\ p \end{array} \quad \mathcal{D}_1 \\
 \hline p \quad \exists\text{-elim (1)}
 \end{array}$$

The formula ‘ $\exists x F(x)$ ’ is maximal in the sense that it is the conclusion of an  $\exists$ -int rule and the premiss of an  $\exists$ -elim rule. There is nothing to stop us taking the proof  $\mathcal{D}_2$  (whose last line is  $F(a)$ ) and moving it bodily to the right to place it above the assumption  $F(a)$  ... at which point we no longer need the ‘ $\exists x F(x)$ ’ and we have the new, simpler proof<sup>4</sup>:

$$\begin{array}{c}
 \vdots \\
 \vdots \quad \mathcal{D}_2 \\
 \vdots \\
 \vdots \\
 F(a) \\
 \vdots \\
 \vdots \quad \mathcal{D}_1 \\
 \vdots \\
 \hline p
 \end{array}$$

Figure 2

The difference between this new proof and the old one is that we have disappeared one occurrence of  $\exists$ -elimination. And that matters to us because the new construction corresponding to the new proof will have one fewer choice. Thus *the elimination of a maximal formula* corresponds to an operation on constructions that removes one choice. This is the fact that if you have a definable witness you don’t need even one choice.

#### 4.2.8 “But I’m making only one choice!”

We need to have a reply to the person who says.

“Very well, as you say, *one* choice is OK. So I make a single choice of a selection function on the family of sets-of-countings of the sets in the family of counted sets. So that’s OK!”

They are correct: it is indeed only one choice. But that doesn’t make it OK; we can make one choice from any nonempty set, agreed; but how do we know

<sup>4</sup>A note for proof-theory sophisticates: the formula ‘ $(\exists e)(e \text{ is an enumeration of } \bigcup_{i \in \mathbb{N}} x_i)$ ’ in Figure 1 is *not* a maximal formula despite being both the conclusion of an  $\exists$ -int and the premiss of an  $\exists$ -elim.

that that set of such selection functions is nonempty? It's one thing to show that the collection of such functions constitutes a set – and we can contract that task out to our set comprehension axioms – but it has to be nonempty. And the assertion that it is nonempty is *prima facie* a consequence of AC, not of the comprehension axioms.

Relevant in this connection is the fairly standard exercise for beginners with AC to demonstrate that the Ordering Principle (OP) implies AC for sets of finite sets ( $AC_{fin}$ ).

rewrite between here and the end of the section

Every set of finite sets has a choice function	( $AC_{fin}$ )
Every set can be given a total (aka <i>linear</i> ) order	(OP)

The ordering principle says that every set has a *total* order (not a *wellorder* – it's weaker than AC).

A common mistake is to say that there must be a choice function on the given set of finite sets, because each of the sets being picked from is finite, and every finite set can be wellordered, so we know which element to pick: the first one. The warning sign is that we have not used the assumption that every set is totally ordered. The mistake is the by-now-familiar equivocation on datatypes. The finite sets before us are **sets** not **ordered sets**, or **lists**. We need to expand them all into **ordered sets** somehow. Can you simultaneously order all the  $A_i$  by ordering only one thing? When you put it like that, it becomes obvious what you have to do: totally order  $\bigcup_{i \in I} A_i$ . OP tells you that this set has a total order. Then, for each  $i \in I$ , the restriction of that order to  $A_i$  is a wellorder (since  $A_i$  is finite, and every total order of a finite set is a wellorder) and you plump for the first element in the sense of that wellorder.

It does involve a choice – a single choice, and an instance of  $\exists$ -elimination. Let  $R$  be an arbitrary total order of  $\bigcup_{i \in I} A_i$ . Then there is a choice function on  $\{A_i : i \in I\}$ . But (by OP) there is a total order of  $\bigcup_{i \in I} A_i$ . So there really is a choice function on  $\{A_i : i \in I\}$ , by  $\exists$ -elimination.

So OP implies  $AC_{fin}$  without any use of AC.

Blend this next bit in

Consider what you can expect when you are told  $(\forall x)(\exists y)\phi(x, y)$ . If you are very lucky there will be a Fairy Godmother who, whenever you say ' $x$ ' to her nicely, will reply with a  $y$  such that  $\phi(x, y)$ . She has a method for doing this, but she doesn't tell you what it is; she's a fairy, after all. You know that  $(\forall x)(\exists y)\phi(x, y)$ , but she – unlike you – is actually acquainted with a function  $f$  such that  $(\forall x)\phi(x, f(x))$ . That is to say: the two assertions  $(\forall x)(\exists y)\phi(x, y)$  and  $(\exists f)(\forall x)\phi(x, f(x))$  are different assertions.

Notice that the fairy godmother only promises to totally order any one set. That is what the ' $\forall$ ' is doing. She's not promising to order all sets, each and every one of them, just any one that you show to her. She has an engine that, on being given a set, will order it (She may even be that engine herself: we don't know) for you. She doesn't make you a present of the engine, merely of its output, which she dribbles out one at a time. Does she, as fairies routinely do, offer to give you *three* wishes? In fact she will even give you *finitely many* wishes. However she does *not* undertake, on being given  $\{A_i : i \in I\}$ , to totally

order every  $A_i$ . (Unless  $I$  is finite, of course). That's because OP is  $(\forall x)(\exists y)(y$  is a total order of  $x)$  rather than the rather scary (infinitary!) expression

$$(\forall x_1)(\forall x_2)(\forall x_3) \cdots (\exists y_1)(\exists y_2)(\exists y_3) \cdots \bigwedge_{i \in \mathbb{N}} (y_i \text{ is a total order of } x_i)$$

### 4.3 Executive Summary

If you always find yourself thinking you are making choices and therefore needing AC, what is probably going on is this. Yes, you are making a choice (as – for example – with formula (A) on p 35 above) but no, you don't need AC, because you are making only a *single* choice. A single choice is justified by  $\exists$ -elimination.

The rule of  $\exists$ -elimination gives us a licence to make choices; the ability to make choices give us a licence to use the rule of  $\exists$ -elimination. If you are happier with one of these licences than the other you can start with the one you feel happier with and venture thence towards the other. There is a parallel here with the situation concerning the two rules of  $\exists$ -elimination and  $\forall$ -introduction; they are equivalent and if – like most people – you are happier with  $\forall$ -introduction than with  $\exists$ -elimination you can start from  $\forall$ -introduction, use it to justify  $\exists$ -elimination and thence the licence to make finitely many choices.

#### 4.3.1 Coda

To a certain extent this chapter is addressed to the concerns of people who realise that in their mathematical praxis they are making choices all the time but mistakenly think they need AC to make them. Such people can find it helpful to become acquainted with the rule of  $\exists$ -elimination. I don't want to exaggerate the usefulness of the idea that a choice corresponds to an occurrence of  $\exists$ -elimination, and I am happy to leave the correspondence at a fairly informal level. However it does help make sense of a number of things. Finitely many choices are always all right because a finite proof can contain finitely many applications of  $\exists$ -elimination. Infinitely many choices are problematic, but then infinite proof objects (and if a proof is to contain infinitely many  $\exists$ -eliminations it will perforce be infinite) are problematic. Theorem 1, which says we can make  $n$  choices for any  $n \in \mathbb{N}$  (and does not use the axiom of choice) is provable because it has only finitely many application of  $\exists$ -elimination. They punch above their weight because they occur inside an induction loop. Finally, constructions-relying-on-having-a-widget do not need a choice when there is a canonical widget available, and in those circumstances the proof associated with the construction does not have an  $\exists$ -elimination at the crucial point.

To pursue this parallel deeper and further and more seriously than is done here one would need to study infinitary proof objects, and such things are not for the nervous beginner (nor the overconfident beginner) being unfortunately much trickier than they appear. Even *finitary* proof theory remains a fairly niche subject familiar only to theoretical computer scientists of a particular stamp. I harbour the hope that the neat correspondence between making-proofs and

eliminating-existentials might encourage missionary-position mathematicians to cast a close eye over Proof Theory.

*Cognoscenti* may be struck by the lack here of any discussion of the  $\epsilon$ -calculus. This is in part because the  $\epsilon$ -calculus is a can of worms, but principally because, altho' a discussion of the axiom of choice can help to shed some light on the  $\epsilon$ -calculus, there seems to be less traffic in the opposite direction. Our aim is instead to explain the functioning of the Axiom of Choice in Ordinary Mathematics, and the nexus between the  $\epsilon$ -calculus and the Axiom of Choice is not illuminating for the mathematician on the Clapham omnibus (or the Wellington cable car).



## Chapter 5

# AC is the principle that monotone nondeterministic transfinite processes will conclude successfully

*Every monotone [continuous] deterministic process can be completed;  
trivial in the finite case; needs Hartogs' thm/Bourbaki-Witt in the  
infinite case;  
Every monotone nondeterministic process can be completed—AC;  
Some non-monotone nondeterministic processes cannot be completed.*

My take on AC is that it says monotone nondeterministic transfinite processes will complete successfully. It's a standard exercise (for the lecturer, not the student!) in set theory courses to show that monotone deterministic transfinite discrete processes complete successfully. (It's actually quite tricky to show properly and details are routinely glossed over, but never mind). Discrete nondeterministic finite processes complete successfully – you can successfully fill in the numbers on your lottery card by tossing coins – but infinite nondeterministic processes are of course a problem.

Transfinite processes indexed by the ordinals have been around in mathematics for ages. Lots of recursive constructions of transfinite length. Nobody turns a hair.

To properly understand AC we need the notion of **discrete transfinite process**. Wossat? Let's start with a nice example.

Start with a closed set of reals. Delete its isolated points to obtain its *derivative*. This, again, is a closed set. Is it perfect-closed? That is, does it have any isolated points? It might, in which case it is not perfect-closed. Very well, remove them to obtain the (second) derivative. It's not hard to see that

there are closed sets for which we can take a derivative infinitely often without reaching a perfect-closed set at any finite stage. Is there a good notion of a limit of this process? Yes there is, because the process is monotone – we are deleting points at each stage and never putting anything back. So the intersection of the finite derivatives is a sensible thing to consider. Is it perfect-closed? Perhaps not, but we can always keep going.

What we have here is an example of a transfinite monotone deterministic process. This particular transfinite monotone deterministic process will terminate when it has reached a perfect closed set, since a perfect closed set is its own derivative. The fact that it will terminate is very important, but it is by no means obvious that it will – even tho’ we take it for granted.

## H I A T U S

Reference needed

[5]

Cantor’s construction is nice in two ways: it is *monotone* and it is *deterministic*. It is monotone in the sense that nothing that is taken away is ever put back. One could say that, thought of as a function from dates/times (or ordinals) to sets (or rather sets<sup>op</sup>) it obeys  $t_1 \leq t_2 \rightarrow f(t_1) \leq f(t_2)$ . It is *deterministic* in the sense that the person executing the process never has to make a choice about which points to remove next. Processes like this that are deterministic and monotone can be unproblematically executed. If they have a termination condition they will terminate. That is the extra-set-theoretical meaning of the lemma of Hartogs’ that says that for every set  $X$  there is a von Neumann ordinal that will not inject into  $X$ : *if a monotone deterministic process ever fails to terminate properly it’s not because you have run out of ordinals*. It may crash for other reasons of course: for example, the thing you are trying to construct might not exist.

In vol 1 we talk about the mathematical meaning of some set-theoretic assertion, Hartogs’ among them

[Consider the project to find an set equal to its power set, in ZF, using Bourbaki-Witt. It fails, and you run out of ordinals, but *that’s not why it fails*: it fails beco’s of unstratified separation which tells you that there is no such fixed point

Try doing it in NF. It fails beco’s the recursion is unstratified and cannot be executed. ]

What sort of situation do we find ourselves in if we drop determinism (while keeping monotonicity)? Let  $\mathcal{X} = \{X_i : i \in \mathbb{N}\}$  be a family of sets, all with at least two members. Let us consider the project of picking one member from each  $X_i$ , with a view to obtaining a function  $f : \mathbb{N} \rightarrow \bigcup \mathcal{X}$  satisfying  $f(i) \in X_i$  for all  $i \in \mathbb{N}$ . The discrete transfinite process before us is pretty straightforward: examine the  $X_i$  in turn, starting at  $X_0$ , and pick a member from each – which we can do because (by assumption) they are all nonempty<sup>1</sup>. This process is nondeterministic because at each stage there is more than one thing we can pick. It’s also clear that it is monotone – at least in the sense that, as we go

<sup>1</sup>But **set** does not support “Give me a random member or an error message if you are empty. Find a way round this, presumably using The Syllogism.



along, we are building a function (a set of ordered pairs) and we add ordered pairs to this function (so that at stage  $n$  we have  $n$  pairs, all of the form  $\langle X_i, x_i \rangle$  with  $x_i \in X_i$ ) and we never remove any pairs. So the process is monotone and nondeterministic; can it be completed?

The first point to make is that the elementary set-theoretic apparatus we have used in persuading ourselves that the process can be run successfully for  $n$  steps (for every  $n \in \mathbb{N}$ ) is not enough by itself to prove that the process can be completed. “The process can be completed” simply does not follow from the fact that it can be run for  $n$  steps for every  $n^2$ . This piece of news will be shocking to many readers, but it really is so. A proof can be found in section ?? below.

The people who will be shocked are the people who say, in pained voices “Why can I not just keep going?”. The best response to this is probably another question “How do you propose to do that?” or “What is it that counts as *keeping-going*?”. These people do not realise there is a problem, and that is because they are overlooking the huge difference between *deterministic* transfinite processes and *nondeterministic* transfinite processes. If the process is deterministic then you can, indeed, just “keep going”. If the process is nondeterministic then the various bits-of-processes between which you are undetermined have to cohere.

What one needs is a kind of coherence principle, something that says that all the finite partial functions can be glued together to obtain an infinite (total) function.

One needs to say: there is a good notion of the completion of a transfinite monotone deterministic process – just take the union of the stages. But if it’s nondeterministic there is no good description of the limit beco’s there is not good description of the stages – co’s they’re nondeterministic.

Naturally – as the appearance of the particle ‘task’ in ‘supertask’ reveals, each supertask has a purpose, to produce something. There is always a question about what the workshop looks like after the supertask has been completed or been abandoned, and there are set-theoretic principles that have something to tell us about this. There are two set-theoretic principles that bring us hope that a supertask will complete successfully. Hartogs’ lemma and Zorn’s lemma.

Hartogs’ lemma tells us that if the supertask doesn’t do what it’s supposed to do then it’s not beco’s we have run out of ordinals. This is beco’s Hartogs’ enables us to prove Bourbaki-Witt. One hesitates to say that that is the true meaning of Hartogs’ lemma since the kind of completion that it promises holds also in NF, where Hartogs’ lemma fails, but it’s not seriously misleading to say it. Perhaps what one should say is that Hartogs’ Lemma is the set theoretic principle which *in ZF* assures us that we never run out of ordinals, and that therefore anything declared by a recursion can be declared by a ecursion on the ordinals

We talk about this in Book 1

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<sup>2</sup>We need to distinguish between “I can do this  $n$  times for every  $n$ ” and “For every  $n$ , if i have done it  $n$  times then I can do it an  $n + 1$ th time” There are easy examples of the first that are not examples of the secon but the point is the even the second isn’t enuff

To make sense of a supertask of transfinite length we need to know, at every limit stage  $\lambda$ , what the situation is in which you make your  $\lambda$ th move. For this one needs a kind of continuity. Monotonicity helps.

Cumulative hierarchy monotone and deterministic, but not cts, so no solution.  $\alpha \mapsto \mathcal{P}(\bigcup_{\beta < \alpha} V_\beta)$  is monotone but not cts.

Bourbaki-Witt sez that cts + monotone + deterministic  $\rightarrow$  success.

Do we need to think about confluence in the context of nondeterminism?

[with thompson's lamp the work-in-progress is not an object but a proposition, a truth-value]

To present Zorn as a principle about completion of supertasks in the same way we need to make three distinctions.

(i) We need to distinguish between deterministic and nondeterministic supertasks.

(ii) We need to distinguish between monotone and non-monotone supertasks.

(iii) We need to distinguish between monotone supertasks that are cts and those that are not. (No good notion of a cts nonmonotone supertask)

So a supertask will be  $\pm$  deterministic,  $\pm$  monotone and – if monotone –  $\pm$  continuous.

So there are 6 possibilities, which we consider in order (roughly) of increasing difficulty.

### 5.0.1 Deterministic and Monotone

Classic early example is Cantor's construction of a perfect closed set starting from a closed set by repeatedly taking the derivative. It's deterministic (every closed set has precisely one derivative) and it's monotone (the derivative of  $X$  is a subset of  $X$ ). This process is deterministic and monotone. It is also continuous at limits (we take intersections)

constructing the transitive closure of a relation  $R$  as  $R \cup R^2 \cup \dots$

Infinitary  $\bigvee$  and  $\bigwedge$ .

These are cts.

Contrast with the attempt to obtain a set identical to its own power set by iteration. It's deterministic and monotone but it's not cts at limits. We not only find that the process doesn't deliver a fixed point; we *know* this process cannot succeed.

Consider the project of obtaining a minimal transitive relation extending  $R$  by picking up ordered pairs  $\langle x, y \rangle$  and  $\langle u, v \rangle$  from the graph of  $R$  and adding  $\langle x, v \rangle$  whenever  $y = u$ . This is monotone, continuous and completes successfully despite being nondeterministic and we can prove the existence of such relations without using AC.

So how does one explain why the supertask to construct a minimal transitive extension of a relation  $R$  is going to succeed? The key observation is that *if* there are any minimal transitive relations extending  $R$  then there is a unique one. So the attempt to construct one by nondeterministic supertask must succeed *despite it being nondeterministic*. First one shows that if  $S_1$  and  $S_2$  are two such relations then  $S_1 = S_2$ . This is in striking contrast to the attempt to find a choice

function on a countable family. Both these supertasks are nondeterministic, monotone and continuous. So why does one need AC and the other not?

The difference between these two situations is not that in one case you need the axiom of choice and the other you don't; both of these supertasks are nondeterministic and need the axiom of choice. The point is that the supertask that constructs the transitive closure is not the reason why the transitive closure exists: it exists for totally other reasons: it's an inductively defined set and exists for the reasons that inductively defined sets always exist (whatever they are). The choice function on the infinite famil

Truncation here...

You don't need the axiom of choice to show that

(i) when the supertask to create  $t(R)$  completes it will have constructed  $t(R)$ ; What you need AC for is to show

(ii) that the supertask does in fact complete. How can i tell? If i look at the various stages of the supertask i can recover a wellordering of  $\text{dom}(R)$  from the logbook.

Do we need to consider confluence and directedness?

## 5.0.2 Deterministic and Nonmonotone

Infinitary XOR.

Thompson's lamp

Attempt to inject  $\omega_1$  into  $\mathbb{R}$  fails beco's it isn't monotone; you are always rearranging. If you do it without rearranging, but putting more copies of  $\mathbb{R}$  on the end you succeed in injecting the second number class into ... the Long Line!

## 5.0.3 Nondeterministic

This is where AC comes in. Given a sequence  $\langle A_i : i \in \mathbb{N} \rangle$  of nonempty sets, how do we feel about the supertask of obtaining a choice function for the sets in the sequence, in the absence of any exploitable order information on the sets which tells us which of their members to pick? We proved by induction (theorem 1 on p.38) that, for any  $n$ , we can find a choice function on  $A_0 \cdots A_n$ . Can we get a choice function for *all* of them?

Failure of continuity at limit stages is part of the problem

Take care when assigning your candidate supertask to one of these classes.

In most cases where it is clear what happens at a limit stage it's beco's the process is cts, but not always (power set, for example). With Thompson's lamp it simply isn't clear what happens at the limit stage. Nor with finding a path thru' the cc-poset of partial selection functions. The role of AC seems to be to refine a nondeterministic process to a deterministic one. Once you make that process deterministic it becomes cts.

Talk of supertasks entered the philosophical literature with *Thompson's Lamp* (see [29]. Mathematicians will know Littlewood [20]. See also [4]. There is a very readable discussion of supertasks in <https://plato.stanford.edu/>

entries/spacetime-supertasks/. It's readable, but not very helpful to readers of this book. Do not be distracted!

At time  $t = 0$  the lamp is off. At time  $t = 1/2$  it is switched on, at time  $t = 3/4$  it is switched off, then on again at time  $t = 7/8$  and so on. The puzzle then is: what is its state at time  $t = 1$ ? The problem is supposed to be that there are compelling reasons to believe that it cannot be on (because every time it is switched on before time  $t = 1$  it is subsequently switched off) and similarly it cannot be off. People like us will say that, because the Thompson's Lamp process is discontinuous, what it does *near*  $t = 1$  tells us nothing about what it does *at*  $t = 1$ , so there is no problem. They are right to say that, but to say that is to miss the larger point that there is a huge assumption in the background that the process *can be completed!*, or perhaps I should say that *sense can be made of the idea of the process being completed*.

This answer is so obvious that some ingenuity is required of the puzzle designers if the reader is to be inveigled into thinking there is a problem. I think it may be that there is a background assumption that all discontinuities have to be *isolated*.

Of course throwing a switch requires a certain minimal amount of energy, so the Thompson's Lamp thought-experiment would involve an infinite amount of energy. So it's not realistic, and so it must be an allegory, a *mock-up* of the real problem. So what is the real problem? Nothing interesting, as it happens.

If the reader feels that theorem 1 justifies the claim that every *countable* set has a selection function then (s)he is probably thinking that a selection function for a countable set can be obtained by performing the following supertask. First count the set (and by assumption this can be done – the set is countable) so that it has become  $\{X_i : i \in \mathbb{N}\}$  and then construct a  $\subseteq$ -increasing sequence of choice functions for longer and longer initial segments of the ordering by acting out the induction in the proof of theorem 1. Completion of this – nondeterministic – supertask would, indeed, give us a choice function for  $\{X_i : i \in \mathbb{N}\}$ . But that is to say that the assumption that we can complete this supertask implies the axiom of choice for countable sets.<sup>3</sup> So we seem to be appealing to a principle that supertasks can be completed. When is this assumption safe?

All the supertasks considered in the literature have the feature that the subtasks that compose them and are executed successively have an order-of-execution relation on them that is a wellordering. The assumption that the reals can be wellordered has a multitude of bizarre consequences [my favourite example – shown me by Imre Leader – is that there is a continuum-sized total ordering with no nonidentity order-preserving injection into itself] obtained typically by constructing the desired bizarre object by recursion on the hypothesised wellordering in a process that can really only be described as a supertask.

Actually it can be given a top-down presentation

In principle one might want to consider supertasks where the order-of-execution relation is not a wellordering, but in the current setting we have

<sup>3</sup>The idea that countable choice relies on a supertask argument goes back at least as far as Schuster, [28], though I gather that he no longer holds the views expressed there.

no need to consider such generality: all supertasks considered here will have an order-of-execution relation that is a wellordering. One could say that for us, here, “supertask” is simply a nice sound-bite (or strapline) for *discrete process of wellordered transfinite length*.

#### 5.0.4 Monotonicity and Determinism

##### BLEND IN

This might be a place to talk about how one can make sense of infinite sums and product in analysis beco’s  $+$  and  $\times$  are monotone.

This use of the word ‘monotone’ here might sound funny to some, so let me illustrate with a couple of examples:

- The riddle of *Thompson’s lamp* [29] see above ...
- Disjunction and conjunction ( $\vee$  and  $\wedge$ ) are commutative and associative, so one can think of them as operations on finite sets of propositions. Thought of as functions from sets-of-propositions to truth values they are *monotone* in the sense that

$$P \subseteq Q \rightarrow \bigvee P \geq \bigvee Q \quad (5.1)$$

and

$$P \subseteq Q \rightarrow \bigwedge P \leq \bigwedge Q \quad (5.2)$$

(setting **false**  $\leq$  **true**).

Further, given an infinite family  $\langle p_i : i \in \mathbb{N} \rangle$  of propositions, both the sequences

$$\langle \bigwedge_{i < n} p_i : n \in \mathbb{N} \rangle \quad (5.3)$$

(That is to say:  $p_0, p_0 \wedge p_1, p_0 \wedge p_1 \wedge p_2 \dots$ )  
and

$$\langle \bigvee_{i < n} p_i : n \in \mathbb{N} \rangle \quad (5.4)$$

(That is to say:  $p_0, p_0 \vee p_1, p_0 \vee p_1 \vee p_2 \dots$ )

– thought of as functions from natural numbers to truth-values – are monotone;

This has the effect that a conjunction (or disjunction) of an infinite set of propositions is well-defined<sup>4</sup>. The limit of 5.3 is **false** as long as even one of the  $p_i$  is false (and **true** otherwise) – the point being that if 5.3 ever takes the value **false** then all subsequent values are **false**. Analogously the limit of 5.4

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<sup>4</sup>Or perhaps – to be on the safe side – one should say that if one wants to think of the infinite con(dis)junction as being well-defined, it is obvious what the answer has to be. This kind of argumentation is familiar from elementary analysis.

is **true** as long as even one of the  $p_i$  is **true** (and **false** otherwise) – the point being that if 5.4 ever takes the value **true** then all subsequent values are **true**.

Contrast this with exclusive-or (**XOR**). **XOR** similarly is associative and commutative and so can be thought of as a function from finite-sets-of-propositions to truth-values. However there is no analogue of 5.1 or 5.2: the sequence

$$\langle \text{XOR}_{i < n} p_i : n \in \mathbb{N} \rangle$$

is not monotone and we do not have a good notion of its limit; one cannot apply **XOR** to infinite sets of propositions; the expression:

$$\text{XOR}_{i \in \mathbb{N}} p_i$$

is not defined: no sense can be made of it.

### BLEND IN

If you have both determinism and monotonicity then you have a good notion of what-happens-in-the-limit. One would like to be able to say that processes that are monotone and deterministic can always be completed, but that is not the whole story. There are monotone deterministic processes that do not complete: the supertask of creating a fixed point for the power set function by iterating transfinitely (trying to reach it “from below”) will never work – because there is no fixed point! So it’s not true that every monotone, deterministic process can be completed. What is true is that – because of Hartogs’ lemma – *if it fails to complete it’s not because you run out of ordinals*, it’s because the thing you want isn’t there. Quite *why* the thing isn’t there remains to be explained in each case. A fixed point for  $\mathcal{P}$  would be a paradoxical object (at least if you have unstratified separation).

we talk about this in Book 1  
and also earlier here.

Is this anything to do with the fact that  $\mathcal{P}$  is not  $\subseteq$ -continuous? Perhaps...

A monotone deterministic supertask is a project that has a starting condition, and instructions to add something at successor stages, and at limit stages take the union of what you have got so far. There will be a termination condition that tells you when you have succeeded (or crashed). Such a project will always succeed (or crash). If the process never halts (because it neither succeeds nor crashes) then the collection of ordinals of stages you have been through will contain all ordinals, and the set of all ordinals is not to be borne.

Lots of examples: inductively defined sets as unions of stages – completely unproblematic.

However if the process is not deterministic then the collection of stages is not totally ordered and we cannot appeal to the Burali-Forti paradox. What might happen? Instead of getting one well-defined sequence of stages, we might find that we have a debouchement of ever-fragmenting sequences of stages none of which ever come to anything, like a mountain stream getting lost in rivulets in a desert.

Consider the process of trying to find a choice function for a countable family of sets, armed only with the finite axiom of choice, theorem 1.

More generally (if your process is not both monotone and deterministic) then it might crash. However, if it does, it's not because you have run out of ordinals.

If you lack one or other of monotonicity and determinism then bad things can happen, or you need special assumptions if you want to be sure that the supertask completes. Thompson's lamp is deterministic but not monotone, and the state of the lamp at time  $t = 1$  is not well-defined; the supertask with  $AC_\omega$  is monotone but not deterministic, and can't be completed without [some] AC. The supertask of embedding the set of countable ordinals into  $\mathbb{R}$  in an order-preserving way is monotone but nondeterministic and cannot be completed *at all* even on the assumption of AC. And this despite the fact that all its proper initial segments can!

The challenge of embedding the countable ordinals into  $\mathbb{R}$  looks a bit like a supertask. It looks like a monotone nondeterministic supertask, so AC (or more specifically Zorn) will tell us that it can be completed: the poset of order-preserving partial maps from the countable ordinals into  $\mathbb{R}$  ordered by  $\subseteq$  is chain-complete and therefore (by Zorn) has maximal elements no problem: the problem is that these maximal elements might not be defined on all countable ordinals – the [modified] identity map that sends the finite ordinal  $n$  to the real number  $n$  is maximal! The supertask can be completed all right (if we have AC); it's just that the result of that completion isn't what we wanted.





## Chapter 6

# Two Distinctions

In this chapter we consider the finite/infinite distinction and a concrete/abstract distinction. These distinctions – and the relation between them – are gadgets that are helpful when we grapple with the axiom of choice.

### 6.1 The finite/infinite Distinction

There is a mediæval and renaissance literature on this distinction going back through the Jesuits and Galileo to Duns Scotus and possibly beyond. From about the 19th century (Cantor onwards) our understanding has stabilised.

Altho' this is not a book about constructive logic, a word or two will nevertheless be in order on the way in which the axiom of choice and excluded middle become entangled. It is said of both of them that they are true in the world of finite things (and that this fact can be established from first principles) while nevertheless being ... *questionable* ... beyond it. There is also the circumstance that the axiom of choice implies excluded middle. See [7].

People say that not understanding the finite/infinite distinction (by which i mean: casually assuming that the infinite resembles the finite in ways that in fact it doesn't) results in errors: no finite set can be equinumerous with a proper subset of itself so one might make the mistake of thinking that this goes for all sets, thereby failing to spot that *Dedekind-infinite* is a coherent idea.

That is a standard example, but there are two other examples which are of more concern to us here: excluded middle and the axiom of choice.

Intuitionists say that believing Excluded Middle to be correct stems from an error of assuming that the infinite slavishly resembles the finite. (Of course Excluded Middle holds in the world of finite things – tho' one has to be careful how one states this fact.)

What exactly do people mean when they say – as they so often do – that excluded middle holds in finite domains? Now it's a simple matter to set up possible world models that believe that every set is inductively finite but in which Excluded Middle fails, so what is meant must be a bit more nuanced. At

the very least we have to assume Excluded Middle for atomics. The following appears to be true.

**THEOREM 2**

Let  $\mathcal{L}$  be a first-order language with finitely many constant symbols  $c_i : 0 < i \leq n$ . Let  $T$  be the  $\mathcal{L}$ -theory  $T$  containing

- (i) all constructive theses;
- (ii) for each atomic formula  $\psi$  that mentions only the constants  $c_i$ , an axiom  $\psi \vee \neg\psi$ ; and
- (iii) an axiom  $(\forall x) \bigvee_{0 < i \leq n} x = c_i$ .

Then  $T$  contains all classical theses of  $\mathcal{L}$ .

*Proof:*

The proof proceeds by induction on the subformula relation.

Clearly true for atomics by assumption.

We trade on the following observations:

- If  $A \vee \neg A$  and  $B \vee \neg B$  then distributivity gives us

$$(A \wedge B) \vee (A \wedge \neg B) \vee (\neg A \wedge B) \vee (\neg A \wedge \neg B).$$

The first three disjuncts give us  $A \vee B$  and the last disjunct gives us  $\neg(A \vee B)$ , whereby we obtain  $(A \vee B) \vee \neg(A \vee B)$ . And  $\wedge$  and  $\rightarrow$  similarly. This means that the propositional fragment obeys classical logic.

- We can eliminate the quantifiers using the two  $T$ -theorems

$$(\exists x)(\psi(x)) \longleftrightarrow \bigvee_{0 < i \leq n} \psi(c_i)$$

and

$$(\forall x)(\psi(x)) \longleftrightarrow \bigwedge_{0 < i \leq n} \psi(c_i)$$

and then appeal to what we have just proved about the propositional fragment. ■

Another story is that paying insufficient attention to the distinction between finite and infinite leads one to think that AC is true everywhere because it is true in the finite world. (See theorem 1.) And of course there is this theorem of Diaconescu:

**REMARK 2** (Diaconescu [7])  $AC \rightarrow \text{Excluded Middle}$ .

*Proof:*

Take AC in the form that every inhabited set of inhabited sets has a choice function. (We say  $x$  is inhabited iff  $(\exists y)(y \in x)$  – rather than  $\neg(\forall y)(y \notin x)$ ).  $\perp$  and  $\top$  are two distinct objects, thought of as **false** and **true**.

Let  $p$  be an arbitrary proposition. We assume AC and deduce  $p \vee \neg p$ .

Let  $a = \{x : x = \perp \vee (p \wedge x = \top)\}$  and  
 $b = \{x : x = \top \vee (p \wedge x = \perp)\}$  and  
 $X = \{a, b\}$ .

Then  $X$  is an inhabited set of inhabited sets and must have a selection function  $f$ . Therefore  $f(a) \in a$  and  $f(b) \in b$ . Further we know  $(f(a) = \perp) \vee (f(a) = \top)$  and  $f(b)$  similarly. Thus there are four possibilities, so we can use proof by cases. If  $f(a) = \top$  then  $p$ ; if  $f(b) = \perp$  then  $p$ . If neither of these happens—so  $f(a) = \perp$  and  $f(b) = \top$ —then at least  $f(a) \neq f(b)$  so  $a \neq b$ . But  $p \rightarrow (a = b)$ , so we infer  $\neg p$ . Thus proof by cases gives us excluded middle. ■

However i offer the suggestion that the questionable confidence that AC and excluded middle hold in the Infinite World is not the result of recklessly extrapolating from the Finite to the Infinite but rather the result of recklessly extrapolating from the Concrete to the Abstract.

## 6.2 A Concrete/Abstract Distinction

This section is speculative: i am not claiming that there is a hard-and-fast distinction that can be used to provide a rigorous explanation. The hope is that what i say here will resonate with a kindred distinction that the reader's mind already houses, and that they may be able to use it in building a picture of their own. When Quine and Goodman [14] wrote “We do not believe in abstract entities.” nobody quibbled that they didn't define what ‘abstract’ meant and what the difference was between the abstract things they didn't believe in and the concrete things that they did believe in. I am hoping that my readers can evaluate my observations *lazily* up to the point where the elucidation starts to shed some light on their picture of the axiom of choice. Then they can go their own way.

It's a bit like the celestial/secular distinction, which is not a good start.

1. The concrete world has agency; the abstract world does not.
2. The concrete world has a time axis; the abstract world does not.
3. The abstract world is deterministic and reproducible; the concrete world is *noisy*.
4. The world of the concrete has complete information (you can ask any question and get an answer);

There is an eerie parallel with fictional worlds. It is famously underdetermined how many children Lady Macbeth had<sup>1</sup>). There are fictionalist

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<sup>1</sup><https://archive.org/details/in.ernet.dli.2015.215175>

Philosophies of Mathematics ... tho' i have not heard this parallel used as an argument in their favour.

the world of the abstract isn't noisy but it lacks complete information – in the sense that it is strongly typed: there are questions that just can't be asked or answered:  $3 \in 5$ ?

5. Truths about *concreta* are contingent; truths about *abstracta* are necessary.

6. *concreta* are located in time and space; *abstracta* are not.

Ad (1). Part of the appeal of AC is agentive. *Homo Faber* evolved in the world of *concreta* and likes to think in terms of agency. But the world of abstracta is not agentive so the appeal is spurious.

Ad (3). Typically it is nondeterminism that causes us to reach for AC; however the world of abstracta is deterministic.

Types are abstract; tokens are concrete.

Part of the appeal of the old protestant gibe about angels dancing on the head of a pin is the way in which it imputes to the target a confusion between abstract (angel) and concrete (pin).

If you confuse the concrete-abstract distinction you can end up losing sleep over the Banach-Tarski paradox and the suite of hat-colouring problems.

Banach-Tarski arose from consideration of the dissection puzzles. Can you chop up a cube into finitely many pieces and reassemble them to get a regular tetrahedron of the same volume? Questions like this are not questions about infinite sets; they are questions about finitary combinatorics.

One of the things one wants to say about Banach-Tarski is that the real problem it throws up is not AC but the idea that regions of space are sets (of ordered triples of real numbers), an error that my colleague Jeremy Butterfield calls *pointillism*, but which might less graphically be characterised as the error of set-theoretic foundationalism, of thinking that everything is a set. If you stain pointillism with AC, you see these Banach-Tarski-shaped splodges under the microscope. AC is not the problem: it is merely the stain that makes the pathology visible. Once you realise that Banach-Tarski is the result of pointillism you no longer care whether it follows from AC or not.

But another take on this is that you might be finding Banach-Tarski odd because you are not distinguishing appropriately between the concrete and the abstract. It is possible to wonder (and some comic material has been developed along these lines) why there isn't – somewhere in gold-mining hill country – a reclusive mathematician (one thinks of Theodore Kaczynski [https://en.wikipedia.org/wiki/Ted\\_Kaczynski](https://en.wikipedia.org/wiki/Ted_Kaczynski)) who for the sake of appearances sometimes pans gold but mostly practices his dark mathematical arts in doubling the size of nuggets ...and occasionally going down to a trading post in the valley to turn the nuggets into cash, to then buy groceries with the proceeds. The point about the decompositions in Banach-Tarski is not that they

rely on the axiom of choice; the point is that they live in the world of *abstracta* rather than *concreta*; the pieces that the nuggets get chopped into are not even measurable! In contrast the dissection problems of the nineteenth century have clear roots in the concrete world, and the solutions can be applied to actual scissors and paper. Banach-Tarski beguiles us by sounding like the perfectly respectable dissection problems we know of old.

There are also hat problems. See [16]. Here is one I found in an example sheet I once taught from.

There is a countable set of people, each wearing a hat. There are two colours of hat. Each person can see all the other hats but they cannot see their own. Can the wearers devise a strategy that will result in all but finitely many of them being able to deduce the colour of their own hat? AC says yes! There is an equivalence relation of finite difference on the family of two-colourings of the set of hats: two colourings are equivalent iff they differ on only finitely many heads. By the axiom of choice there is a selection function on the set of equivalence classes. In fact there are lots. The hat-wearers agree to fix one such choice function. A hat wearer can look around them and deduce which equivalence class their colouring is in. The choice function will direct their attention to one particular colouring  $\chi$  belonging to that equivalence class, and they then bet that the colour of their hat is the colour assigned by  $\chi$ . They all see  $\chi$ . The actual colouring in which the hat wearers find themselves differs from the colouring  $\chi$  identified by the choice function at only finitely many places, so only finitely many of them can be wrong.

This looks very odd indeed. (That is why it is a good example sheet question). It certainly looks at first blush as if the oddity is something to do with the axiom of choice. But it might be more helpful to see the oddity as arising from the trick of narrating a fact about uncountable choice as if it were a fact about concrete well-lit middle-distance medium-sized objects in the real world, wearing hats. Reflect that the hat wearers – who are supposed to be ordinary people after all – nevertheless are deemed to be able to assemble in finite time the infinitely many assignments of colours to hats. No concrete creature has access to an infinite amount of information. The oddity is nothing to do with the axiom of choice; the oddity is the suggestion that the hat-wearers can process an infinite amount of information.

A bit vague from here on

Bits of Mathematics that concern the interface between concrete and abstract are always problematic. Probability! Computability!

People always say that reproducibility is the key notion in scientific practice; on this picture it's the key notion in mathematics. But then mathematics is the Queen of the Sciences.

Being seduced by the apparent possibility of supertasks is a consequence of not distinguishing the concrete from the abstract. Supertasks require agency, and there is agency in the world of *concreta* but not the world of *abstracta*.

Not grasping the concrete/abstract distinction results in taking supertasks seriously. Also thinking that the axiom of choice is obviously true. (the error

lies not in thinking that it's *true*; the error lies in thinking that it's *obviously*<sup>2</sup> true.

### 6.3 Try not to confuse them

*In this section we consider the errors of misidentification, and their consequences*

[It's not blindingly obvious that the world of *concreta* is finite, tho' it might be ... it might be compact. But we should expand on the two mistakes, how they are mistakes, but not the *same* mistake, because the world of *concreta* is not the same as the world of finite things.]

Equipped with these two distinctions one finds a raft of possible mistakes to make. One might neglect to distinguish the concrete from the abstract, or neglect to distinguish the finite from the infinite. At times one might think that one of the distinctions is in play when it is in fact the other: All these mistakes have consequences, and some of those consequences involve the axiom of choice.

Let us start with Russell's parable of the countably infinite collection of pairs of shoes and the countably infinite collection of pairs of socks. The striking feature of this parable is that it seems obvious that the family of pairs of socks has a transversal. But the reason why that seems obvious isn't that the family is finite: it isn't! It is that socks are concrete (well, cotton or wool) objects and concrete objects can always be distinguished by grubby everyday details such as their position in space. In the everyday world we can wellorder everything. The pack of cards with which i play bridge with my friends is not an abstract object in the way that the game of bridge is an abstract object: the cards are grubby physical objects with creases and taco-sauce stains on the reverse that cardsharps can use to tell which card is  $K\spadesuit$  and which is  $A\clubsuit$ . So the thought is that the axiom of choice is true in the everyday world not beco's the everyday world is finite (tho' it presumably is) but because it is *concrete*, and we are all of us cardsharps<sup>3</sup>. In the world of *abstracta* (by which i mean really hardcore abstracta, like the real numbers or the natural numbers) one can make a case for saying that it's not at all obvious that one can wellorder everything. The real line is the unique complete ordered field. Now there is nothing in that intelligence that tells you how to wellorder it. So, if you can wellorder it, it must be for some completely other reason. (Such as the axiom of choice, one might think. But if *that* is why the reals are wellordered then we can't use the fact that the reals are wellordered to argue for AC without begging the question.) But why might we expect there to be such a completely other reason? We wellordered the socks beco's they live in a noisy world of blooming buzzing confusion replete with extraneous detail that we cardsharps can exploit. The reals don't live in that kind of world. So the point is not that AC is true-in-the-finite-world-but-not-true-in-the-infinite-world, the point is rather that it is true in the concrete world but not true in the world of *abstracta*.

<sup>2</sup>There might be things that make it obvious, but this isn't one of them.

<sup>3</sup>Need to say something about  $P+NP$  in this context. Most interesting NP problems have extra structure which can be exploited.

[this may be where the connection to *grue* comes in. What are these on them! completely other reasons for believing that  $\mathbb{R}$  can be wordered? Everything that we've examined can be wordered? But the only things that we can examine are in the world of *concreta*, where we can worder things by contemplating them.]

## H I A T U S

Need to collate all the material on concealment, agency, formalisation and abstraction into one document which can then be inserted into ACpedagogy and dialethismarticle.tex. Mind-projection fallacy, tournament chess, supertasks, AxInf true only in the world of abstract objects. Believing you can perform supertasks (or even believing that performing a supertask is a thought-experiment worth thinking) is an error you make when you forget that you are in the world of abstract objects rather than in the world of *concreta* in which we actually live. No nondeterminism in the world of abstract objects, no *noise*. And no time axis: there are linear orders all right, but no time. Nondeterminism is failure of reproducibility.

One might be deploying in the world of *abstracta* intuitions that were evolved to help you cope with the world of *concreta* and which work well there. (And I do mean evolved: natural selection can work on any inherited strategies we might have for dealing with the concrete world; it is only at one remove that it can work on strategies for reasoning about *abstracta*.)

*Homo Faber* think they can wellorder everything by hand. Admittedly in the finite world they can (we saw in theorem 1 how AC is true in the finite world) but that isn't quite the point. The point is that AC seems to be true in the world of *concreta*. (Notice that Mathematical Physics never mentions AC). The world of *concreta* seems at first blush to consist of things that are not only finite but also quite comically small (for example it will apparently take only  $10^{106}$  years for all the black holes in the universe to evaporate, and no self-respecting Ramsey theorist will so much as get out of bed for a number that puny) but one wouldn't want to bank on it being *exclusively* finite. After all, we might want to conceptualise *abstracta* in such a way that some of the things that do *not* get swept up into it (and therefore get left in *concreta*) are not finite: in principle the concrete world might be infinite. The point is rather that in the world of *concreta* in which we live there is always enough grubby detail to enable us to make the choices we want to make. It's not just Old Masters that have decayed varnish and coffee-stains that give them individuality. This is why it seems obvious (for example) that there is a choice function on the set of pairs of socks. Every sock has a *patina*. Russell's thought-experiment is a concretisation of the axiom of choice for socks, I mean pairs. It is the axiom of choice for pairs narrated in the world of *concreta*. Russell's thought-experiment makes us think about the difference between the world of *concreta* and the world of *abstracta*.

We just drop *Homo Faber* in it

What is so clever about Russell's parable is that it exploits our confusion of the abstract and the concrete to make AC<sub>2</sub> look extremely plausible.

It then pulls the rug out from under us by pointing out the difference between the shoes and the socks.

rhetoric has agency; logic doesn't.

When you abstract away from everyday life to the arcane world of abstract entities you leave behind (you *conceal!*) the fussy details that enabled you to wellorder things in the real world.

Two mistakes:

- (i) Overestimating the extent to which an understanding of the infinite can be had by extrapolation from the finite;
- (ii) Overestimating the extent to which an understanding of the abstract can be had by extrapolation from the concrete.

These two errors overlap because 'finite' and 'concrete' overlap. (ii) results in trying to express a lot of abstract mathematics as constructions... a mistake to which *Homo Faber* is particularly drawn. *H. Faber* evolved to live in the world of *concreta* and has habits that reflect that history; *H. Faber* likes constructions.

Both errors result in a blithe assumption that an infinite monotone abstract construction (a supertask) can be conceptualised in the same way as a finite concrete one. This assumption is wrong but not necessarily disastrous, at least as long as one is considering monotone deterministic constructions (supertasks). Things start to go wrong with your mathematics once you insist on thinking of it in terms of supertasks and thinking that those supertasks are nondeterministic. Cantor's supertask is deterministic.

(This is not a reason for having a go at constructivists, or at least i don't think so. Should Say something about this.)

Somewhere here we need a chapter boundary  
 Constructions are a useful concept in the world of *concreta*, so *Homo Faber* likes to think in terms of constructions.

If you make the mistake of overestimating the resemblance between the world of *abstracta* and the world of *concreta* then you will be vulnerable to the seductions of supertasks. Transfinite recursion. The proof Cantor gave of his theorem that every closed set of reals has a largest closed subset without isolated points involves a transfinite recursion, and that is the way the story is always told. But it can be told in a "top-down" *at-a-stroke* way.

Recall that  $D(x)$  is  $x$  minus its isolated points. Start with a closed set  $X$ , do  $D$  at successor stages and take  $\bigcap$  at limit stages...and eventually obtain a perfect closed set.

This is the way the story is told, and the story of ordinals grew out of the telling of it. It is ordinals that we use to number the pages of the worksheet that we follow in order to construct our answer. *Homo Faber* likes this sort of thing very much. It not only tells us that the set we desire exists, it even tells us how to find it, while getting our hands upliftingly dirty in the process – we have *earned* the answer! Honest toil, indeed.



There are some inconvenient and annoying questions one can ask about this process ... “How do we know that we don’t run out of ordinals?” for one. Notice that there is nothing in Cantor’s story to promise us that this supertask concludes successfully. That’s just assumed.

All good stuff. However there are plenty of people who are unaware that the story can be told without ordinals. The perfect closed set sought by Cantor can be obtained by a single set existence axiom...

$$\bigcap \bigcap \{Y \subseteq \mathcal{P}(X) : X \in Y \wedge D“Y \subseteq Y \wedge (\forall A \subseteq Y)(\bigcap A \in Y)” \quad (A)$$

If you have the axiom scheme of separation then it is obvious that this object is a set<sup>4</sup>. There is an actual set abstract because the construction is deterministic.

Thinking that it’s not only *possible* to prove the existence of perfect closed sets by means of this kind of transfinite recursion but that this is the *correct* or perhaps even the *only* way is a mistake. It’s the mistake of thinking that the abstract resembles the concrete. It’s not a grave mistake because the result we are trying to prove can be proved without making it. We are relying on the hope that monotone discrete deterministic transfinite processes always halt – just as discrete deterministic finite processes always do. And our hope is not disappointed: as it happens we are in luck. Monotone discrete deterministic transfinite processes do, indeed, always halt. (This is beco’s the output of such a process can always be described by a set abstract along the lines of (A) above.) So far our mistake has not got us into trouble. The key ingredient is ‘deterministic’. If we drop it then we find that blah Discrete nondeterministic finite processes in the world of concreta always halt. blah

In this section we have seen how one might come to believe the Axiom of Choice by being confused about the abstract/concrete distinction and the finite/infinite distinction. In the following section 6.4 we consider some lines of thought that might cause one to not believe the axiom.

## 6.4 Why might we not believe it?

It is possible to make the axiom of choice look highly implausible, or perhaps a better word is *unappealing*. I am not going to claim that the patten that follows is a convincing refutation, but it is worth thinking about.

If the Wellordering Principle is true then in particular  $\mathbb{C}$  is wellordered. Now the complexes have an automorphism generated by swapping  $i$  and  $-i$ , but no wellordering of the complexes can have an automorphism: wellorderings are rigid.

This doesn’t mean that the complexes cannot have a wellordering, but it does mean that no wellordering of  $\mathbb{C}$  can be described within that part of complex analysis for which complex conjugation is an automorphism.

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<sup>4</sup>It may look as if you need power set but you don’t: you quantify over all subsets of  $X$  but you don’t need the collection of them to be a set.

Now the reals (and the complexes) are a rather special case. One might feel that the fact that  $\mathbb{R}$  can be completely characterised as the unique complete ordered field, and that  $\mathbb{C}$  is its algebraic closure, means that somehow everything we know about them must arise from that characterisation. And if that characterisation doesn't deliver a wellordering then that must be because there is none. This argument (such as it is) relies on the special character of  $\mathbb{R}$  and  $\mathbb{C}$  and isn't susceptible of wider application.

Some sets, like  $\mathbb{N}$  and the second number class, are naturally wellordered, and we don't need AC to wellorder them. Many other sets are not naturally wellordered in this way, and for them is neither obvious that they should be naturally wellordered nor obvious that they shouldn't. For example, there doesn't seem to be anything in the definition of  $H_{\aleph_1}$  to say that it should be wellordered, but neither does it have any characterising structure that admits an automorphism in the way that  $\mathbb{C}$  does and would tell us that it isn't.

The intuition behind this point about the automorphism of  $\mathbb{C}$  tells us that if there is such a thing as a wellordering of  $\mathbb{R}$  or  $\mathbb{C}$  this fact is not really a fact about the reals, not something that emerges from the innermost nature of the reals; rather it is something dropped on  $\mathbb{R}$  by a passing seagull . . . *not part of the Mathematics*. And where did the seagull get it from, one might ask?

This was very much the kind of response the axiom of choice received a hundred-odd years ago when it was sprung on us by Zermelo. *If you think the axiom of choice is plausible then you are thinking of mathematics in a completely different way from me.*

In short:

If  $\mathbb{C}$  is wellordered then – since  $\mathbb{C}$  has an automorphism – the wellordering must be completely invisible to the arithmetic – indeed the *Analysis* – of  $\mathbb{C}$ . However in some sense there is no more to  $\mathbb{C}$  than Complex Analysis. There is no more to  $\mathbb{C}$  than being the algebraic closure of  $\mathbb{R}$ , and *that* doesn't tell us how to wellorder  $\mathbb{C}$ .

Of course this doesn't tell us that AC is *false* . . .

On this view asking whether  $|\mathbb{R}| = \aleph_1$  or  $\aleph_2$  is as daft as asking  $\aleph_3 \in 5$ ? And don't forget that is surprisingly fiddly to show that  $|\mathbb{R}| = 2^{\aleph_0}$ . How easy is it to prove constructively??

There are lots of stories about where the reals come from: abstraction is a wondrous and many splendoured thing. However we don't need to spell them all out and decide which one is the best beco's it won't matter: *none* of them prepare the ground for the news that  $\mathbb{R}$  can be wellordered. Doesn't mean that it can't of course but, to the extent that the abstraction stories – whatever they are – are the roots of the mathematics of  $\mathbb{R}$ , we are drawn to the conclusion that the wellordering – if there is one – isn't part of that mathematics.

It may be that this shrinking away is part of the same phenomenon as the widely felt distaste for Set Theory: *it isn't Mathematics*. strongly felt but never sufficiently clearly articulated.

In the – concrete – world from which abstract entities are abstracted there is a huge amount of detail (some of which can be used to wellorder things) but a

lot of that information is discarded (concealed) in the process of the abstraction. The wellorderings are not relations for which the equivalence relations we take the quotients are congruence relations, so they don't appear in the quotients.

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If you implement the reals as sets then there is a fighting chance that you might import information about the internal structure of those sets that enables you to distinguish between them and thereby wellorder the set of them. This is in fact precisely what happens in  $ZF + V=L$ .

Natural numbers arise from finite sets thru' equipollence. There's nothing about numbers that tell you which of them should be members of which others. There's nothing about where reals come or what they do that tells you how to wellorder them.

It's always a bit risky to pronounce on the contents of other minds but it's probably safe to say that a lot of people believe AC beco's they believe they can just wellorder any set by brute force – in a *supertask*. This raises questions about Agency which is problematic beco's Pure Mathematics has no agents. But let's engage with this idea anyway and see what emerges<sup>5</sup>.

If you can wellorder everything then in particular you can worder  $\mathbb{R}$ . When people spell this out we are told that there are models of set theory in which  $\mathbb{R}$  is wellordered and also models in which it isn't. These models are good and useful things, but they don't show that it is both consistent that  $\mathbb{R}$  be wellordered and consistent that it not be wellordered. The thing that they say is wellordered (or not, as the case may be) is not  $\mathbb{R}$  but an implementation of it. An implementation of the reals in our preferred foundational system may be wellordered – or made of green cheese, or whatever – but that doesn't tell us anything about  $\mathbb{R}$  itself. If  $\mathbb{R}$  had a natural wellordering (in the way that it has a dense total ordering, for example) then this would surely be captured by any implementation, so the fact that there are implementations of real arithmetic that make the set of reals not wellordered is pretty compelling evidence that the reals, in at least one good sense, simply aren't wellordered.

Or do they want to say that a purported implementation of  $\mathbb{R}$  into set theory isn't an implementation unless it's wellordered? That being wordered is part of the spec? It clearly doesn't follow from the specification of  $\mathbb{R}$  as a complete ordered field. So how does it come to be part of the spec? But nobody seems to want to say that an implementation of a complete ordered field must be wellordered – ZFistas certainly say that there are “models of set theory” in which  $\mathbb{R}$  is not wellordered.

What are the reals anyway? Set theoretic foundationalists would have you believe that  $\mathbb{R}$  is a pure set, a set of pure sets, that every real number is a set.

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<sup>5</sup>This is the kind of situation that the paraconsistentists would say invites us to drop non-contradiction

Pointillism! But this is a mistake. Reals are not sets, they are numbers. The question is not “can we wellorder the pure set that implements  $\mathbb{R}$  in Set Theory? The answer to this yes if our set theory has Gödel’s axiom  $V = L$  (which nobody believes, as it happens) but otherwise possibly not. The reals are the unique complete ordered field, and the question is “Is the unique complete ordered field wellordered?”

\*\*\*\*\*

OLD STUFF BELOW HERE

### 6.4.1 Grue Emeralds

Does every perfect binary tree have an infinite path? Think of the tree  $\{0, 1\}^{<\omega}$  of finite sequences of 0s and 1s partially ordered by end-extension. That is the prototypical perfect binary tree and the stream of zeroes is an infinite path in it. We haven’t examined every perfect binary tree of course, but what we can say is that every perfect binary tree *so far examined* has had an infinite path. Isn’t that evidence that every perfect binary tree has an infinite path? There is a curious echo here of a famous puzzle of Nelson Goodman’s: the grue emeralds. An emerald is grue (see [13]) if either

- (i) it is examined before 1/i/2500 and found to be green; or
- (ii) is unexamined before 1/i/2500 and is blue.

There is much to debate in this gruesome scenario, but one thing is clear: the fact that every emerald so far examined has turned out to be grue is not evidence that all unexamined emeralds are grue.

I offer the thought that the mere fact that every perfect binary tree so far conceived has an infinite path is not evidence that all perfect binary trees have such paths. Granted, every perfect binary tree so far examined has had an infinite path . . . just as every emerald so far examined has been grue. The act of examining the emerald makes it grue, and the act of examining a tree expands it from an object of type **naked-tree** to an object of datatype **tree-with-an-embedding-into- $E^3$** .

Expand this section; think it through properly

[The parallel to keep an eye on is: examining a set wellorders it in the same way that examining the emerald makes it grue.]

A potentially useful thought . . . The examined (and therefore wellordered) set is a different ADT from the unexamined (and therefore naked) set. But the unexamined and examined emeralds are of the same ADT.

There are many subtleties in the problem of the grue emeralds, and this is no place for a thorough examination of them (even were I competent to do it). The key point for us is that the

- (i) grueness (or otherwise) of the emerald, and
  - (ii) our investigation of it for grueness
- are not independent events in the way that

(iii) the greenness (or otherwise) of an emerald and  
 (iv) our investigation of it for greenness  
 are independent. We do not render an emerald green by examining it, tho' we do prevent it from being grue by examining it before 2500.

Examining a perfectly ordinary emerald makes it grue.

Conceiving of a set makes it wellordered. So 'wellordered' is a grue predicate.  
 [speculation here]

A set is wellorderable\* if it is examined and found to be wellorderable, or is unexamined and not wellorderable.

If you examine a set and find it to be wellorderable then you have perforce wellordered it ... expanded it into a wellordering ... haven't you? In fact, if you examine it closely enough to ascertain whether or not it is wellorderable then you perforce wellorder it. So every set is wellorderable\*. Oops, not quite. You might be a wellordered set that hasn't been examined. Perhaps the definition we want is " $x$  is wellorderable\* if it is either unexamined or discovered to be wellordered"

What is going on here? We can't imagine a set without imagining it as a wellordered set? Surely that's not true – think of  $\mathbb{R}$ . But perhaps by imagining it we turn it into something that might be wellordered at some later stage? Perhaps we are simply mistaken in thinking that we can imagine it. It can be argued that we cannot imagine any uncountable wellorderings.

The suggestion is that

- (i) thinking that the fact that all perfect binary trees so far encountered have infinite branches is evidence for  $AC_2$

is the same mistake as

- (ii) thinking that there is inductive support for "all emeralds are grue".

Can we conceive of perfect binary trees with no infinite branches? Certainly there are models of set theory containing such special trees, but of course they do not *really* lack infinite branches. There are infinite branches all right, just not in the special corner of the universe that is the model containing the special tree. So we haven't succeeded in conceiving of a perfect binary tree with no infinite branch; what we have managed to conceive is a perfect binary tree all of whose infinite branches can be overlooked or mislaid ... are in other words *deniable*.

imagining is *concretising*

Does this mean that a perfect binary tree lacking an infinite branch is inconceivable? And does its inconceivability mean it's impossible? These avenues of enquiry will remind some readers of Berkeley's Master Argument for Idealism. Berkeley leads his readers by the nose through a thicket of

"it's inconceivable that anything should exist unconceived"

and its like to

“it is impossible that anything should exist unconceived”.

The parallels are strong, and they are not encouraging for the advocate of AC, since the general view nowadays is that Berkeley’s Master Argument is deeply flawed, and that successful repairs – if any – won’t capture the sense of the original exercise<sup>6</sup>. There are parallels between the Master Argument on the one hand, and – on the other – the thought that AC is obvious because one cannot imagine a set that can’t be wellordered; an exploration of these parallels may be useful to both parties. I don’t think anyone has considered the act of expanding (in the model theoretic sense) a mathematical object to be a mental construction in the way one would in this context, but – altho’ it might be helpful – I do not have the stomach for it. But i’ll have a tentative stab in section 6.4.2 which now follows.

Get the reference right

Thinking about the project of counting a union of countably many countable sets might cause a reader to start thinking about supertasks. My position on this is that it is not a helpful move. *supertask* is not a useful concept (beguiling tho’ some of its instances are) and – even if it were – it isn’t helpful in coming to grips with AC. Indeed the idea that it is helpful in that setting is a result of another error of mistaking something for something else. It comes of thinking of that project in a too concrete –too *agentive*– way.

However, the *aperçu* that moves one to make the mistake of thinking that AC is connected with supertasks is a good one. AC indeed turns out to be a principle about transfinite processes. It is the principle that every monotone nondeterministic process concludes successfully.

#### H I A T U S

Why might we believe it? Why might we not believe it? I think there is a concrete/abstract distinction to be made, and that one of the most widespread causes for people to believe in AC is neglect of this distinction. So, before we tackle either horn of the do-we-(not)-believe dilemma, we should have a discussion of this distinction.

We discuss the two distinctions. Concrete/Abstract and Finite/Infinite. Ignoring the second results in thinking there can’t be infinite sets; ignoring the first results in believing AC. These are different mistakes.

We are forced to consider supertasks beco’s the ‘keep on going’ (KBO<sup>7</sup>) argument for countable choice relies on the plausibility of a supertask story.

Supertasks smuggle in agency but that’s OK as long as you are on your guard.

We want our concept of supertask to be of a discrete kind of thing lest anything done in time become a supertask and we get trapped by a Zeno-like paradox.

<sup>6</sup>I am indebted to Maarten Steenhagen for directing my attention to [27] (specifically pp 127ff.)

<sup>7</sup>And it doesn’t mean *Korean Baseball Organisation*. Ask Google, and mention Churchill.

So a supertask is something discrete, run over infinitely many ordinals.

Many supertasks are formatted to look seductively like infinitely long versions of things of the kind one might actually do, in time. However the separability of  $\mathbb{R}$  means that the realistic feel of supertasks of uncountable length is illusory.

Thus a supertask is a task that involves doing infinitely many things. But not *merely* infinitely many things: obtaining  $\{n^2 : n \in \mathbb{N}\}$  from  $\mathbb{N}$  by squaring everything in  $\mathbb{N}$  is not a supertask: since no single act of squaring interferes with any other, all the squarings can be done independently and simultaneously – in parallel. You have a supertask on your plate only when the infinitely many things cannot be done in parallel but have to be done in succession.

[other considerations? Insert here]

So a supertask is a discrete transfinite process, indexed by ordinals. So it has to halt and when it has halted it's supposed to have achieved something. Output something.

What features do we need to consider in trying to ascertain whether or not a supertask will conclude? Various things to consider: deterministic/nondeterministic, monotone/nonmonotone, cts/not-cts

There will be theorems telling us that a s'task satisfying some of these conditions will halt and succeed. AC will be one of those theorems.

Supertask of trying to build a free complete boolean algebra with countably many generators. (one can build a free complete Heyting algebra with countably many generators.)

They concern us on two fronts:

(i) transfinite processes: can we complete them.

(ii) But also supertasks of *expansion* expanding a naked set into a counted-set

AC tells us that certain supertasks which o/w might not conclude successfully do in fact conclude successfully. To state its consequences clearly we need to identify 3 features.

'Supertask' is a wonderful piece of Adman's jargon. More prosaically a supertask is a discrete task of transfinite length. It is a task spread over infinitely many stages, and those stages are clocked by ordinals. (one has to insist on the ordinals lest any continuous process over the real line should count as a supertask).

So perhaps we want to tease apart two things. We want to construct a Wombat; it looks as if the Wombat can be obtained as a result of a supertask. We design the supertask, and it completes *jez'* fine, but sadly we were mistaken: the output is not the Wombat. An order-preserving injection from the second number class into  $\mathbb{R}$  is a case in point.

**The Axiom of Dependent Choices** (DC) is the principle that every [non-deterministic] supertask of length  $\omega$  can be completed. In its usual formulation it says that,

for any set  $X$  with a binary relation  $R$  satisfying  $(\forall x \in X)(\exists y \in X)(R(x, y))$  there is a sequence  $\langle x_1, x_2 \dots x_n \dots \rangle$  where, for all  $i$ ,  $R(x_i, x_{i+1})$ .

Hang on, what if  $n > \omega$ ?

Consider the supertask in [20] p 26, which we recapitulate here. We have a bag, and infinitely many beads. Our points in time and our beads are both indexed by countable ordinals. At time  $t = 0$  the bag is empty; at time  $t = n$  beads with numbers  $n \cdot 10$  to  $n \cdot 11$  are put into the bag, and the bead with number  $n$  is removed. At time  $t = \omega$  the bag is empty again; every bead that has been put in before  $t = \omega$  is removed before time  $t = \omega$ . (“let  $b$  be an arbitrary bead . . .” then  $b$  is not in the bag at time  $t = \omega$ ). In fact the bag is empty uncountably often. There is no real mathematical significance to this; the point to the trick is that the mathematically naïve can be spooked by the fact that, although at every stage you put in more beads than you remove, nevertheless there are stages at which you have removed everything that you have put in. There is nothing wrong with this really, except the fact that the function  $n \mapsto$  the cardinality of  $\{k : \text{the bead with number } k \text{ is in the bag at time } n\}$  is not a monotone function from the class of countable ordinals to  $\mathbb{N}$ .

I bring supertasks up really only in order to wave them away – I think they are a conjuring trick: they entertain but do not enlighten. However one cannot simply ignore them, since appeal to supertask intuitions underlies many people’s belief in the truth of the Axiom of Choice. They derive their plausibility from beguilingly familiar features which are actually mathematically entirely irrelevant. Using a supertask argument is like working a conjuring trick, or telling a joke well: they have to direct – or rather *misdirect* – the audience’s attention. This is something we shall see again. They also trade on an assumption that the default is for the infinite to resemble the finite. Not in *all* respects it doesn’t – clearly! – so this is a *false* assumption and it needs to be smuggled in subliminally rather than adopted openly so we can all see it. Better still – as in all good joke-telling – the audience should be induced to make the necessary background assumptions *themselves*. Complicity!

### The Separability of $\mathbb{R}$

It is central to the idea of a supertask that the subtasks be done *in succession*. But succession in what? Both the Thompson’s lamp supertask and the countable choice supertask seem to be (notionally) executed in time – actual, physical time. It is this reassuring air of concreteness that lends them what plausibility they have. But one shouldn’t be led by this concreteness to an acceptance that all supertasks are executable. It is a consequence of the separability of  $\mathbb{R}$  that we cannot embed any uncountable wellordering into the reals in an order-preserving way, and this means that we cannot conceive of any transfinite process (supertask) of uncountable length as taking place *in time*.

This has implications for the possibility of arguing for AC on the basis of supertasks. I want to argue that if you sensibly believe AC – in particular that an arbitrary uncountable set can be wellordered, then it isn’t beco’s of supertasks. You can’t wellorder  $\mathcal{P}(\mathbb{R})$  by a supertask since supertasks take place in (notional) time. Once one has taken that on board it seems hardly necessary to follow up with the special case that you can’t wellorder even the reals by supertasks beco’s if you did, then, the reals being uncountable, the



dates at which you pointed to the various reals would form an increasing  $\omega_1$  sequence in the time line, and that is impossible.

If you want to say that your supertasks take place in some other kind of time then you are severing the last link to intuitive motivation.

### 6.4.2 Supertasks: Expansions and Forcible Wellordering

In this section we discuss the possibility of datatype expansions being performed by supertasks. These are ‘expansions’ in the model-theory sense in which the rationals as an ordered field are an expansion of the rationals as an ordered set<sup>8</sup> We expand a countable **naked-set** into a structure of type **counted-set** by performing the (mental) supertask of counting it...or do we?

Actually we have to be **very** careful here. If the supertask consists in non-deterministically pulling members out of the set like rabbits out of a hat, in a discrete wellordered sequence of pulls clocked by the ordinals, then there is no reason to expect that we will exhaust it in  $\omega$  steps (tho’ we do know we will exhaust it in *countably many* steps). To be sure of exhausting it in precisely  $\omega$  steps we’d have to know the enumeration in advance, and be merely *reciting* it, and of course such a recitation effects nothing. In contrast, embedding a perfect binary tree into the plane, or the socks in  $E^3$ , are both supertasks of length  $\omega$ .

It might be worth noting the following dispiriting facts.

- (i) Can’t count a countable set just on being told that it’s countable;
- (ii) Can’t count a countable set on being given a wellordering of it;
- (iii) Can’t count a countable set even if all its members are reals.

need references for these

We consider them in turn.

(i) Every proper initial segment of the set of countable ordinals (the second number class) is countable. If we could count a countable set on being told that it is countable we would have a function that, to each countable ordinal  $\alpha$ , replied with a counting of the ordinals below  $\alpha$ . That would be enough to prove that the set of countable ordinals has no countable unbounded subset, and it is known (since at least [12] and surely earlier) that any proof of this fact must use at least some AC. It’s not even as if there is some strategy we can use on a set  $X$ , some project on which we embark, which is guaranteed – *if  $X$  is countable* – to produce a counting of it (and which produces something uninformative if  $X$  is not countable). We can try picking elements from  $X$  as we run through the ordinals. If  $X$  is countable we will run out of members of  $X$  – and therefore stop – at some countable stage, but there is no guarantee that the output of this process is a counting of  $X$ . It’ll be a *wellordering* all right, but it might not be a *counting*. It is a deep fact that there is no definable way of recovering a counting of a countable set from a wellordering of it.

<sup>8</sup>See [https://en.wikipedia.org/wiki/Model\\_theory](https://en.wikipedia.org/wiki/Model_theory).

(ii) If we could count a countable set on being given a wellordering of it we would have a function that, to each countable ordinal, replied with a counting of the ordinals below it, and this, as we have just seen, needs some AC.

(iii) If we could use the structure of the reals to count a countable set of reals then we would be able to prove that  $\mathbb{R}$  is not a union of a countable set of countable sets, and it is known that we can't.

Even once one has taken on board the idea that **naked-set** is a different type from **counted-set** one can still fall into the trap of thinking (and i have heard people say this) that some objects [ $\mathbb{N}$  is the obvious example] are obviously counted sets whereas some are obviously merely countable. That's the wrong way to think. It's not that  $\mathbb{N}$  is obviously a **counted set**, it starts off as a **naked-set** like everyone else; it's that the **naked-set**  $\mathbb{N}$  *happens to* have a counting [the identity map will do nicely] that is rather more salient than the counting of – for example –  $\mathbb{Q}$ , or – to take a more extreme example – the set of recursive ordinals.

need more discussion here

Interestingly to describe this situation properly we seem to need a concept of a structure –  $\mathbb{N}$ – which we are at liberty to think of as set, a worder, a ...

Stuff deleted from here and  
moved to counting.tex

Perhaps there is some profit to be derived from the exercise of applying an analysis like this to the counting of countably infinite sets. This gets very murky indeed, since it involves supertasks and the axiom of choice. It's a situation where one has to make risky assumptions simply to get off the ground.

OK, so i have a set  $X$  which happens to be countable. How do i count it? I attempt to build a bijection between it and my collection of natural-number-representatives, of counter sets. This is legitimate as long as **set** is the kind of datatype that supports random access and replacement (which is OK) but we also need to be able to perform the supertask of indicating members of  $X$  (novel members of  $X$  indeed) infinitely often. The assumption that we have this kind of ability is a nontrivial choice principle, since it implies that every infinite set has a countable subset.

And even if that is all right (which i don't think it is, but never mind) we are still not satisfied, and here's why. Let  $X$  be an infinite set, and let us pair off members of  $X$  with counter sets. That shows that  $X$  has a countable subset. It doesn't show that  $X$  is countable, because there might be stuff left over *even if  $X$  is – in fact – countable*. How are we to ensure that the process of counting, of matching up members of  $X$  with counter sets, exhausts  $X$  and the set of counter sets simultaneously? Clearly the only way of ensuring that is for  $X$  to come equipped with a wellordering of length  $\omega$ . Notice that even having  $X$  being actually countable, plus being equipped with a wellordering doesn't help us. There is no effective route from a wellordering of a countable set to a counting of that set.

### 6.4.3 A Parallel with Berkeley's Master Argument?

One conceives the socks (like the shoes) as scattered through 3-space (how else is one to conceive them?). But once one has done that, one has perforce wellordered them. Can one conceive a counterexample to the axiom of choice? Doesn't that sound rather like the task of conceiving "of a tree or house existing by itself, independent of, and unperceived by any mind whatsoever"? A parallel with Berkeley's Master Argument is something one could do without. There seems to be general agreement in the literature that Berkeley's argument is fallacious, and there seems to be general agreement, too, about roughly where the fallacy is located, but no agreement on its precise nature.

This suggests that one can (attempt to) argue for the axiom of choice in the same way that Berkeley argued for idealism: it is impossible to conceive of a counterexample. One can argue about how good Berkeley's argument was in the first place, but it is also worth thinking about how good the parallel is.

Can't conceive any uncountable wellorderings

If we conceive of the countably many pairs of socks then we conceive the set of socks as countable. So the thought is not that Russell's parable smuggles in spurious information, the thought is rather that the information which is smuggled in was inevitably going to be dragged in by the process of imagining the socks. Suppose you don't imagine the socks as socks, but as proxies for the members of the pairs in the countable set of pairs (for which they were of course proxies all along). Then you are imagining a countable family of pairs, in effect a countable family of pairs of *reals* since we imagine these things as living in space (don't we?) and since 3-space is in bijection with  $\mathbb{R}$  and  $\mathbb{R}$  has a total order this gives us a choice function on the pairs. So a countable set of pairs that lacks a choice function is inconceivable.

However, we should beware. If the plan is to promote the axiom of choice by saying that counterexamples are inconceivable, then the notion of inconceivability we will need is too broad and the argument proves too much. The point is that if *conceivable* means anything remotely like *visualisation* then we have to remember that all uncountable wellorderings are inconceivable . . . in that sense. (It is a point worth making to beginners in set theory, particularly strong beginners, that there is no point in trying to imagine the set of countable ordinals; being used to real analysis they need to be told, right from the outset, that the second number class is a very different beast from  $\mathbb{R}$ ). And we don't need the axiom of choice to prove the existence of uncountable wellorderings. All that is needed is the axiom of power set and some minimal amount of separation such as can be found in Mac Lane set theory or even KF. So this argument proves too much. Well, too much for *this* writer's taste. There may be people who draw up their skirts in horror at the thought of uncountable wellorderings, but one would prefer to keep inconceivability-of-counterexamples arguments for AC uncoupled from sensitivities about the existence of uncountable wellorderings. After all, there are plenty of people who want to be able to argue for the axiom of choice and simultaneously believe in the existence of uncountable wellorderings. Arguing for AC by claiming the inconceivability of counterexamples is not

We haven't described these theories...

an option for them.

But i suppose there might be a difference between the way in which counterexamples to AC are inconceivable and the way in which uncountable wellorderings are inconceivable. The obvious people to tease out this difference are people who want to both argue that AC is true (because counterexamples to it cannot be conceived) and that uncountable wellorderings exist. I am not such a person.

This needs to be developed

Summary: If we can find a notion of conceivability according to which uncountable wellorderings are conceivable but counterexamples to AC are not then one can run an analogue of Berkeley's Master Argument to show that AC is true. A lot of potholes!

## 6.5 Some Subtleties

So what is one to make of the line of patter routinely sold to undergraduates, the purported proof that a union of countably many countable sets is countable? It's not a proof [that a union of countably many countable sets is countable] because we know that there is no such proof: it's possible for the premisses to be true but the conclusion false. So what is it?

Is it an ellipsis? If it were an ellipsis, it would be flagged as such, and a subsequent unpacking would be promised. But no such explanation is provided, just a pretence that everything is going to be OK, like the Blonde Expedition to the Sun "It'll be OK, we're landing at night".

Telling them that is a proof doesn't just give them false beliefs about the axiom of choice [which is bad enough] it gives them false ideas about the nature of proof, and that is much more serious.

Since it's not an ellipsis for a valid proof it's a defective proof, otherwise known as a *fallacy*. What nature of fallacy? A: Fallacy of equivocation.

It's very striking that there should be people who think that AC is obviously true and obviously indispensable but nevertheless don't want to tell their students anything about it beyond the bare minimum needed to secure their compliance. This is rather the way that priestcrafts treat their lay flock.

Some things are simultaneously too important to be altogether withheld from the lay flock yet also so fraught with complications that one shouldn't risk trying to explain them to the lay flock. A delicate balancing act is called for.

However, if the institutions are to persist through time, at some point you have to identify those members of the lay flock that are going to go on to be priests, and let them into the secret. Most religions have got the hang of it. Mathematics hasn't.

Edit this

Christianity would never have got where it is today if the first christians had said to themselves "The death of Jesus on the cross is this hugely important event that gives us all the possibility of Eternal Life – *but we're not going to tell anyone about it*".

This policy of not telling students about AC is not a considered result of a set of deliberations; it's a continuation of what would have been a sensible policy

before we understood the rôle of AC, compounded by a *post-hoc* rationalisation of bad practice that should have been abolished by the discovery of the axiom. rewrite this para

### 6.5.1 Banach-Tarski

Also Vitali,  $\mathbb{R}$  as a VS over  $\mathbb{Q}$

This has been moved...

### 6.5.2 Infinite Exponent Partition Relations

There is a theorem [find references] that says that infinite exponent partition relations violate choice. Conversely, if choice fails, one can sometimes find models in which some infinite exponent partition relations hold. What is going on? If one is undecided about AC what is one to believe? What is the mathematical content of these nonexistence proofs? I think the correct response is to say that the possibility of the truth of AC means that there are no algorithms [even in an extended sense] for finding infinite monochromatic sets for partitions of infinite sets...no constructive proof of their existence. Indeed there is a useful parallel here with the critique of classical logic by the constructivists (the exhibitionists). You don't have to discard the classical proof altogether, you just have to start thinking of it as a proof of something else.

### 6.5.3 $\text{AC}_\omega^\omega$

If we make the countably many choices in advance then, in executing the zigzag algorithm, all we are doing is passively executing a deterministic process; (we are the machine on which it runs, and we are not making any choices at all) we are merely proving that a counted union of counted sets is counted. At stage  $\binom{m+n}{2} + m$  we take the  $m$ th thing from the  $n$ th counted set.

Nathan says this should be incorporated into the section on supertasks

However it looks as if there is another way of doing it, by making infinitely many choices *in succession* rather than simultaneously. There are people who have taken on board the fact that one needs countable choice to prove that a union of countably many countable sets is countable, but haven't fully grasped the manner in which AC is put to use in proving it. If you are trying to prove that a union of countably many countable set is countable then, you might think, when you visit the  $x$ th set to get its  $y$ th member you have to invoke the axiom of choice to obtain that  $y$ th member – because that  $x$ th set is merely countable not counted. As we've just seen, that is not in fact how the axiom of choice is used in this proof. The endeavour to give a formal description of this strategy results in a story along the following lines.

explain DC

“First you count the index set, so you have a family  $\{A_i : i \in \mathbb{N}\}$ . As we have observed, this costs nothing. Then at stage 1 you ask all the  $A_i$  for an element, using a choice function  $f_1$ . Where does this  $f_1$  come from? Well, there is this choice principle called  $\text{AC}_\omega^\omega$  that says that if I have a countable family  $\{A_i : i \in \mathbb{N}\}$  of countable

sets then there is a function picking one element from each. This function can be thought of as an  $\omega$  sequence  $i \mapsto f_1(A_i)$  of elements from the union of the  $A_i$ . Replace each  $A_i$  by  $A_i \setminus \{f_1(A_i)\}$  and do the same thing, this time using  $f_2$ , which is a choice function that  $\text{AC}_\omega^\omega$  tells you is to be had for the family  $\{A_i \setminus \{f_1(A_i)\} : i \in \mathbb{N}\}$ .

Subsequently you iterate, at each stage using a function defined on the (set of the) remains of the  $A_i$ s, concatenating the  $\omega$ -sequence you have just obtained onto the end of the sequence you have been constructing so far, so that after  $n$  steps you have a wellordering of length  $\omega \cdot n$  of a subset of  $\bigcup_{i \in \mathbb{N}} A_i$ . You keep doing this – possibly transfinitely – until the  $A_i$  are all used up. Of course there is no guarantee that the  $A_i$  all run out at stage  $\omega$ , so the process might be of transfinite length. But at least, when it stops, you have a wellordering of  $\bigcup_{i \in I} A_i$ .”

Interestingly this doesn’t prove that  $\bigcup_{i \in I} A_i$  is countable. *Wellordered* yes, but that isn’t enough to show that it is countable. We will return to this later. For the moment our concern is to understand how AC is used in the proof that a union of countably many countable sets is countable.

It turns out that here we are using more than merely  $\text{AC}_\omega^\omega$ . The axiom we are using in this construction is actually the (presumably much stronger) “There is a global function that assigns to each countable family of countable sets a choice function”. But observe that even if you have this your desired end will not be reliably achieved. Suppose I use this function  $\omega$  times, what have I achieved? I have a wellordering of length  $\omega^2$  that, for each  $i$ , contains infinitely many elements of  $A_i$ . I don’t know that I have got everything in  $A_i$ . I can persist with this process, and run it transfinitely, and eventually I will have wellordered the whole of  $\bigcup_{i \in \mathbb{N}} A_i$  – but there is no visible countable bound on how long this process will run. It is true that each  $A_i$  will be exhausted in countably many stages – at stage  $\alpha_i$ , say, to give it a name – but how do we know that the set  $\{\alpha_i : i \in \mathbb{N}\}$  is bounded below  $\omega_1$ ? That allegation follows from countable choice, as we know, but the obvious proof (try it) exploits the power to pick representatives from countable families of *uncountable* sets. All we have proved is that a countable union of countable sets is wellordered and of size  $\aleph_1$  at most.

Thus it seems that  $\text{AC}_\omega^\omega$  doesn’t (at least not straightforwardly) prove that a union of countably many countable sets is countable. What can we actually do with it? The obvious thing to do is to try the doomed strategy above and see how far we can go with it. Consider the special case where our countable family of countable sets is in fact a family of *finite* sets. Socks! Clearly our axiom will tell us that we can count the set of socks in the attic. We use the axiom once to simultaneously pick one sock from each of the  $\aleph_0$  pairs. Each pair then has only one sock left, and we are done.

### THEOREM 3

$\text{AC}_\omega^\omega \vdash$  For every  $n \in \mathbb{N}$ , every countable family of sets all of which are of size

$n$  at most has a sumset of size  $\aleph_0$ .

*Proof:*

By induction on  $n$ .

The theorem is clearly true for  $n = 1$ . For the induction step suppose  $\mathcal{F}$  is a countable family of sets all of size  $n + 1$  at most, and suppose that any countable family of sets all of which are of size  $n$  at most has a sumset of size  $\aleph_0$ . By  $\text{AC}_\omega^\omega$  we have a selection function  $f$  that picks one element from every set in  $\mathcal{F}$ .  $\mathcal{F}' = \{x \setminus \{f(x)\} : x \in \mathcal{F}\} \setminus \{\emptyset\}$  is now a family to which the induction hypothesis can be applied (all of its members are of size  $n$  at most) so it has a sumset of size  $\aleph_0$ . But  $\bigcup \mathcal{F}$  is  $\bigcup \mathcal{F}' \cup f''\mathcal{F}$ , and  $f''\mathcal{F}$  is clearly countable. So  $\bigcup \mathcal{F}$  is the union of two countable sets and is countable. ■

What if  $\mathcal{F}$  is a countable family of sets all of them finite, but with no finite bound on their size? This theorem tells us nothing about this situation at all!

Nathan says we can prove thm 3 as follows. Suppose we have a countable family of finite sets. Associate to each set the (countable!) set of its total orderings. Use  $\text{AC}_\omega^\omega$  to pick an ordering for each, and then concatenate them. This actually shows that a union of countably many finite sets is countable.

This works because each set of total orders is finite. If each set in the family is infinite then it won't work.

Suppose we know that a union of countably many countable sets is wordered. (This is compatible with failure of countable choice beco's  $\omega_1$  can be singular.) Then  $\text{AC}_\omega^\omega$  follows, because a global wellordering on the union provides a choice function on the summands. So, clearly,  $\text{AC}_\omega^\omega$  doesn't imply that a countable union of countable sets is countable. If it did, we would have an outright proof that  $\omega_1$  were regular.

#### 6.5.4 Agency

Most of the ways of pointing up the use of AC involve making fine distinctions that seem to invoke the concept of agency, and this is something of which mathematicians are suspicious, and rightly so: Mathematics is agent-independent. It's one of the reasons why the constructivist critique of classical mathematics gets a cool reception. When we say "it may be that it can be counted, but not by you" it looks as if we are relativising mathematics to agents. On the face of it agency is clearly involved in the constructive critique. ... but it isn't really; what we are actually doing is invoking a concept of *information*.

See the chapter in `logicrave.tex` about Agency

Perhaps this belongs with supertasks

Curry-Howard reeks of agency; anything to do with computability reeks of agency. But remember that a function can be computable even if the agent doesn't know how to compute it. If computation involves agency, who is the agent? The Turing machine?

AC looks plausible in some versions and implausible in others. There are plenty of people who find it entirely plausible that every surjection should have

a right inverse but baulk at the tho'rt of every set being wellordered. How can you wellorder  $\mathbb{R}$ , after all? If you think propositions  $A$  and  $A'$  have different features then clearly you think they are different propositions. If you find one version of AC plausible and the other one implausible then these two alleged versions of AC cannot both be versions of AC, and you are misidentifying at least one of them – and, if one, then perhaps both...? Why not?

If you think it is implausible that every set can be wellordered then you are probably thinking that the wellordering must be in some sense definable. If you think it plausible that every surjection has a right-inverse then you are probably thinking of the set as having useful added structure. In general you are probably equivocating over different concepts of set.

Of course it might also be that the reason why you don't think that  $\mathbb{R}$  can be wellordered is that you can't imagine a wellordering of the reals, and the reals look so familiar that you expect that if there were one, you would be able to imagine it.

Paradoxically you might find yourself more inclined to believe that arbitrary sets can be wellordered than that  $\mathbb{R}$  can be wellordered! Is this an example of the conjunction fallacy?

## Envoi

With the best will in the world it is hard to see a project to justify AC by appeal to supertasks as anything other than a fancy way of depicting an uncritical and unjustifiable extrapolation of finite behaviour to the infinite. The infinite doesn't resemble the finite closely enough to justify AC.

Using the axiom of choice isn't bad mathematics so much as bad *practice*.

Banach-Tarski is loaves-and-fishes.

Why do we ignore AC? It's not because (like Ax Power set) it's straightforwardly true and there's nothing to stress about. AC is not straightforwardly true in that way.

When confronted with entirely novel stimuli we reach for the tools that we have to hand, however inadequate they be, and try to see the new data as substrates for those old tools. This results in our applying perfectly good intuitions to material for which those intuitions were not designed. It also results in us performing fallacies of equivocation.

Dually when presented with new tools we try to apply them to old problems. When some clever bugger invented the hammer there was a mad rush to go through old outstanding problems to see if any of them were nails.

As Ben Garling says, we often find that old foundational crises leave behind them scars in the form of expository/pedagogical problems.



As Oron says: “Why didn’t mathematicians work this out ages ago? Why do they understand so little?” I think the answer is that for most of mathematical practice a clear understanding of the axiom of choice is not really required. Most of mathematics is the study of the finite, and AC holds in finite domains. Some things that are easy to understand are also easy to *misunderstand* and – unless there is an immediate and dire cost to misunderstanding that thing – one can continue to misunderstand it for a long time, while nevertheless continuing to enjoy the nice warm feeling brought to one by that misunderstanding-which-one-mistakes-for-an-understanding – which of course is *phenomenally* the same as the nice warm feeling one obtains from *actual* understanding. In these circumstances there are no cues to tell one that one is going down the wrong path.

But perhaps this is a good moment to remind ourselves that AC is not a proposition but a licence: it is not in *proofs* that choices are to be found: it is in *constructions*. Mathematics is not a body of truths/propositions, but a body of constructions, and that is the only way to understand AC. It’s not declarative but performative : it confers a licence. Think of Euclid’s *Elements*: it’s not a body of theorems but a *recipe book*, a body of *instructions for doing things*. You are allowed various tools: for example you are empowered/authorised to draw a line through two points; to draw a circle whose centre is at  $x$  and has  $y$  on the circumference, and so on. In that spirit AC allows you to wellorder anything.

Explain ‘performative’

## 6.6 A section on Skolemisation?

### HIATUS

blend these two

This is an important topic, and there is an extensive technical literature on it. A good place for the determined interested reader to start would be <https://plato.stanford.edu/entries/epsilon-calculus/>

Let  $\phi(,)$  be a binary relation such that  $(\forall x)(\exists y)\phi(x, y)$ . A skolem function for  $\phi$  is a function  $f$  such that  $(\forall x)\phi(x, f(x))$ . The assertion that for any such  $\phi$  we can find a skolem function does look very much like an application of the axiom of choice. Remarkably one does not need the axiom of choice if one wishes to pretend that such  $\phi$  have Skolem functions. What is going on is this. Suppose  $T$  is a first-order theory in  $\mathcal{L}(\phi)$ , the language that contains the expression  $\phi$ , and  $T \vdash (\forall x)(\exists y)\phi(x, y)$ . Suppose further that we expand  $\mathcal{L}(\phi)$  by adding a symbol ‘ $f$ ’ and an axiom  $(\forall x)\phi(x, f(x))$ , giving us a new theory  $T'$  in the expanded language. Then  $T'$  is consistent if  $T$  is, and there is no use of the axiom of choice in the proof!

Sadly the matter involves some fairly technical logic (proof theory in particular) and is probably not to the taste of most readers of this book. However the need clarification cannot be disregarded altogether, since the thoughtful reader will immediately want to apply this relative consistency result to the “countable union of countable sets is countable” situation. Let us suppose we are reasoning

in some theory  $T$  that empowers us to perform certain manipulations on sets. Let  $\{X_i : i \in \mathbb{N}\}$  be a counted family of countable sets. Saying that the  $X_i$  are all countable is to say that  $(\forall i)(\text{there is a counting of } X_i)$ . But then we can consistently suppose that there is a Skolem function  $f$  sending each  $i$  to a counting of  $X_i$ , and we can use  $f$  in the zigzag construction to obtain a counting of the union. What's not to like? What's not to like is that the authorisations  $T$  gave us to do whatever-it-was that it authorises do not extend to manipulating  $f$  since  $f$  is not mentioned in the language that  $T$  lives in.

Say something about skolemisation in resolution proofs in first-order logic!

## 6.7 Isn't it simplest just to believe it?

The case for the axiom of choice is made by appeal to thought-experiments involving scenarios that turn out on close inspection to be implausible in the extreme. The perfect binary tree must have a branch. To contort this into a case for AC you have to have a perfect binary tree about which you know so little (in which you have taken so little interest) that you haven't even got a Hasse diagram for it ("shows how much you care!") but which is nevertheless so important to you that you ardently desire an infinite path for it, and for which you must therefore be allowed to use AC. I say: show me such a tree and I'll start thinking there is a case for AC as an axiom. I'm still waiting.

"Suppose there is a countable family of countable sets that we don't know how to count ..." Why should I suppose such a thing? How likely is it that I might run into one?

I say to believers in AC: yes, you occupy the rhetorical high ground, so you don't need any arguments. But suppose you didn't occupy the rhetorical high ground; how do you – a believer in AC – occupy and defend it?

### 6.7.1 AC keeps things simple

So that everybody is called 'Bruce'. [2] Finite sets obey AC, and mostly the infinite sets we encounter in mathematics do too. Isn't it a reasonably sensible simplifying assumption to make that unobserved sets will behave like observed sets? There are two replies to this. One is that it might be that if we made more strenuous efforts to observe the unobserved sets one might observe them and make interesting discoveries about them. The second is that not every observed set is observed to be wellordered anyway! Don't forget that the best information we have about wellordering the reals is that we *know* there is no definable relation that can be proved to wellorder them. (This allows there to be definable relations that might, in suitable models, wellorder the local *simulacrum* of the reals, and this is in fact the case.)

blend these two paragraphs

There are people who do not have a philosophical position on the nature of sets and mathematical entities but who just want to get on with their mathematics. They need a reason to jump one way or the other on the question of the axiom of choice. One suggestion that might carry some weight with such people

is that the axiom of choice is a good thing because it *keeps things simple*. If AC fails there are these annoying objects around: infinite sets without countable subsets, countable sets of pairs of socks without a counting of the socks, and so on. Who needs them? Aren't they just a pain?? Why not adopt the axiom of choice and be shot of them all?

One reply that one would like to use, but can't, is that this flies in the face of a widely used (and thoughly bad) line of talk about set-theoretic axioms, namely that one should populate the mathematical universe with everything one can. This line of talk is terrible, because *Extremalaxiomen* of this kind basically never make sense. But this reply may be worth using nevertheless, since there are people who are susceptible to maximisation principles of this kind, and might be induced to adopt ... infinite sets without countable subsets, countable sets of pairs of socks without a counting of the socks, and so on, as above.

Widespread though this view is, and appealing though it undoubtedly is, it really is entirely without merit. The choiceless family of pairs of socks is a pain, no doubt, and it seems we would be better off without it. But then the paradoxical decomposition of the sphere is a pain too, and you get that if you adopt AC. Not only is it a pain, but it is a pain of a very similar stamp: the pathological sock collection and the paradoxical decomposition of the sphere alike have the twin features of not only being initially counterintuitive but also – even on inspection – lacking any motivation in what one might tempt fate by calling *ordinary mathematics*. However the point is not so much the tit-for-tat point that the Axiom of choice has some pathologies that are as gross as the pathologies associated with its negation; the point is that it is a mistake to try to anticipate what mathematics will throw at us. We simply can't dam' well ignore things we don't like. Perhaps there *just are* bad families of pairs of socks, in the way that (at least according to AC) there *just are* paradoxical decompositions of the sphere. Granted: the paradoxical decomposition of the sphere no longer looks paradoxical, but the fact that something that looked paradoxical *then* no longer looks paradoxical *now* serves only to remind us that something that looks pathological at the moment might look a lot less pathological in fifty or a hundred years' time.

It may well be that the wisest course in relation to the axiom of choice is the same course as the  $\sqrt{2}^{\sqrt{2}}$  story leads us to in relation to the law of excluded middle. Use it sometimes, but bear in mind that there may be other times when the news it brings you is useless to you. And to always – *always* – prefer proofs that do not use it to proofs that do.

The current situation with AC is that the combatants have agreed to differ. People who are fully signed up to the modern consensus realist view of sets as arbitrary objects-in-extension believe – almost without exception – that the axiom of choice is true. There is a smaller party – consisting largely of constructivists of various flavours – who have a subtly different – and more intensional – concept of set and who in consequence do not accept the axiom of choice. However it has to be said that the philosophy of mathematics generally subscribed

to by constructivists does not accord Set Theory the foundational role (i nearly wrote *leading role* there) many others do. In consequence they do not have a lot to say about the axiom of choice in its set-theoretic garb.

As well as the agreement (between the camps) to disagree there appears to be agreement within each camp. The emergence of the axiom of determinacy (which contradicts AC) caused a few flutters among the platonists: the axiom couldn't simply be ignored: it was far too interesting for that. And to accept it would be to reject AC. They found instead a way of domesticating it: certain large cardinal hypotheses imply that it is true in a natural substructure of the universe. That way they get the best of both worlds.

## 6.8 Are there Principled Reasons for Believing AC to be true?

We don't seem to be getting very far with making AC look plausible by deducing obvious truths from it. So can one argue for it directly? Are there principled reasons for believing AC to be true?

As we have just noted, it seems to be the case that most of the people who believe that the Axiom of Choice has a truth-value at all tend to believe that that truth-value is 'true'. I think this is a common-cause phenomenon: the forces that lead people to believe that the axiom of choice has a truth value tend also to make them think that that truth-value is 'true'. The forces at work here are various kinds of belief in the ultimate reality of mathematical objects, and ways of thinking about those objects. If a set is real, then you can crawl all over it and get into all its nooks and crannies. And, by doing that, you perforce wellorder it. After all, if – having time on your hands as one does when one is trying to fall asleep by counting sheep – you count members of your set then you will wellorder it. You never run out of ordinals to count the sheep with (that is Hartogs' lemma) so your endeavour to wellorder the set cannot fail. And if you didn't count Tweedledum before you counted Tweedledee that can only be because you counted Tweedledee before you counted Tweedledum. Again, a supertask.

On this view the axiom of choice is just plain true, and the intuitive argument for it is that one can boldly go and straightforwardly just bloody well wellorder the universe *by hand* as it were. To be more precise, the axiom of choice (on this story) follows from realism about mathematical objects. (We saw earlier – p. 9 – how it looks plausible that AC follows from second-order set comprehension) The force of this story derives from the plausibility of the idea that we can just go on picking up one thing after another until we have picked up everything. We can do it with material objects and so – being realists about sets as we are – we expect to be able to do it to sets.

If you are platonist you believe that every set is out there, somewhere, to be pawed and pored over. If you paw it long enough you can probably wellorder it. If you examine the set of pairs of socks long enough, you will be able to pick

one sock from each pair. At least that's what it looks like to most platonists. If you are a platonist you believe that it is possible (at least for a suitably superior intelligence) to know everything there is to know about a mathematical object such as a set, so you know how to wellorder any set. Why should you be able to wellorder it? Nobody seems to know. It's probably something to do with an ill-formulated intuition about the ultimately deterministic nature of mathematical entities. This intuition may have the same roots as the intuition behind what philosophers call *bivalence* ... and it may of course be a mistake! There just might be mathematical objects that are of their essence sufficiently nondeterministic for us not to be able to wellorder them but we don't seem to be able to imagine any at the moment. Indeed we might not be able to *imagine* any – ever. If we could imagine them, one feels, one would be able to wellorder them. (Might this be something to do with the fact that 'imagine' seems to mean 'visualise' and once we visualise something we can wellorder it? In this connection see the discussion on the significance of our intuitions of space on page ??.) There is an echo here of the phenomenon of *self-refutation* as in "It is raining and I don't believe it"; "I can't say 'breakfast'" and perhaps Berkeley's master argument for idealism.

But this is just the supertask mistake

This way of thinking about sets is nevertheless entirely consonant with the way in which the *sutra* of the socks is recounted. To the mathematical realist it seems perfectly clear that the set of socks is countable, even at the same time as it is clear to the realist that lesser mortals might be unable to count them and might well come to believe that they form an uncountable or Dedekind-finite collection. The Bounded Being remains unconvinced that the set of socks is countable but that is only because the Bounded Being has incomplete information. Should the Bounded Being ever be given the full story about the socks (s)he will see immediately that the socks are wellordered. Sets are like that: I can hear siren voices saying things like ... "*being wellordered is part of our conception of set*"; or "*if you can conceive it you can wellorder it*"; or "*if you can't wellorder it then it's a not a completed totality*".

There are two things wrong with this story. The first is that the imagery of picking things out of a set *in time* is restricted to sequences of choices whose length can be embedded in whatever it is that measures [our conception of] time, presumably  $\mathbb{R}$ . We cannot embed into  $\mathbb{R}$  any wellorderings of uncountable length so this story never tells us how to wellorder uncountable sets.<sup>9</sup> This doesn't mean that the story is wrong, but it does demonstrate that its intuitive plausibility – beguiling tho' it is – is entirely spurious – even if you believe in supertasks.

The other problem is this. For it to be plausible that we can wellorder the universe by brute force we have to be sure that as long as we can pick  $\alpha$  things for every  $\alpha < \lambda$  then we can pick  $\lambda$  things. This is all right if  $\lambda$  is a successor ordinal: as long as there is something left after we have picked  $\alpha$  things then we can pick an  $(\alpha + 1)$ th thing. That's just straightforwardly true, and it's the

<sup>9</sup>An embedding of an uncountable wellordering into  $\mathbb{R}$  would partition  $\mathbb{R}$  into uncountably many half-open intervals, each of which would have to contain a rational. There aren't enough rationals to go round.

argument we saw in the proof of theorem 1. Our realist intuitions get us this far, and thus far they are correct. The problem is that this is not enough: we still have to consider the case when  $\lambda$  is limit, and then we need something that says that all the possible ways of picking  $\alpha$  things for  $\alpha < \lambda$  can be somehow stitched together. And for that one needs the axiom of choice. The point is that, at each successor stage the assertion “I can pick something” is just syntactic sugar for “there is still stuff left”; the difference between the two sounds substantial but it isn’t. If one makes the successor step look *more* significant than it really is (by using syntactic sugar) then a side effect is that the difference between the successor stage and the limit stage is made to seem *less* significant than it really is.

We are back in exactly the situation we saw on page 38. Realism doesn’t get you the axiom of choice: what it gets you is the right to tell the story on page 38 in beguilingly concrete terms. This argument for the axiom of choice derives all its plausibility by artfully concealing the assumption that *the infinite resembles the finite in the way required*. The italicised assumption turns out to be precisely the axiom of choice. This is not to say that the platonists are wrong when they claim that AC holds for their conception of set, merely that this story isn’t an argument for it.

Finally here is something one can say to people who think the axiom of choice is true for sets. *Take very seriously the idea that there might be lots of datatypes of set; what you are currently thinking of as one datatype is in fact many..* Is their concept of set the absolutely rock-bottom concept of **naked-set**? Or is it not perhaps one of the assorted richer datatypes of **decorated-set**? [decoration unspecified for the present] If there is a rock-bottom, minimalist type of **naked-set** then it is plausible that AC might fail for that type even if it holds for others, so the intuition that AC holds for sets might instead be an intuition about one of the datatypes of **decorated-set**. That’s not to say that, of all the various concepts of set we need in mathematics, the rock-bottom concept of **naked-set** is the one that will loom largest in the thinking of mathematicians, but it does offer a way of representing the believers and the unbelievers as not actually disagreeing. Of course this the old point about a fallacy of equivocation from section 3.2.

### The Consistency of the Axiom of Choice?

Let us return to the idea that, if we have perfect information about sets, we can well-order them. This may be wrong-headed, but it does give rise to an idea for a consistency proof for the axiom of choice. Recall the recursive datatype WF: its sole constructor adds at each stage *arbitrary* sets of what has been constructed at earlier stages. If we modify the construction so that at each stage we add only those sets-of-what-has-been-constructed-so-far about which we have a great deal of information, then with luck we will end up with a model in which every set has a description of some sort, and in which we can distinguish socks *ad libitum*, and in which therefore the axiom of choice is true. This even gives rise to an axiom for set theory (due to Gödel) known as “ $V = L$ ”.  $V = L$  is the

Does this belong in the other volume?

axiom that asserts that every set is *constructible* in a sense to be made clear. No-one seriously advocates this as an axiom for set theory: none of the people who think that formulæ of set theory have truth-values believe that  $V = L$  is true; it is taken rather as characterising an interesting subclass of the family of all models of set theory.

There are various weak versions of the axiom of choice that the reader will probably need to know about. **The axiom of countable choice** (“AC<sub>ω</sub>”) says that every countable set of (nonempty) sets has a choice function.

Both these versions are strictly weaker than full AC. DC seems to encapsulate as much of the axiom of choice as we need if we are to do Real Analysis – well, the minimal amount needed to do it sensibly. DC does not imply the various headline-grabbing pathologies like Vitali’s construction of a nonmeasurable set of reals nor the Banach-Tarski paradoxical decomposition of the sphere. There are also other weakened versions of AC, but these two are the only weak versions that get frequently adopted as axioms in their own right.

In this connection one might mention that people have advocated adopting as an axiom the *negation* of Vitali’s result, so that we assume that every set of reals is measurable. Since this is consistent with DC we can adopt DC as well, and continue to do much of Real Analysis as before, but without some of the pathologies. Indeed one might even consider adopting as axioms broader principles that imply the negation of Vitali’s result – such is the Axiom of Determinacy. However that is a topic far too advanced for an introductory text like this.

### 6.8.1 IBE and some counterexamples

Can we argue for AC by IBE? There is a *prima facie* problem in that there are some consequences of AC that people have objected to at one time or another. We have already mentioned Vitali’s theorem that there is a non-measurable set of reals, and the more recent and striking Banach-Tarski paradox<sup>10</sup> on the decompositions of spheres. Nor should we forget that when Zermelo [31] in 1904 derived the wellordering theorem from AC the reaction was not entirely favourable: the wellordering of the reals was then felt, initially, to be as pathological as Banach-Tarski was later.

However, one can tell a consistent and unified story about why these aren’t really problems for AC. There is, granted, a concept of set which finds these results unwelcome, but that concept is not the one that modern axiomatic set theory is trying to capture. The view of set theory that objects to the three results mentioned in the last paragraph is one that does not regard sets as fully extensional and arbitrary. How might it come about that one does not like the idea of a non-measurable set of reals, or a Banach-Tarski-style decomposition of the sphere, or a wellordering of the reals? What is it that is unsatisfactory

<sup>10</sup>Q: What is a good anagram of ‘Banach-Tarski’?

A: ‘Banach-Tarski Banach-Tarski’.

about the set whose existence is being alleged in cases like these?<sup>11</sup> It's fairly clear that the problem is that the alleged sets are not in any obvious sense definable.<sup>12</sup> If you think that a set is not a mere naked extensional object but an extensional-object-with-a-description then you will find some of the consequences of AC distasteful. But this means that in terms of the historical process described in section ?? you are trapped at stage (2). Once you have achieved the enlightenment of stage (3) these concerns evaporate. Nowadays mathematicians are happy about *arbitrary* sets in the same way that they are happy about *arbitrary* reals.

### Constructive Mathematicians do not like AC

There are communities that do not accept the axiom of choice, and the reasons they have are diverse.

One such community is the community of constructive mathematics. If one gets properly inside the constructive world view one can see that it requires us to repudiate the axiom of choice. However, getting properly inside the constructive world-view is not an undertaking for fainthearts, nor by any to be taken in hand lightly or unadvisedly, and it is not given to all of us to succeed in it. Fortunately for unbelievers there is a short-cut: it is possible to understand why constructivists do not like the law of excluded middle or the axiom of choice, and to understand this without taking the whole ideology of constructive mathematics on board. It comes in two steps.

#### First we deny excluded middle

First we illustrate why constructivists repudiate the law of excluded middle.

Some readers may already know the standard horror story about  $\sqrt{2}^{\sqrt{2}}$ . For those of you that don't – yet – here it is.

Suppose you are given the challenge of finding two irrational numbers  $\alpha$  and  $\beta$  such that  $\alpha^\beta$  is rational. It is in fact the case that both  $e$  and  $\log_e(2)$  are transcendental but this is not easy to prove. Is there an easier way in? Well, one thing every schoolchild knows is that  $\sqrt{2}$  is irrational, so how about taking both  $\alpha$  and  $\beta$  to be  $\sqrt{2}$ ? This will work if  $\sqrt{2}^{\sqrt{2}}$  is rational. Is it? As it happens, it isn't (but that, too, is hard to prove). If it isn't, then we take  $\alpha$  to be  $\sqrt{2}^{\sqrt{2}}$  (which we now believe to be irrational – had it been rational we would have taken the first horn) and take  $\beta$  to be  $\sqrt{2}$ .

$\alpha^\beta$  is now

$$(\sqrt{2}^{\sqrt{2}})^{\sqrt{2}} = \sqrt{2}^{\sqrt{2} \cdot \sqrt{2}} = \sqrt{2}^2 = 2$$

<sup>11</sup>The (graph of the) wellordering of the reals and the (collection of pieces in the) decomposition of the sphere are of course sets too.

<sup>12</sup>There is a very good reason for this, namely that there is no definable relation on  $\mathbb{R}$  which provably wellorders  $\mathbb{R}$ . This theorem wasn't known in 1904 but people in 1904 could still realise that they didn't know of any wellorderings of  $\mathbb{R}$ .



which is rational, as desired. However, we haven't met the challenge. We were asked to *find* a pair  $\langle \alpha, \beta \rangle$  of irrationals such that  $\alpha^\beta$  is rational, and we haven't found such a pair. We've proved that there *is* such a pair, and we have even narrowed the candidates down to a short list of two, but we haven't completed the job.<sup>13</sup>

What does this prove? It certainly doesn't straightforwardly show that the law of excluded middle is *false*; it does show that there are situations where you don't want to reason with it. There is a difference between proving that there is a widget, and actually getting your hands on the widget. Sometimes it matters, and if you happen to be in the kind of pickle where it matters, then you want to be careful about reasoning with excluded middle. But if it doesn't matter, then you can happily use excluded middle – or AC.

### The Axiom of Choice implies Excluded Middle

In proving this we must play fair: the classical concept of *nonempty set* multifurcates into lots of constructively distinct properties. Constructively  $x$  is **nonempty** if  $\neg(\forall y)(y \notin x)$ ;  $x$  is **inhabited** if  $(\exists y)(y \in x)$ , and these two properties are distinct constructively: the implication  $(\neg\forall\phi \rightarrow \exists\neg\phi)$  is not good in general.

Clearly if every family of nonempty sets is to have a choice function then if  $x$  is nonempty we can find something in it. This would imply that every nonempty set is inhabited. We shall not resort to such smuggling. If we are to eschew smuggling we will have to adopt AC in the form that every set of *inhabited* sets has a choice function.

Let us assume AC in this form, and deduce excluded middle. Let  $p$  be an arbitrary expression; we will deduce  $p \vee \neg p$ . Consider the set, the *doubleton*,  $\{0, 1\}$ , and the equivalence relation  $\sim$  defined by  $x \sim y$  iff  $x = y \vee p$ . Next consider the quotient  $\{0, 1\} / \sim$ . The suspicious might wish to be told that this set is

Check this definition: it got garbled at one point.

$$\{x : (\exists y)((y = 0 \vee y = 1) \wedge (\forall z)(z \in x \longleftrightarrow z \sim y))\}.$$

This is an inhabited set of inhabited sets. Its members are the equivalence classes  $[0]_\sim$  and  $[1]_\sim$  – which admittedly may or may not be the same thing – but they are at any rate inhabited. Since the quotient is an inhabited set of inhabited sets, it has a selection function  $f$ . We know that  $[0]_\sim \subseteq \{0, 1\}$  so certainly  $(\forall x)(x \in [0]_\sim \rightarrow x = 0 \vee x = 1)$ . Analogously we know that  $[1]_\sim \subseteq \{0, 1\}$  so certainly  $(\forall x)(x \in [1]_\sim \rightarrow x = 0 \vee x = 1)$ . So certainly  $f([0]_\sim) = 0 \vee f([0]_\sim) = 1$  and  $f([1]_\sim) = 0 \vee f([1]_\sim) = 1$ .

This gives us four possible combinations.  $f([0]_\sim) = 1$  and  $f([1]_\sim) = 0$  both imply  $1 \sim 0$  and therefore  $p$ . That takes care of three possibilities; the remaining possibility is  $f([0]_\sim) = 0 \wedge f([1]_\sim) = 1$ . Since  $f$  is a function this tells us that

<sup>13</sup>We can actually exhibit such a pair, and using only elementary methods, at the cost of a little bit more work.  $\log_2(3)$  is obviously irrational:  $2^p \neq 3^q$  for any naturals  $p, q$ .  $\log_{\sqrt{2}}(3)$  is also irrational, being  $2 \cdot \log_2(3)$ . Clearly  $(\sqrt{2})^{(\log_{\sqrt{2}}(3))} = 3$ .

$[0]_{\sim} \neq [1]_{\sim}$  so in this case  $\neg p$ . So we conclude  $p \vee \neg p$ .<sup>14</sup>

Observe, however, that if we define the family of  $N$ -finite sets recursively by:

The empty set is  $N$ -finite;  
if  $X$  is  $N$ -finite and  $x \notin X$  then  $X \cup \{x\}$  is  $N$ -finite.

then we can prove by structural induction on the  $N$ -finite sets that every  $N$ -finite set of inhabited sets has a choice function. This is theorem 1.

There is a moral to be drawn from this: whether or not you want to include AC (or excluded middle) among your axioms depends at least in part on the use you are planning to put those axioms to. (This is of course a completely separate question from the question of whether or not AC (or excluded middle) is *true*).

Uplifting though this moral is, it is not the point that I was trying to make. The fact that AC implies excluded middle and that there are principled reasons sometimes to eschew excluded middle means that there are principled reasons for (sometimes) wishing to eschew the axiom of choice.

## 6.9 Weak forms

The difference between various forms becomes obscured. Countable choice is a very different beast from full choice.

- Analysis without  $AC_{\omega}$  is a disaster area. Analysis without full AC but with  $AC_{\omega}$  is a very interesting prospect, particularly if we add nice things like LM – there are even people who advocate it; (Imre says that all such people are logicians);
- WQO theory without countable choice is a train-wreck – but very very few theorems in WQO theory need full choice;
- NF refutes full choice but doesn't seem to refute  $AC_{\omega}$ . (Not sure about the various choice-contradicting nice conditions on models of TZT);
- We need  $AC_{\omega}$  to identify the two definitions of wellfoundedness – and thereby to make sense of games of finite length. Games of length  $\omega$  are supertasks.
- Don't we need  $AC_{\omega}$  to do forcing?

## 6.10 Some thoughts about certificates

[a lot of the talk below is about freeness. There is also the point that, on one way of declaring the von Neumann ordinals, the vN ordinals is the same as the

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<sup>14</sup>Thanks to Douglas Bridges for the right steer on this exercise! The theorem is due to Diaconescu [7].

rectype of its certificates. Certainly true for vN naturals: the certificate that  $n$  is a natural is the set  $[0, n]$ , which is of course  $n$  itself. For transfinite ordinals there is the constructor that takes arbitrary increasing sequences, so of course we get more certificates]

My point of departure is the idea of a recursive datatype or *rectype* for short. A rectype has *founders* and is built up by *constructors*. All the usual examples are **free** in the sense that each object in the rectype is denoted by a unique word in the constructors. Examples are the natural numbers, or lists and trees. Such rectypes are always initial objects in a suitable category. Computer scientists, for obvious reasons, tend to be interested only in rectypes of **finite character**: finitely many founders and finitely many constructors each of finite arity. However there is no mathematical reason not to consider rectypes of infinite character, and the cumulative hierarchy of sets is a natural example. It has no founders at all, and has one constructor – **set-of** – of unbounded arity. This is a free rectype and is well-behaved. slight change here

So, thus far, we have two parameters with which we classify rectypes. They may be of finite character vs infinite character, and they may be free vs not-free.

	Free	Not Free
Finite Character	The naturals lists, trees	?? ??
Infinite Character	The cumulative hierarchy of sets	The ordinals

Need to fill in the question marks.

Next I need the idea of a *certificate* or *proof*. If you are a member of a rectype there is always a good reason for you to be, and a certificate-or-proof is that reason – presented as a mathematical object. If the rectype is free (so it's an initial object in a suitable category) every object in it has a unique certificate. If the rectype is not free there may be a multiplicity of certificates. (Or there may even be none, as we shall see). Notice that even if the rectype  $R$  is not free, the rectype of certificates-for- $R$  is always free. Perhaps I should be a bit more explicit about what a certificate is to be. A certificate-that- $x$ -belongs-to-the-rectype is: the constructor used in the last step in the construction of  $x$ , together with a list of arguments to that constructor, with certificates for each of those arguments. So a certificate is a word in the constructors and founders. And the family of certificates for a rectype is another rectype – indeed a free rectype.

Now we need a slightly finer distinction, within the family of rectypes of infinite character. Specifically I shall be interested in the following rectypes.

1. The collection of wellfounded hereditarily countable sets. The single constructor is countable-set-of. This collection is often called  $HC$ <sup>15</sup>;
2. The rectype whose founder is the ordinal number 0, with constructors **successor** and **sup-of- $\omega$ -sequence-of**. This is a substructure of the ordinals;

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<sup>15</sup>I prefer ' $H_{\aleph_1}$ '

3. The retype whose founders are all the countable sets, and whose constructor is `union-of-countable-set-of`;
4. The retype whose founders are all the  $\omega$ -sequences and whose constructor is  `$\omega$ -sequence of`;
5. The cumulative hierarchy of sets.

(1), (4) and (5) are free. (2) and (3) are not.

Now any retype admits a wellfounded quasiorder which in my introductory book [10] “Logic Induction and Sets” I call the **engendering relation**, and which is the transitive closure of the relation that  $x$  bears to each of things it is immediately constructed from. For example, in  $\mathbb{N}$  the engendering relation is  $<$ . (I am a bit worried by the fact that there doesn’t seem to be a standard name for this engendering relation in the literature .... If one isn’t needed, when I think it is, then I must have misunderstood something very badly). The engendering relation is wellfounded, and has a canonical rank function, which is a map to the ordinals, whereby the rank of any object in the retype is the least ordinal bigger than the ranks of all the things that bear the engendering relation to it. In case (5) the retype rank is literally the same as the set-theoretic rank.

Now let’s think about free retypes of infinite character, but *bounded* character, so their constructors have bounded appetites.

Jech [18] has a wonderful theorem that says that every set in  $HC$  has rank less than  $\omega_2$ . It’s very important that the proof does not use AC at all. It exploits the fact that the retype  $HC$  is free: each object has a unique certificate. I think that in general Jech’s theorem shows that in any free retype of countable character every object must have rank  $< \omega_2$ .

The freeness is important here. It is a theorem of Gitik [12] that the retype (2) can contain all ordinals.

There is another result that is useful in this connection. I noticed it myself, but i’m sure it’s folklore. If  $AC_\omega$  holds, then  $|HC| = 2^{\aleph_0}$ . In a sense this isn’t really the theorem; the theorem that underlies it goes like this:

Each of these retypes is the least fixed point for a suitably chosen operation  $\mathcal{O}$ . So if you can find another fixed point (“pick a fixed point, any fixed point!”) or even something  $x$  with  $|x| = |\mathcal{O}(x)|$  you should be able to embed the retype in it and thereby bound its size. (There’s a certain amount of small print to this: not all of which have I checked). Consider not  $HC$  but the retype (3). The reals is the same size as the set of  $\omega$ -sequences of reals. That means that we can define by recursion on the retype (3) an injection into the reals. We need the freeness of (3) to get an *injection*. If we can choose a certificate for each hereditarily countable set then we can embed  $HC$  into the reals. Hence the fact that  $|HC| = 2^{\aleph_0}$ .

I don’t think this depends on special properties of countable sets; i think Jech’s argument can be generalised to apply to all free retypes of bounded character. I think it will say something like: if  $\kappa$  is an aleph then in any free retype generated by fewer than  $\kappa$  founders and fewer than  $\kappa$  constructors each

of arity less than  $\kappa$  every object has rank  $< \kappa^{++}$  and the rectype itself is of power  $2^\kappa$ . Something like that, anyway.

Moral: every free rectype of *bounded* character is a set. and by Jech's argument we have tight control of the ranks of the ordinals used.

But what about the non-free rectypes? One would expect that even in a non-free rectype every object should have a certificate. How could this not be true? Since everything in the rectype is there for a good reason, there must be a good reason one can point to. Although this is true for rectypes of finite character it appears not to be straightforwardly true for rectypes of infinite character. It seems that unless we assume AC we have no reason to suppose that a rectype of infinite character is a projection (in the obvious way) of its rectype of certificates. For example, in the model of Gitik's where every limit ordinal has cofinality  $\omega$  the rectype (2) generated from 0 by *succ* and  $\omega$ -sups contains all ordinals, and the rectype of certificates for it is a free rectype of countable character, so every certificate has rank  $< \omega_2$ . In those circumstances we cannot rely on ordinals beyond  $\omega_2$  having certificates.

Free rectypes of infinite bounded character are well-behaved, but we need AC to show that every infinite rectype is a surjective image of a free one. So in the absence of AC the task of establishing the sethood of a non-free rectype of infinitary-but-bounded character is nontrivial. For example in NF we do not know if the rectype (2) is the universe. And this despite the fact that we know that not every set can be a projection of a member of rectype (3).

Presumably AC is equivalent to the assertion that every rectype is a surjective image of its rectype of certificates.

So I think my questions to you are along the lines: (i) how much of this is known? Can I improve bits of it by expressing it in a more category-theoretic way..? Any helpful comments gratefully received...

Dear Thomas,

Thanks for your "Letter to Jamie". If I've understood it correctly, an answer is this:

Note that AC in the category of sets is that every surjection has a right inverse. I.e. if  $f : A \rightarrow B$  then there exists a  $g : B \rightarrow A$  such that  $f \circ g : B \rightarrow B$  is the identity.

There's a powerful theory of initial algebras which gives functions from free objects (essentially, your certificates) to other objects (your non-free rectypes). AC implies (and almost certainly is equivalent with) the property that every one of these functions has a right inverse.

So what you describe probably can all be expressed in categorical language. I think what you've written amounts to observing (correctly or falsely I cannot judge off-the-cuff) that in the category of sets without AC, there are certain functions which do not have initial algebras, but they do have initial algebras in the category of sets with AC. This makes sense; in the category of sets without AC there are simply fewer functions!

There's one little niggling thing. An object in the category of sets is a set because it's an object in the category of sets. You might have to set up two categories; a category of sets ("small things") and a category of collections ("big things").

Dear Thomas,

FYI here's the page about the axiom of choice in Set (the category of sets)

[http://books.google.co.uk/books?id=KaXmMjwBulgC&pg=PA17&lpg=PA17&dq=axiom+of+choice+epi+split+epi&source=web&ots=kuyJgzb9\\_v&sig=UJdRbYAOHZVXbzxiYjWE9pXhl=en&sa=X&oi=book\\_result&resnum=4&ct=result](http://books.google.co.uk/books?id=KaXmMjwBulgC&pg=PA17&lpg=PA17&dq=axiom+of+choice+epi+split+epi&source=web&ots=kuyJgzb9_v&sig=UJdRbYAOHZVXbzxiYjWE9pXhl=en&sa=X&oi=book_result&resnum=4&ct=result)

Blend these two sections properly

### 6.10.1 AC and Certification

Once we have taken on board the rôle played the concept of *datatype* in explaining the difference between the theorem that needs choice and the theorem that does not need choice, one can see that the axiom of choice weaves its magic by showing how, when we are given an object of one datatype (a **naked set** that happens to be countable), we can see it as a reduct of an object of the richer datatype **counted set**.

The next step after consciously acknowledging that mathematical objects typically and usefully have identifiable datatypes is the step of thinking of those datatypes as mathematical objects themselves. When we do this we find another rôle for the axiom of choice.

Let us help ourselves to the concept of *certificate*. It is useful primarily in connection with recursive datatypes [...] but it is actually slightly more general.

A certificate that a particular object is a member of a particular datatype is something that will convince a skeptical reader that the object in question is, indeed, an object of the datatype it is alleged to belong to. If we want to set up a subtype of **naked-set** called **countable-naked-set** a certificate for an object of that type would be a counting of it. Similarly, a certificate for a counted set is a counting of it. This makes it sound as if the two types **counted-set** and **countable-set** are the same, but they aren't, and the certificate-talk gives us a way of illustrating the difference. If  $x$  is a **counted-set**, the certificate that  $x$  is so is part of the object  $x$ ; if  $x$  is **countable-set** it isn't. A **countable-set** is a **naked-set** that *could* be expanded by decorating it with a counting (and that counting is a certificate that it is of type **countable-set**), but it remains a **naked-set** and the counting is not a part of it – it hovers around attentively but is not part of the kit; in contrast a **counted-set** is a **naked-set** that *has been* expanded by decorating it with a counting... and that counting is a certificate that the expanded object – of which it is a part – is of type **counted-set**).

Clearly objects of either of these types have certificates.

Now consider the datatype

**set-that-is-a-union-of-countably-many-countable-sets.**

It is a subtype of the type **naked-set**. Let's call this type **C** for short. A certificate that an object  $X$  really is of type **C** must be a counted set  $\{\langle C_i, X_i \rangle : i \in \mathbb{N}\}$

of pairs where each  $X_i$  is a countable set with  $C_i$  a counting of it, such that  $\bigcup_{i \in \mathbb{N}} X_i = X$ .<sup>16</sup>

Now this certificate will give rise to a counting of  $X$ , by means of the zigzag construction on the  $C_i$ . So if every object of type  $\mathbf{C}$  has a certificate it follows that a union of countably many countable sets is always countable, so every element of  $\mathbf{C}$  is actually countable.

There are two more illustrations, slightly less unnatural. There is  $H_{\aleph_1}$ , the (wellfounded) hereditarily countable sets, aka HC; there is also the class of hereditarily wellordered sets.

A certificate that  $x$  is a member of HC is a counted subset  $X$  of HC such that  $x = \bigcup X$ , equipped with a function that assigns to each  $y \in x$  a certificate that  $y \in \text{HC}$ .

**REMARK 3** *If every element of HC has a certificate then every member of HC has a countable transitive closure.*

*Proof:* By induction on set-theoretic rank. ■

The significant feature common to all these cases is of course the fact that these rectypes are not free.

Explain freeness

Thus, existence of certificates implies choice principles! Consideration of other, more complex datatypes will show [i think!] that the principle “for every datatype, every object of that datatype has a certificate” will imply full AC. AC should follow from the assertion that there is a global function assigning to each hereditarily wellordered set a wellordering of it. First step would be to show that AC follows from the assertion that there is a function assigning to each wellordered set a wellordering of it.

## H I A T U S

### Dependent Choice

You need to be clear about what you are picking *from*. You can go on picking from a set as often as you like – through all the ordinals, even – as long as you don’t remove them once you’ve picked them. After all, there’s nothing wrong with a function from a wellordered set  $X$  to a set  $Y$ ! If you remove your chosen element each time (so you pick a different member of  $X$  every time) then you are constructing a wellordered subset of  $X$ , and of course the size of any such subset is bounded by  $|X|$ .

“But” (i can hear the reader exclaiming) “the second choice is made from a set that is *different* from the first set!” If the first set is  $X$ , and  $x$  is chosen from it then the second choice is made from  $X \setminus \{x\}$ . This leads us to principles of *dependent* choice, where the set on which the choice function is being defined has some structure that arises from the choice function itself.

But this has no bearing on the axiom of choice, because AC talks about making choices from lots of *different* sets.

<sup>16</sup>Or perhaps just a countable set of such certificates. . . ?

## 6.11 Leftovers

There is the point that the counterexamples to AC are things that it's impossible to describe completely, simply because of the order structure built into our language.

That is to say, the negation of AC is a sort-of self-refuting sentence like “I can't say ‘breakfast’” which cannot be true if uttered and “It is raining and I do not believe it” which cannot be true if believed by the speaker.  $\neg$ AC resembles them in that the point is not that it can't be true, it's that it cannot be understood – or perhaps that if understood cannot be believed.

### H I A T U S

It might be claimed the picture above misrepresents the thought processes of the people who think that the axiom of choice is obvious. Yes – it will be admitted – there is a danger of a fallacy of equivocation as sketched above, but the argument for choice relies on the cases where it is provable. It's a different kind of IBE: the reason why we can prove all these instances of the axiom of choice is simply that the axiom of choice is true. We can't prove that the set of socks is wellordered but that's only because we have not been given enough information about it. Any set about which we know enough reveals itself – under the close examination that we are able to give it – to be wellordered. Why is this? Is this just coincidence on a cosmic scale? Of course not! There is a simple explanation: the truth of the axiom of choice.

However this line of talk isn't really supported by the data. Not all observed sets are observed to be wellordered. Some sets provably have selection functions: the power set of the naturals for example. But some don't *provably* have them: the power set of  $\mathbb{R}$  for example. (One could try claiming that the power set of the reals is not observable in the relevant sense, but since the only reason for arguing this is that it fails to support this argument, this would be too obviously circular for most tastes).

What is the correct concept of “observable” here? (We obviously don't mean *literally* “observable”! (It's worth thinking about whether or not the only things that are observable in the relevant sense are things that have enough order structure: you perforce wellorder a set in the course of observing it.)

We might mean something like

observed-to-be- $\phi$  = provably  $\phi$

observable set = definable set

Clearly we are thinking here of sets-in-intension, or descriptions of sets. Perhaps we can here put to good use the expression from possible-world rhetoric. ‘ $V_\omega$ ’ is a **rigid designator** (it denotes the same thing in all standard models); ‘ $\mathbb{R}$ ’ is not.

So what precisely is the general observation whose truth is to be explained by the axiom of choice? It's not the fact that every observable set is known to be wellordered, since that is not a fact. Nor is it the fact that every definable set can consistently be wellordered, since AC would explain a lot more than



that, and Inference to the *Best* Explanation would point us not at AC itself but rather at *the statement that AC is consistent*. Sadly that last observation is something we already know and don't need any arguments for. If we want an argument for AC we won't find it here.

The argument isn't really IBE at all (in contrast to the genuine IBE argument used for replacement, for example) but is a kind of induction by simple enumeration, or whatever is the argument that we use to refute the scepticism that says that unobserved objects might suddenly go out of existence, or misbehave in other ways, like the unobserved wallpaper in the drawing room of the magician Mr. Leakey in [15].

This is an attempt to tar with a radical sceptic's brush the people who say that you know AC to hold only for sets (finite, definable etc) for which you have privileged information. All observed sets are wellordered, so all unobserved sets are wellordered too.

(Is there a parallel here with attempts to prove that all emeralds are grue?)

There is a problem with arguments for the truth or falsity of set theoretic axioms. It is fairly general, but we can illustrate it here with the axiom of choice, since that is the axiom under discussion.

If you believe that the axiom of choice is the kind of thing that has a truth-value then you probably believe that it is noncontingent. If it's true it's necessarily true and if it's false it's necessarily false. If you have house room for such ideas of metaphysical necessity then you probably try to capture them by talk about possible worlds. Conveniently there are obvious candidates for the possible worlds we would use to explain the necessary truth or necessary falsehood of AC, namely models of set theory, or perhaps *standard* models of set theory only. This terminology also gives us vocabulary to say things like “ $V_\omega$  is a rigid designator” and “ $\mathbb{R}$  is not a rigid designator” which (if our possible worlds are standard models) enable us to capture some things that set theorists recognise as facts.

How inconvenient it is, therefore, that on this account AC turns out to be true in some possible worlds and false in some others, and therefore not to be noncontingent after all. Not only that, but we don't know which of these models is the actual world, so we have no idea whether it is true *simpliciter* or not.

Clearly there is some explaining to be done

Can we argue that it is false? Argument to the effect that if it were true then counterexamples would be unimaginable?

OK, even if we cannot argue that the axiom of choice is true (at least by arguments like this) is there nevertheless a case to be made for adopting it as an axiom? (You would have reached this stage long ago if you had never been that kind of realist and never believed you had any epistemic access to arbitrary infinite extensional objects).

What are the pros and cons? On the pro side is the point that it makes the arbitrary infinite extensional objects behave like the cuddly familiar, nonarbitrary finite ones and thereby makes the world a tidy place. (Well, there is still this fact about  $\mathbb{R}$  but believers in AC are greedy) It is true that  $\mathbb{R}$  is not naturally wellordered, but if anything this is a point in AC's favour, since by

wellordering  $\mathbb{R}$  it gives us another way of reasoning about  $\mathbb{R}$  and proving things about it.

On the con side is the fact that models in which every set of reals is measurable are quite cute in various ways.

I want to make a connection here with what I was telling you in the first lecture about the three stages entities go through on their way to becoming mathematical objects. We have noted that AC for *finite* objects is true. So it's only *infinite* objects we are ever going to get our knickers in a twist about. And we didn't start manipulating/calculating-with infinite objects until the days of Cantor and Dedekind. At the earliest stages of this process the infinite objects we had to deal with were all naturally motivated, naturally occurring, objects with enough internal structure – so we find that all the instances of AC that we wanted were true. Well, almost all of them:  $\mathbb{R}$  has no definable wellordering as we have seen.

What this means is that, with hindsight, we should have expected people to notice that they needed the Axiom of Choice as an extra principle at precisely that stage when they started reasoning about/calculating-with *arbitrary infinite objects-in-extension*. And this is indeed exactly what happened.

Let's take an example: Vitali's construction of a non-measurable set. This could only arise once one had the concept of an arbitrary set of reals.

So if you only ever deal with finite sets and sets with enough internal structure you will hardly ever encounter one that doesn't come ready wellordered, and the issue doesn't arise. The question about whether or not AC is *true* can arise for you only if you are a realist about arbitrary infinite extensional objects.

Tie together Grue emeralds with expansion and self-refutation connect with Berkeley

AC and regimentation. No accident that you use BPI to prove a representation theorem

### AC implies the existence of God

The following comes from Meyer [21].

One direction: God implies Choice, since if God existed, it would be possible to construct a choice set for each set, since God could just think about it for a bit and do the choosing, being omnipotent and all. The other direction: take a causal sequence – by Zorn's lemma (an equiv. of Choice), it will have a unique first member. Thus we have a cosmological argument which establishes a First Cause. (God, of course).

Another example of people using AC when they don't need to.

How do you prove that there is no subset of  $\mathbb{R}$  that is of order-type  $\omega_1$  in the inherited order? Suppose there were such a set,  $X$ . Then  $\mathbb{R} \setminus X$  is partitioned into open intervals, each of which must contain a rational, so pick a rational from each one, using AC. Then we have an uncountable set of rationals, which is impossible. (I have actually had students say this to me). But AC is not

needed: since  $\mathbb{Q}$  is countable we can (one choice!) pick an enumeration of it and select the first rational in each interval according to that enumeration.

Connect with logic-and-rhetoric the point that: just beco's the  $p$  i am telling you about isn't (as it happens) going to be a problem for you (beco's of your choice of pathway) it doesn't follow that  $p$  wasn't true!

Be sure to find some rude things to say about axioms of plenitude. What is an axiom of plenitude anyway? What is an axiom of restriction, a *Beschränktheitsaxiom*?

Conversation with Peter Smith 7/iii/2018

EXplain propetly why it was only after we acquired the idea of arbitrary set/function-in-extension that AC could blow up in our faces (B-T)

It's beco's of the belief that the infinite resembles the finite that they believe AC. All else is post-hoc rationalisation.

Look up traffic on stackexchange

Talk about skolem functions?

If  $\sim$  is a congruence relation for an infinitary [total] operation  $f$  then in general you need AC to show that  $f$  is total on the quotient. Two examples:

- (i) Infinitary sums of cardinals and ordinals – “multiplicative” axiom!
- (ii) The Cauchy reals are order-complete. (perhaps we can do this without choice)
- (iii) Also power set axiom in APG models

There are natural examples where you can't *just keep buggering on*. There are games where Player II can choose to stay alive for  $n$  steps, for any  $n$ , but is doomed to lose sooner or later.

The fact that WF is rigid sets you up for AC, because the existence of a definable wellorder of  $V$  enforces rigidity.



## Chapter 7

# Appendices etc etc

### 7.1 Glossary

Wellordering

performative

Contrastive explanation



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