The Axioms of Set Theory

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Preface

This is 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want to adopt them (in the light of the uses to which they can be put) say a bit (but only a bit, for this is not a historical document) on how we came to adopt them, and explain their mutual independence.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 explaining what the axioms say and why one might want to adopt them.

Finally I must cover myself by pointing out in my defence that I am not an advocate for any foundational rôle 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 like the one I have given myself here include

[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.

Mycielski [35] and Shoenfield [44]. My effort is both more elementary and more general than theirs are.[2] 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 in a silver threepenny bit for one of them one of 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: [17]. The fact that [17] is an *undergraduate* text should calm the fears of readers concerned that they might not be getting a sufficiently elementary treatment.

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

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

2

The Cumulative Hierarchy

The *axioms of set theory* of my 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

$$V_\alpha =: \bigcup_{\beta<\alpha} \mathcal{P}(V_\beta) \tag{2.1}$$

... 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 conception of sets is more-or-less explicit in Mirimanoff [33], but is usually associated with Von Neumann [54]. He noticed that the cumulative hierarchy gives an **inner model** of set theory.[1] Von Neumann

[1] For these purposes an inner model is a definable proper class that is a model of

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 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 graphically 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 $\beth_{\alpha+1} := 2^{\beth_\alpha}$.

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.

3

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, 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 issues that cannot be ducked altogether and which we will cover briefly here.

3.1 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 of course mounted, and—although the points made in favour of the defendants in the two cases were of course 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. 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.

3.2 Intension and Extension

The intension-extension distinction is a device of mediæval philosophy which was re-imported into the analytic tradition by Church (see [13] p 2) and Carnap [9] in the middle of the last century, probably under the influence of Brentano. However, in its passage from the mediævals to the moderns it has undergone some changes and it might be felt that the modern distinction 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.

3.3 What is a Mathematical Object?

We are not yet finished with philosophical prolegomena, sadly. 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 [37] had a *bon mot* which indicates memorably where lies the importance of the concept being 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. 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 2) 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 not to have a hole in it in the first place. Even infinite time is of no help to her.)

(ii) If widgets are legitimate well-defined objects one can quantify over them. The literature of philosophical logic contains numerous aftershocks of Quine's ([36] "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: [48].

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. 6) derives from the clear account of identity-between-sets that it provides. We will see more of this in chapter 2.

So how can they acquire this status? Typically they seem to go through a 3-step process.

(i) 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.

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

(iii) 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 he **unit** for an operation * if $(\forall x)(*(x, \clubsuit) = x)$. For example: 0 is the unit for addition; 1 is the unit for multiplication; the empty string is the unit for concatenation; the identity function is the unit for composition of functions, and so on. By "it becomes possible" what I mean is that until you are considering arbitrary widgets and operations on them then the empty widget is unlikely to attract your attention. How could it, after all? The fact that it's a unit for various algebraic operations on widgets becomes important only once you are considering operations on widgets and this is more likely once you have arbitrary widgets. [3]
[4]

[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.

[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

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.

3.4 The Worries about Circularity

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 . . . ?

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 we feel that *it must mean something*. Something it could be taken to mean is that set theory is metaphysically prior to the rest of mathematics, or in some other sense pro-

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."

[46] 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. This view of Mathematics is sometimes parodied as *Mathematics as stamp collecting* or *Mathematics as butterfly collecting*; I prefer *Mathematics as egg-stealing*.

vides a foundation for it. This second claim is far from obvious and *does* invite points 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 retained the currency 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 spectacluarly 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

[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.

numbers aren't sets and they don't do membership. A thoroughgoing foundationalism about sets (of the kind that says that all mathematical object 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 does make the point that the whole foundation project is a bit more subtle than one might expect, and that the cirularities which launched this digression are not really pathologies, but a manifestation of the fact that life is complicated.

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

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

4

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

The Axioms of Set Theory go back to an article by Zermelo [57] of about 100 years ago, and in very nearly their present form. The most significant difference between Zermelo's axiomatisation in [57] 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. They 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" to which the paradoxical sets palpably did not belong. 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

[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.

to be harbouring hidden dangers. Quite where the boundary between safe collections and dodgy collections lies 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 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 a 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.

4.1 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

[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 riposte is required.

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.

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 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 $x = \{x\}$. 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.[3]

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.

4.1.0.1 Ordinals

It has probably by now struck the astute reader that the usual way of narrating the cumulative hierarchy (as in section 2) makes essential use of ordinals. Can this be avoided? No. Does this matter? Again, no. There are two ideas that we must keep separate. One should not allow the (fairly 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 14.) 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. This use of the word 'monotone' here might sound funny to some, so let me illustrate with a couple of examples:

(i) The riddle of *Thompson's lamp* [49] concerns a process that is *not* monotone. At time $t = 0$ the lamp is switched on; at time $t = 0.5$ it is switched off; then at time $t = 0.75$ it is switched on again, and so on. What is its state at time $t = 1$? Is it on or off? It can't be on because on every occasion before $t = 1$ at which it is switched on it is turned off again before $t = 1$. By the same token it cannot be off either!

(ii) Disjunction and conjunction ($\vee$ and $\wedge$) are commutative and associative, so one can think of them as operations on finite sets of propositions. Thought of as functions from sets-of-propositions to truth values they are *monotone* in the sense that

[3] See Menzel [32] where he implements ordinals as atoms, and even arranges to have a *set* of atoms—by weakening replacement

$$P \subseteq Q \longrightarrow \bigvee P \geq \bigvee Q \tag{4.1}$$

and

$$P \subseteq Q \longrightarrow \bigwedge P \leq \bigwedge Q \tag{4.2}$$

(setting `false` $\leq$ `true`).

Further, given an infinite family $\langle p_i : i \in \mathbb{N} \rangle$ of propositions, both the sequences

$$\langle \bigwedge_{i<n} p_i : n \in \mathbb{N} \rangle \tag{4.3}$$

(That is to say: p_0, $p_0 \wedge p_1$, $p_0 \wedge p_1 \wedge p_2 \ldots$)

and

$$\langle \bigvee_{i<n} p_i : n \in \mathbb{N} \rangle \tag{4.4}$$

—thought of as their truth-values—are monotone;

This has the effect that a conjunction (or disjunction) of an infinite set of propositions is well-defined. The limit of 4.3 is `false` as long as even one of the p_i is false (and `true` otherwise)—the point being that if 4.3 ever takes the value `false` then all subsequent values are `false`. Analogously the limit of 4.4 is `true` as long as even one of the p_i is `true` (and `false` otherwise)—the point being that if 4.4 ever takes the value `true` then all subsequent values are `true`.

Get the XOR formula to display the subscripting properly

Contrast this with exclusive-or (`XOR`). `XOR` similarly is associative and commutative and so can be thought of as a function from finite-sets-of-propositions to truth-values. However there is no analogue of 4.1 or 4.2: the sequence

$$\begin{gathered}\texttt{XOR}\\ \langle i < np_i : n \in \mathbb{N} \rangle\end{gathered}$$

is not monotone and therefore one cannot apply `XOR` to infinite sets of propositions:

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 45). This is by no means obvious, and not everybody will want to work through the proof. (Those who do can see the discussion in [17].) 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 [14] calming.

However, none of this gives us any clue about how to think of ordinals as *sets*. I shall not here explain how to do that, since it is one of the things that is explained in every book on set theory written in the last 80 years so the reader is guaranteed to learn it anyway. In contrast this is 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 2 p. 6 above, and the circularities worries discussed around page 14 would come back with a vengeance..

4.1.1 Set Pictures

According to this view, sets are the things represented by accessible pointed digraphs, or APGs.[4] 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.

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 express an interest in it are usually directed to Aczel [1]—though the seminal paper is the hard-to-get [21] 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 [21]:

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)

[4] So they should really be AP*D*s, but the notation is now standard.

(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 30.

5

Listing the Axioms

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.

5.1 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 4.1. Why does this give us extensionality? One direction is easy. Clearly sets with distinct members must be distinct sets, by the identity of indiscernibles. 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 mebers 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 fo extensionality for sets that we have just seen is merely the version of this princip[le for sets.

5.2 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:

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

  In some ways it might be clearer what is going on if we were to have an axiom scheme of existence of arbitrary finite sets:

  $$(\forall x_1 \ldots x_n)(\exists z)(\forall w)(w \in z \longleftrightarrow \bigvee_{1 \leq i \leq n} w = x_i).$$

  It is not hard to see that this scheme can be deduced from the axiom of pairing and the axiom of sumset. The advantage of thinking in terms of the whole scheme is that it can be seen as an axiom (scheme) arising from the insight that finite collections of objects are not going to be problematic and can be safely assumed to be sets.(The Russell class is clearly not going to be finite, for example). This will prepare us for the principle of *limitation of size* to be seen below.

  The axiom of pairing is so basic that hardly anyone ever thinks about what life would be like without it. See [31]. Without pairing we cannot construct ordered pairs to order, so we cannot discuss relations of arity greater than 1.
- **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 '$\bigcup x$'.
- **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. 35. If safety is to be found in smallness, then any subset of a safe thing is also safe.
- **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) but not *dangerously* bigger. Again this is an axiom that could arise only once one had the idea of sets as objects-in-extension. 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.
- **Axiom scheme of Replacement**

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

(ϕ represents a function, and replacement says "the image of a set in a function is a set").

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).

5.3 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:[1]

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

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

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!".

It is a standard observation that once one has the axiom of infinity then one can prove the existence of the empty set by separation.

[1] There is a literature (see for example [48]) 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. 11, once one accepts arbitrary widgets-in-extension one has accepted null widgets.

Once one thinks of these two axioms as bearing two messages of the same kind, one might ask if there are other messages of the same kind—to be obtained by an iteration of the step that took us from the axiom of empty set to the axiom of infinity. It turns out that there are.

We needed the axiom of empty set because we noticed that without it the universe might not contain anything. We then needed the axiom of infinity because we noticed that if we assumed only the axiom of empty set then there might not be any infinite sets. 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 $(\exists x)(\phi(x))$.

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.

5.4 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 section to itself, and the axiom of choice merits a whole chapter.

5.5 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. 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.[2] There are others who, while having a more inclusive view of what sets are or might be, nevertheless feel that there is nothing to be gained by remaining receptive to the possibility of extra sets violating the axiom of foundation, simply because the illfounded sets bring us no new Mathematics. This is a much less straightforward position, but of course also much less contentious. 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 [20].

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.[3]

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

[2] Perhaps not *inescapably* (see Forster [19]) but certainly *plausibly*.
[3] Thanks to Peter Johnstone for reassurance on this point.

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 Rusell-and-Whitehead [41]

Then one might be receptive to the result that the two extensions
(i) ZF + foundation and
(ii) ZF + AFA
of ZF + Coret's axiom are both conservative for stratified 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 [21];
(ii) Church's Universal Set theory [11];
(iii) the NFU conception of illfounded set;
(iv) the positive set theory of Hinnion's school in Brussels.

All these theories can be interpreted into ZF (or natural enhancements of ZF). This creates an opening for the rhetorical move that says: all these things can be interpreted into ZF so they can be seen as mere *epiphenomena*. The difficulty for people who wish to adopt this point of view is that there are interpretations in the other direction as well: ZF can be interpreted in all these theories (or natural enhancements of them as before). So which conception is primary? One is reminded of what philosophers call the "paradox of analysis"[4]

This interpretation argument (for what it is worth) cannot be mounted against the illfounded sets of Quine's NF, since no interpretation of Quine's NF into ZF-like theories is known: the consistency question for NF is open. On the other hand that very fact gives cautious working

[4] "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." [26] p 323.

mathematicians the perfect excuse to ignore NF altogether—though presumably they will find it prudent not to burn their boats by adopting a view that would be refuted by a consistency proof for NF should anyone ever find one. The unavailability of the interpretability argument against NF is scarcely an argument in favour of NF, but the arguments in favour of NF are not our brief here anyway.

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 6.3.4). 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 where they arise from. 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 clases.

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 saw in the case of the number 5. However, there is nothing to stop us implementing the mathematical object as the *set of all its instances of minimal set-theoretic rank*. The object answering to the italicised description is a set by the axioms of ZF[5], 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 6.3.6.2). 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. 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. This example serves to underline the importance of Scott's trick. Gauntt [23] showed that if we assume neither the axiom

[5] And we do really mean ZF here, not Zermelo set theory. It seems that replacement is need to get the set-theoretic rank function to behave properly.

of foundation nor the axiom of choice then we can find a model which has no implementation of cardinals.

5.5.1 The Remaining Axioms

We have postponed detailed discussions of the axiom of choice and the axiom scheme of replacement. Each of these is a large enough subject to merit a chapter all to itself. (The axiom scheme of separation has not had a detailed discussion yet either, but it will be treated as a side effect of the rôle of the principle of limitation of size in motivating the axiom scheme of replacement.) We turn first to the axiom scheme of replacement.

6

The Axiom Schemes of Replacement and Collection

Important role played by replacement in the study of L

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

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

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:

$(\forall x \in X)(\exists y)(\psi(x, y)) \longrightarrow (\exists Y)(\forall x \in X)(\exists y \in Y)(\psi(x, y))$, where ψ is any formula, with or without parameters.

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

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. So now we can consider the proposal to adopt them as axioms.

Does the conception of set that we are trying to axiomatise force the scheme of replacement into our trolley? There are various ways in which it might.

(i) The weakest of the cases one might make would be one that says that it's useful;
(ii) secondly we might be able to persuade ourselves that the axiom scheme is true in the cumulative hierarchy of sets (and we believe that the universe of sets *is* the cumulative hierarchy);
(iii) thirdly–and this would be more difficult—it might be that we could argue that our concept of set from ordinary mathematics forces the axiom scheme upon us. Ordinary mathematics was suspicious of big sets from the outset, so perhaps that insight can give us an argument for replacement?

6.1 Limitation of Size

Let us take the third of these first, since it will enable us to get out of the way an overdue discussion of the axiom scheme of separation.

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.

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.

Limitation of size—in either version—has some plausibility. None of the paradoxes concern small sets. Historically the motivation for formalising set theory was to investigate $\mathbb{R}$ and $\mathbb{N}$ (and the other sets that arise from them in Analysis) more carefully. All these sets are small. So as long as we adopt only axioms that respect LOS we ought to be able to help ourselves to as many as we want without getting into trouble (but see point (3) below.)

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

6.1.1 Church's distinction between high and intermediate sets

We have now reached a point at which we need to invoke the distinction between *large* sets (as in *large* cardinals) and *big* sets which are collections that in most jurisdictions are not sets at all but proper classes. Within the big sets we might want to distinguish a subspecies of **intermediate** classes. Church [11] makes a distinction between **low** sets (a low set is a set the same size as a wellfounded set) and **co-low**[1] All other sets he calls **intermediate**. Our *big* sets include his co-low (high) and intermediate sets. Examples of intermediate classes are things like the

[1] Church calls them **high** but this seems to me to be profligate of notation.

Russell class and its congenors[2], the collection of wellfounded sets, and the collection of all Von Neumann ordinals.

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

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

(There is yet a third candidate for a characterisation of the intermediate classes: they are those proper classes whose complements are also proper classes. (Notice that the complement of V is a set; whether or not the complement of the Russell class is a set will depend in part on whether or not the axiom of foundation is true). The assertion that all big classes (the intermediate classes included) are nevertheless the same size as V has highly nontrivial consequences. Von Neumann took up this assertion as an axiom: see 9.0.2 in chapter 9.)

The moral that Church points is that the *aperçu* about paradox and big sets doesn't—on reflection—give an argument for an axiom that says that any class is a set as long as it's small enough: since the paradoxical sets are all intermediate rather than co-low, it could actually be an argument for an axiom of complementation. There are systems of axiomatic set theory (such as the system in [11]) in which the complement of a set is a set. This system is known to be consistent relative to ZF so it's

[2] $\{x : x \not\in^2 x\}$, $\{x : x \not\in^3 x\}$ and so on.

clear that size is not by itself a problem. (This is a point against LOS-2 rather than LOS-1).

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

Church's point can be answered, or at least side-stepped. The paradoxes certainly do not involve *small* sets, so a principle (such as LOS-1) that restricts us to small sets will certainly enable us to steer clear of the paradoxes.

6.1.2 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.

rewrite this section completely

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 has 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 (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.

6.1.3 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.

6.1.4 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 not, one feels, be formalisable in such a way as to have consequences that are untrue to its spirit. It might be of course, that Limitation of size is a sensible fundamental principle but that replacment is not a a formalisation of it, but nobody seems to draw this moral.

The fact that replacement enables us to prove the existence of sets vastly larger than $\mathbb{N}$ not only makes it difficult for us to defend it on the grounds that it is a formal version of the limitation of size principle, it's not immediately clear whether the manufacture of supersized sets like $\bigcup\{\mathcal{P}^n(\mathbb{N}) : n \in \mathbb{N}\}$ is a point in its favour (to be exploited in an IBE argument) or a point against it. Are these sets part of the consensus that we are trying to capture? Or are they part of the same quagmire as the Russell class? If your view of set theory is of something that arose from the study of the continuum you might feel that sets of such excessive size

[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.

are so remote from your motivations as to not be legitimated by them, and accordingly—*prima facie* at least—to belong to the quagmire rather than to Mathematics. However, that appears not to have been the view of Russell and Whitehead or Skolem, at any rate. Russell and Whitehead ([41] 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.[4] Skolem ([47] p 297.) took 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 6.3.1.

6.2 Is Replacement perhaps true in the Cumulative hierarchy?

Could one perhaps persuade oneself that replacement is true in the cumulative hierarchy? Attempts have been made, but all those known to me look very *post hoc*. I suspect that all these attempts are in fact exercises in bad faith, 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.

There is a literature on this, but I find it very difficult to give it a sympathetic presentation. 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. This is more-or-less the argument on p 239 of Shoenfield [45]. This sounds to me like replacement under another name . . . a bit circular. Readers more patient than me might wish to consult that reference and also Shoenfield [44] on page 324 of the Barwise Handbook [2].

[4] "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."

Randall Holmes [25] has considered this question, and argues that the full axiom scheme of replacement does not arise from the cumulative hierarchy conception, but only the instances that are Σ^2.

But in any case there is not very much at stake in this section, since—as we shall see—the IBE case for adopting the axiom scheme of replacement is so strong that no other is needed.

6.3 Reasons for adopting the Axiom Scheme of Replacement

We saw in section 3.1 how it can be legitimate to argue for the adoption of an axiom by pointing to desirable consequences of it not easily inferrable by other means. This is what we will do for replacement in the next few sections.

6.3.1 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 rational 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. (iii) is standard (see section 8.1). (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.[5]

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. The clinching argument against this view is surely the following point. 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 is time-invariant, so objections on the basis that something is not part of ordinary mathematics are simply not mathematically substantial.

In any case, this is not where the real debate should lie. A lot of attention has been paid to, and ink spilled over, the question of whether or not these strong set-existence consequences of the axiom scheme of replacement are points in its favour, and much less has been made of the other reasons for adopting replacement. This is partly because the other compelling reasons for adopting replacement are quite logical and technical in flavour and quite recondite—in sharp contrast to points like the deducibility of Borel determinacy from replacement, which are points of striking simplicity whose significance can be understood even by people who cannot follow the proofs of the theorems. Appreciation of the significance of implementation-sensitivity arguments, the normal form arguments, the omnibus inductively-defined-set argument and so on (to be seen later in this chapter) require much more logical sophistication, and those considerations are therefore less likely to be useful in a debate where some of the participants lack that sophistication and are therefore unlikely to be impressed by them. The kind of weapons brought to bear in a public debate inevitably depend on the nature of the public. To the extent that the opponents of replacement are people who are nei-

[5] A good place to start looking is the foundations of mathematics mailist list run by Martin Davis

ther interested in logic nor sensitive to its claims the logical arguments available to the proponents will not be useful and will not be brought to bear. Despite that, I shall spell them out in the rest of this chapter. After all, if you have as many pearls at your disposal as we have, you should not be grudging when it comes to casting them before swine.

6.3.2 Gödel's Argument

In the naïve, hand-wavy picture of the genesis of the cumulative hierarchy as in chapter 2 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 4.1.0.1—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 $V_{\omega+\omega}$ (foundation 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 correspond to them. 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 [59].

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.

6.3.2.1 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 23.

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 6.3.6.2) to prove it, and that will need the axiom scheme of replacement.

6.3.3 The Argument from the Normal Form Theorem for Restricted Quantifiers

Another reason for adopting replacement 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 [17] 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 '$(\forall x)(x \in y \longrightarrow \ldots)$' 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[6]: 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 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 '$(\forall x)(R(x,y) \longrightarrow \ldots)$', and R is the engendering relation. We find that Δ_0 formulæ behave in many ways as if 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).

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.

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

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^T_n or Σ^T_n.

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

6.3.4 The Argument from Implementation-invariance

Very little of what passes for set theory is in fact pure set theory. Most of it involves defined terms that implement ideas from elsewhere in mathematics, such as *integer, real, ordered pair* and so on. This is not an unwelcome complication but a welcome one: it is a consequence of the fact that set theory is a sort of protean universal language for mathematics, and this is delight not a pain. The implementations of these mathematical (extra-set-theoretical) ideas have bedded down over time and people have got used to them and become set in their ways. You will even hear people loosely saying that $\langle x, y\rangle$ (the ordered pair of x and y) *is* $\{\{x\},\{x,y\}\}$ when of course it is nothing of the sort. Ordered pairs can be implemented in lots of different ways and although in practice it generally doesn't matter there is a point of principle: it ought not to make much difference how we implement things like numbers and ordered pairs in our set theory. Interestingly it turns out that if we wish to be loftily indifferent about how we choose implementations then we have to adopt the axiom scheme of replacement. (See Forster [18]). Let us look at a couple of cases.

6.3.4.1 Existence of Cartesian Products

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

$$\texttt{pair}(x,y) = \texttt{pair}(x',y') \longrightarrow x = x' \land y = y',$$
$$\texttt{fst}(\texttt{pair}(x,y)) = x \text{ and}$$
$$\texttt{snd}(\texttt{pair}(x,y)) = y.$$

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[7] 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, as Adrian Mathias has pointed out to me, 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.

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 operation can be defined using only separation and no replacement.

6.3.4.2 Cardinal Numbers

There is a widespread and fairly uncontroversial understanding of cardinal arithmetic as an abstraction from set theory: facts about cardinals and operations on cardinals are just facts (or *can be interpreted* as facts) about sets and operations on sets. Thus, to see that cardinal multiplication is commutative—that $a \cdot b = b \cdot a$—it is sufficient to observe that $A \times B$ is naturally the same size as $B \times A$. One does not have to implement cardinal numbers as sets to see this.

However not all of cardinal arithmetic can be interpreted back into set theory in this implementation-free way. Consider Euler's totient function $\phi(n)$[8]. $\phi(n)$ is the cardinal of a set of numbers, so quite which actual set $\phi(n)$ will be the cardinal of will depend on how cardinals are implemented. However it is not hard to see that all the sets of which it might be the cardinal are the same size, so it doesn't make any difference what the implementation is. As the following diagram illustrates, two implementations I_1 and I_2 of cardinals will generate a bijection π between two subclasses of the universe (the ranges of I_1 and I_2), and by replacement the restriction of π to any sets will be a set, so that π will be a bijection between the two candidates for the job of being the set of units mod n. Thus it comes about that facts about Euler's totient function turn out to be implementation-insensitive too.

[7] f"x is $\{f(y) : y \in x\}$.

[8] which is of course $|\{m : (m < n) \wedge (m \text{ and } n \text{ are coprime})\}|$

$$\begin{tikzcd} V \arrow[r, "I_1"] \arrow[dr, "I_2"'] & V \arrow[d, "\pi"] \\ & V \end{tikzcd}$$

Sadly there are assertions about cardinals which are not implementation-insensitive in this way: an example is '$3 \in 5$'.

To make sense of '$3 \in 5$' not only do we have to implement numbers as sets (5 clearly has to be a set if there is to be any hope of 3 being a member of it) but the truth value it finally receives will depend with exquisite sensitivity on the implementation of naturals that we use. '$3 \in 5$' is true for von Neumann naturals but not for Zermelo naturals or Scott's trick naturals.[9] Although assertions like '$3 \in 5$' can be plausibly argued not to belong to cardinal arithmetic, we do have to come to some sort of decision about what to do about them: even is it is to ignore them. And the decision must be reasoned and defensible.

Thus it seems that there are three kinds of assertions about cardinals.

(i) those which do not require an implementation;
(ii) those which require an implementation but which are not implementation-sensitive; and
(iii) those which are implementation-sensitive.

It turns out that the difference between (iii) on the one hand and (i) and (ii) on the other can be captured by a typing discipline, which we will now explain. There are two types: `set` and `cardinal`, and every variable must have precisely one of them. We also have a cardinal-of function, written with two vertical lines. The typing rules are

(i) In "$y = |x|$", 'y' is of type `cardinal` and 'x' is of type `set`;
(ii) In "$x \in y$", 'y' is of type `set`.

The formulæ that are well-typed according to this scheme turn out to be precisely those whose truth-values are insensitive to choice of implementation. The proof that well-typed formulæ are invariant uses the axiom scheme of replacement. Let us sketch an instance of this, using Euler's totient function as above. As we observed there, quite which

[9] Purists about the strong typing of mathematics might feel that "$3 \in 5$" lacks a truth-value and that therefore a proper implementation of cardinal arithmetic should capture this fact. They will not find this illustration as helpful as the argument of the previous section.

set $\phi(n)$ turns out to be the cardinal of will depend on what our implementation of cardinal is, but all the candidate sets should at least be the same size. That is to say, given any two candidates, we want the graph of the obvious bijection between them ("send cardinal-first-style of x to cardinal-second-style of x") to be a set, and the obvious way to do it is to use on the set of cardinals-first-style the function that sends cardinal-first-style of x to the ordered pair $\langle$ cardinal-first-style(x), cardinal-second-style(x)$\rangle$; the set of such ordered pairs is the desired graph of the obvious bijection.

As far as I know no-one has bothered to prove the converse (which would complete the analogy with the result of Mathias of the previous section).

Notice that nothing has been said here about the availability of implementations. It is actually a non-trivial task to implement arbitrary mathematical objects in set theory. We saw this in section 5.5.

The considerations about implementations raised in this section make a very powerful argument for Replacement. It is, as we all know, deeply unimportant that we should opt for one ordered pair function rather than another, or for one implementation of cardinal rather than another. However, the fact that these choices are unimportant it itself an important mathematical fact. If we do not adopt replacement as an axiom scheme then this important mathematical fact goes uncaptured in our set theory. Given the serious foundational aspirations of Set Theory this would be a glaring shortcoming.

6.3.5 Existence of Inductively defined sets

As some writers in a modern computer science tradition have emphasised—see e.g. [51]—part of the significance of the axiom scheme of replacement is that it is a kind of omnibus existence theorem for recursive datatypes. Here is an illustration. 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

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

(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. What about closure under infinitary operations? This depends sensitively on the nature of the operation. If the operation is frankly second order we should expect paradox. The collection WF of all wellfounded sets is paradoxical, as is the collection of hereditarily transitive sets (it's the proper class of all von Neumann ordinals). In general, whenever f is monotone and injective then the $\subseteq$-least set X such that $\mathcal{P}(f(X)) \subseteq X$ is a member of itself if and only if it isn't. Try it!

Collections inductively defined by closure under operations of character that is at least *bounded* even though not actually finite can typically be proved to be sets by means of replacement. For example the collection of sets hereditarily of size less than κ can be obtained by iterating as follows:

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

Establishing that eventually we reach a fixed point requires more work than would be proper here; for us the point is merely that we can construct the sequence of "approximations from below" by means of replacement applied to the ordinals.

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 78.

The existence of inductively defined sets is important in lots of different ways, as we will see in the remaining sections of this chapter.

6.3.5.1 Reflection

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 [51].

6.3.5.2 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[10]. 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 *gedanken-experiment* of you and me 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 two constructions we build an isomorphism between them.[11]

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

[11] Second-order arithmetic has an obvious model, the *standard* model that axiomatic arithmetic was intended, all along, to describe. Let us call this model $\mathcal{M}$. Second-order arithmetic includes as one of its axioms the following:

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

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

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 [59].) So whenever $\mathcal{M}$ and $\mathcal{M}'$ 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.

6.3.6 Existence of Transitive Closures

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

Inconveniently, the expression 'transitive closure' has two meanings, and we must not confuse them. (See the glossary p. 90). 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 this catchy menomonic (and pædagogically useful) character of this terminology it has almost completely passed out of use and everybody nowadays writes of *transitive closures* instead.

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

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 $\mathcal{M}$". Therefore $\mathcal{M}$ is the only model of second-order arithmetic.

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 [17] 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. 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.

6.3.6.1 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 27. 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 real 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 Neuman implementation. What (iii) is trying to tell us is that there is a set that contains all Von Neumann naturals. The set of Von Neumann naturals itself is the $\subseteq$-least set witnessing (iii).

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. So (ii) $\longrightarrow$ (iii). Evidently (iii) $\longrightarrow$ (i) since the Von Neumann $\mathbb{N}$ is manifestly Dedekind-infinite. It's the other direction ((i) $\longrightarrow$ (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. 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 reminiscent of the way we have just obtained $TC(x)$. 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 [31]. (Also Boffa [4]; and [16] p 178; and [53] p. 296.)

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

6.3.6.2 Mostowski

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 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. This is one of the reasons why Zermelo set theory is unsatisfactory. A much more widely-used system—by those who 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.

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.

7

The Axiom of Choice

Without any doubt the most problematic axiom of set theory is the axiom of choice. It has generated more bad arguments (both for it and against it) and more ill-tempered discussions than all the other axioms put together. There is no space here to even pretend to do justice to those discussions: entire books have been devoted to them. See Moore [34]. There is room here for no more than a brief outline of the issues and some pointers into the literature.

There are many, many versions of the axiom of choice, and they are spread across many branches of mathematics. So many, in fact, that Rubin and Rubin [39] could fill a whole book listing equivalent forms and detailing proofs of equivalence between many of them. The fact that the axiom of choice has all these different manifestations all over the place shows that it is a deep mathematical principle, and that fact is by itself enough to force it upon our attention. It doesn't mean that it is *true*, of course (since the negation of the axiom of choice has exactly as many versions and in the same places!) but it does mean that it is highly significant, and is something that we have to take seriously one way or another.

We shall start with a form of it that is particularly simple, to make it easier for the reader to see what is being claimed, and perhaps see whether or not they want to believe it.

The version we consider is the axiom that Russell [40] called the *multiplicative axiom.*[1]

[1] Russell wanted to define the product $\alpha \cdot \beta$ of two cardinals as the size of a set that was the union of α sets each of size β. (For some reason he didn't define it as the size of the cartesian product of a set of size α with a set of size β.) However, as the socks example in section 7.2.1 makes clear, we need a certain amount of AC to ensure that all sets that are unions of α sets each of size β are the same size. Interested readers can consult [18] or [40]).

One pleasing feature of this version of the axiom is that is purely set-theoretical and doesn't need any extra notation.

Let X be a nonempty family of disjoint nonempty sets, so that $(\forall y, z \in X)(y \cap z = \emptyset)$. Then there is a set Y such that, for all $y \in X$, $Y \cap y$ is a singleton. (M)

Y is said to be a *transversal* set.

I can remember thinking—when I first encountered this axiom—that this must be a consequence of the axiom scheme of separation that says that any subcollection of a set is a set. The Y that we are after (once we are given X) is obviously a subcollection of $\bigcup X$, and *that's* a set all right. This is true, but it doesn't help, since there is no obvious way of finding a property ϕ so that Y is $\{w \in \bigcup X : \phi(w)\}$. Contrast with the existence of a bijection between $A \times B$ and $B \times A$: we can specify such a bijection *without knowing anything about A and B*—just flip the ordered pairs round. To find such a Y, given X, it seems that we need to be given a lot of information about X. For an arbitrary X we do not have that kind of information; accordingly we cannot prove (M) above for arbitrary[2] X; this leads us to the conclusion that if we want to incorporate M and its logical consequences in our theory then we will have to adopt it as an axiom.

The problem seems to be that in order to obtain Y we have to select an element from every member of X and we need information about X (and its members) to guide us in making our choice. At this point I shall revert to a more usual version of the axiom:

if X is a set of nonempty sets then there is a function $f : X \longrightarrow \bigcup X$ such that $(\forall x \in X)(f(x) \in x)$. (AC)

f is said to be a **choice function** for X. It's not hard to see that (AC) is equivalent to (M).

M acquires a certain plausibility if one considers cases where X has, say, two members: x_1 and x_2. For in that case we just take any ordered pair $\langle u, v\rangle$ from $x_1 \times x_2$ and form the pair $\{u, v\}$, and this pair is obviously a transversal. A slightly more complicated argument will work if X has three, or four members. Indeed, we can prove by induction on n that M—and AC—hold for all finite X. The proof is simple—*very* simple in fact—and provides a useful test-bed for people's intuitions about choice.

[2] The sudden appearance of the word 'arbitrary' at this juncture is an indication that the stage at which we need to make the axiom of choice explicit at precisely the stage where we acquire the concept of an arbitrary set... in-extension!

THEOREM 4. *For every $n \in \mathbb{N}$ if X is a set of nonempty sets with $|X| = n$ then X has a choice function.*

Proof:

The base case is $n = 0$. The empty function is a choice function for any empty set of nonempty sets.

Now for the induction step. Suppose X has a choice function. How do we find a choice function for $X \cup \{x\}$? If $x \in X$ then any choice function for X will do. The other case is when x is nonempty and is not already a member of X. If x is nonempty then it has a member, y say. But then if f is a choice function for X then $f \cup \{\langle x, y\rangle\}$ is a choice function for $X \cup \{x\}$. ■

There are several subtleties in this proof.

(i) The first point to note is that although we have assumed X to be a finite set we have made no assumptions about the cardinalities of the *members* of X. Every finite set has a choice function: X can be a finite set of sets-as-big-as-you-please.

(ii) Secondly, classroom experience has taught me that many people think we have used the axiom of choice in this proof, by picking y from x. But we haven't. All we have said is that, **if** f is a choice function for X and y is a member of x, **then** $f \cup \{\langle y, x\rangle\}$ is a choice function for $X \cup \{x\}$; and that much is true. We are appealing to nothing more than the validity of the inference

$$\frac{(\forall x)(F(x) \longrightarrow p) \quad (\exists x)(F(x)}{p}$$

in the case where $F(x)$ says that x is the ordered pair $\langle f, y\rangle$ where f is a choice function for X and y is a member of x, and p says that there is a choice function for $X \cup \{x\}$. We haven't claimed that there is a uniform construction of choice functions for all finite sets; we have claimed merely that each finite set has *a* choice function. A finite set will typically have lots,[3] and without knowing much more about the internal structure of the set there is no reliable uniform way of identifying one in advance. Indeed it is important that we can perform the induction *without* having a uniform construction of choice functions for all finite sets: if there were somehow a way of canonically extending choice functions for finite sets then we would be able to prove the axiom of choice for countable sets.

[3] The number of choice functions X has is simply the product of the cardinalities of the members of X.

For let $\{X_i : i \in \mathbb{N}\}$ be a countable family of nonempty sets. If there were a canonical way of assigning uniformly, for each $i \in \mathbb{N}$, a choice function f_i that picks a member from all X_j with $j < i$ and f_i extends f_j with $j < i$ then the function $\bigcup\{f_i : i \in \mathbb{N}\}$ would be a choice function for the family $\{X_i : i \in \mathbb{N}\}$. But this last assertion is the countable axiom of choice, and that axiom is known to be independent of the other axioms. (See p. 60.)

(iii) Thirdly, a point to ponder. This fact revealed by this proof is often expressed by some formulation like "We can always make finitely many choices: to make infinitely many choices we need AC". How do you count choices? Well, you have to count them in such a way that in the proof by induction for theorem 4 that we have just sat through there are only finitely many choices. Presumably exactly one. And let us remind ourselves again that what matters is the number of times one makes a choice, not (this is point (i) again) the number of elements in the set from which one is choosing. Even choosing from a pair can be hard, as the example of the socks (section 7.2.1) shows. Buridan's Ass points the same moral. *How happy could I be with either, were t'other dear charmer away.*

There are various weak versions of the axiom of choice that the reader will probably need to know about. **The axiom of countable choice** ("AC_ω") says that every countable set of (nonempty) sets has a choice function. **The axiom of dependent choices** (DC) says that for any set X with a binary relation R satisfying $(\forall x \in X)(\exists y \in X)(R(x, y))$ there is a sequence $\langle x_1, x_2 \ldots n_n \ldots\rangle$ where, for all i, $R(x_i, x_{i+1})$.

Both these versions are strictly weaker than full AC. DC seems to encapsulate as much of the axiom of choice as we need if we are to do Real Analysis—well, the minimal amount needed to do it sensibly. DC does not imply the various headline-grabbing pathologies like Vitali's construction of a nonmeasurable set of reals nor the Banach-Tasrki paradoxical decomposition of the sphere. There are also other weakened versions of AC, but these two are the only weak versions that get frequently adopted as axioms in their own right.

In this connection one might mention that people have advocated adopting as an axiom the *negation* of Vitali's result, so that we assume that every set of reals is measurable. Since this is consistent with DC we can adopt DC as well, and continue to do much of Real Analysis as before, but without some of the pathologies. Indeed one might even consider adopting as axioms broader principles that imply the negation of Vitali's result—such is the Axiom of Determinacy, However that is a topic far too advanced for an introductory text like this.

In the rest of this chapter we will consider briefly the various positions people have adopted in relation to the Axiom of Choice and how one might come to hold those positions. First we consider the prospects for an IBE argument that adopting the axiom of choice might be a sensible unifying thing to do.

7.1 IBE and some counterexamples

Can we argue for AC by IBE? There is a *prima facie* problem in that there are some consequences of AC that people have objected to at one time or another. We have already mentioned Vitali's theorem that there is a non-measurable set of reals, and the more recent and striking Banach-Tarski paradox[4] on the decompositions of spheres. Nor should we forget that when Zermelo [58] in 1904 derived the wellordering theorem from AC the reaction was not entirely favourable: the wellordering of the reals was then felt, initially, to be as pathological as Banach-Tarski was later.

However, one can tell a consistent and unified story about why these aren't really problems for AC. There is, granted, a concept of set which finds these results unwelcome, but that concept is not the one that modern axiomatic set theory is trying to capture. The view of set theory that objects to the three results mentioned in the last paragraph is one that does not regard sets as fully extensional and arbitrary. How might it come about that one does not like the idea of a non-measurable set of reals, or a Banach-Tarski–style decomposition of the sphere, or a wellordering of the reals? What is it that is unsatisfactory about the set whose existence is being alleged in a case such as this?[5] It's fairly clear that the problem is that the alleged sets are not in any obvious sense definable.[6] If you think that a set is not a mere naked extensional object but an extensional-object-with-a-description then you will find some of the consequences of AC distasteful. But this means that in terms of the historical process described in section 3.3 you are trapped at stage (2).

[4] Q: What is a good anagram of 'Banach-Tarski'?
A: 'Banach-Tarski Banach-Tarski'.

[5] The (graph of the) wellordering of the reals and the (collection of pieces in the) decomposition of the sphere are of course sets too.

[6] There is a very good reason for this, namely that there is no definable relation on $\mathbb{R}$ which provably wellorders $\mathbb{R}$. This theorem wasn't known in 1904 but people in 1904 could still realise that they didn't know of any wellorderings of $\mathbb{R}$.

Once you have achieved the enlightenment of stage (3) these concerns evaporate. Nowadays mathematicians are happy about *arbitrary* sets in the same way that they are happy about *arbitrary* reals.

7.1.1 Constructive Mathematicians do not like AC

There are communities that do not accept the axiom of choice, or who at the very least regard some of the alternatives to it as not completely beyond the pale, and the reasons that have been found are diverse.

One such community is the community of constructive mathematics. If one gets properly inside the constructive world view one can see that it requires us to repudiate the axiom of choice. However, getting properly inside the constructive world-view is not an undertaking for fainthearts, nor by any to be taken in hand lightly or unadvisedly, and it is not given to us all to succeed. Fortunately for unbelievers there is a short-cut: it is possible to understand why constructivists do not like the law of excluded middle or the axiom of choice, and to understand this without taking the whole ideology of constructive mathematics on board. It comes in two steps.

7.1.1.1 First we deny excluded middle

First we illustrate why constructivists repudiate the law of excluded middle. Some readers may already know the standard horror story about $\sqrt{2}^{\sqrt{2}}$. For those of you that don't—yet—here it is.

Suppose you are given the challenge of finding two irrational number α and β auch that α^β is rational. It is in fact the case that both e and $log_e(2)$ are transcendental but this is not easy to prove. Is there an easier way in? Well, one thing every schoolchild knows is that $\sqrt{2}$ is irrational, so how about taking both α and β to be $\sqrt{2}$? This will work if $\sqrt{2}^{\sqrt{2}}$ is rational. Is it? As it happens, it isn't (but that, too, is hard to prove). If it isn't, then we take α to be $\sqrt{2}^{\sqrt{2}}$ (which we now believe to be irrational—had it been rational we would have taken the first horn) and take β to be $\sqrt{2}$.

α^β is now

$$(\sqrt{2}^{\sqrt{2}})^{\sqrt{2}} = \sqrt{2}^{\sqrt{2}\cdot\sqrt{2}} = \sqrt{2}^{2} = 2$$

which is rational, as desired. However, we haven't met the challenge. We were asked to *find* a pair $\langle\alpha, \beta\rangle$ of irrationals such that α^β is rational, and we haven't found such a pair. We've proved that there *is* such a pair, and we have even narrowed the candidates down to a short list of two, but we haven't completed the job.[7]

What does this prove? It certainly doesn't straightforwardly show that the law of excluded middle is *false*; it does show that there are situations where you don't want to reason with it. There is a difference between proving that there is a widget, and actually getting your hands on the widget. Sometimes it matters, and if you happen to be in the kind of pickle where it matters, then you want to be careful about reasoning with excluded middle.

7.1.1.2 The Axiom of Choice implies Excluded Middle

In proving this we must play fair: the classical concept of *nonempty set* multifurcates into lots of constructively distinct properties. Constructively x is **nonempty** if $\neg(\forall y)(y \notin x)$; x is **inhabited** if $(\exists y)(y \in x)$, and these two properties are distinct constructively: the implication $(\neg\forall\phi \longrightarrow \exists\neg\phi)$ is not good in general.

Clearly if every family of nonempty sets is to have a choice function then if x is nonempty we can find something in it, This would imply that every nonempty set is inhabited. We shall not resort to such smuggling. If we are to eschew smuggling we will have to adopt AC in the form that every set of *inhabited* sets has a choice function.

Let us assume AC in this form, and deduce excluded middle. Let p be an arbitrary expression; we will deduce $p \vee \neg p$. Consider the set $\{0, 1\}$, and the equivalence relation $\sim$ defined by $x \sim y$ iff p. Next consider the quotient $\{0, 1\}/\sim$. (The suspicious might wish to be told that this set is $\{x : (\exists y)((y = 0 \vee y = 1) \wedge (\forall z)(z \in x \longleftrightarrow z \sim y))\}$). This is an inhabited set of inhabited sets. Its members are the equivalence classes $[0]$ and $[1]$—which admittedly may or may not be the same thing—but they are at any rate inhabited. Since the quotient is an inhabited set of inhabited sets, it has a selection function f. We know that $[0] \subseteq \{0, 1\}$ so certainly $(\forall x)(x \in [0] \longrightarrow x = 0 \vee x = 1)$. Analogously we know that $[1] \subseteq \{0, 1\}$ so certainly $(\forall x)(x \in [1] \longrightarrow x = 0 \vee x = 1)$. So certainly $f([0]) = 0 \vee f([0]) = 1$ and $f([1]) = 0 \vee f([1]) = 1$. This gives us four possible combinations. $f([0]) = 1$ and $f([1]) = 0$ both imply $1 \sim 0$ and therefore p. That takes care of three possibilities; the remaining

[7] We can actually exhibit such a pair, and using only elementary methods, at the cost of a little bit more work. $log_2(3)$ is obviously irrational: $2^p \neq 3^q$ for any naturals p, q. $log_{\sqrt{2}}(3)$ is also irrational, being $2 \cdot log_2(3)$. Clearly $(\sqrt{2})^{log_{\sqrt{2}}(3)} = 3$.

possibility is $f([0]) = 0 \wedge f([1]) = 1$. Since f is a function this tells us that $[0] \neq [1]$ so in this case $\neg p$. So we conclude $p \vee \neg p$.[8]

There is a moral to be drawn from this: whether or not you want to include AC (or excluded middle) among your axioms depends at least in part on the use you are planning to put those axioms to. (This is of course a completely separate question from the question of whether or not AC (or excluded middle) is *true*).

Uplifting though this moral is, it is beside the point that I was making. The fact that AC implies excluded middle and that there are principled reasons sometimes to eschew excluded middle means that there are principled reasons for (sometimes) wishing to eschew the axiom of choice.

7.2 IBE and a Fallacy of Equivocation

If you are going to get involved in set theory you will find yourself working with people who think the axiom of choice is true, *simpliciter*. The purpose of this next section is not to argue that the axiom of choice is false—whatever that means—but to make the point that at least some of the reasons for its widespread public acceptance are bad.

It is common practice in the teaching of mathematics at university level to gloss over applications of the axiom of choice, and proclaim such standard propositions as—for example—"A countable union of countable sets is countable" with some sketchy argument which does not render explicit the use of the axiom, and indeed might not even mention it by name at all. The students in consequence do not form a mental image of the axiom, and tend subsequently not to recognise when it is being used. Not surprisingly, they end up defending the axiom by IBE: since by then they believe all its consequences, they see in it a single reason for believing them. Typically when confronted with it later on in their education they either deny that it is being used at all, or—if they acknowledge that it is being used—will go on to say there is no problem, since the axiom is obviously true.

Let us consider some of these apparently obvious truths.

7.2.1 Socks

In [40] (p 126) we find the *sutra* of the millionaire whose wardrobe contains a countable infinity of pairs of shoes and a countable infinity of

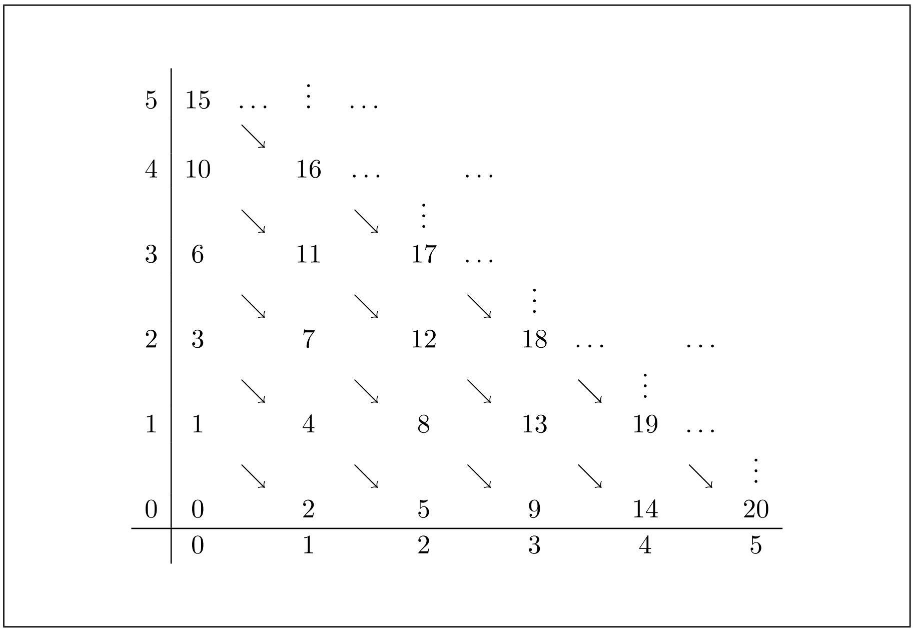
[8] Thanks to Douglas Bridges for the right steer on this exercise! The theorem is due to Diaconescu [15].

pairs of socks. It is usually felt to be obvious that there are countably many shoes and countably many socks in his attic. Most people will claim that it is obvious that there is a bijection between the shoes and the natural numbers, and obvious too that there is a bijection between the socks and the natural numbers. Obviousness is all very well but it does not reliably signpost a path to understanding. Fortunately with a bit of prodding most students can be persuaded to say that the left shoe from the nth pair can be sent to $2n$ and the right shoe from the nth pair can be sent to $2n+1$. This indeed shows that there are countably many shoes. "And the socks?" one then asks. With any luck the student will reply that the same technique will work, at which point the victim can be ribbed for being a sad mathmo with odd socks. Old jokes are the best. In fact this joke is so good that it even survives being explained.

If we have succeeded in finding a bijection between the set of socks and the natural numbers then we have given each sock a number. This means, at the very least, that we now have a uniform way of choosing one sock from each pair, namely the one with smaller number. Conversely, if we have a uniform way of choosing one sock from each pair, then we can send the chosen sock from the nth pair to $2n$, and the rejected sock to $2n+1$. These two observations show that the set of socks is countable iff there is a choice function on the pairs of socks. Analogously the set of shoes is countable iff there is a choice function on the pairs of shoes. Clearly there is a way of choosing one shoe from each pair, because we can uniformly distinguish left shoes from right. But socks? Clearly there is no *uniform* way of telling socks apart. So we need the axiom of choice to tell us that the set of socks is countable nevertheless.

7.2.2 A union of countably many countable sets is countable

Let $\{X_i : i \in \mathbb{N}\}$ be a family of countable sets. Is its sumset, $\bigcup\{X_i : i \in \mathbb{N}\}$, countable? The usual answer is: yes. Draw the X_i out in a doubly infinite array, and then count them by zigzagging. What could be more obvious? The picture below illustrates how we do it. Let $x_{i,j}$ be the jth member of X_i. Put the members of X_i in order in row i, so that $x_{i,j}$ is the jth thing in the ith row. Then we can count them by zigzagging.



A Counted Union of Counted Sets is Counted

What could be simpler?! If—as they tell us—we need the axiom of choice to prove this, then we'd better assume it.

7.2.3 Every perfect binary tree has an infinite path

A perfect binary tree is a tree with one root, wherein every node has precisely two children. If one has a blackboard to hand when telling this story, one is tempted to start off drawing a perfect binary tree:

```
      *
     / \
    /   \
   *     *
  / \   / \
 *   * *   *
/
```

... which makes it obvious that this perfect binary tree at least (the one you have started drawing) has an infinite path. Always choose the leftmost branch. What could be easier!? Again we are told that this needs the axiom of choice. Very well, let us therefore adopt the axiom of choice.

7.3 The Fallacy of Equivocation

However, in all three cases we have a fallacy of equivocation. Let us take the binary tree case first, since it is fresh in our minds.

7.3.1 Perfect binary trees

The fact that is obvious is not the fact that

A perfect binary tree has an infinite path; (i)

but the fact that

A perfect binary tree *with an injection into the plane* has an infinite path; (ii)

since we cannot follow the rule "take the leftmost child in each case" unless we can tell what the leftmost child is, and this information is provided for us not by the tree itself but by its injection into the plane. It is true that to make sense of "leftmost" we have to pick an injection into the plane but this is a single choice not infinitely many. (see (iii) p. 60.)

Are not (i) and (ii) the same? They certainly will be if any two perfect binary trees are isomorphic. And aren't any two perfect binary trees isomorphic? Isn't that obvious?

No, it isn't: what is obvious is *not*

Any two perfect binary trees are isomorphic; (iii)

but

Any two perfect binary trees *equipped with injections into the plane* are isomorphic; (iv)

and (iii) and (iv) are not the same. We are back where we were before. We should not fall into a *petitio principii*.

7.3.2 The Countable Union of Countable Sets

The most illuminating discussion of this known to me is one I learned from John Conway (oral tradition). Conway distinguishes between a **counted set**, which is a structure $\langle X, f \rangle$ consisting of a set X with a bijection f onto $\mathbb{N}$, and a **countable set**, which is a naked set that just happens to be the same size as $\mathbb{N}$, but which does not come equipped with any particular bijection. As Conway says, elliptically but memorably:

A counted union of counted sets is counted; (v)

A countable union of counted sets is countable; (vi)

but a counted union of countable sets, and *a fortiori* a countable union of countable sets could–on the face of it–be anything under the sun.

In other words, that which is obvious is not

A union of countably many counted sets is countable; (vii)

(v) is of course a shorthand for the claim that a counted set of counted sets has a union with an obvious counting: indeed a counting that can be recovered—in the way displayed by the diagram on page 66—by combining the counting of the set and the countings of its members. Although (v) and (vi) are provable without choice—and the diagram gives a visual proof of (vi)—(vii) is not. The fallacy of equivocation is to mistake (vii) for one of (v) and (vi).

7.3.3 Socks

There are at least two lines of thought that might lead one to think that there are countably many socks.

7.3.3.1 All countable sets of pairs look the same

"Surely to goodness", one might think, "All sets that are unions of countably many pairs must be the same size! Just replace the members of the nth pair of socks by the members of the nth pair of shoes, successively . . . ". In particular there must be the same number of socks in the attic as there are shoes. We know we can count the shoes (left shoes go to evens, right shoes go to odds) so we must be able to count the socks too.

But why should we believe that all sets that are the union of countably many pairs are the same size?

If I want to show that the union of countably many pairs of blue socks is the same size as the union of countably many pairs of pink socks I want to pair off the blue socks in the nth pair of blue socks with the pink socks in the nth pair of pink socks, and—sadly—I can do this in two ways at each stage. Therefore we will need AC-for-pairs to pick one of the two ways, and we have made no progress.

This does make one useful point. In order to know whether a countable set of pairs has a countable sumset or not one needs to know what the pairs are pairs *of*: one needs to be able to look inside the members of the pairs. This does seem odd. Adopting the axiom of choice banishes this oddity.

7.3.3.2 Intuitions of space

There is another line of thought that leads us to believe that the set of socks is countable. The very physical nature of the setting of the parable has smuggled in a lot of useful information. It cues us to set up mental pictures of infinitely many shoes (and socks) scattered through space. The shoes and socks are—all of them—extended regions of space and so they all have nonempty interior. Every nonempty open set contains a rational, and the rationals are wellordered. This degree of asymmetry is enough to enable us to choose one sock from each pair, as follows. In any pair of socks, the two socks have disjoint interiors[9] and both those interiors contain rational numbers. Consider, for each sock, that rational number in its interior which is the first in some standard wellorder of the rationals. This will distinguish between the socks. The physical intuitions underlying this last argument make it very clear to us that we can pick one sock from each pair—as indeed we can. Space is just sufficiently asymmetrical for us to be able to explicitly enumerate the socks in countably many pairs scattered through it.

So we have another example of a fallacy of equivocation, this time between:

Every countable set of pairs has a choice function (viii)

and

Every countable set of pairs of open subsets of E^3
has a choice function (ix)

[9] All right! Your pair of socks might be folded into each other the way your mother used to do it, so their interiors are not disjoint. However even in these circumstances their interiors S_1 and S_2 are at least *distinct*, .The least rational in the symmetric difference $S_1 \Delta S_2$ will belong to one of the two sock, and pick that sock!

As before, it is the first member of the pair that needs the axiom of choice, but it is the second member of the pair that is obvious.

From a pedagogical point of view it may be worth making the following observation, even though it may initially seem to have no direct bearing on the above argument. When we reflect how straightforward is the construction that matches left shoes with evens and right shoes with odds, one is struck by how difficult it is to induce students to come out with it—or with anything like it. This suggests that the reason why students think the set of shoes is countable is because they think they can count it directly, in exactly the way they think they can count the socks directly—namely by illegitimately exploiting the physical intuitions cued by the background information in the parable.

A kind of *supertask* in fact.

Perhaps this does have direct bearing on the argument of this section after all, for—by drawing attention to the importance of physical intuitions in lending plausibility to mathematical claims—it underlines how easy it is to commit the fallacy of equivocation that got us into this mess.

7.4 Isn't it simplest just to believe it?

There are people who do not have a philosophical position on the nature of sets and mathematical entities but who just want to get on with their mathematics. They need a reason to jump one way or the other on the question of the axiom of choice. One suggestion that might carry some weight with such people is that the axiom of choice is a good thing because it *keeps things simple*. If AC fails there are these annoying objects around: infinite sets without countable subsets, countable sets of pairs of socks without a counting of the socks, and so on. Who needs them? Aren't they just a pain?? Why not adopt the axiom of choice and be shot of them all?

Widespread though this view is, and appealing though it undoubtedly is, it really is entirely without merit. The choiceless family of pairs of socks is a pain, no doubt, and it seems we would be better off without it. But then the paradoxical decomposition of the sphere is a pain too, and you get that if you adopt AC. Not only is it a pain, but it is a pain of a very similar stamp: the pathological sock collection and the paradoxical decomposition of the sphere alike have the twin features of not only being initially counterintuitive but also—even on inspection—lacking any motivation in what one might tempt fate by calling *ordinary mathematics*. However the point is not so much the tit-for-tat point that the Axiom of choice has some pathologies that are as gross as the pathologies associated with its negation: the point is that it is a mistake to try to anticipate what mathematics will throw at us. We can't simply ignore things we don't like. Perhaps there just *are* bad families of pairs of socks, in the way that (at least according to AC) there just are paradoxical decompositions of the sphere. Granted the paradoxical decomposition of the sphere no longer looks paradoxical, but that only serves to remind us that something that looks pathological now might look a lot less pathological in fifty or a hundred years' time.

It may well be that the wisest course in relation to the axiom of choice is the same course as the $\sqrt{2}^{\sqrt{2}}$ story leads us to in relation to the law of excluded middle. Use it sometimes, but bear in mind that there may be other times when the news it brings you is useless to you.

The current situation with AC is that the contestants have agreed to differ. People who are fully signed up to the modern consensus realist view of sets as arbitrary extensional object believe—almost without exception—that the axiom of choice is true. There is a smaller party—consisting largely of constructivists of various flavours—who have a subtly different—and more intensional—concept of set and who in consequence do not accept the axiom of choice.

As well as the agreement to disagree there appears to be agreement within each camp. The emergence of the axiom of determinacy (which contradicts AC) caused a few flutters among the platonists: the axiom couldn't simply be ignored: it was far too interesting for that. And to accept it would be to reject AC. They found instead a way of domesticating it: certain large cardinal hypotheses imply that it is true in a natural substructure of the universe. That way they get the best of both worlds.

7.5 Are there Principled Reasons for Believing AC to be true?

We don't seem to be getting very far with making AC look plausible by deducing obvious truths from it. So can one argue for it directly? Are there principled reasons for believing AC to be true?

As we have just noted, it seems to be the case that most of the people who believe that the Axiom of Choice has a truth-value at all tend to believe that that truth-value is 'true'. I think this is a common-cause phenomenon: the forces that lead people to believe that the axiom of choice has a truth value tend also to make them think that that truth-value is 'true'. The forces at work here are various kinds of belief in the ultimate reality of mathematical objects, and ways of thinking about those objects. If a set is real, then you can crawl all over it and get into all its nooks and crannies. And by doing that, you perforce wellorder it. After all, if—having time on your hands as one does when one is trying to fall asleep by counting sheep—you count members of your set then you will wellorder it. You never run out of ordinals to count the sheep with (that is Hartogs' theorem) so your endeavour to wellorder the set cannot fail. And if you didn't count Tweedledum before you counted Tweedledee that can only be because you counted Tweedledee before you counted Tweedledum. supertask

On this view the axiom of choice is just plain true, and the intuitive argument for it is that one can boldly go and straightforwardly just wellorder the universe *by hand* as it were. To be more precise, the axiom of choice (on this story) follows from realism about mathematical objects. The force of this story derives from the plausibilty of the idea that we can just go on picking up one thing after another until we have picked up everything. We can do it with material objects and so—being realists about sets as we are—we expect to be able to do it to sets.

If you are platonist you believe that every set is out there, somewhere, to be pawed and pored over. If you paw it long enough you can probably wellorder it. If you examine the set of pairs of socks long enough, you will be able to pick one sock from each pair. At least that's what it looks like to most platonists. If you are a platonist you believe that it is possible (at least for a suitably superior intelligence) to know everything there is to know about a mathematical object such as a set, so you know how to wellorder any set. Why should you be able to wellorder it? Nobody seems to know. It's probably something to do with an ill-formulated intuition about the ultimately deterministic nature of mathematical entities. This intuition may have the same roots as the intuition behind what philosophers call *bivalence*, and it may be a mistake of course. There just might be mathematical objects that are of their essence sufficiently nondeterministic for us not to be able to wellorder them but we don't seem to be able to imagine any at the moment. Indeed we might not be able to *imagine* any—ever. If we could imagine them, one feels, one would be able to wellorder them. (Might this be something to do with the fact that 'imagine' seems to mean 'visualise' and once we visualise something we can wellorder it? In this connection see the discussion on the significance of our intuitions of space on page 69.) There is an echo here of the phenomenon of *self-refutation* as in "It is raining and I don't believe it"; "I can't say 'breakfast"' and perhaps Berkeley's master argument for idealism.

This way of thinking about sets is nevertheless entirely consonant with the way in which the *sutra* of the socks is recounted. To the mathematical realist it seems perfectly clear that the set of socks is countable, even at the same time as it is clear to the realist that lesser mortals might be unable to count them and might well come to believe that they form an uncountable or Dedekind-finite collection. The Bounded Being remains unconvinced that the set of socks is countable but that is only because the Bounded Being has incomplete information. Should the Bounded Being ever be given the full story about the socks (s)he will see immediately that the socks are wellordered. Sets are like that; "being wellordered is part of our conception of set"; "If you can conceive it you can wellorder it"; "if you can't wellorder it then it's a not a completed totality".

There are two things wrong with this story. The first is that the imagery of picking things out of a set *in time* is restricted to sequences of choices whose length can be embedded in whatever it is that measures time, presumably $\mathbb{R}$. We cannot embed into $\mathbb{R}$ any wellorderings of uncountable length so this story never tells us how to wellorder uncountable sets.[10]

The other problem is this. For it to be plausible that we can wellorder the universe by brute force we have to be sure that as long as we can pick α things for every $\alpha < \lambda$ then we can pick λ things. This is all right if λ is a successor ordinal: as long as there is something left after we have picked α things then we can pick an $\alpha + 1th$ thing. That's just straightforwardly true, and it's the argument we saw at the beginning of this chapter. Our realist intuitions get us this far, and this far they are correct. The problem is that this is not enough: we still have to consider the case when λ is limit, and then we need something that says that all the possible ways of picking α things for $\alpha < \lambda$ can be somehow stitched together. And for that one needs the axiom of choice. The point is that, at each successor stage the assertion "I can pick something" is just syntactic sugar for "there is still stuff left"; the difference between the two sounds substantial but it isn't. If one makes the successor step look more significant than it really is (by using syntactic sugar) one can make

[10] An embedding of an uncountable wellordering into $\mathbb{R}$ would partition $\mathbb{R}$ into uncountably many half-open intervals, each of which would have to contain a rational. There aren't enough rationals to go round.

the difference between the successor stage and the limit stage seem less significant than it really is.

We are in exactly the situation we saw earlier in this chapter (page 59). Realism doesn't get you the axiom of choice: what it gets you is the right to tell the story on page 59 in beguiling concrete terms.

The argument for the axiom of choice derives all its plausibility by artfully concealing the assumption that the infinite resembles the finite in the way required. This assumption turns out to be precisely the axiom of choice. This is not to say that the platonists are wrong when they claim that AC holds for their conception of set, merely that this story isn't an argument for it.

7.5.1 The Consistency of the Axiom of Choice?

Let us return to the idea that, if we have perfect information about sets, we can well-order them. This may be wrong-headed, but it does give rise to an idea for a consistency proof for the axiom of choice. Recall the recursive datatype WF: its sole constructor adds at each stage *arbitrary* sets of what has been constructed at earlier stages. If we modify the construction so that at each stage we add only those sets-of-what-has-been-constructed-so-far about which we have a great deal of information, then with luck we will end up with a model in which every set has a description of some sort, and in which we can distinguish socks *ad libitum*, and in which therefore the axiom of choice is true. This even gives rise to an axiom for set theory, due to Gödel and known as "$V = L$". $V = L$ is the axiom that asserts that every set is *constructible* in a sense to be made clear. No-one seriously advocates this as an axiom for set theory: none of the people who think that formulæ of set theory have truth-values believe that $V = L$ is true; it is taken rather as characterising an interesting subclass of the family of all models of set theory.

8

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 clearly 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 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* 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 5.

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

In this I am following the notation of Boffa [4].

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 wellfounded

[1] Some writers prefer to say that A is independent of T only if $T + \neg A$ and $T + A$ are both consistent.

induction. We can use this induction to argue that everything 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 '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.

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.

8.1 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 get $V_{\omega+\omega}$.

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

8.2 Power set

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

The obvious way of proving that $H_{\aleph_1}$ is a set is to use transfinite iteration of the function $x \mapsto \mathcal{P}_{\aleph_1}(x)$, taking unions at limits, so that (as on page 51) we define:

$$X_0 = \emptyset;$$

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

ω-cts??

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 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 6.3.4 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 possibility of implementing ordered pairs as Wiener-Kuratowski

8.3 Infinity

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

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

8.4 Sumset

Recall the definition of beth numbers from chapter 2. Then $H_{\beth_\omega}$ proves the independence of the axiom of sumset. A surjective image of a set of size strictly less than $\beth_\omega$ is also of size strictly less than $\beth_\omega$. This ensures that $H_{\beth_\omega}$ is a model of replacement. Next we notice that there are well-orderings of length $\omega + \omega$ inside $H_{\beth_\omega}$, and that every $V_{\omega+n}$ is in $H_{\beth_\omega}$. Therefore by replacement $\{V_\alpha : \alpha < \omega + \omega\}$ is a set. Indeed it is hereditarily of size less than $\beth_\omega$. However, its sumset $\bigcup\{V_\alpha : \alpha < \omega+\omega\}$ is $V_{\omega+\omega}$ which is of course of size $\beth_\omega$ and is not in $H_{\beth_\omega}$. I omit the details of the proof that the other axioms are satisfied.

8.5 Foundation

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

If $\langle V, R\rangle$ is a structure for the language of set theory, and 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 stratified formula then $\langle V, R\rangle \models \Phi$ iff $\langle V, R_\pi\rangle \models \Phi$. Not all the axioms are stratified, but it is quite easy to verify the unstratified instances of replacement, and the first version of the axiom of infinity on page 55 is stratified. Foundation fortunately is not stratified! The π we need is the transposition $(\emptyset, \{\emptyset\})$. In $\mathcal{M}^\pi$ 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 $\mathcal{M}^\pi$ is a model for all the axioms of ZF except foundation.

8.5.1 Antifoundation

There is another way of proving the independence of the axiom of foundation and that is to prove the consistency of an axiom of *antifoundation*. To this end let us return to the ideas of section 4.1.1. 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 8.7.

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

8.6 Extensionality

First, some slang. If T is a name for a system of axiomatic set theory (with extensionality of course), then TU is the name for the result of weakening extensionality to the assertion that *nonempty* sets with the same elements are identical. 'U' is for 'Urelemente'—German for 'atoms' (see p. 19).[2]

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 $f : V \longrightarrow V$ 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 (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 80.

[2] 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 ....

8.6.1 More about Extensionality

In the light of this result and the discussion on page 20 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 not known whether or not Quine's NF is consistent, though it is known to be consistent if extensionality is weakened to allow atoms—but only flavour 1 atoms; the consistency proof doesn't work with Quine atoms! There may be more to this atom business than meets the eye.

The relative strength of extensionality and its negation is quite sensitive to other considerations too. Does the language contain an abstraction operator? See Scott [42] where he shows that a version of ZF without extensionality can be interpreted in Zermelo set theory! See also [22].

8.7 Choice

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 ill-founded 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!!)[3]

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 6. *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

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

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. ■

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

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 6) 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!

8.8 Pairing

Pairing is not independent of the other other axioms of ZFC, since it follows from the axioms of empty set, power set and replacement. ($\mathcal{P}^2(\emptyset)$ (the power set of the power set of the empty set) has two elements, $\emptyset$

and $\{\emptyset\}$, and every other pair can be obtained as a surjective image of it.) However pairing is independent of the other axioms of Zermelo. Roughly, let $\mathfrak{M}$ and $\mathfrak{N}$ be two supertransitive models of Zermelo such that neither is a subset of the other. ($\mathfrak{M}$ is supertransitive if it is transitive and every subset of a member of $\mathfrak{M}$ is also in $\mathfrak{M}$). Then $\mathfrak{M} \cup \mathfrak{N}$ is a supertransitive model of all of Zermelo except pairing. See section 13 of Mathias [31] 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 26.

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).

9

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

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

$$(\forall X_1 \ldots X_n)(\exists Y)(\forall z)(z \in Y \longleftrightarrow \phi(z, X_1 \ldots X_n)) \qquad (9.1)$$

We rewrite all the axioms of *ZF* except replacement and separation by resticting 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 9.1 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 [43].

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 decidable set of axioms (so that one can recognise an axiom when one sees one) nothing sensible can be done anyway, and if one does have 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. (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.

In this context it might be worth noting the result of W. Craig that any theory with a semi-decidable set of axioms has a decidable set of axioms.

Morse-Kelley is actually stronger than GB, and although the details

[1] It was actually first spelled out by Wang [56], who called the system 'NQ'.

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

are hard, it is not hard to see why this might be true. 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 9.1 where all variables must range over sets only is a subscheme of 9.1. 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.

9.0.1 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.

9.0.2 Von Neumann's axiom

The introduction of the device of proper classes into Set Theory is usually credited to Von Neumann [54]. One of the axioms to be found there is:

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

This 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. (We will use separation and power set)

L $\longrightarrow$ R

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.

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 π"x of WF, which is a set by replacement. But π"x, being a set of wellfounded sets, is wellfounded itself, so x is the same size as a wellfounded set.

R $\longrightarrow$ 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 this axiom implies some form of antifoundation: after all any Quine atom is strictly smaller than the universe. However there is a missing step. All it shows is 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 stratified formula then every $\sim$-equivalence class contains a wellfounded 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 [55].

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:

$$\dfrac{\dfrac{\begin{array}{c}[A]\\ \vdots\\ B\end{array}}{A \longrightarrow B}\ \to\text{-int} \qquad A}{B}\ \to\text{-elim} \tag{9.2}$$

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

and

$$\dfrac{\dfrac{A \qquad B}{A \wedge B}\ \wedge\text{-int}}{A}\ \wedge\text{-elim} \tag{9.3}$$

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

$$\begin{array}{c}A\\ \vdots\\ B\end{array} \tag{9.4}$$

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: $x = \{x\}$.

Stratified Formula

A formula in the language of set theory is stratified 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 *stratified* 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 $n \in \mathbb{N}$.

(These definitions are not strictly correct, but are equivalent to the correct definitions as long as countable choice (p. 60) holds.)

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Further Reading

A quick glance at the bibliography will show that there are several volumes alluded to more than once. The Van Heijenoort collection is essential to anyone interested in the history of Logic; The Barwise volume 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. Although the book by Hallett and the book by Tiles are not alluded to in the body of the text, they are still definitely worth a read. The Väänänen article could be profitable consulted by those interested in pursuing second-order categoricity. Quine's *Set theory and its Logic* [38] is eccentric but valuable. Although modern readers will find Quine's notation an obstacle—and may 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.