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

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Is this the kind of book you would like your student to read?!—Mervyn Griffith-Jones

Contents

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

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.

Preface

These two slender volumes are not intended to be an introductory text in set theory: there are plenty of those already. It's 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.[1] 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want to adopt them (in the light of the uses to which they can be put) say a bit (but only a bit, for this is not a historical document) on how we came to adopt them, and explain their mutual independence.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.

Other essays with a brief resembling the one I have given myself here include Mycielski [38], Maddy [**?**], [33] and Shoenfield [47]. My effort is both more

[1] 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 mention them only to the extent that they can shed light on the mainstream material.

elementary and more general than theirs are.[2]

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 complains 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: [14]. The fact that [14] 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, but in both cases there is a conception underlying the game which the rule was intended to make explicit, or to implement[3]. One effect of this is that there can be *good* or *bad* LBW laws.

[2] Despite the promising-sounding title Lemmon [27] is a technical work.

[3] 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.

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 just can't agree about what is true of them. But what is actually happening is 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.

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

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

...where the Greek letters range over ordinals. What this mouthful of a formula says is that the αth 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_β. 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

Where do i in fact use it?

This conception of sets is more-or-less explicit in Mirimanoff [36], but it is more usually associated with Von Neumann [56]. He noticed that the cumulative hierarchy gives an **inner model** of set theory.[1] 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

[1]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**—sometimes more evocatively described as its **birthday**: the rank of x is the least α 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_α, is defined to be $\beth_\alpha$. Since $V_{\alpha+1}$ is the power set of V_α, Cantor's theorem tells that that all the V_αs are different sizes. In fact $\beth_0 := \aleph_0$; and $\beth_{\alpha+1} := 2^{\beth_\alpha}$.

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.

2.1 Inference to the best explanation

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 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 [30] for an excellent treatment.

The IBE defence was probably more important for Replacement than for Choice. Advocates of the Axiom of Choice have stoutly maintained that it is obviously true. (And the IBE case for AC is weak, as we shall see). In contrast, advocates of the 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 [10] p 2) and Carnap [7] 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 shares little more than a name with the mediæval idea.

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 corresponds 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 [40] 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. 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.)

Otherwise there are endless possibilities of fallacies of equivocation

(ii) If widgets are legitimate well-defined objects one can quantify over them. The literature of philosophical logic contains numerous aftershocks of Quine's ([39] "On what there is") observation that "to be is to be the value of a variable". This has usually been read as an 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: [51].

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_α 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 $\mathbb{R} \to \mathbb{R}$ for the mathematicians of the eighteenth century; proofs and formulæ for the average modern mathematician;[1] chemical elements for chemists even today.[2]

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 *a* correct formalisation. 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.

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

[1] My *Doktorvater* Adrian Mathias says that a logician is someone who thinks that a formula is a mathematical object.

[2] 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.

on widgets becomes important only once you are considering operations on widgets and this is more likely once you have arbitrary widgets. [3] [4]

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?

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

[3] 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.

[4] 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."

[49] 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.

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.[5]

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 appear 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 thoroughgoing 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

[5] 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 unrecognised nails and it is perfectly sensible to review lingering unsolved problems to see if any of them are, in fact, nails.

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 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 [59] of about 100 years ago, and in very nearly their present form. The most significant difference between Zermelo's axiomatisation in [59] 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 they could be taken as reminders that there was a lot of work still to be done.

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!)[1] 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: people weren't interested in them anyway 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

[1] 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 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"—to quote Russell. 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.[2] 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 all the gadgetry? 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.

Is this the place to explain expansions and reductions?

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 it is the 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 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

Even thinking of the rationals as a set is not *entirely* straightforward—think B-T.

[2] 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 required.

more attracted than they are now to the idea that set theory should accomodate 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[3]. 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.[4]

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 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

[3] At least if you working in a standard theory of wellfounded sets not something like Quine's NF.

[4] See Menzel [35] where he implements ordinals as atoms, and even arranges to have a *set* of atoms—by weakening replacement

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 *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.

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

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 α 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 α? 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 α times and then do it β 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 α times the task of doing-X β-times.

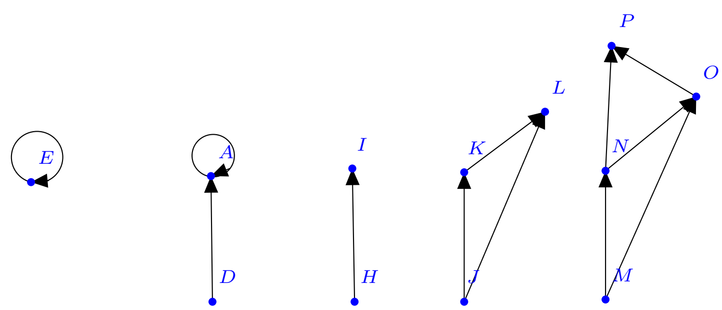
In fact these properties of ordinals all follow from the three assumptions that (i) 0 is an ordinal and (ii) if α is an ordinal, so is $\alpha + 1$; and (iii) if everything in A is an ordinal, then $\sup(A)$ is an ordinal too. This last is because if I have performed some task at least α times for every α 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 54). This is by no means obvious, and not everybody will want to work through the proof. (Those who do can see the discussion in [14].) Readers from a theoretical computer science background will 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 [11] calming.

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

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 possibly the last time the reader will have made to him or her 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 circularities 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.[5] 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.

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 with entities abstracted somehow from the isomorphism classes. This gives rise to some fun mathematics, and readers who

[5]So they should really be APDs, but the notation is now standard.

express an interest in it are usually directed to Aczel [1]—though the seminal paper is the hard-to-get [19] 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 [19]:

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 of 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 36.

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 ordeed 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. 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 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 *un*ordered 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) and there is a sensible alternative. It just so happens that that alternative is not one that we wish to pursue, but it's worth outlining, if only to give a contrastive explanation of the role of the axiom. The meaning of the axiom of pairing is that *there is only one flavour of set*.

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 *compatible*; they can cohabit in a small 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 compatible, the fox and the goose not so!

The relation R is evidently symmetric. And—as long as every set belongs to *something*—it will be reflexive. But the axiom of power tells us that every set belongs to its power set so R is indeed reflexive. The axiom of pairing is quite simply the assertion that R is the universal relation.

What else might it be, if not the universal relation? Well, so far it is at least symmetrical and reflexive thereby making it what is sometimes called a *fuzzy*. The first question to ask of any fuzzy is whether or not it is also transitive, which would make it an actual equivalence relation. The scenario in which R is merely a fuzzy and not an equivalence relation doesn't seem to be sensible. (Bear in mind that the universal relation is an equivalence relation). So what if R is an equivalence relation other than the universal relation?

Well, it will have more than one equivalence class. And the equivalence classes are little microuniverses inhabited by pairwise compatible sets. 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[1] and similarly the existence of $\bigcup x$ means that any two members-of-members of x are equivalent. Indeed, for any concrete n, any two members-of^n 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 [44]. 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). In fact one can set up a strongly typed theory of sets in the language $\mathcal{L}(\in, =)$ of set theory, with no extra type predicates.

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 a choice for a one-sorted theory of sets instead of a many-sorted theory of sets.

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 '$\bigcup x$'.

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. 43. 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 definite enough to be a set, and ϕ is something definite enough to be written down, then the subset of x containing those things that are ϕ is also definite enough to be a set.

Definiteness loomed large in the early literature—as an undefined 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

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

The power set ("$\mathcal{P}(x)$") of x is bigger than x (that's Cantor's theorem—reference??) 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

[1] If you have spotted that this means that R is the universal relation after all, since any set x is equivalent to the empty set (they both inhabit $\mathcal{P}(x)$ after all) then go to the top of the class. You need special clauses for the empty set(s) if you are to develop typed set theory succcessfully. But that is not our purpose here.

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 that way of thinking is part of the object-in-extension package.

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:

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

This is a scheme rather than a single axiom because we have one instance for each formula ϕ 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 ϕ 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:[2]

- **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.

[2]There is a literature (see for example [51]) 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. 15, once one accepts arbitrary widgets-in-extension one has accepted null widgets.

- **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!".

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. It turns out that there are. The way in is to think about ways of iterating of 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 ϕ 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.

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. We might spice this up to say that $(\forall x)(\mathrm{clos}(\{x\}, f)$ exists). If we write '$\mathrm{clos}(\{x\}, f)$' as '$F(x)$' then we are saying that V is closed under F.

BAD JOIN

This suggests a strategy for developing a sequence of axioms of infinity. At each stage one devises the next axiom of infinity by thinking of a natural property ϕ 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 ϕ to be? We need a sensible way of dreaming up such a ϕ. 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_ω, the collection of hereditarily finite sets. It is true that every set in V_ω (and therefore every set in what the universe might be) is finite, so "being not-finite" is certainly a candidate for ϕ. However we can say more than that: V_ω 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 $V_?$ of the wellfounded universe that is a model of the axioms we have so far, and find a ϕ that is true of $V_?$ 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.

[3]

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.

[3]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

Chapter 5

The Axiom of Foundation

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 theere is no need to go over that here.

tho' I could be talked into providing such an explanation

What is its status? Set theorists (most of them) would have you believe that it is a fundamental fact about the nature of God's creation, but it's really nothing of the sort. Fraenkel rather let the cat out of the bag when he said, quite early on, that it 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. Blah sunset clauses blah Britain goes metric yeah right. and meanwhile it has become entrenched. Boolos has the grace to admit "There does not seem to be any argument 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".

find this

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.

It 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.[1] There are others who, while having a more inclusive view of what sets are or

[1] Perhaps not *inescapably* (see Forster [16]) but certainly *plausibly*.

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 [17] and Coret [12]

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.[2]

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 [44].

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

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

"conservative"? For the index?

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 [19];
(ii) Church's Universal Set theory [9];
(iii) The NFU conception of illfounded set;
(iv) The positive set theory of Hinnion's school in Brussels.

Do we *really* need to talk about GPC?

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

[2]Thanks to Peter Johnstone for reassurance on this point.

primary? One is reminded of what philosophers call the "paradox of analysis"[3] 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 bjiection 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

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

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

[3]The phrase first appears in [26] 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*.

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[4], since it can be obtained by separation from the set $V_{\alpha+1}$, where α 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.

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 so, since as long as we have foundation there is always Scott's trick[5]. This example serves to underline the importance of Scott's trick. Gauntt [21] 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 restriction

[4] 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.

[5] 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.

Significance outside set theory implementation-invariance.]

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

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:

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

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

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 a human 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:

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

where ψ 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 by each French impressionist painter.

quantifier classes?

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

In general, for most natural classes Γ of formulæ, the two schemes of replacement-for-formulæ-in-Γ and collection-for-formulæ-in-Γ 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, Γ 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* $\models$ *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 ϕ 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. 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 general, for most natural classes Γ of formulæ, the two schemes of replacement-for-formulæ-in-Γ and collection-for-formulæ-in-Γ 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, Γ 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 replacment; that is not a crime. However if you propose to pick a fight with people who annoy you by saying that Replacement really is indispensible then the time-is-short argument does not let you off the hook. *Hic Rhodos hic salta.*

There are people who mutter that the axiom scheme of replacement is

(i) not needed for *ordinary mathematics*, and that

(ii) it is only sad marginal people such as set-theorists who take any interest in it.

Readers of this book should not take (i) personally; it is a mistake, and for two reasons: for one thing it isn't true that it's not of any use in ordinary mathematics, and (for another) even if it were true it wouldn't matter. (ii) can be disposed of more quickly than (i), so let's get it out of the way first. Since what mathematicians actually do will change from time to time, 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 mathematically substantial. 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:

Let G be a graph where, for each vertex v, the collection $N(v)$ of neighbours of v is a set[1].

(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.*

We deal with (i) 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 endebted to Robert Black[2] for a pretty good 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 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."*

[1] Here our graphs ave 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'.

[2] Prsonal Communication.

A similar argument is to be found on p 239 of Shoenfield [48] and also Shoenfield [47] on page 324 of [2]. The assumption underlying this sounds to me rather like replacement under another name . . . a bit circular.

Randall Holmes [24] 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 Σ^2.

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. "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.

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, 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. And (this is a separate point) even if one takes limitation-of-size in its usual (literal) form, replacement flies in its face by delivering large sets in industrial quantities.

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 amyway? 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. 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 [9] 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) 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 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 from a model of ZF(C). Roughly, one exploits the fact that any model of ZF(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.

Recent work (by Tim Button, so far unpublished) 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 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: 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 (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[3]. 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. This does make it look as though LOS is not 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.

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 deined 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 *negative* and we have known since Aristotle that attempts to define things negatively are not good. However there is a way of capturing this negativity in a legitimate formal way as follows, which says

$$n \text{ is a natural number if } (\forall y)(0 \in y \wedge (\forall m \in y)(m + 1 \in y) \to n \in y)$$

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

[3]It has to be admitted that we here make essential use of the axiom of sumset, but *that* axiom, at least, is not controversial.

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

(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 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 *nth 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 (nowadays *transitive closure*) 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.—[53]). 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

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

(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 ω (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.

Actually with this example there is no problem in proving that the inductively defined collection is a set, since it is a subset of $\mathcal{P}(\mathbb{R})$. 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, ω_1 steps suffice, but there is a little bit of work to be done. We won't do it 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_κ of sets hereditarily of size less than κ. This set can be obtained "from below" by defining a sequence of approximants indexed by an initial segment of the ordinals as follows:

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

Armed with replacement we can be confident that, for any ordinal α, 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 α 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 α such that any countable subset of $\bigcup_{\beta<\alpha} X_\beta$ is already a subset of one of
ry entry for cofinality the X_β. Since the cofinality of ω_1 is ω_1 then ω_1 is such an α.

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 Z, even though it is only of size $2^{\aleph_0}$ and Z proves the existence of much bigger sets than that. See page 74.

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[4]. 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. The moves available to you are the same as the moves available to me, so as we procede with our fit in the word 'deterministic'
two constructions we build an isomorphism between them.[5]

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 axioma-

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[4]Open Question number 24 (the last in the list) at the end of Appendix B of [8] (p 514) is "Develop the model theory of second- and higher-order logic"!

[5]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.

tise 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.

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 urelemente and (ii) the height of the model. (This is in Zermelo [61].) 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. *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.

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 1930 Grenzzahlen und Mengenbereiche was interested in 2nd order categoricity.

Existence of Transitive Closures

We have already used this idea in the section on Δ_0 formulae

Inconveniently, the expression 'transitive closure' has two meanings, and we must not confuse them. (See the glossary p. 93). 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 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. 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, 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 [14] for the details, since we are trying here to keep technicalities to a minimum.

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 ([44] 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 ω_2 and generally $\aleph_\nu$ and ω_ν, where ν 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 ([50] 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 ([6] 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^n-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^n 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 7.2); (i) is agreed. So a natural example of facts about sets in $V_{\omega+\omega}$ that can be proved only by reasoning about big 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.[6]

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 41).

[6] A good place to start looking is the foundations of mathematics mailing list run by Martin Davis.

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_αs 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.

Thee 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_αs 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 such as $\omega + \omega + \omega$ but does not contain the V_αs that we should be able to construct by recursion over those wellorderings. If set theory is to 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 α, then we want to be able to construct all V_β with $\beta < \alpha$. The argument that the universe should be closed under $\alpha \mapsto V_\alpha$ is in [61].

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 unto 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 that will need the axiom scheme of replacement.

6.5.5 The Normal Form Theorem for Restricted Quantifiers

Another reason for adopting replacement (Forster [14]) 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 [14] 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 Δ_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 '$(\forall x)(x \in y \to \ldots)$' or '$(\exists x)(x \in y \wedge \ldots)$'. Thereafter a Σ_{n+1} (respectively Π_{n+1}) formula is the result of binding variables in a Π_n (repectively Σ_n) formula with existential (respectively universal) quantifiers. We immediately extend the Σ_n and Π_n classes by closing them under interdeducibility-in-a-theory-T, and signal this by having 'T' as a superscript so our classes are Σ^T_n and Π^T_n.

This linear hierarchy of complexity for formulæ will be very useful to us in understanding T if we can be sure that every formula belongs to one of these classes[7]: it is standard that we can give a Π^{n+1} truth-definition for Σ_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 Π^T_n and Σ^T_n 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 ϕ is a Π^T_n formula what kind of a formula is $(\exists x \in y)\phi$? It would be very simple if it, too, were Π^T_n. It's plausible that it should be Π^T_n (it has the same number of blocks of unrestricted quantifiers after all) but it 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 $(\forall x < n)(\ldots)$.) In general, when dealing with a recursive datatype, we can define Δ_0 formulæ—as above—as those with no unrestricted quantifiers, where we take restricted quantifiers to be '$(\exists x)(R(x, y) \wedge \ldots)$' and '$(\forall x)(R(x, y) \to \ldots)$', and R is the engendering relation. We find that Δ_0 formulæ behave in many ways as if

[7] well, *lots* of these classes: after all if ϕ is in Σ^T_n it is also in Π^T_{n+1}.

they contained no quantifiers at all. 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 Δ_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 Σ_1 formulæ and Π_1 formulæ. They generalise upwards and downwards in the same way *as long as $\mathfrak{M}$ and $\mathfrak{M}'$ are both transitive models.* Δ_0 formulæ of course generalise both upward and downward and are therefore **absolute**.

The study of the various naturally occurring recursive dataypes 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 Σ_n and Π_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 ψ 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 ϕ of the language of set theory there is an expression ϕ' 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 Σ_n is closed under restricted universal quantification. Dually we infer that Π_n is closed under restricted existential quantification. It is of course immediate that Σ_n is closed under restricted existental quantification and that Π_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 Π_n^T or Σ_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 Φ 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 Φ that sends an n-place predicate to a subset of M^n (and function letters similarly) then $\Phi^{\mathfrak{M}}$ (the *interpretation of* Φ *in* $\mathfrak{M}$) is the formula we get from Φ by applying the following rules recursively to Φ:

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

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

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

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

Subject to some small print (concerning cases where the language of $\mathfrak{M}$ is not the same as the language of which Φ 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* Φ.

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

Unless ϕ is Δ_0, there is no reason to expect that there are any γ that reflect ϕ. The **reflection principle** says that there is nevertheless always such a γ. In fact one can prove the following.

Δ_0 not defined yet ...

Theorem 3. *For every ϕ, ZF proves*
$\phi \longleftrightarrow (\exists$ *a closed unbounded class of* $\alpha)\phi^{V_\alpha}$.

Proof. See Lévy [28], [29].

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 ϕ) then there is a V_α that is closed under ϕ. 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 [53].

Corollary 1. *ZF is not finitely axiomatisable.*

Proof:

If ZF were finitely axiomatisable, then by reflection there would be an ordinal α such that $\langle V_\alpha, \in\rangle$ were a model of ZF. This V_α is a set. This is important because, once we have a Gödel numbering of formulæ, the assertion that every formula in some semidecidable set Σ 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 ϕ 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 Σ has a model and is free of contradiction. We know because of Gödel's incompleteness theorem that no theory can prove its own consistency, so no initial segment $\langle V_\alpha, \in\rangle$ can 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. ■

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

(i) There is a Dedekind-infinite set;
(ii) V_ω 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 32. 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_ω exists one can obtain from it the set of all Von Neumann naturals. (Every Von Neumann natural is a member of

V_ω). So (ii) $\to$ (iii). Evidently (iii) $\to$ (i) since the Von Neumann $\mathbb{N}$ is manifestly Dedekind-infinite. It's the other direction ((i) $\to$ (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_ω or the Von Neumann $\mathbb{N}$ from this? The obvious way to obtain V_ω 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 [34]. (Also Boffa [5]; and [13] p 178; and [55] p. 296.)

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

Thus by adopting the axiom scheme of replacement we erase all need for concern about which form of the axiom of infinity we are using. Finally—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 functions 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 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 Neuman 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 Neuman 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_ω is a model of Zermelo set theory containing wellorderings not isomorphic to any von Neumann ordinal. 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.

This 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 Π_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 α. This α will be the range of the recursively defined function π where $\pi(x) =: \pi``\{x' : x' <_X x\}$. So "$y = \pi(x)$" will be

> $(\forall \sigma)((\langle min(X), 0\rangle \in \sigma) \wedge (\forall x \in X)(\sigma \restriction \{x' : x' <_X x\}$ is total $\to \langle x, \sigma``\{x' : x' <_X x\}\rangle \in \sigma) \to \langle x, y\rangle \in \sigma)$.

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

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

This isn't quite right

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

6.6 Implementation-invariance

Perhaps readers will forgive me for starting with a self-quotation (from [18]).

> "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 ω, (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."

Once one has appreciated that what one is doing is *implementation* not *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. This is a possibility that most mathematicians would never even consider, it's so obviously absurd. But this 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 just a theorem about some special sets that happen to implement reals in the mock-up of the real line that i keep in my attic?? 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`© and the reals as implemented by the

`Blue Real Company`© are non-isomorphic. The best way to do that is to adopt axioms that ensure that [in all situations like this] there is an isomorphism. This isomorphism that bijects the set of `Pink Real`©s with the set of `Blue Real`©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.

This is treated in [18] 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.

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

$\texttt{pair}(x, y) = \texttt{pair}(x', y') \to x = x' \wedge y = y'$,
$\texttt{fst}(\texttt{pair}(x, y)) = x$ and
$\texttt{snd}(\texttt{pair}(x, y)) = y$.

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 $\{\texttt{pair}(x, y) : R(x, y)\}$ 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[8] 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

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

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

[8] f“x is $\{f(y) : y \in x\}$.

Then if $Y = X \times \{\emptyset\}$ exists for this new kind of ordered pair we can recover $F``X$, 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 is a very arresting wee result, and is educationally valuable but not everybody is convinced. Randall Holmes says[9] 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) spec also behaves properly—you will need replacement. Well yes; so what![10] Holmes may be right, but some writers assume that the existence of cartesian products follows without any need to assume replacement. In Godement [22] 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 theonly thing thatmatters 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.

Setlike not defined yet

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. But first we need some definitions and some discussion.

REMARK 1.
Suppose that whenever b and p are two classifiers for an equivalence relation $\sim$ on a proper class X then the function π 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 π is just h, and is 1-setlike by the assumption. So h (which was arbitrary) is 1-setlike. This is replacement.

■

DEFINITION 1. *A classifier for an equivalence relation $\sim$ is a function f s.t* $(\forall xy)(f(x) = f(y) \longleftrightarrow x \sim y)$

[9] Personal communication

[10] But this is simply to say that the pairing and unpairing functions must be 1-setlike. In fact, if Holmes' analysis is the right way to go, then one probably wants pairing/unpairing functions to be fully setlike.

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

From NF studies we have the concept of a *setlike* function.[11]

Definition 2. *A (unary) function f is*

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

An n-ary function f is 1-setlike if $f``(X_1 \times \ldots X_n)$ is a set whenever $X_1 \ldots X_n$ 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.

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,

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

and

$$f``X, \mathcal{P}(f``X), \mathcal{P}^2(f``X) \ldots \mathcal{P}^n(f``X)$$

are isomorphic.

This last fact is related to the fact that the concept of *setlike* arose from the need to state correctly a completeness theorem for Rieger-Bernays permutation models. If $\mathfrak{M}$ is a structure for $\mathcal{L}(\in, =)$ (the language of set theory) and σ 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 ϕ 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 arbitary 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.*

[11] The idea (tho' not the terminology) goes back to [12]. 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.

Proof:

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

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

Remark 3. *(Mac Lane Set Theory)*
Suppose our pairing and unpairing functions are setlike. If f is 1-setlike, then f is locally a set.

Proof:

Suppose f is 1-setlike. Then $f``x$ is a set if $x$ is; $x \times f``x$ is a set since our pairing function is setlike, and $f \restriction x$ is now a subclass of $x \times f``x$, and it will be a set because we have Δ_0 separation. ■

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 σ is perforce setlike, since $\sigma(x)$ has to be $\sigma``x$, so $\sigma``x$ is always defined.

Remark 4. *(Mac Lane Set Theory) : (Coret [12])*
If f is 1-setlike then $j(f)$ is 1-setlike.

Proof:

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

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

which is a set by separation. ■

Corollary 2. *(Mac Lane Set Theory)*
Every 1-setlike function is setlike.

Proof:

If f is 1-setlike then, by remark 4, $j(f)$ is 1-setlike as well. This powers an induction that shows that $j^n(f)$ is 1-setlike for all n, which is to say that f is setlike. ■

We allude above to a notion of *typing* which is explained elsewhere in this document but which, too, we reprise as part of the effort to make this section self-contained. The typing we are concerned with arises [for example] when we have new entities that arise from equivalence relations—e.g. arithmetic of natural numbers arising from equipollence between finite sets and operations for which equipollence is a congruence relation. The new language is [potentially] typed in the sense that [for example] one is not [might not be] allowed to place a '$\in$' to the left of a variable ranging over numbers. Some assertions in this language are so strongly typed that the number variables can be rewritten out of them altogether: the assertion that addition of natural number is commutative is such an example. There are some sentences from which number variables cannot be removed, and which accordingly require us to decide on a classifier for equipollence... an example would be the assertion that every natural number has only finitely many predecessors. This particular sentence is well-behaved in the sense that its truth-value does not depend on the choice of classifier; it is well-typed in the above sense—no integer variable is preceded by an '$\in$' for example. Then there are assertions like Rosser's Axiom of Counting, that says that every natural number n has precisely n predecessors. To make sense of this expression one has to actually use a classifier, but one would expect that its truth-value does not depend on a choice of classifier. Finally there are assertions—such as '$3 \in 5$'—whose truth-value emphatically does depend on choice of classifier, and which are clearly untyped. The purpose of this section is to relate the typing to insensitivity-to-choice-of-classifier.

THEOREM 4. *(Zermelo, KF?)*

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

Proof:

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

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

Let us call this bijection π. We want to establish that π is setlike. Because of lemmas 3 and 4 it will suffice to show that π is 1-setlike. Suppose $P \subset p``X$; we want $\pi``P$ to be a set. Now $\pi``P$ is a subcollection of the set $b``X$, so we can aspire to use separation to prove it a set. And indeed we can: it is $\{y \in b``X : (\exists x \in P)(y = b(x))\}$. ■

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

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

So if $b``X$ and $p``X$ are proper classes there is some work to do. Is it sufficient that b and p be setlike? Consider the relation $(\exists X')(P' = p``X \wedge B' = b``X)$

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

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

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

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

Let α_p be a pink ordinal. There is a wellordering $\mathcal{A} = \langle A, <_A \rangle$ whose pink ordinal is α_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 α_p to be not only total but setlike. Why should it be?]

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

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

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

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

The stricter the typing the easier it is to show that typed 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.

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!

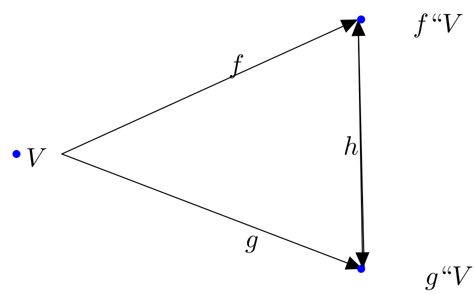
What set-theoretic axioms do we need, and what conditions on classifiers, to ensure that Rosser's Axiom of Counting holds?

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 $H_{\beth_\omega}$.



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 $h \restriction Y$ 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 Σ_1 separation so it cannot be done in KF. But even if we upgrade to Zermelo (so we have full separation) we still have a problem with finding a suitable set A from which to separate. If $f``V$ is a set we can use it to be A.

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

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

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

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

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

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

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

Lemma 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 Δ_0 separation (as in KF). Let f be a set of ordered pairs that bijects x and y. If $x' \subseteq x$ then $f``x' \subseteq y$ so $f``x'$ is a set by separation. So $j(f)$ is a bijection between $\mathcal{P}(x)$ and $\mathcal{P}(y)$. And it is a set—again by separation—being a subset of $\mathcal{P}(x \times y)$. This last object is a set according to KF—at least if our ordered pairs are Wiener-Kuratowski. ■

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

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.

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 axiom being proved independent. "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[1]. Our consistency proofs below will

[1]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 ϕ, where ϕ is a one-place predicate. The intuition is that x is hereditarily ϕ if everything in $TC(x)$ is ϕ. (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_ϕ, the class of things that are hereditarily ϕ, and it is easy for the beginner to become confused. I am going to start with my favourite definition:

Definition 3.

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

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

The first thing to notice with this definition is that everything inside H_ϕ under this definition will be wellfounded. This is because H_ϕ is a recursive datatype and comes equipped with a principle of induction. We can use this induction to argue that every set in H_ϕ is wellfounded.

A word is in order on the definition and the notation involved. The two uses of the set-forming bracket in the *definiens* of 'H_κ' and 'H_ϕ' 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 depends on there being such an x.

One could write:

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

and

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

this up

Of course H_ϕ 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_ϕ as the intersection of all sets x such that $\mathcal{P}_\phi(x) \subseteq x$, since there are none; the interesection 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_ϕ 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 ϕ as the set of things x s.t. $TC(x)$ is ϕ. This is a bad notation for various reasons. For one thing it makes sense only when ϕ is a property which is preserved under subsets (like being smaller than κ) 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 countably 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.

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consistent.

7.1 Extensionality

7.2 Replacement

$V_{\omega+\omega}$ is a model for all the axioms except replacement. It contains well-orderings of length ω 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[2]

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

7.3 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 48) we define:

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

(as on p 48

This function—$x \mapsto \mathcal{P}_{\aleph_1}(x)$—is not ω-continuous, since new countable subsets might appear at ω-limits: X_ω could have countable subsets that are not subsets of any X_n with n finite. This means we will have to iterate the construction of the X_β up to a stage α such that any countable subset that is present at stage α was created at some earlier stage. By use of countable choice we can show that the first such α is ω_1. So we iterate ω_1 times and then use replacement to conclude that X_{ω_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 Δ_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 Winer-Kuratowski pairing function which is the one most commonly used—the well-orderings will be

[2]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!

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.

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.

7.4 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.

7.5 Sumset

Recall the definition of beth numbers from chapter 1. Recall from **??** 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. qqA 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 $H_{\beth_\omega}$.

7.6 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 π 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^π. Alternatively, we could define Φ^π as the result of replacing every atomic wff $x \in y$ in Φ 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 Φ 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 57 is stratifiable. Foundation fortunately is not stratifiable! The π we need 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: $x \in_\pi \emptyset \longleftrightarrow x \in \pi(\emptyset) = \{\emptyset\}$. So $x \in_\pi \emptyset \longleftrightarrow x = \emptyset$. So $\mathfrak{M}^\pi$ is a model for all the axioms of ZF except foundation.

7.6.1 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 7.8.

7.7 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).[3]

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

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

[3] 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 . . . .

(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 76.

7.7.1 More about Extensionality

Mention Benedikt's synonymy result here

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. This wraps up the situation if you believe in the axiom of foundation. Interestingly in the Quine systems matters are not so straightforward. It is now believed that Quine's NF is consistent (tho' the proof is very difficult and remains unpublished at time of writing [**?**]). However it has for some time been known to be consistent if extensionality is weakened to allow atoms—but only flavour 1 atoms (see [**?**]); the consistency proof doesn't work with Quine atoms! There may be more to this atom business than meets the eye.

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

The relative strength of extensionality and its negation is quite sensitive to other considerations too. Does the language contain an abstraction operator? See Scott [45] where he shows that a version of ZF without extensionality can be interpreted in Zermelo set theory! See also [20].

7.8 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 σ 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 σ 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!!)[4]

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

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

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

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

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

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

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!

7.9 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 [34] 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 **??**.

blend this in

In section 4.2.1 we floated the idea of a world in which pairing fails, and the set 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 on those models—because of the typing restrictions.

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

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 $\phi(x, \vec{y})$:

$$(\forall X_1 \ldots X_n)(\exists Y)(\forall z)(z \in Y \longleftrightarrow \phi(z, X_1 \ldots X_n)) \tag{8.1}$$

We rewrite all the axioms of *ZF* except replacement and separation by restict-ing 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.[1] However, if we restrict the scheme 8.1 to those instances where ϕ 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 [46].

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)[2] 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 decidable sets of axioms and not just to those with finitely many 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

[1] It was actually first spelled out by Wang [58], who called the system 'NQ'.

[2] The old terminology—still very much alive in this area—speaks of *recursively axiomatisable theories*.

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 [56]. 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 Coret'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 π. 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[3], 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!

[3]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.

Chapter 9

Set-theoretic principles whose significance lies 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.

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. We have seen how an important part of the mathematical meaning of the scheme of replacement is implementtion-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. Hartogs' lemma tell us that if a transfinite deterministic monotone process fails to complete it's not because you have run out of ordinals. Replacement and choice are axiom (schemes) but Hartogs' lemma is not. 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. 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 IO

. . . means that one can make as many disjoint copies of a given structure as one wants. After all, if we want to make κ-many disjoint copies of a structure

$\mathfrak{A}$ we need a set $K = \{\{k\} : k \in K'\}$ of singletons, with $|K| = \kappa$. Then $\{\mathfrak{A} \times \{k\} : k \in K'\}$ is a family, of size κ, of copies of $\mathfrak{A}$.

Check the primes; my head is spinning

One frequently needs this kind of freedom of manœuvre in algebra, tho' the example I am about to give is not literally a case in point. . . one often wants to take a family of structures and make copies of all the structures in the family in such a way that the copies are pairwise disjoint. A pertinant example is the inference of AC from the multiplicative axiom. Here's how it goes. Assume the multiplicative axiom, and let X be a family of nonempty sets. We wish to infer the existence of a choice function for X. If X is a family of nonempty sets, then $\{x \times \{x\} : x \in X\}$ is a family of pairwise disjoint nonempty sets, which means that there is a transversal for this family (by the multiplicative axiom). Just what does this transversal consist of? For each $x \in X$ the transversal contains precisely one member of $x \times \{x\}$; such an ordered pair picks one element from x, so the transversal is in fact literally a selection function for X. The point is that the multiplicative axiom applies only to families of *pairwise disjoint* sets, whereas AC makes a claim about *all* families: we have to be able to obtain a pairwise-disjoint family from an arbitrary family in an information-preserving way.

Remark 5. *(KF)*
IO implies Hartogs'.

Proof:
Given a set X, we seek a wellordered set W that does not inject into X. We implement ordered pairs somehow. It doesn't much matter how we do it, as long as "$x = \langle y, z\rangle$" is stratifiable with 'y' and 'z' having the same type. Let us suppose that this type is k greater than the type given to 'x'. We know (for reasons i won't rehearse) that $k \geq 0$. Consider the set $\mathcal{W}$ of isomorphism classes of wellorderings of subsets of X. The assertion

"*calW* is the set of isomorphism classes of wellorderings of subsets of X."

This is stratifiable with '$\mathcal{W}$' being given a type $k+2$ greater than the type given to 'X'. $\mathcal{W}$ is certainly a set by the KF axioms. By IO, $\mathcal{W}$ is the same size as a set of singletons $\iota``\mathcal{W}'$, and $\mathcal{W}'$ (think of this as $\iota``\mathcal{W}^{(1)}$) is the same size as a set of singletons $\iota``\mathcal{W}''$ (think of this as $\iota``\mathcal{W}^{(2)}$) and so on up to $\mathcal{W}^{(k)}$ so that $\mathcal{W}$ is the same size as $\iota^{k+2}``\mathcal{W}^{(k)}$). $\mathcal{W}^{(k)}$ is now the set W that we seek.

■

IO also legitimates a rather nice and useful way of thinking about partial orderings. An ordernesting[1] is a poset $\langle X, \subseteq\rangle$ whose order relation is $\subseteq$. Consideration of the equivalence relation on $\bigcup X$ defined by $u \sim v$ iff $(\forall x \in X)(u \in x \longleftrightarrow v \in x)$ leads us to wonder whether or not it is $\mathbb{1}_{\bigcup X}$, the identity relation restricted to $\bigcup X$. It might and it might not. Let's itemize these two possibilities.

[1]I learnt this terminology from Allen Hazen, but I don't know who invented them or where the word comes from. Allen drops the name 'Sierpinski' in this connection.

Definition 4. *A poset $\langle X, \subseteq\rangle$ is a* **type-1 ordernesting** *if the equivalence relation on $\bigcup X$ defined by $u \sim v$ iff $(\forall x \in X)(u \in x \longleftrightarrow v \in x)$ is $\mathbb{1}_{\bigcup X}$, the identity relation restricted to $\bigcup X$. Otherwise it is a* **type-2 ordernesting**.

We shall show that IO is a kind of representation theorem, in that it says that every partial ordering is isomorphic to a type-1 ordernesting. We have to be careful how we state this, because it sounds very like Stone-Birkhoff, and *that* needs BPI. Given a boolean algebra $\langle B, \leq_B\rangle$ consider the function defined on B by $b \mapsto \{x \in B : x \leq b\}$. This makes $\langle B, \leq_B\rangle$ isomorphic to a Type-1 ordernesting as desired—and without using BPI—but it does not respect complementation.

Remark 6.
IO is equivalent to "Every partial order is isomorphic to a type-1 ordernesting".

Proof:
L $\to$ R:
Let $\langle X, \leq_X\rangle$ be a partial order. By IO, there is a set Y and a bijection f such that $\iota``Y = f``X$. The map sending each $x \in X$ to $\bigcup\{f(x') : x' \leq x\}$ is defined by a homogeneous formula and is an isomorphism between $\langle X, \leq_X\rangle$ and a type-1 ordernesting that is a substructure of $\langle \mathcal{P}(Y), \subseteq\rangle$.

R $\to$ L:
Suppose that every partial order is isomorphic to a type-1 ordernesting and let X be an arbitrary set. Equip X with the identity relation $\mathbb{1}_X$ to obtain a partial order. Any type-1 ordernesting isomorphic to $\langle X, \mathbb{1}_X\rangle$ has a carrier set containing only singletons. Thus X is in bijection with this set of singletons.
■

We also have

Remark 7.
"Every set is the same size as a set of pairwise disjoint sets"
is equivalent to
"Every partial order is isomorphic to a type-2 ordernesting".

Proof:
Very similar to the proof of remark 6.
L $\to$ R:
Let $\langle X, \leq_X\rangle$ be a partial order. By assumption, there is a set Y of pairwise disjoint sets and a bijection f such that $f``X = Y$. The map sending each $x \in X$ to $\bigcup\{f(x') : x' \leq x\}$ is now an isomorphism between $\langle X, \leq_X\rangle$ and a Type-2 ordernesting that is a substructure of $\langle \mathcal{P}(\bigcup Y), \subseteq\rangle$.

Check this: cut-and-paste makes for errors

R $\to$ L:
Suppose that every partial order is isomorphic to a Type-2 ordernesting and let X be an arbitrary set. Equip X with the identity relation $\mathbb{1}_X$ to obtain a partial order. Any Type-2 ordernesting isomorphic to $\langle X, \mathbb{1}_X\rangle$ has a carrier

set consisting of pairwise disjoint sets. Thus X is in bijection with this set of pairwise disjoint sets. ■

Check this: cut-and- makes for errors

A subtlety to do with IO. IO implies that you can make disjoint copies of things. Indeed it implies that, for all X, and all cardinals κ, there is a family of copies of X of size κ, *and the family forms an indiscrete category*. This is easy. Let ι"K be a set of singletons of size κ. Then, for each $k \in K$, $X \times \{k\}$ is an object of the category and the unique morphism from $X \times \{k\}$ to $X \times \{k'\}$ is the obvious map $\langle x, k \rangle \mapsto \langle x, k' \rangle$.

Indeed there is even a converse. Suppose i have an indiscrete category of size κ; then i can find κ-many singletons. Worth spelling out.

Let $\mathcal{K}$ be the indiscrete category of size κ and $\hookrightarrow_{i,j}$ the unique morphism from K_i to K_j, objects of $\mathcal{K}$. Let k be some random element of some random object K_i of $\mathcal{K}$. Then $\{\{\hookrightarrow_{ij}(k)\} : K_j \in \mathcal{K}\}$ is a set of singletons of power κ.

N.B. if i weaken the assertion about copies to merely: "there is a κ-sized family of things of size X" then i don't get back IO.

Include the essay called 'workspace' on the importance of being able to make disjoint copies. In it should go

(i) the discussion of IO and the consistency of ZF relative to str(ZF) + IO.

Also

(ii) the inference of AC from the multiplicative axiom.

Also

(iii) the permutation models of NF containing sets equal to their own power set. You need sets disjoint from their own power sets...

(iv) You can make as many disjoint copies as you want.

Another common situation is the one where we have a family $\{\mathfrak{M}_i : i \in I\}$ of structures. If they are not already distinct, we reach for $\{\mathfrak{M}_i \times \{i\} : i \in I\}$. If they are distinct but not disjoint, or merely given as a *family* not as an *indexed family* we reach for $\{\mathfrak{M}_i \times \{\mathfrak{M}_i\} : i \in I\}$, which we might write as $\{\mathfrak{M} \times \{\mathfrak{M}\} : \mathfrak{M} \in \mathcal{F}\}$ where $\mathcal{F}$ is the set of structures.

9.2 Certificates

This point about counted sets *vs* 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` $\rightarrow$ `indiscrete-category` $\times$ `indiscrete-category-certificate`.

(One should smuggle in here the word *paper-trail*.)

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 Σ_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 *un*pack 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

Dear Jamie, I am writing this article in the form of a letter to you. There are two reasons for this. The intelligent comments i seek can only be had from someone who knows both theoretical Computer science and some set theory. The other is that you might feel you owe me one beco's of the time i have spent on your TCS article (have the buggers finally accepted it BTW..?) and would therefore be a soft touch. The third reason ("The Spanish Inquisition is famous for two things!") is that you might actually be interested!!

So here goes.

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 tress 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 reaason 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 with 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      |    The naturals
character   |
            |    lists, trees,
            |    a la ML
            |
------------|-----------------------------------------------
            |
infinite    |    The cumulative
character   |    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 who founders are all the ω-sequences and whose constructor is ω-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 ω_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 (All uncountable cardinals can be singular Israel J of Mathmatics **35** (1980) pp. 61–88.) that the recursive data type 2 can contain all ordinals

There is another result. I noticed it, but i'm sure it's folklore. 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 data type in it and thereby bound its size. Consider not HC but the recursive data type (3). The reals is the same size as the set of ω-sequences of reals. That means that we can define by recursion on the recursive data type (3) an injection into the reals. We need the freeness of (3) to ensure that the map we are defining is an *in*jection.

Moral: every free recursive data type 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 ω the recursive data type (2) generated from 0 by `succ` and ω-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[2]. 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.

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...

[2]Tho' we do know that it is a set.

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 [57].

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 ω-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 α is naturally a wellordering of length α. 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?

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:

$$\cfrac{\cfrac{\begin{array}{c}[A]\\ \vdots \\ B\end{array}}{A \to B}\ \to\text{-int} \qquad A}{B}\ \to\text{-elim} \tag{9.1}$$

where the '$A \to B$' is the result of an $\to$-introduction and at the same time the major premiss of a $\to$-elimination

and

$$\cfrac{\cfrac{A \qquad B}{A \wedge B}\ \wedge\text{-int}}{A}\ \wedge\text{-elim} \tag{9.2}$$

where 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

$$\begin{array}{c} A \\ \vdots \\ B \end{array} \tag{9.3}$$

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:

$$h(0, x_1 \ldots x_n) =: f(x_1 \ldots x_n);$$

$$h(y+1, x_1 \ldots x_n) =: g(h(y, x_1 \ldots x_n), y, x_1 \ldots x_n)$$

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 a label such that in every subformula '$x \in y$' the label given to 'x' is one lower then 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 member 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 ω-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 ω-sequence $\langle x_n : n \in \mathbb{N} \rangle$ with $x_0 = x$ and $x_{n+1} \in x_n$ for all $n \in \mathbb{N}$.

Must define countable choice

(These definitions are not strictly correct, but are equivalent to the correct definitions as long as countable choice holds.)

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fit in an allusion to this in the body of the text

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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 [55] 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 [37]. Although the book [23] by Hallett and the book [52] by Tiles are not alluded to in the body of the text, they are still definitely worth a read. The Väänänen article [54] could be profitably consulted by those interested in pursuing second-order categoricity. Quine's *Set theory and its Logic* [41] 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.