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

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Contents

Introduction; Understanding the axioms; what is a choice; completing nondeterministic monotone processes; finite/infinite vs concrete/abstract

Chapter 1

Introduction

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

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

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

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

expand this

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

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

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

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

So who am I to lift my head above the parapet and write the book that no-one else does? What is my excuse? The peculiar history of my mathematical education resulted in my being a dedicated student of a distinctly odd set theory, namely Quine's *New Foundations*. One of its oddities is that it refutes the axiom of choice, so that anybody who wishes to explore the world it describes has to be prepared to eschew the axiom of choice altogether, and if you are to be able to do that you have to be able to detect when you are using it. Subsequently hanging out with theoretical computer scientists (initially as a postdoc) inevitably made me more sympathetic to constructivists than an unreconstructed Quinean would be, and compounded my scepticism of AC. It also made me think in terms of datatypes, and (as I hope to show in what follows, starting in [9]) arriving at a proper understanding of AC relies on first unpicking fallacies of equivocation about datatypes. Finally a logician in a mathematics department is forced to think about AC and explain it to their students, even if only because no-one else will.

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

The situation is not that

(i) it's evident that AC and ZL are the same proposition but we don't know whether we believe it or not;

it's the much more alarming

(ii) they appear to be distinct even tho' we know they're equivalent.

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

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

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

Chapter 2

Understanding the Axiom

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

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

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

Y is said to be a *transversal* set[1].

I can remember thinking – when I first encountered this axiom – that it must be a consequence of the axiom scheme of separation, which says that any subcollection of a set is a set. The Y that we are after (once we are given X) is obviously a subcollection of $\bigcup X$, and *that's* a set all right. There are people who say that every subclass of a set *however specified or conceived* is a set, and on this view the axiom of choice, in the above formulation, is a straightforward consequence of comprehension. We will return to this later (p 87), but for the moment let as assume we have only first-order axioms of set existence. If we were to deduce the axiom of choice from them we would have to be able to present Y as the set of those things in $\bigcup X$ satisfying some property or other. Unfortunately there is no obvious way of finding a property 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 specific to X. For an arbitrary X

[1] However one should bear in mind that 'transversal set' has additionally other meanings/uses.

we do not have that kind of information; accordingly we cannot prove (M) above for arbitrary[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 choices.

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

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

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

Equally simple might be: every surjection has a right inverse

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

Is this the right place to be making this point?

2.1 Misunderstanding the axiom

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

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

[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! NOT THE PLACE FOR THIS REMARK

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

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

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

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

Chapters 4 and 3 are inspired by these two errors.

Not sure which chapter should come first

Chapter 3

Getting the right datatype, and the fallacy of equivocation

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

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

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

3.1 Four Examples

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

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

[1] The stress is on the second syllable; this isn't something you find under a Christmas tree.

> (possibly every infinite subset of a separable space) has a countably infinite partition. One is tempted to believe that every infinite set has a countably infinite subset beco's one thinks one can count them. This is a supertask error rather than a fallacy of equivocation.

3.1.1 A Countable Union of Countable Sets is Countable

People who get into a tangle about this matter typically present with a history of having been taught that a union of a countable family of countable sets can be proved to be countable by a simple zigzag construction without any need for further assumptions.

Let $\{X_i : i \in \mathbb{N}\}$ be a family of countable sets. We want $\bigcup\{X_i : i \in \mathbb{N}\}$ to be countable. And we all remember the proof(!)

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

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

Clearly x and y are unique by construction. So we can think of n as the pair $\langle x, y \rangle$. Thus, at stage n, we declare that the nth thing in the union is to be the xth member of X_y.

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

$$
\begin{array}{c|ccccccccccc}
5 & 15 & \dots & \vdots & \dots & & & & & & & \\
 & & \searrow & & & & & & & & & \\
4 & 10 & & 16 & \dots & & \dots & & & & & \\
 & & \searrow & & \searrow & \vdots & & & & & & \\
3 & 6 & & 11 & & 17 & \dots & & & & & \\
 & & \searrow & & \searrow & & \searrow & \vdots & & & & \\
2 & 3 & & 7 & & 12 & & 18 & \dots & & \dots & \\
 & & \searrow & & \searrow & & \searrow & & \searrow & \vdots & & \\
1 & 1 & & 4 & & 8 & & 13 & & 19 & \dots & \\
 & & \searrow & & \searrow & & \searrow & & \searrow & & \searrow & \vdots \\
0 & 0 & & 2 & & 5 & & 9 & & 14 & & 20 \\
\hline
 & 0 & & 1 & & 2 & & 3 & & 4 & & 5
\end{array}
$$

3.1.2 Socks

Should have an Erewhonian joke somewhere about having the socks

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

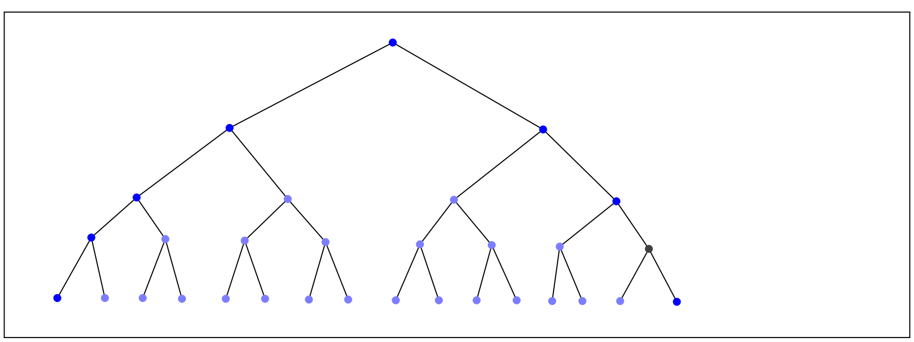
Very well: you have proved that there are countably many shoes; *how many socks?*

3.1.3 Every Perfect Binary Tree has an Infinite Path

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

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

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



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

3.1.4 Lagrange's Theorem

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

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

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

3.2 The Fallacy of Equivocation

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

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

$$\frac{\text{Bronze is a metal; all metals are elements}}{\text{Bronze is an element.}}$$

This argument (as we say) "*equivocates* on" the word 'metal'.

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

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

3.3 Datatypes: an informal Introduction

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

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

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

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

The idea goes back at least as far as Tarski's idea of a *similarity type*, and surely has roots in the informal notions the Viennese positivists employed to give an explanation of the oddity of "This stone is thinking about Vienna" (one of their examples). This is of course false, but false not for reasons to do with the way the world is, but for reasons to do with misuse of abstract datatypes; it thereby furnished an example of what they called a 'category mistake". The idea became richer and more explicit in Edinburgh in the 1970s. As an explanatory/analytical device it is much more part of the currency of Theoretical Computer Science than of Mathematics. Datatypes crop up at this juncture because they are a nice way of explaining the four conundra we introduced in this chapter. However, this is a book about Set Theory, and the ADT of greatest importance to us will be that of `set`, so we may as well get a discussion of that out of the way now.

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

A throwaway remark of Kripke's (quoted by Boolos [1]) has vivified an ageing narrative[2] of an *iterative* datatype of sets. You throw a lasso *and then* you tap with a wand the contents you have just captured to obtain thereby a `set`

[2]From the German... *Narr*: a fool.

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

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

unless it's the one-armed bandit of []

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

1. The question of what constructors (and perhaps destructors) `set` has;

2. The question about what operations the ADT `set` supports; and finally

3. questions about which axioms the world of `set`s obeys.

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

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

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

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

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

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

references

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

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

Two points to make about Kripke's lasso.

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

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

The lasso picture tells us that `set` supports "¿$x \in y$?" and "¿$x = y$?" but we mustn't jump to the conclusion that it supports

> "Give me a member, or an error message if you are empty".

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

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

what datatypes did mathematicians have before they invented sets?
Is the constructive ADT `set` different from the classical ADT `set`?
What operations does the ADT `situated set` support?

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

$$\texttt{set} < \texttt{multiset} < \texttt{list}$$

or

$$\texttt{group} < \texttt{ring} < \texttt{ordered ring} < \texttt{ordered field} < \texttt{complete ordered field}$$

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

expansion is defined on p 22.

So what operations does the ADT `set` support?

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

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

Have we defined this game??

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

duplication

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

Distinguishing between these various manifestations of the reals seems a very unnatural move to most working pure mathematicians; they are not interested in thinking of the reals (or for that matter anything else) as a set, but rather as a rich and complex structure with more aspects than you can shake a stick at, and certainly more than you can be bothered to count[3] Indeed, the very idea that mathematical structures are sets-with-knobs-on couldn't even get started until mathematicians acquired/invented the concept of set less than two centuries ago. If you think of the entire history of human mathematics as compressed into a single day, Set Theory appears a few minutes before midnight, just after the extinction of the trilobites.

For them the natural point of departure is the reals themselves. The reals can do, and act, many things – "one man in his time plays many parts" after all – so one can think of the reals as . . . a field, as an ordered set, as a topological space, as a vector space . . . but the tendency is to think that the reals remains the same though all these retellings, and to think that there is no significance – no *cost* – attached to the decision to change one's viewpoint. This is an *error of attachment*.

To use another bit of logical (well, *philosophical*) jargon, our picture of the reals is rather *intensional* . . . we think of the reals as an entity that has a soul, and a soul in which all the other manifestations of $\mathbb{R}$ – complete ordered field, vector space over $\mathbb{Q}$, etc etc – somehow partake. How else are we to explain the common mathematical parlance of [for example] "Consider the reals as an additive abelian group"? I am not saying that there is anything *wrong* with this way of thinking; what i am saying is that there are times when one has to stand back from it.

Of course $\mathbb{R}$ is only one example of a structure that has lots of reducts and natural expansions, all of which one naturally wants to think of as being the same thing; there are other examples. And it is in (some of) these other settings that the intensional way of thinking can obscure uses of the axiom of choice and lead us into error.

We need to move this following paragraph to somewhere earlier in this chapter

These distinctions that standard mathematical practice tends to blur are actually in themselves legitimate objects of mathematical study, and we often use the word "(Abstract) Data Type" (or *ADT*) to describe the kinds of structure that conventional practice equivocates between: *Group, ring, vector space, list, tree, . . .* and there has been – since the 1960's – an interesting and growing literature on the subject, mainly generated by (theoretical) Computer Scientists.

Let us now consider what this identification-of-different-manifestations looks like from the point of view that considers all these manifestations to be distinct mathematical objects, mathematical objects distinguished by their datatypes. Any attempt to reason in a way that identifies these (now officially distinct) entities commits . . . a *fallacy of equivocation*.

If, Dear Reader, you are an ordinary working mathematician, you probably

[3] This reluctance to think of the reals as a `naked-set` – or even to contemplate the `naked-set` that you obtain from their conception of reals by throwing away all the extra (fun!) structure – is exemplified by the striking lack of interest that most mathematicians show in the properties that the reals has *qua set*. It is remarkable how many people with degrees in mathematics from reputable institutions do not know that the cardinality of the reals so much as has a *name* let alone that that name is '$2^{\aleph_0}$' rather than – say – '$\aleph_1$'. Ask around, you'll see what I mean. And we should not be surprised by this: most of the interesting questions about the set-with-knobs-on that is $\mathbb{R}$ concern the knobs not the naked set.

think of the distinction-between-datatypes to which I am pointing as a mere contrivance, a pointless and annoying artefact of post-nineteenth-century mathematics. I hope to persuade you that it is quite useful, and that it is really quite straightforward: no structure is the same as any of its proper reducts – and it is only by grasping this fact that you can come to understand the independence results concerning AC.

3.4 Expansion and Reduction

The rationals as an ordered set are an ordered set; the rationals as an abelian group are a group. They have the same underlying set but are distinct structures. Many mathematicians grumble about being leant on to observe the distinction between a set and a set-with-knobs-on, but the distinction does make a lot of things clear. A group is not the same as the set of its elements. An ordered set is not the same as a naked set. Model theory has the wonderful word **reduct** which is very useful here. For example, the rationals as an ordered set are a reduct of the rationals as an ordered group. The converse operation is **expansion**, and the rationals as an ordered group are an expansion of the rationals as an ordered set. The rationals as an ordered set are a *reduct* of the rationals as an ordered group because one obtains the first object from the second by "*throwing away*" some structure; the rationals as an ordered group are an *expansion* of the rationals as an ordered set because one obtains the former from the latter by *adding* some structure. A key observation is that a structure and an expansion of it remain distinct even if one can be turned into the other in only one way. The abelian group of natural numbers less than p (p prime) with addition mod p can be turned into a field in only one way, but – even so – that field is not the same thing as the abelian group, let alone the set $[0, p]$ of natural numbers less than p.

Compare the way in which Kripke/Boolos conceals the wand; mathematics is the art of concealment

This isn't quite right

An ADT can be a subtype of another type: `naked tree decorated with a map into` $\mathbb{R}^2$ is a subtype of `naked tree`. If `A` is a subtype of `B` then often things of type `A` can be turned into things of type `B` by decorating them with stuff; this is called *expansion*; conversely things of type `B` can be turned into things of type `A` by discarding decorations; this is called *reduction*. The rationals as a field are an expansion of the rationals as an abelian group – which of course are a reduction of the rationals as a field. A two-coloured graph is an expansion of a two-colourable graph.

rewrite this properly

3.5 Back to our four examples

Thus armed, we can return to the four examples we considered at the beginning of this chapter.

3.5.1 The Perfect Binary Tree

The fact that is obvious is not the fact that

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

but the fact that

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

since we cannot follow the rule "take the leftmost child in each case" unless we can tell what the leftmost child is, and this information is provided for us not by the tree itself but by its injection into the plane. Let us coordinatise the plane: equip it with an origin and two axes. Then the two children of any one node have two distinct addresses that are ordered pairs of reals. When constructing an infinite path we extend it from a given bud node by proceding to the child node whose address is the lexicographically first of the two addresses of the two children.

Are not (i) and (ii) the same? They certainly will be if any two perfect binary trees are isomorphic, since if even one of them is equipped with a bijection into the plane that one will have an infinite path and you can then use the isomorphisms to find copies of that infinite path in other perfect binary trees. And aren't any two perfect binary trees isomorphic? Isn't that obvious?

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

Any two perfect binary trees are isomorphic; (iii)

but

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

and (iii) and (iv) are not the same. A perfect binary tree is not the same thing as a perfect binary tree equipped with an injection into the plane.

Perhaps Say something about König's Infinity Lemma

3.5.2 The Countable Union of Countable Sets

We need to be alert to the difference between *countable set* and *counted set*. A countable set is a naked `set` that just happens to be countable – there is in the universe somewhere a bijection between it and $\mathbb{N}$, but the whereabouts and nature of this bijection have not been revealed to us; we are like the hero in the mediæval romance who knows there is somewhere in the universe a magic sword to cut the head off the dragon that guards the ring, but he has not been told where it is nor what it looks like. A `counted set` is not a mere naked `set` at all, but is a structure consisting of a `set` actually equipped with a bijection with $\mathbb{N}$. According to Classical Logic **knowing of a naked `set` that it can be counted is not the same as being in possession of a designated counting of it; your knowledge might have been obtained from a nonconstructive proof.** The set and the set-equipped-with-a-counting are two different kinds of object, different ADTs.

What operations does the ADT `counted-set` support? Presumably all the operations supported by `set` (whatever they are) but in addition we can ask it for its nth member. Is it a random access device or a serial access device? (*i.e.*, do we obtain the nth member from it by walking along a trail consisting of the first $n-1$ members?) For our purposes it won't matter.

(Brief remark: we are overloading "union" below to mean all four operations: countable/counted sets of countable/counted sets.)

Given that we can now distinguish between countable `sets` and `counted sets`, our original claim *A countable union of countable sets is countable* multifurcates into:

(Ci): A counted union of counted sets is counted;
(Cii): A countable union of counted sets is countable;
(Ciii): A counted union of countable sets is countable.
(Civ): A countable union of countable sets is countable.

and of course (Civ) is the formulation with which we started[4]. The contrast is between (Ci)–(Cii) – which are both provable from first principles – and the last two, which aren't.

These four assertions sound so similar that it is easy for the incautious to confuse them. Fortunately we are now in a position to disentangle them. The zigzag construction from page 14 will be essential.

Let start with (Ci). It sounds snappy but unfortunately it's not literally true as stated. It's a snappy shorthand for the – perfectly correct – observation that there is a canonical way of obtaining a counting of the sumset of a counted family of counted sets: the zigzag algorithm. I shall follow general practice in using this shorthand.

Now let's think about (Cii). The zigzag algorithm gives us a way of taking a counted bundle of countings and returning a counting of the union of the sets counted. Reflect that the zigzag construction wants its input to be a counted set of counted sets. Let $\{A_i : i \in \mathbb{N}\}$ be a counted set of counted sets. At stage $n = \binom{x+y+1}{2} + x$ the algorithm says to A_x: "Give me your yth element". The zigzag algorithm is the function f in the syllogism. '$A(x)$' says that x is a counted set of countings, and '$B(y)$' says that y is a counting of the union of the sets counted. So (Cii) is straightforwardly provable from first principles, and we have made no use of the axiom of choice.

For each n, do the following. Decode n as $\binom{x+y+1}{2} + x$, obtaining x and y. Ask A_x for its yth member. All these actions can be performed simultaneously, since they do not interfere with each other.

Observe that the execution of the zigzag algorithm is not what we will, later – in section 6.4.2 – come to call a *supertask*: a supertask is a sequence of tasks of transfinite length, done in sequence not in parallel. All the actions in a run of the zigzag algorithm can be done simultaneously since, for any n, we can ask a counted set for its nth member without first having asked it for its kth member,

[4] I learnt this formulation from Conway – hence the 'C'.

with $k < n$. (This is true even if `counted set` is a serial access device). Just send the number $\binom{x+y+1}{2} + x$ to the xth member of the yth counted set.

> Those drawn to realizability semantics for constructive logic might like to think of the zigzag algorithm as a *realizer* of the universally quantified conditional (Cii). A realizer[5] of a conditional $A \to B$ is a function from the set of realizers of A to the set of realizers of B.

Now consider (Ciii). We have a countable family of countable sets. OK, let's count the family, so we can think of it as $\{A_i : i \in \mathbb{N}\}$. We can do that with a single choice, so we are not using AC.

This is why we have to have the datatype section after the one-choice-is-allowed section

Reflect that the zigzag construction wants its input to be a counted set of counted sets, as it was in case (Cii), where everything was tickety-boo. In contrast, here, faced with a counted set $\{A_i : i \in \mathbb{N}\}$ of merely count*able* sets it won't run. Let's think a bit about this.

ZIGZAG ALGORITHM (*to* $\mathbf{A}_1$)	"Give me your first element".
$\mathbf{A}_1$ (*bursting into tears*)	"I'm a *set* not a *wellordering* – I don't *do* "first" elements; I don't need this; I want my mummy!!"

In computer science terms what happens is that we here encounter a `typecheck_error`, and we would get a message from the operating system saying something along the lines:

`I expected a counted-set, but I found a naked-set.`

The reader may think that that fact – that A_1 is a `set` not a list – is a mere detail, that you can turn A_1 into a `list` (and then ask for its first member) at run-time. But you can't. You have to *expand* it into a list – at *compile-time* as it were. Expanding all the A_i to `lists` simultaneously at compile-time is where you use the axiom of choice.

This next paragraph doesn't have to be tied to this example

The reader may by now be thinking: "OK, so what are these ADTs? I've had quite a lot dangled in front of me: `set`, `counted-set`, `binary-tree`, `binary-tree–with–an–injection` .... Can we have a shopping list please?" 'Fraid not. We make up these ADTs as we go along, invoking them locally to provide contrastive explanations. As we remarked on p 17, ADT is an open category.

> This is probably the correct juncture to make the point that the only reason why so many people are confused about this is because they were lied to about the zigzag construction. Some such explanation is required, because it is not natural to be confused about things that are this easy. And the lie is a salient candidate for the correct explanation.
> Incidentally this is the first place where the *lie* is mentioned.

[5]The 'z' in this word is not a violation of the spelling rules of the British English in which I am writing this essay: 'realizer' (as in constructive logic) is an American loan word and we retain the original spelling in order to flag its distinctive use.

OK, so how *do* we use AC?

Let us take AC in the form we first encountered it here: every set of pairwise disjoint nonempty sets has a transversal. If we have to invoke AC here, the question is, which family of pairwise disjoint nonempty sets is it that we desire a transversal for?

A moment's reflection will persuade the reader that we can assign a counting to each A_i by applying AC to the (countable) family of sets-of-countings; for each A_i there are plenty of countings – $2^{\aleph_0}$ of them to be precise – and we need to decorate each A_i with one of them in order to expand them into counted sets suitable for the zigzag construction.

In effect we have used AC to get us back into situation (Cii); we use AC to expand every member of a (counted) family of objects of type `naked-set` into a counted family of objects of type `counted-set`.

In fact this use of AC is neccessary: it cannot be proved in pure set theory that a union of countably many countable sets is countable, but demonstrating this unprovability is a nontrivial task. We discuss below (section 6.5.3) what can be said in the absence of AC.

Do we Say something about independence of AC in vol 1?

The fallacy of equivocation here is between (Cii) and (Ciii).

Somewhere here emphasise the elementary point that for all $n \in \mathbb{N}$ a union of n countable sets is countable.

3.5.3 Lagrange

Every coset of H is a copy of H. Every $g \in G$ gives rise to a coset $\{gh : h \in H\}$ and the function $h \mapsto gh$ is a bijection between H and gH. So every coset is of size $|H|$. And they're all disjoint, and they're all the same size – co's they're all the same size as H. So $|H|$ divides $|G|$.

We want to say that $|H|$ divides $|G|$, of course. That is to say that there is a set C such that $|C| \cdot |H| = |G|$. That – in turn – is to say there is a bijection between $C \times H$ and G, since that is how multiplication of cardinals is defined ...so every element of G can be represented by an ordered pair $\langle h, c\rangle$ with $h \in H$ and $c \in C$.

But what is this set C?

If we cannot find such a C then all we can say is that G is the union of a certain number – c, say – of things all the same size h. But in the absence of AC the expression "the cardinality of a union of c-many things each of size h" cannot be relied upon to denote the cardinal $c \cdot h$. The union of countably many pairs (e.g. of socks, yes) cannot be assumed to be of size $2 \cdot \aleph_0$ (which of course is $\aleph_0$) in the absence of AC.

How do we find such a set C? This is an instance of a general problem, but in this case it's clear what we have to do. Every left coset of H is a bijective copy of H, so for each such coset H' we pick $g \in G$ such that $gH = H'$ and call it $g_{H'}$. Then the set C we want is $\{g_{H'} : H' \text{ is a left-coset of } H\}$. Then every $g \in G$ really does correspond to a unique pair $\langle h, g\rangle$ with $h \in H$ and $g \in C$.

What a lot of faff! The average pure mathematician revolts at the thought. Why do they revolt? Because they have been happily equivocating between two data types, and are now being told not to. Indeed even being forced to listen to talk like 'data type' is an unwelcome distraction. In this case the two data types are

> (i) the data type of (naked) `set`, which is a set of group elements, and the other
>
> (ii) is that of `decorated set` which is a coset $C \subseteq G$ decorated with one of the gs such that $C = \{gh : h \in H\}$.

It's rather like the difference between countable `set` and `counted-set`. In fact in one sense it is *exactly the same distinction*: it's the distinction between two datatypes.

That is why I was right to use the letter 'H'' for the coset, rather than write 'gH'. 'gH' is not really a natural notation for denoting a coset, but it is a natural notation for denoting a *decorated coset*.

Of course if there is such a C you can take it to be the set of H-cosets.

Change the Definition of Multiplication . . . ?

The thoughtful and suspicious reader might look at this explanation and say that the difficulty to which the axiom of choice purports to be the answer arises only beco's of the manner in which we have defined multiplication. We have been saying of three cardinals $\mathfrak{a}$, $\mathfrak{b}$ and $\mathfrak{c}$ that $\mathfrak{a} = \mathfrak{b} \cdot \mathfrak{c}$ if and only if there are sets A, B and C such that $\mathfrak{b} = |B|$, $\mathfrak{c} = |C|$ and $\mathfrak{a} = |B \times C|$. Perhaps we should instead say that $\mathfrak{a} = \mathfrak{b} \cdot \mathfrak{c}$ if and only if a set of size $\mathfrak{a}$ can be expressed as a union of $\mathfrak{b}$ things each of size $\mathfrak{c}$. On the face of it, this sounds sensible. However, for this definition to succeed we would have to show that:

> whenever A_1 and A_2 are two sets both of which can be written as a union of $\mathfrak{b}$ things each of size $\mathfrak{c}$ then $|A_1| = |A_2|$. (Eqn)

. . . and unfortunately we need extra assumptions to prove (Eqn)

The point is that if you define multiplication in this – wrong – way then the product of the two cardinals $|A|$ and $|B|$ isn't well defined, since it doesn't just depend on the two cardinals $|A|$ and $|B|$ but depends on A and B as well. Proper (cardinal) multiplication corresponds to an operation on sets (cartesian product) for which equipollence is a congruence relation. This new operation on cardinals does not correspond to any operation on sets for which equipollence is a congruence relation.

The reader has probably guessed by now that proving equation (Eqn) requires the axiom of choice.

It might be an idea to supply a discussion

> The reader might also think (as I did, for a while) that the fact that the axiom of choice enables us to prove that these two definitions of cardinal multiplication are equivalent is what lies behind the name "The Multiplicative Axiom". That is not so. The significance of AC in this context is that it enables us to prove that *infinite* products (for cardinals) are

> defined: a product of nonempty sets is nonempty. If we want to show that a product $\Pi_{i\in I}\mathfrak{a}_i$ of a family $\langle \mathfrak{a}_i : i \in I\rangle$ of cardinals is well defined, we use AC to pick a representative set A_i from each $\mathfrak{a}_i$ and then use AC *again* to show that the direct product $\Pi_{i\in I}A_i$ is nonempty. Of course the product of the family $\langle \mathfrak{a}_i : i \in I\rangle$ of cardinals is $|\Pi_{i\in I}A_i|$. In fact we need AC even to define infinite *sums* of cardinals, because we need it to pick a representative from each of the cardinals being summed. But that's all we need it for: we don't need it (in the way we have just used it for infinite products) to show that the answer is non-degenerate.

3.5.4 Socks

How many shoes does the squillionaire have if he has countably many pairs of shoes? You want to say "countably many" and you are right, but why are you right? Let's go back to basics. What is a countable set? One that is in bijection with $\mathbb{N}$– one that has a *counting*. So, given that we want to infer that the set of shoes is countable from the information that the set of *pairs* of shoes is countable, what we should be doing is arguing from the existence of a counting for the set of *pairs* to a counting for the set of *shoes*. The appropriate response to a challenge to do X is to do X; classroom experience teaches me, however, that that is not what most mathematicians actually do when confronted with this challenge. They tend to say things like "it's obvious" or "I can count them"; "just pick one, and then another ...". It's not clear to me (and i suspect not to them either) whether they think (i) that they have risen to the challenge, or have instead (ii) justified their decision not to. A certain amount of tact is required on the part of those who insist (as I do in these circumstances) that if you claim that a set is countable then you have issued an IOU that commits you to paying the bearer on demand with a counting of it. Merely *outlining* an answer is not the same as actually *supplying* an answer. At some point you have to either exhibit an enumeration or admit that you don't know how to. No stonewalling!

ME	What does it mean to say that a set is countable?
VICTIM	It means you can count it.
ME	The set of shoes is countable?
VICTIM	Yes.
ME	So it can be counted?
VICTIM	Yes!
ME	I challenge you to count it.
VICTIM	[*flannel and bluster*]
ME	OK! If you know how to count it (but don't want to show me) at least tell me which shoe is the first shoe according to your scheme. While we're about it, which is the 15th?
VICTIM	[*silence*]
ME	I don't believe you can count it.
VICTIM	[*more flannel and bluster, plus indignation*]
ME	Prove me wrong in front of witnesses!

[*to the audience*]
He can't count the shoes! He has a Ph.D. in Mathematics and he can't count shoes?[6] He probably can't even tie the laces on them! Can anyone here show him how to do it?

The point is easily missed. It is of course true that it doesn't matter in the least (for the rube's purposes or mine) which counting you use. But it does matter a very great deal that there is a counting, so it matters that you should be able to produce one on demand – even tho' it doesn't matter which one you produce. People who are affronted by the demand that they produce a counting are confusing two things, one of which matters and the other of which doesn't. It's another fallacy of equivocation.

It doesn't matter which bijection I use

is not the same as

It doesn't matter whether I can produce a bijection or not.

The first is true and the second is not.

Sophisticates might discern here a connection of ideas with the constructive critique of classical mathematics. Constructivists never admit that $(\exists x)F(x)$ has been proved until something that is F has been produced. My insistence here, that people who say the set of shoes is countable should be willing to exhibit a counting, isn't really based on these (what a student of mine used to call) *exhibitionist* scruples – I'd be perfectly happy with a nonconstructive proof that there is a counting of the shoes; it's just that prompting the rube to produce a constructive proof is more effective polemically and pædogogically; in any case all the obvious proofs that the set of shoes is countable are in fact straightforwardly constructive so there is no additional cost to the victim in demanding of them that the proof they come up with should be constructive.

A combination of cajolerie and threats of public humiliation will – eventually – persuade most mathematicians to get off their high horse and condescend to say, out loud:

"Yes, the left shoe from the nth pair can be sent to $2n$ and the right shoe from the nth pair can be sent to $2n + 1$."

(Tho' this is often done with bad grace, as much as to say that any insistence on an actual bijection is the height of unreason). This does, indeed, show that there are $\aleph_0$ shoes. Good! Blood from a stone is better than no blood at all (in fact it's the best blood there is). The point of insisting – at gunpoint – that the rube actually come up with a way of counting the shoes is essential for what lies ahead, for it is only once they have done that that they will appreciate the significance of their inability to do the same for the socks when the time comes...

...which it does now. How many socks? One wants to say "countably many" of course. This invites the same challenge: "Count them" and, with any luck, the victim will provide (or at least initially reach for) the same answer as they eventually reached with the shoes: send the left sock from the nth pair to $2n$ etc. Of course, for that to work you have to have always a readily identifiable *left* sock and a readily identifiable *right* sock, and of course only mathematicians have odd socks. Old jokes are the best.

With the shoes you have an algorithm that you can hand on to unskilled unsupervised labour so that you can bunk off for a leisurely pint at the restaurant at the end of the universe where they will join you at the End of Time when they've finished. With the socks there is no such algorithm.

Part of the attraction of this parable (for the preacher) is that, at first blush, the two cases – socks and shoes – look essentially equivalent, and this renders all the more striking the revelation that they are not equivalent after all. How can they look so similar when they are so different? The answer is that the very physical nature of the setting of the parable has smuggled in a lot of useful information. It cues us to set up mental pictures of infinitely many shoes (and socks) scattered through space. The shoes and socks – all of them – are (or can with only a minimal amount of abstraction be thought of as) extended regions of space and – as such – they all have nonempty interior. Every nonempty open set in E^3 contains a rational point (a triple all of whose entries are rational), and the set of rational points has a standard wellordering. This degree of asymmetry is enough to enable us to choose one sock from each pair, as follows. In any pair of socks, the two socks have disjoint interiors[7] and both those interiors contain rational points. Consider, for each sock, that rational point in its interior S which is the first in some standard wellorder of the rational points, fixed in advance. For example there will be a least natural number that appears as the denominator of a coordinate of a rational point in S, and only finitely many rational points decorated with that minimal natural, and it's not hard to develop this idea into a choice function on open subsets of 3-space. This will distinguish between the socks, since one will have been given a rational point earlier in the canonical ordering than the rational point given to the other. The physical intuitions underlying this last argument make it very clear to us that we can pick one sock from each pair – as indeed we can. Space is *just sufficiently* asymmetrical for us to be able to explicitly enumerate the socks in countably many pairs scattered through it.

> There is another proof, at least if we can shrink each sock to a point. Every set of isolated points in a Polish space is countable, even without choice

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

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

and

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

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

(P') does not need the axiom of choice, and it ought to be obvious. However I suspect it's worth banging the drum for a proof, as we have just done. In contrast (P) *does* need the axiom of choice, but it looks obvious if you confuse it with (P').

. . . should Say something about how P' is obvious to us beco's of our physical intuitions rather than for any mathematical reasons. This might matter.

In summary:

> **If you find yourself thinking of some familiar assertion that it looks obviously true without recourse to the axiom of choice, while nevertheless having at the same time access to expert testimony to the effect that AC is needed for it, then you should go looking for a fallacy of equivocation.**

It may be helpful to think of many of these fallacies of equivocation as failures to attend to the question of which datatype one is using. That is arguably the situation with the four cases we have considered.

- In the case of the socks we are failing to distinguish between the ADT `naked-set` [of socks] and the ADT `set–equipped-with–injections – into-space` [of socks embedded in space];
- In the case of the perfect binary trees we are failing to distinguish between things of datatype `tree` and things of datatype `trees–equipped-with–an-injection-into-the-plane`.
- In the case of the countable union of counted sets we are failing to distinguish between `naked sets` and `counted sets`.
- In the case of Lagrange's theorem it's a failure to distinguish between cosets as `naked-sets` and cosets as `decorated-sets`.

3.6 How the fallacy gets committed

Whatever else it is, mathematics is at least a social activity, and a part of becoming a mathematician is learning how to talk like a mathematician. People learning mathematics who have learnt to say things like "let gH be a (left-)coset of H in G" think they are merely learning how mathematicians talk, but they are actually taking on board a great deal more than that. Several philosophical communities have the expression 'analytic truth' which means [roughly!] something whose truth is ascertainable merely by analysing it (rather than by checking the way the world is). Ever since Quine made the point a lifetime ago it has been a commonplace among philosophers in his tradition that your decisions about which propositions are to be analytic are made as soon as you choose a language. For those who want a way in to this literature `https://plato.stanford.edu/entries/analytic-synthetic/` is as good a place to start as any. They are learning a language all right, but it is a language that has induced its users to assume the axiom of choice by artfully bundling it into the machinery they use. gH is not a mere left-coset of H in G – a `naked-set`; it is a left-coset *decorated with a certificate*. That is no more the same thing as a mere left-coset than the rationals as an ordered field are the same thing as the rationals as an abelian group. Such people are not merely learning a language, they are unwittingly adopting unacknowledged assumptions. Sometimes when you eat a mushroom you are taking in more than just butter and garlic.

Not mentioned certificates yet

OK, so how *does* it get committed?

Mathematics is not revealed, anew and afresh, to each generation. It is transmitted by toiling professionals – whose expositions may be trapped in local optima – to bright young minds that cannot be expected to absorb in a handful of lectures all the facts and all the ill-articulated pitfall-avoidance skills that their elders have somehow accumulated. It is never possible, when handing on the torch to the next generation, to tell them everything at once. You paint a broad picture, leave some details out, tell a few jokes to make them comfortable and – above all – you *don't frighten them*. Respecting the need for omissions sometimes results in the telling of outright lies, and the way in which first-year students are told that a union of countably many countable sets is countable is a case in point. N.B. the lie is not "a union of countably many countable sets is countable"; the lie is the claim that the usual story is a *proof*. Reasonable people can disagree about whether the axiom of choice ought to be embraced; reasonable people can disagree about whether a policy of lying to children is defensible in this case; what reasonable people are *not* free to disagree about is whether or not the story is a lie. I can imagine that some readers will baulk at this claim, but they shouldn't. We know that some invocation of a choice principle is needed to prove that a countable union of countable sets is countable, so any purported proof that doesn't invoke any such principle is defective. Nowadays we have objective criteria for whether or not a proof is correct, in the form of computer-verification of proofs. And we all know what the verdict would be.

Say something about how one feels sympathy with people who confront the expository problem.

Hidden Curriculum!

[If you want to defend current practice (and it may well be entirely defensible) you have to do it on the basis that it is one of those cases where it is all right to lie to children. Part of that will be an argument that there is no other way of doing it. I will argue that there is a way of doing it without lying, and that is to explain datatypes and casting.]

Even if the mathematics lecturer is not him/herself in the grip of the fallacy of equivocation, the elisions and glossings-over caused by the pædogogical need to press on to the mainstream subject matter of the course have the effect that the students are led to commit the fallacy on their own account. This is because committing the fallacy is the simplest way of exhibiting the behaviour – namely not worrying about AC – that is being exhibited to them by their lecturer.

This is the primrose path. The lecturer can be forgiven for feeling that the distinction between `counted-set` and a countable `naked-set` is one that serves no *immediate* purpose for the first year student; thus you start off by eliding uses of AC from the proof, on those grounds, and you thereby leave to lecturers of subsequent years the task of explaining what is going on.

Thus it happens that the first-year students are told that a countable union of countable sets is countable, and they are shown the zigzag picture . . . and then the caravan moves on without further explanation. They subsequently learn on the grapevine that there are these sad weirdos called *logicians* who insist that this – by now familiar – **fact** (namely that a countable union of countable sets is countable) apparently needs this axiom called *"the axiom of choice"*(!?) Mostly they aren't told why, so – rather than make use of the countable/counted distinction (which in any case they haven't been shown) – they innocently and uncomprehendingly embrace the axiom by an invocation of Inference-to-the-best-explanation.

So what happens in their second year, when someone should be dotting the i's and crossing the t's on the dodgy first-year proof that a countable union of countable sets is counted? They are then lectured by someone *who has been through the same process, and made the same mistake.* (A policy of *putting off the explanation until some suitable later date* is always likely to run into trouble if there is no-one whose responsibility it is to ensure that the explanation is ultimately provided.) Never telling the student these things is a sort of tail-event; at any stage there is the possibility of providing an explanation, so it can always be put off. It is only at the end of time that one knows one has failed, and by then it is too late.

'Lying' . . . Isn't that a bit harsh? Perhaps nobody has been actually *lied to*, not literally, not *strictly*, but they have been victims of a policy of telling judiciously selected half-truths – albeit with the best intentions. Such policies do not reliably achieve their ends. I remember being told the story of the child that was told that if it sucked its thumb it would swell up and burst. The child takes this on board, as children always do. Next day the child sees on the bus a woman who is 7-months-pregnant and says to her reproachfully "Anybody can see what you've been doing".

There are people who brazenly defend this policy. One of my colleagues (Imre Leader) says that one doesn't routinely flag uses of the axiom of pairing when giving a proof, so why should one have to flag AC? The answer is as follows. The only reason for wanting to flag all uses of the axiom of pairing is if one is trying to reconstruct mathematics inside set theory and is concerned

See the chapter in vol 1. Rewrite this

to keep track of which bits of set theory one is doing, in order to prove a point. That is a very special situation to find oneself in, and most of us don't find ourselves in it, so we don't flag uses of the axiom of pairing nor – usually – any other axioms of set theory. The reason why we flag uses of AC is not that AC is a set-theoretic axiom and we want to flag all uses of the axioms of set theory, rather it's because AC is an important distinct mathematical principle: if you're carrying it, you'd better declare it.

Chapter 4

Finitely many Choices ... but what is a Choice?

4.1 Thinking you need the axiom when you don't

If nobody brought you up to recognise the axiom of choice when you see it, but you learnt behind the bikesheds that this kind of thing goes on, you might startle at shadows. For example you might think that you need AC in the proof of the familiar fact that

$$\binom{n+1}{k+1} = \binom{n}{k} + \binom{n}{k+1} \tag{A}$$

Plenty of people do!

If you were one of the people who are spooked by this equation then this chapter is for you.

Cast your thoughts back to the old proof of this fact that you have stored somewhere in the back of your mind. If we want to select $k+1$ things from a set X of $n+1$ things then we arbitrarily pick one of $n+1$ things, as a sort of pivot – in fact let's call it 'p' for $\underline{p}$ivot – and we then either

(i) select a further k things from $X \setminus \{p\}$; or
(ii) select $k+1$ things from $X \setminus \{p\}$.

Process (i) gives us $\binom{n}{k}$ things (the first summand).
Process (ii) gives us $\binom{n}{k+1}$ things (the second summand).

Process (i) gives us all the unordered $(k+1)$-tuples that contain the pivot p, and process (ii) gives us all the unordered $k+1$-tuples that do *not* contain p. So we add the two terms to sweep up all the $(k+1)$-tuples we need. But how do we decide which thing to choose for a pivot?[1]?? It is true that the aggregate number comes out the same whichever pivot we *in fact* choose, but

[1]It is possible to worry about excluded middle here .... We do assume that every tuple either does or does not contain the pivot p. However that is not our concern here.

(a) We still have to choose some element, don't we …? and
(b) Aren't we going to need the axiom of choice to tell us that we can in fact choose some element?

Of course the answer to (a) is *yes* – as expected – but the answer to (b) is *no*. This 'no' answer to (b) requires some explanation.

Before we go to a dissection of this mistake let's have another example. This one is a bit more sophisticated, but it has connection to a proof we will see soon of a special case of AC, and it will be good to keep it in view.

Challenge 1
If $f : X \to \bigcup X$ is a choice function, so that $f(x) \in x$ for all $x \in X$, and $A \notin X$ is a nonempty set, then f can be extended to a choice function for $X \cup \{A\}$.

You have to pick a member of A, don't you!… don't you..?

The key to understanding why you are not using the axiom of choice on those occasions when you mistakenly think you do – like equation (A) above – lies in some very basic logic. (Indeed the mild disdain with which many mathematicians regard formal logic probably has a significant rôle in perpetuating this misunderstanding)

In elementary Logic classes students are invited to take sentences of ordinary language and regiment them into the language of first-order logic. The aim of the exercise is of course to introduce the student to the idea of formalisation: bringing-out and dealing-rigorously-with the features of interest, while concealing everything else.

(1) If there is a Messiah then we are saved.

(2) If there is even one person in this room with COVID then we are in trouble.

(3) If there is even one righteous man in the Cities Of The Plain then God will not fry the city.

Consider (1). Using an obvious lexicon such as '$M(x)$' for 'x is a Messiah' and 's' a propositional constant for 'We are saved' we get

$$(\exists x)(M(x)) \to s \quad \text{or perhaps} \quad (\forall x)(M(x) \to s)$$

Or, looking at (2), writing '$C(x)$' for 'x is a person in this room with COVID' and p for 'we are in trouble' we get

$$(\exists x)(C(x)) \to p \quad \text{or perhaps} \quad (\forall x)(C(x) \to p)$$

The fact that in each of these examples there are two apparently quite different formulations is a reflection of the fact that the two following formulæ are logically equivalent:

$$((\exists x)\phi(x)) \to A \qquad (\forall x)(\phi(x) \to A) \tag{1}$$

By 'logically equivalent' we mean that once we have determined what ϕ and A are then the two results have the same truth value.

Attend closely to where the brackets open and close: the first formula is of the form $A \to B$ (top level connective is an if-then); the second formula is of the form $(\forall x)$ stuff ... (the top level connective is a universal quantifier.)

The fact that the two formulæ in (1) are equivalent means that the following inference is good, whatever ϕ and A are.

$$\frac{(\exists x)\phi(x) \qquad (\forall x)(\phi(x) \to A)}{A} \tag{S}$$

The letter 'S' here is intended to suggest 'syllogism'. (It's not a proper syllogism in the classical Greek sense, but never mind[2]). This inference is the crucial one to bear in mind when considering situations that look like applications of the axiom of choice that aren't, and the reader is advised to stare at it for a good long while. It's telling you that if, for any x, the ϕ-ness of x is sufficient for A to be the case, then any x will do to ensure A ... *you don't need to know which one is* ϕ and *you don't need to nominate one to do the job – its mere existence is sufficient*!

We can use this syllogism to shed some light on situations where some people think we need the axiom of choice. Let us return to one of our earlier examples.

Let's deal with equation (A) first. We are given a set X of $n+1$ things, and we want to prove that the number of $k+1$-sized subsets of X is $\binom{n}{k} + \binom{n}{k+1}$. We notice that, for any $x \in X$, there is a partition of the set of $k+1$-sized subsets of X into two pieces (when wondering which piece to put a subset into, ask whether or not the pivot p is a member of the subset in question) of sizes $\binom{n}{k}$ and $\binom{n}{k+1}$. Notice that 'p' does not appear in these formulæ for the sizes of the two pieces, so we get the same answer whichever pivot we use. This fact gives us the equality we desire – always assuming that there is such a p. But of course there is such a p – in fact there are $n+1$ of them.

Let us recall challenge 1:

> If $f : X \to \bigcup X$ is a choice function, and $A \notin X$ is a nonempty set then f can be extended to a choice function for $X \cup \{A\}$

Consider now the assertion:

> If $f : X \to \bigcup X$ is a choice function, and $a \in A \notin X$, then $f \cup \{\langle A, a\rangle\}$ is a choice function for $X \cup \{A\}$.

This is pretty straightforwardly true.

Using quantifier-speak it becomes

[2] You could obtain something like a syllogism:
There are men
All men are mortal
There are mortals

> $(\forall X)(\forall$ choice functions $f : X \to \bigcup X)(\forall A \notin X)(\forall a \in A)(\exists x)(x$ is a choice function for $X \cup \{A\})$

which (assuming we are allowed to mix our languages for the sake of telling a story) is unexceptionable. The x in question is of course $f \cup \{\langle A, a\rangle\}$. However when we do the above manipulation to the $\forall a$ quantifier we get

> $(\forall X)(\forall$ choice functions $f : X \to \bigcup X)(\forall A \notin X)((\exists a)(a \in A) \to (\exists x)(x$ is a choice function for $X \cup \{A\})$

which rewrites to

> $(\forall X)(\forall$ choice functions $f : X \to \bigcup X)(\forall A \notin X)(A \neq \emptyset \to (\exists x)(x$ is a choice function for $X \cup \{A\})$

which might suggest that we have smuggled in a choice of a member of A. In a sense we have, but what this shows is that *one* choice is all right!

(It might help to consolidate this in your mind by reminding yourself that, in example (2) above, if there is even one person in this room with COVID then we are in trouble . . . *even if we don't know who that person is.*)

So the moral to be drawn is that in order to prove things like (A) at the start of this chapter then, yes, you do have to make a choice, but that the act of making that choice is authorised – by first-order logic if you want to think of it that way. We can certainly formalise a proof of (A) in first-order arithmetic.

This means we can upgrade Challenge 1 to an actual lemma!

Lemma 1 $(\forall X)(\forall$ *choice functions* $f : X \to \bigcup X)(\forall A \notin X)(A \neq \emptyset \to (\exists g)(g$ *is a choice function for* $X \cup \{A\}))$.

We are now in a position to prove

Theorem 1 *(The Finite Axiom of Choice)*
Every finite set has a choice function. For every $n \in \mathbb{N}$*, if* X *is a set of nonempty sets with* $|X| = n$ *then* X *has a choice function.*

Proof:
The proof is by induction on $\mathbb{N}$.

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

> If you are not happy about the empty function (and you might not be) then start instead with the case $n = 1$. In this case X is $\{x\}$ for some nonempty x. But then, for any $y \in x$, the singleton $\{\langle x, y\rangle\}$ is [the graph of] a selection function for X.

For the induction step we use lemma 1.

■

Knowledge of this fact goes back quite a long way. There is a proof in Russell and Whitehead [26] volume 2, as theorem *120.63.

We note for future reference that this proof is acceptable to intuitionists/constructivists; it uses no logic principles they would find suspect.

4.2 What is a choice? How do we individuate Choices?

The fact revealed by theorem 1 is often expressed by some formulation like the *aperçu* :

> "We can always make finitely many choices:
> to make infinitely many choices we need AC" (F)

(F) is extremely arm-wavy, but the thing it is waving towards is important and true. It would be nice to know what a choice is, what choices are, and how we count them. There is a famous remark of Quine's: "No entity without identity" which is very much to the point here. If we haven't got identity criteria for widgets, so that we don't know how to tell whether or not two widgets are the same widget, then we don't really know what a widget is, and that means that we can't use our concept of widget to explain anything; an explanation that makes essential use of the vague concept of widget will itself be too vague to be of any use as an explanation.

If we are to make sense of (F) then we'd better have a way of individuating choices. What is a choice? One pointer towards answers to these challenges comes from reflecting on the fact that theorem 1 can be proved without using AC, and therefore – if the *aperçu* is correct – while making only finitely many choices. So what is a choice? On the face of it we make a choice every time we go through the induction loop. How are we to reconcile this with the desire to think of this proof as making only finitely many choices? Well, one could try saying that that for each actually finite set one needs only that many actual choices to obtain a choice function. I don't like the sound of this beco's it has the feeling of conflating the single assertion that every finite set has a choice function with the scheme of assertions that every set of concrete finite size has a choice function. Fortunately there is another approach.

Places where we seem to be making choices are typically places where we invoke the syllogism, so I am going to risk doing enough logic to analyse the syllogism. It would be very good for the reader's soul to learn some basic Natural Deduction, but of all the multitude of rules it harbours (two for each connective or quantifier, for starters) the one that will concern us most here is the rule of $\exists$-elimination, which is the rule that tells us how to exploit – when building a proof – the information wrapped up in an assertion like $\exists x F(x)$.

A bit of Proof Theory

4.2.1 The Rule of $\exists$-elimination

The picture below is a hugely simplified picture of a proof of an expression p using $\exists$-elimination. The rule of $\exists$-elimination is famously intimidating to beginners so I am hoping that my readers will not feel that their intelligence is being insulted if I offer a few words of patter. The rule is telling us that

if

(i) we can deduce p from the news that x has property F – and that the security of the deduction doesn't depend on x, that any x will do – *and*

(ii) that we know (somehow) that there is something that is F,

then

we can deduce p.

The calligraphic '$\mathcal{D}$' names a $\mathcal{D}$erivation of p from the assumption $F(t)$, represented by the vertical dots that it stands next to. The square brackets round the '$F(t)$' mean that that assumption is "used up" by the $\exists$-elimination that is the last line of the proof. That is to say, although $F(x)$ was an assumption in the proof $\mathcal{D}$ (of p) that eventually got processed into our proof of p, it is no longer an assumption of that final proof, of which $\mathcal{D}$ is a proper part. $\exists x F(x)$ is an assumption in our (displayed) proof of p, but $F(x)$ isn't. The '(1)' connects the application of the rule to the premiss being discharged (there may be lots of other $\exists$-eliminations in the stuff $\mathcal{D}$ abbreviated by the vertical dots.)

$$\dfrac{\exists x F(x) \qquad \begin{array}{c}[F(t)]^{(1)}\\ \vdots\ \mathcal{D}\\ p\end{array}}{p}\ \exists\text{-elim (1)}$$

4.2.2 The Rule of $\forall$-introduction

There is also the rule of $\forall$-introduction:

$$\dfrac{\begin{array}{c}\vdots\\ A(t)\end{array}}{(\forall x)(A(x))}\forall\text{–int}$$

To prove that everything has property A, reason as follows.

Let t be an object about which we know nothing; we reason about it for a bit and deduce that t has A; remark that no assumptions were made about t; conclude therefore: *all* xs must therefore have property A.

But it is important that x should be an object about which we know nothing, otherwise we won't have proved that *every* x has A, merely that A holds of all those x's that satisfy the conditions x satisfied and which we exploited in proving that x had A. The rule of $\forall$-introduction therefore has the important side condition: 't' **not free in the premisses**. The idea is that if we have proved that A holds of an object x *selected arbitrarily*, then we have actually proved that it holds for *all* x.

explain free variable

The rule of ∀-introduction is often called **Universal Generalisation** or **UG** for short; readers may know it under that name. It is a common strategy and deserves a short snappy name. It even deserves a joke.[3]

Remark 1 *(Universal Anarchist theorem).* Every government is unjust.

Proof:
Let G be an arbitrary government. Since G is arbitrary, it is certainly unjust. Hence, by universal generalization, every government is unjust. ■

This is of course also a fallacy of equivocation.

4.2.3 Some remarks about ∃-elim and ∀-int

There are some remarks about ∃-elim and ∀-int which are commonplace in Natural deduction circles.

(i) They are "dual";
(ii) They have the same *side conditions*; and
(iii) ∀-int is easier to understand (or perhaps I mean *accept*) than ∃-elim.

(i) In what sense are they dual?

∃-elim says that if you can deduce p from $F(a)$, then you can deduce p from $\exists x F(x)$. (Modulo side conditions) In the following proof we use the rule of *classical negation* which says that if we can derive a contradiction from $\neg p$ then we may deduce p.

$$\dfrac{\dfrac{\dfrac{\dfrac{[\neg p]^1}{\vdots}}{\neg F(a)}}{(\forall x)\neg F(x)}\;\forall\text{-int} \qquad \neg(\forall x)(\neg F(x))}{\dfrac{\bot}{p}\;\text{classical negation (1)}}\;\to\text{-elim} \tag{4.1}$$

and compare it with the proof by ∃-elimination on p 40. That proof contained a deduction of p from $F(a)$. But if there is such a deduction then, by contraposition, we can easily obtain from it a deduction of $\neg F(a)$ from $\neg p$.

The proof displayed above shows how, if you can deduce p from $F(a)$, then you can deduce p from $\exists x F(x)$ (Modulo the same side conditions) using ∀-int (instead of ∃-elim) as long as you have a rule of classical negation and accept that $\exists x F(x)$ is the same as $\neg(\forall x)(\neg F(x))$. (*You* might, Dear Reader, but not everybody does.)

Moral: if you are happy with ∀-introduction, you should be happy with ∃-elimination.

[3]Thanks to the late Aldo Antonelli `https://en.wikipedia.org/wiki/G._Aldo_Antonelli`.

(ii) They have the same side-conditions

t not free in premisses!!!

(∀-elim and ∃-int have no side-conditions.)

Do we mention them o/w? Make a joke about how the wags call them *off-side* conditions, co's nobody understands them.

(iii)

The significance of the duality lies partly in point (ii). If you are happier with ∀-int than ∃-elim then you might find the demonstration in the proof on p. 40 above helpful in making ∃-elim acceptable.

4.2.4 Back to The Syllogism

What has all this got to do with The Syllogism? Answer: if we try to corall an argument that uses The Syllogism into anything like Natural Deduction form then we find occurrences of the rule of ∃-elimination appearing wherever we needed The Syllogism. Here is a proof of The Syllogism in Natural Deduction style.

$$\dfrac{\dfrac{\dfrac{(\forall x)(F(x) \to p)}{F(a) \to p}\;\forall\text{ elim} \qquad [F(a)]^{1)}}{p}\;\to\text{-elim} \qquad (\exists x)(F(x))}{p}\;\exists\text{-elim}(1) \tag{4.2}$$

Suppose we have a proof of a proposition p and at some point in the proof we need there to be a thing which is F. Specifically, if a is such a thing then the deduction $\mathcal{D}_1$ will lead us from $F(a)$ to the conclusion p as desired. We don't care which thing is F (and we may not even know) but we do at least know there is one. This is the assumption '$\exists x F(x)$'.

There will be an instance of the rule of ∃-elimination at any stage in the proof where our construction needs a thing that is F and we know there are some but we haven't identified any. We will be applying ∃-elim to the formula '$\exists x F(x)$'. On our analysis, this is where we make a (single) choice.

With this in mind let us look closely at the proof of theorem 1.

4.2.5 Another look at the proof of theorem 1

It is a proof by induction on 'n' that every unordered n-tuple of nonempty sets has a choice function. The base case is clear enough, so let us consider the induction step.

Our induction hypothesis is that every set $\mathcal{X}$ of nonempty sets with $|\mathcal{X}| = n$ has a choice function. We wish to deduce that every set $\mathcal{X}$ of nonempty sets with $|\mathcal{X}| = n + 1$ has a choice function.

Let $\mathcal{X}$ be a set of nonempty sets with $|\mathcal{X}| = n + 1$. Since $|\mathcal{X}| = n + 1$, there is an $X \in \mathcal{X}$ with $|\mathcal{X} \setminus \{X\}| = n$. Choose one such X. (Clearly we are going to be doing an ∃-elimination using this X). Since $|\mathcal{X} \setminus \{X\}| = n$ we apply the induction hypothesis to $\mathcal{X} \setminus \{X\}$ to infer that it has a choice function. (This conclusion is going to be the premiss of another $\exists$-elimination)

Now suppose f is a choice function for $\mathcal{X} \setminus \{X\}$. By assumption, X is a nonempty set. Then, whenever $x \in X$, we have that $f \cup \{\langle x, X\rangle\}$ is a choice function for $\mathcal{X} \cup \{X\}$. X is nonempty by assumption, so there is, in fact, such an x so (by $\exists$-elimination – again!) there is a choice function for $\mathcal{X}$. But $\mathcal{X}$ was an arbitrary $n+1$-sized set so, by UG, every $i+1$-sized set has a choice function.

Thus the inference from "Every set with n nonempty members has a choice function" to "Every set with $n+1$ nonempty members has a choice function" makes three uses of $\exists$-elimination. Thus, if n is a concrete natural number we can prove that every set with n nonempty members has a choice function using $3n$ uses of $\exists$-elimination. And we can prove it in some very very minimal set theory without even any arithmetic. The proof of theorem 1 uses only three instances of $\exists$-elim but they sit inside an induction loop and the proof wherein that loop resides is in a stronger system with at least some arithmetic.

Thus the fact that you can make *one* choice is a gift of first-order logic (*constructive* first-order logic indeed – we don't need excluded middle or anything even remotely suspect like that); the fact that you can make *any concrete finite number of* choices, too, is a gift of first-order logic (yes – *constructive* first-order logic, again); the fact that you can make any arbitrary finite number of choices is a theorem of (a suitably spiced up) arithmetic. You prove by induction on n that, for every n, you can make n choices. But this is not a theorem of pure logic (it can't be: pure logic does not know the concept of arbitrary natural number). What about infinitely many choices?

4.2.6 Infinitely many Choices

Taking up the idea that

(i) there is a correspondence between constructions ("recipes" à la Euclid) and proofs, and that
(ii) making a choice corresponds to performing a $\exists$-elimination,

what sort of proof might correspond to a construction that makes infinitely many choices? Well, it's going to have to be an infinite proof, since one has to somehow fit in infinitely many uses of $\exists$-elimination. In this frame of mind let us look at the proof that a union of countably many countable sets is countable.

It will use the rule of infinite conjunction and have a $\exists$-e in each branch. It would look a bit like this:

The application of infinite conjunction is subsumed by the rule for zigzag. should Say something about ...

$$
\begin{array}{c}
\dfrac{\dfrac{\begin{array}{cc}
\left[\begin{array}{l} c_1 \text{ is an enum-} \\ \text{eration of } x_1 \end{array}\right]^1 \ldots & \left[\begin{array}{l} c_i \text{ is an enum-} \\ \text{eration of } x_i \end{array}\right]^i \ldots \\
\vdots & \vdots \\
\vdots & \vdots \\
\multicolumn{2}{c}{\text{zigzag}\{c_i : i \in \mathbb{N}\} \text{ is an enumeration of } \bigcup_{i \in \mathbb{N}} x_i}
\end{array}}{(\exists e)(e \text{ is an enumeration of } \bigcup_{i \in \mathbb{N}} x_i)}\ \exists\text{-i} \qquad E_1,\, E_2,\, \ldots E_i \ldots}{(\exists e)(e \text{ is an enumeration of } \bigcup_{i \in \mathbb{N}} x_i)}\ \exists\text{-elim}
\end{array}
$$

where 'E_i' is short for '$(\exists e)(e$ is an enumeration of $x_i)$'.

Figure 1

The single occurrence of $\exists$-elimination discharges simultaneously all the infinitely many assumptions "c_i is an enumeration of x_c".

Return to the topic of countable unions of countable sets, and the proof in Figure 1 above. If you have an f such that $\forall i \in \mathbb{N}$, $f(i)$ is a counting of x_i then the corresponding construction simply zigzags through the c_i in the way you always thought you were supposed to, and allowed to.

If, for each $i \in \mathbb{N}$, we can actually supply a counting c_i, then we can simplify that proof to:

$$
\begin{array}{cccccc}
\begin{array}{l} c_1 \text{ is a} \\ \text{counting} \\ \text{of } x_1 \end{array} & \begin{array}{l} c_2 \text{ is a} \\ \text{counting} \\ \text{of } x_2 \end{array} & \begin{array}{l} c_3 \text{ is a} \\ \text{counting} \\ \text{of } x_3 \end{array} & \ldots & \begin{array}{l} c_i \text{ is a} \\ \text{counting} \\ \text{of } x_i \end{array} & \ldots \\
\vdots & \vdots & \vdots & & \vdots & \\
\multicolumn{6}{c}{\text{zigzag}\{c_i : i \in \mathbb{N}\} \text{ is a counting of } \bigcup_{i \in \mathbb{N}} x_i}
\end{array}
$$

Figure 2

and the infinitary occurrence of $\exists$-elim has disappeared. The proof is still infinitary, but it makes no choices. All the x_i are now *counted sets* not mere *countable sets*. And a counted union of counted sets is, as they say, counted.

4.2.7 Maximal Formulæ

In a proof a formula is said to be a *maximal* formula iff it is both the conclusion of an introduction rule and the major premiss of the corresponding elimination rule. There are some technical terms in there for possible future reference; for the moment what matters is that these technicalities *direct our attention to formulæ that can be got rid of.* In this proof

$$\dfrac{\dfrac{\begin{matrix}[p]^1\\ \vdots \\ q\end{matrix}}{p \to q}\ \to\text{-int (1)} \qquad p}{q}\ \to\text{-elim} \tag{4.3}$$

the formula ‘$p \to q$’ is maximal within the meaning of the act (since it is the conclusion of a $\to$-introduction rule and the major premiss of a $\to$-elimination rule. And there is an obvious manipulation that will turn the proof into

$$\begin{matrix} p \\ \vdots \\ q\end{matrix} \tag{4.4}$$

...which has the same premisses and the same conclusion but less stuff in the middle; the *maximal formula* has gone. That maximal formula was the conclusion of an $\to$-introduction and the premiss of an $\to$-elimination. Of more concern to us is the following example of a maximal formula that is the conclusion of an $\exists$-introduction and the premiss of an $\exists$-elimination.

$$\dfrac{\dfrac{\begin{matrix}\vdots\ \mathcal{D}_2\\ F(a)\end{matrix}}{\exists x F(x)}\ \exists\text{-int} \qquad \begin{matrix}[F(a)]^{(1)}\\ \vdots\ \mathcal{D}_1 \\ p\end{matrix}}{p}\ \exists\text{-elim (1)}$$

The formula ‘$\exists x F(x)$’ is maximal in the sense that it is the conclusion of an $\exists$-int rule and the premiss of an $\exists$-elim rule. There is nothing to stop us taking the proof $\mathcal{D}_2$ (whose last line is $F(a)$) and moving it bodily to the right to place it above the assumption $F(a)$ ... at which point we no longer need the ‘$\exists x F(x)$’ and we have the new, simpler proof[4]:

---

[4]A note for proof-theory sophisticates: the formula ‘$(\exists e)(e$ is an enumeration of $\bigcup_{i\in\mathbb{N}} x_i)$’ in Figure 1 is *not* a maximal formula despite being both the conclusion of an $\exists$-int *and* the premiss of an $\exists$-elim.

$$\begin{array}{c} \vdots\ \mathcal{D}_2 \\ F(a) \\ \vdots\ \mathcal{D}_1 \\ \hline p \end{array}$$

Figure 2

The difference between this new proof and the old one is that we have disappeared one occurrence of $\exists$-elimination. And that matters to us because the new construction corresponding to the new proof will have one fewer choice. Thus *the elimination of a maximal formula* corresponds to an operation on constructions that removes one choice. This is the fact that if you have a definable witness you don't need even one choice.

4.2.8 "But I'm making only one choice!"

We need to have a reply to the person who says.

> "Very well, as you say, *one* choice is OK. So I make a single choice of a selection function on the family of sets-of-countings of the sets in the family of counted sets. So that's OK!"

They are correct: it is indeed only one choice. But that doesn't make it OK; we can make one choice from any nonempty set, agreed; but how do we know that that set of such selection functions is nonempty? It's one thing to show that the collection of such functions constitutes a set – and we can contract that task out to our set comprehension axioms – but it has to be nonempty. And the assertion that it is nonempty is *prima facie* a consequence of AC, not of the comprehension axioms.

Relevant in this connection is the fairly standard exercise for beginners with AC to demonstrate that the Ordering Principle (OP) implies AC for sets of finite sets (AC_{fin}).

rewrite between here and the end of the section

Every set of finite sets has a choice function	(AC_{fin})
Every set can be given a total (aka *linear*) order	(OP)

The ordering principle says that every set has a *total* order (not a *well*order – it's weaker than AC).

A common mistake is to say that there must be a choice function on the given set of finite sets, because each of the sets being picked from is finite, and every finite set can be wellordered, so we know which element to pick: the first one. The warning sign is that we have not used the assumption that every set is totally ordered. The mistake is the by-now-familiar equivocation on datatypes. The finite sets before us are `sets` not `ordered sets`, or `lists`. We need to expand them all into `ordered sets` somehow. Can you simultaneously order all the A_i by ordering only one thing? When you put it like that, it becomes obvious what you have to do: totally order $\bigcup_{i \in I} A_i$. OP tells you that this set has a total order. Then, for each $i \in I$, the restriction of that order to A_i is a wellorder (since A_i is finite, and every total order of a finite set is a wellorder) and you plump for the first element in the sense of that wellorder.

It does involve a choice – a single choice, and an instance of ∃-elimination. Let R be an arbitrary total order of $\bigcup_{i \in I} A_i$. Then there is a choice function on $\{A_i : i \in I\}$. But (by OP) there is a total order of $\bigcup_{i \in I} A_i$. So there really is a choice function on $\{A_i : i \in I\}$, by ∃-elimination.

So OP implies AC_{fin} without any use of AC. Blend this next bit in

Consider what you can expect when you are told $(\forall x)(\exists y)\phi(x, y)$. If you are very lucky there will be a Fairy Godmother who, whenever you say 'x' to her nicely, will reply with a y such that $\phi(x, y)$. She has a method for doing this, but she doesn't tell you what it is; she's a fairy, after all. You know that $(\forall x)(\exists y)\phi(x, y)$, but she – unlike you – is actually acquainted with a function f such that $(\forall x)\phi(x, f(x))$. That is to say: the two assertions $(\forall x)(\exists y)\phi(x, y)$ and $(\exists f)(\forall x)\phi(x, f(x))$ are different assertions.

Notice that the fairy godmother only promises to totally order any one set. That is what the '∀' is doing. She's not promising to order all sets, each and every one of them, just any one that you show to her. She has an engine that, on being given a set, will order it (She may even be that engine herself: we don't know) for you. She doesn't make you a present of the engine, merely of its output, which she dribbles out one at a time. Does she, as fairies routinely do, offer to give you *three* wishes? In fact she will even give you *finitely many* wishes. However she does *not* undertake, on being given $\{A_i : i \in I\}$, to totally order every A_i. (Unless I is finite, of course). That's because OP is $(\forall x)(\exists y)(y$ is a total order of $x)$ rather than the rather scary (infinitary!) expression

$$(\forall x_1)(\forall x_2)(\forall x_3)\cdots(\exists y_1)(\exists y_2)(\exists y_3)\cdots \bigwedge_{i \in \mathbb{N}} (y_i \text{ is a total order of } x_i)$$

4.3 Executive Summary

If you always find yourself thinking you are making choices and therefore needing AC, what is probably going on is this. Yes, you are making a choice (as – for example – with formula (A) on p 35 above) but no, you don't need AC, because you are making only a *single* choice. A single choice is justified by ∃-elimination.

The rule of ∃-elimination gives us a licence to make choices; the ability to make choices give us a licence to use the rule of ∃-elimination. If you are happier with one of these licences than the other you can start with the one you feel happier with and venture thence towards the other. There is a parallel here with the situation concerning the two rules of ∃-elimination and ∀-introduction; they are equivalent and if – like most people – you are happier with $\forall$-introduction than with $\exists$-elimination you can start from $\forall$-introduction, use it to justify $\exists$-elimination and thence the licence to make finitely many choices.

4.3.1 Coda

To a certain extent this chapter is addressed to the concerns of people who realise that in their mathematical praxis they are making choices all the time but mistakenly think they need AC to make them. Such people can find it helpful to become acquainted with the rule of $\exists$-elimination. I don't want to exaggerate the usefulness of the idea that a choice corresponds to an occurrence of $\exists$-elimination, and I am happy to leave the correspondence at a fairly informal level. However it does help us make sense of a number of things. Finitely many choices are always all right because a finite proof can contain finitely many applications of $\exists$-elimination. Infinitely many choices are problematic, but then infinite proof objects (and if a proof is to contain infinitely many $\exists$-eliminations it will perforce be infinite) are problematic. Theorem 1, which says we can make n choices for any $n \in \mathbb{N}$ (and does not use the axiom of choice) is provable because it has only finitely many application of $\exists$-elimination. They punch above their weight because they occur inside an induction loop. Finally, constructions-relying-on-having-a-widget do not need a choice when there is a canonical widget available, and in those circumstances the proof associated with the construction does not have an $\exists$-elimination at the crucial point.

To pursue this parallel deeper and further and more seriously than is done here one would need to study infinitary proof objects, and such things are not for the nervous beginner (nor the overconfident beginner) being unfortunately much trickier than they appear. Even *finitary* proof theory remains a fairly niche subject familiar only to theoretical computer scientists of a particular stamp. I harbour the hope that the neat correspondence between making-proofs and eliminating-existentials might encourage missionary-position mathematicians to cast a close eye over Proof Theory.

Cognoscenti may be struck by the lack here of any discussion of the ϵ-calculus. This is in part because the ϵ-calculus is a can of worms, but principally because, altho' a discussion of the axiom of choice can help to shed some light on the ϵ-calculus, there seems to be less traffic in the opposite direction. Our aim is instead to explain the functioning of the Axiom of Choice in Ordinary Mathematics, and the nexus between the ϵ-calculus and the Axiom of Choice is not illuminating for the mathematician on the Clapham omnibus (or the Wellington cable car).

Chapter 5

AC is the principle that monotone nondeterministic transfinite processes will conclude successfully

> *Every monotone [continuous] deterministic process can be completed; trivial in the finite case; needs Hartogs' thm/Bourbaki-Witt in the infinite case;*
> *Every monotone nondeterministic process can be completed–AC;*
> *Some non-monotone nondeterministic processes cannot be completed.*

My take on AC is that it says monotone nondeterministic transfinite processes will complete successfully. It's a standard exercise (for the lecturer, not the student!) in set theory courses to show that monotone deterministic transfinite discrete processes complete successfully. (It's actually quite tricky to show properly and details are routinely glossed over, but never mind). Discrete nondeterministic finite processes complete successfully – you can successfully fill in the numbers on your lottery card by tossing coins – but infinite nondeterministic processes are of course a problem.

Transfinite processes indexed by the ordinals have been around in mathematics for ages. Lots of recursive constructions of transfinite length. Nobody turns a hair.

To properly understand AC we need the notion of **discrete transfinite process**. Wossat? Let's start with a nice example.

Start with a closed set of reals. Delete its isolated points to obtain its *derivative*. This, again, is a closed set. Is it perfect-closed? That is, does it have any isolated points? It might, in which case it is not perfect-closed. Very well, remove them to obtain the (second) derivative. It's not had to see that there are closed sets for which we can take a derivative infinitely often without reaching a perfect-closed set at any finite stage. Is there a good notion of a limit of this process? Yes there is, because the process is monotone – we are deleting points at each stage and never putting anything back. So the intersection of the finite derivatives is a sensible thing to consider. Is it perfect-closed? Perhaps not, but we can always keep going.

What we have here is an example of a transfinite monotone deterministic process. This particular transfinite monotone deterministic process will terminate when it has reached a perfect closed set, since a perfect closed set is its own derivative. The fact that it will terminate is very important, but it is by no means obvious that it will – even tho' we take it for granted.

H I A T U S

Reference needed

Dauben, [5]

Cantor's construction is nice in two ways: it is *monotone* and it is *deterministic*. It is monotone in the sense that nothing that is taken away is ever put back. One could say that, thought of as a function from dates/times (or ordinals) to sets (or rather sets^{op}) it obeys $t_1 \leq t_2 \to f(t_1) \leq f(t_2)$. It is *deterministic* in the sense that the person executing the process never has to make a choice about which points to remove next. Processes like this that are deterministic and monotone can be unproblematically executed. If they have a termination condition they will terminate. That is the extra–set-theoretical meaning of the lemma of Hartogs' that says that for every set X there is a von Neumann ordinal that will not inject into X: *if a monotone deterministic process ever fails to terminate properly it's not because you have run out of ordinals*. It may crash for other reasons of course: for example, the thing you are trying to construct might not exist.

In vol 1 we talk about the mathematical meaning of some set-theoretic assertion, Hartogs' among them

[Consider the project to find an set equal to its power set, in ZF, using Bourbaki-Witt. It fails, and you run out of ordinals, but *that's not why it fails*: it fails beco's of unstratified separation which tells you that there is no such fixed point

Try doing it in NF. It fails beco's the recursion is unstratified and cannot be executed.]

What sort of situation do we find ourselves in if we drop determinism (while keeping monotonicity)? Let $\mathcal{X} = \{X_i : i \in \mathbb{N}\}$ be a family of sets, all with at least two members. Let us consider the project of picking one member from each X_i, with a view to obtaining a function $f : \mathbb{N} \to \bigcup \mathcal{X}$ satisfying $f(i) \in X_i$ for all $i \in \mathbb{N}$. The discrete transfinite process before us is pretty straightforward: examine the X_i in turn, starting at X_0, and pick a member from each – which we can do because (by assumption) they are all nonempty[1]. This process is nondeterministic because at each stage there is more than one thing we can pick. It's also clear that it is monotone – at least in the sense that, as we go

[1]But `set` does not support "Give me a random member or an error message if you are empty. Find a way round this, presumably using The Syllogism.

along, we are building a function (a set of ordered pairs) and we add ordered pairs to this function (so that at stage n we have n pairs, all of the form $\langle X_i, x_i\rangle$ with $x_i \in X_i$) and we never remove any pairs. So the process is monotone and nondeterministic; can it be completed?

The first point to make is that the elementary set-theoretic apparatus we have used in persuading ourselves that the process can be run successfully for n steps (for every $n \in \mathbb{N}$) is not enough by itself to prove that the process can be completed. "The process can be completed" simply does not follow from the fact that it can be run for n steps for every n.

For one thing "I can do this n times for every n" does not imply that "For every n, if I have done it n times then I can do it an $n+1$th time". Here is a simple counterexample.

Suppose I am in a room with countably many doors, any one of which I may enter. The nth door opens into a tunnel of length n. So, for any n, I can take n steps away from the origin. However it isn't true that if I have taken n steps I can take an $n+1$th; I might be in the nth tunnel.

But even if "I can do this n times for every n" implied that "For every n, if I have done it n times then I can do it an $n+1$th time" we still wouldn't be home. This is the part that is hard to stomach. To illustrate this gap we consider the Infinite Wardrobe from Russell's discussion of the axiom of choice in [25]. Let us assume that the collection of socks is infinite but Dedekind-finite. (It is known that this is a consistent possibility). The sock collection is divided into countably many pairs $\{S_i : i \in \mathbb{N}\}$. The millionaire's Valet and Maid play a game. They pick socks from the collection, playing alternately, the Valet going first. (Perhaps"indicate" would be better than 'pick'.) The first servant to indicate a sock already indicated earlier in the game loses. Draws are impossible because the game cannot go on for ever: an infinite play would be a countable subset of S, and by assumption there is no such subset. The Valet has a nondeterministic winning strategy: indicate a sock not yet indicated: there are always plenty. Indeed his strategy is to indicate a sock – either will do – from the first pair not yet used. Incredibly the Maid has a winning strategy too, and hers is actually *deterministic*! All she has to do is indicate the sock that is the mate of the sock pointed to by `I` in the move to which she is replying.

> In fact she is better off even than this. Whenever it is the Valet's turn to play, an even number of socks have been indicated, and when it is the Maid's turn to play, an odd number of socks have been indicated. Therefore there is at least one unchosen sock whose mate has been chosen; she should pick the first such sock [2]. Thus not only does she have a winning strategy in the sense that there is a rulebook she can follow to win, but even if she strays from the winning path she can always find her way back.

What matters for us, the bystanders, is not that the Maid has a winning strategy, but rather the fact that the Valet, despite the fact that whenever he has made n plays can always make an $n+1$th, nevertheless – for all that – *he cannot "keep going" for ever*.

---

[2] This may remind the reader of the proof that there infinitely many primes congruent to -1 mod 4; in both cases you prove that a finite set is nonempty by showing that its cardinality is odd

This isn't a *proof* that "For every n, if I have done it n times then I can do it an $n+1$th time" does not imply "I can keep going for ever"; it's a vivid *illustration*. The proof comes from the independence of AC.

Of course in the kind of cases where people think they can *just keep going* don't have this obvious cut-off feature of our tunnel example. But there may be cut-offs nevertheless. The best response to "Why can i not just keep going?" is probably another question "How do you propose to do that?" or "What is it that counts as *keeping-going*?". These people do not realise there is a problem, and that is because they are overlooking the huge difference between *deterministic* transfinite processes and *nondeterministic* transfinite processes. If the process is deterministic then you can, indeed, just "keep going". If the process is nondeterministic then the various bits-of-processes between which you are undetermined have to cohere.

What one needs is a kind of coherence principle, something that says that all the finite partial functions can be glued together to obtain an infinite (total) function.

One needs to say: there is a good notion of the completion of a transfinite monotone deterministic process – just take the union of the stages. But if it's nondeterministic there is no good description of the limit beco's there is no good description of the stages – co's they're nondeterministic.

Naturally – as the appearance of the particle 'task' in 'supertask' reveals, each supertask has a purpose, to produce something. There is always a question about what the workshop looks like after the supertask has been completed or been abandoned, and there are set-theoretic principles that have something to tell us about this. There are two set-theoretic principles that bring us hope that a supertask will complete successfully. Hartogs' lemma and Zorn's lemma.

Hartogs' lemma tells us that if the supertask doesn't do what it's supposed to do then it's not beco's we have run out of ordinals. This is beco's Hartogs' enables us to prove Bourbaki-Witt. One hesitates to say that that that is the true meaning of Hartogs' lemma since the kind of completion that it promises holds also in NF, where Hartogs' lemma fails, but it's not seriously misleading to say it. Perhaps what one should say is that Hartogs' Lemma is the set theoretic principle which *in ZF* assures us that we never run out of ordinals, and that therefore anything declared by a recursion can be declared by a ecursion on the ordinals

We talk about this in Book 1

To make sense of a supertask of transfinite length we need to know, at every limit stage λ, what the situation is in which you make your λth move. For this one needs a kind of continuity. Montonicity helps.

Cumulative hierarchy monotone and deterministic, but not cts, so no solution. $\alpha \mapsto \mathcal{P}(\bigcup_{\beta<\alpha} V_\beta)$ is monotone but not cts.

Bourbaki-Witt sez that cts + monotone + deterministic $\rightarrow$ success.

Do we need to think about confluence in the context of nondeterminism?

[with thompson's lamp the work-in-progess is not an object but a proposition, a truth-value]

To present Zorn as a principle about completion of supertasks in the same way we need to make three distinctions.

(i) We need to distinguish between deterministic and nondeterministic supertasks.

(ii) We need to distinguish between monotone and non-monotone supertasks.

(iii) We need to distinguish between monotone supertasks that are cts and those that are not. (No good notion of a cts nonmonotone supertask)

So a supertask will be $\pm$ deterministic, $\pm$ monotone and – if monotone – $\pm$ continuous.

So there are 6 possibilities, which we consider in order (roughly) of increasing difficulty.

So we should have some sections here called

Deterministic and monotone and continuous
Deterministic and monotone and discontinuous
Deterministic and nonmonotone
Nondeterministic and monotone and continuous
Nondeterministic and monotone and discontinuous
Nondeterministic and nonmonotone

That's 6 possibilities

5.0.1 Deterministic, Monotone and Continuous

Classic early example is Cantor's construction of a perfect closed set starting from a closed set by repeatedly taking the derivative. It's deterministic (every closed set has precisely one derivative) and it's monotone (the derivative of X is a subset of X). This process is deterministic and monotone. It is also continuous at limits (we take intersections)

constructing the transitive closure of a relation R as $R \cup R^2 \cup \ldots$

Infinitary $\bigvee$ and $\bigwedge$.

These are cts.

Contrast with the attempt to obtain a set identical to its own power set by iteration. It's deterministic and monotone but it's not cts at limits. We not only find that the process doesn't deliver a fixed point; we *know* this process cannot succeed.

Consider the project of obtaining a minimal transitive relation extending R by picking up ordered pairs $\langle x, y\rangle$ and $\langle u, v\rangle$ from the graph of R and adding $\langle x, v\rangle$ whenever $y = u$. This is monotone, continuous and completes successfully despite being nondeterministic and we can prove the existence of such relations without using AC.

So how does one explain why the supertask to construct a minimal transitive extension of a relation R is going to succeed? The key observation is that *if* there are any minimal transitive relations extending R then there is a unique one. So the attempt to construct one by nondeterministic supertask must succeed *despite it being nondeterministic*. First one shows that if S_1 and S_2 are two such relations then $S_1 = S_2$. This is in striking contrast to the attempt to find a choice function on a countable family. Both these supertasks are nondeterministic, monotone and continuous. So why does one need AC and the other not?

The difference between these two situations is not that in one case you need the axiom of choice and the other you don't; both of these supertasks are nondeterministic and need the axiom of choice. The point is that the supertask that constructs the transitive closure is not the reason why the transitive closure exists: it exists for totally other reasons: it's an inductively defined set and exists for the reasons that inductively defined sets always exist (whatever they are). The choice function on the infinite famil

Truncation here...

You don't need the axiom of choice to show that

(i) when the supertask to create $t(R)$ completes it will have constructed $t(R)$; What you need AC for is to show

(ii) that the supertask does in fact complete. How can i tell? If i look at the various stages of the supertask i can recover a wellordering of $dom(R)$ from the logbook.

Do we need to consider confluence and directedness?

5.0.2 Deterministic and Nonmonotone

Infinitary `XOR`.

Thompson's lamp

5.0.3 Nondeterministic

This is where AC comes in. Given a sequence $\langle A_i : i \in \mathbb{N} \rangle$ of nonempty sets, how do we feel about the supertask of obtaining a choice function for the sets in the sequence, in the absence of any exploitable order information on the sets which tells us which of their members to pick? We proved by induction (theorem 1 on p.38) that, for any n, we can find a choice function on $A_0 \cdots A_n$. Can we get a choice function for *all* of them?

Failure of continuity at limit stages is part of the problem

Take care when assigning your candidate supertask to one of these classes.

In most cases where it is clear what happens at a limit stage it's beco's the process is cts, but not always (power set, for example). With Thompson's lamp it simply isn't clear what happens at the limit stage. Nor with finding a path thru' the cc-poset of partial selection functions. The role of AC seems to be to refine a nondeterministic process to a deterministic one. Once you make that process deterministic it becomes cts.

Talk of supertasks entered the philosophical literature with *Thompson's Lamp* (see [29]. Mathematicians will know Littlewood [20]. See also [4]. There is a very readable discussion of supertasks in `https://plato.stanford.edu/entries/spacetime-supertasks/`. It's readable, but not very helpful to readers of this book. Do not be distracted!

At time $t = 0$ the lamp is off. At time $t = 1/2$ it is switched on, at time $t = 3/4$ it is switched off, then on again at time $t = 7/8$ and so on. The puzzle then is: what is its state at time $t = 1$? The problem is supposed to be that there are compelling reasons to believe that it cannot be on (because every time it is switched on before time $t = 1$ it is subsequently switched off) and similarly it cannot be off. People like us will say that, because the Thompson's Lamp process is discontinuous, what it does *near* $t = 1$ tells us nothing about what it does *at* $t = 1$, so there is no problem. They are right to say that, but to say that is to miss the larger point that there is a huge assumption in the background that the process *can be completed!*, or perhaps I should say that *sense can be made of the idea of the process being completed.*

This answer is so obvious that some ingenuity is required of the puzzle designers if the reader is to be inveigled into thinking there is a problem. I think it may be that there is a background assumption that all discontinuities have to be *isolated*.

Of course throwing a switch requires a certain minimal amount of energy, so the Thompson's Lamp thought-experiment would involve an infinite amount of energy. So it's not realistic, and so it must be an allegory, a *mock-up* of the *real* problem. So what is the real problem? Nothing interesting, as it happens.

If the reader feels that theorem 1 justifies the claim that every *countable* set has a selection function then (s)he is probably thinking that a selection function for a countable set can be obtained by performing the following supertask. First count the set (and by assumption this can be done – the set is countable) so that it has become $\{X_i : i \in \mathbb{N}\}$ and then construct a $\subseteq$-increasing sequence of choice functions for longer and longer initial segments of the ordering by acting out the induction in the proof of theorem 1. Completion of this – nondeterministic – supertask would, indeed, give us a choice function for $\{X_i : i \in \mathbb{N}\}$. But that is to say that the assumption that we can complete this supertask implies the axiom of choice for countable sets.[3] So we seem to be appealing to a principle that supertasks can be completed. When is this assumption safe?

All the supertasks considered in the literature have the feature that the subtasks that compose them and are executed successively have an order-of-execution relation on them that is a wellordering. The assumption that the reals can be wellordered has a multitude of bizarre consequences [my favourite example – shown me by Imre Leader – is that there is a continuum-sized total ordering with no nonidentity order-preserving injection into itself] obtained typically by constructing the desired bizarre object by recursion on the hypothesised wellordering in a process that can really only be described as a supertask.

Actually it can be given a top-down presentation

In principle one might want to consider supertasks where the order-of-execution relation is not a wellordering, but in the current setting we have no need to consider such generality: all supertasks considered here will have an order-of-execution relation that is a wellordering. One could say that for us, here, "supertask" is simply a nice sound-bite (or strapline) for *discrete process of wellordered transfinite length.*

[3] The idea that countable choice relies on a supertask argument goes back at least as far as Schuster, [28], though I gather that he no longer holds the views expressed there.

5.0.4 Monotonicity and Determinism

BLEND IN

This might be a place to talk about how one can make sense of infinite sums and product in analysis beco's $+$ and $\times$ are monotone.

This use of the word 'monotone' here might sound funny to some, so let me illustrate with a couple of examples:

• The riddle of *Thompson's lamp* [29] see above ...

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

$$P \subseteq Q \to \bigvee P \geq \bigvee Q \tag{5.1}$$

and

$$P \subseteq Q \to \bigwedge P \leq \bigwedge Q \tag{5.2}$$

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

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

$$\langle \bigwedge_{i<n} p_i : n \in \mathbb{N} \rangle \tag{5.3}$$

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

and

$$\langle \bigvee_{i<n} p_i : n \in \mathbb{N} \rangle \tag{5.4}$$

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

– thought of as functions from natural numbers to truth-values – are monotone;

This has the effect that a conjunction (or disjunction) of an infinite set of propositions is well-defined[4]. The limit of 5.3 is `false` as long as even one of the p_i is false (and `true` otherwise) – the point being that if 5.3 ever takes the value `false` then all subsequent values are `false`. Analogously the limit of 5.4 is `true` as long as even one of the p_i is `true` (and `false` otherwise) – the point being that if 5.4 ever takes the value `true` then all subsequent values are `true`.

Contrast this with exclusive-or (`XOR`). `XOR` similarly is associative and commutative and so can be thought of as a function from finite-sets-of-propositions to truth-values. However there is no analogue of 5.1 or 5.2: the sequence

[4] Or perhaps – to be on the safe side – one should say that if one wants to think of the infinite con(dis)junction as being well-defined, it is obvious what the answer has to be. This kind of argumentation is familiar from elementary analysis.

$$\langle \mathop{\texttt{XOR}}_{i<n} p_i : n \in \mathbb{N} \rangle$$

is not monotone and we do not have a good notion of its limit; one cannot apply XOR to infinite sets of propositions; the expression:

$$\mathop{\texttt{XOR}}_{i \in \mathbb{N}} p_i$$

is not defined: no sense can be made of it.

BLEND IN

If you have both determinism and monotonicity then you have a good notion of what-happens-in-the-limit. One would like to be able to say that processes that are monotone and deterministic can always be completed, but that is not the whole story. There are monotone deterministic processes that do not complete: the supertask of creating a fixed point for the power set function by iterating transfinitely (trying to reach it "from below") will never work – because there is no fixed point! So it's not true that every monotone, deterministic process can be completed. What is true is that – because of Hartogs' lemma – *if it fails to complete it's not because you run out of ordinals*, it's because the thing you want isn't there. Quite *why* the thing isn't there remains to be explained in each case. A fixed point for $\mathcal{P}$ would be a paradoxical object (at least if you have unstratified separation).

Is this anything to do with the fact that $\mathcal{P}$ is not $\subseteq$-continuous? Perhaps... we talk about this in Book 1 and also earlier here.

A monotone deterministic supertask is a project that has a starting condition, and instructions to add something at successor stages, and at limit stages take the union of what you have got so far. There will be a termination condition that tells you when you have succeeded (or crashed). Such a project will always succeed (or crash). If the process never halts (because it neither succeeds nor crashes) then the collection of ordinals of stages you have been through will contain all ordinals, and the set of all ordinals is not to be borne.

Lots of examples: inductively defined sets as unions of stages – completely unproblematic.

However if the process is not deterministic then the collection of stages is not totally ordered and we cannot appeal to the Burali-Forti paradox. What might happen? Instead of getting one well-defined sequence of stages, we might find that we have a debouchement of ever-fragmenting sequences of stages none of which ever come to anything, like a mountain stream getting lost in rivulets in a desert.

Consider the process of trying to find a choice function for a countable family of sets, armed only with the finite axiom of choice, theorem 1.

More generally (if your process is not both monotone and deterministic) then it might crash. However, if it does, it's not because you have run out of ordinals.

If you lack one or other of monotonicity and determinism then bad things can happen, or you need special assumptions if you want to be sure that the supertask completes. Thompson's lamp is deterministic but not monotone, and the state of the lamp at time $t = 1$ is not well-defined; the supertask with AC_ω is monotone but not deterministic, and can't be completed without [some] AC.

Injecting the second number class into $\mathbb{Q}$

This will give us an example of an iterative construction where ... The advantage of this case is that the impossibility of injecting the second number class into the rationals is nothing to do with AC.

Pick up stuff from ordinals-forwell.tex

If you do it by recursion on countable ordinals you either squash it down every now (in which case it's not monotone) or you put copies of $\mathbb{Q}$ on the end (in which case it is monotone but possibly unsatisfactory in other ways, and give you an injection into the [rational version of] the Long Line instead of $\mathbb{Q}$)

Any proper initial segment of the second number class can be injected into $\mathbb{R}$ in an order-preserving way. In fact it can even be injected into $\mathbb{Q}$. Fix a countable ordinal α, and let I_α be the ordinals below α. $I_\alpha \times \mathbb{Q}$ lexicographically ordered is a countable dense linear order and accordingly is isomorphic to $\mathbb{Q}$. We send $\beta < \alpha$ to the zero in the βth copy of $\mathbb{Q}$ in the product. ■

Long experience teahes me however that students want to prove this by induction on the countable ordinals.

The challenge of embedding the countable ordinals into $\mathbb{R}$ looks a bit like a supertask. It looks like a monotone nondeterministic supertask, so AC (or more specifically Zorn) will tell us that it can be completed: The poset of order-preserving partial maps from the countable ordinals into $\mathbb{R}$ ordered by $\subseteq$ is chain-complete and therefore (by Zorn) has maximal elements no problem: the problem is that these maximal elements might not be defined on all countable ordinals – the [modified] identity map that sends the finite ordinal n to the real number n is maximal! The supertask can be completed all right (if we have AC); it's just that the result of that completion isn't what we wanted.

Attempt to inject ω_1 into $\mathbb{R}$ fails beco's it isn't monotone; you are always rearranging. If you do it without rearranging, but putting more copies of $\mathbb{R}$ on the end you succeed in injecting the second number class into ... the Long Line!

There is more to say about this case. You start off by sending 0 to something (0 probably) and then you always send the next ordinal to a real larger than all those you have used so far. A limits you use the limit of the reals you have used (you don't have to, but it makes it nice and smooth). If at any point you find that the image is unbounded you just plonk another copy of $\mathbb{R}$ on the end. That way you never have to change your mind about the destination of any ordinal. So it's monotone. Monotone and continuous – what's not to like!? What's not to like is that by the time you have embedded the whole of the second number class you have embedded it not into $\mathbb{R}$ but into The Long Line. `https://en.wikipedia.org/wiki/Long_line_(topology)`

Chapter 6

Two Distinctions

In this chapter we consider the finite/infinite distinction and a concrete/abstract distinction. These distinctions – and the relation between them – are gadgets that are helpful when we grapple with the axiom of choice.

6.1 The finite/infinite Distinction

There is a mediæval and renaissance literature on this distinction going back through the Jesuits and Galileo to Duns Scotus and possibly beyond. From about the 19th century (Cantor onwards) our understanding has stabilised.

Altho' this is not a book about constructive logic, a word or two will nevertheless be in order on the way in which the axiom of choice and excluded middle become entangled. It is said of both of them that they are true in the world of finite things (and that this fact can be established from first principles) while nevertheless being . . . *questionable* . . . beyond it. There is also the circumstance that the axiom of choice implies excluded middle. See [7].

People say that not understanding the finite/infinite distinction (by which i mean: casually assuming that the infinite resembles the finite in ways that in fact it doesn't) results in errors: no finite set can be equinumerous with a proper subset of itself so one might make the mistake of thinking that this goes for all sets, thereby failing to spot that *Dedekind-infinite* is a coherent idea.

That is a standard example, but there are two other examples which are of more concern to us here: excluded middle and the axiom of choice.

Intuitionists say that believing Excluded Middle to be correct stems from an error of assuming that the infinite slavishly resembles the finite. (Of course Excluded Middle holds in the world of finite things – tho' one has to be careful how one states this fact.)

What exactly do people mean when they say – as they so often do – that excluded middle holds in finite domains? Now it's a simple matter to set up possible world models that believe that every set is inductively finite but in which Excluded Middle fails, so what is meant must be a bit more nuanced. At the very least we have to assume Excluded Middle for atomics. The following appears to be true.

THEOREM 2
Let $\mathcal{L}$ be a first-order language with finitely many constant symbols $c_i : 0 < i \leq n$. Let T be the $\mathcal{L}$-theory T containing

(i) all constructive theses;
(ii) for each atomic formula ψ that mentions only the constants c_i, an axiom $\psi \vee \neg\psi$; and
(iii) an axiom $(\forall x) \bigvee_{0<i\leq n} x = c_i$.

Then T contains all classical theses of $\mathcal{L}$.

Proof:
The proof proceeds by induction on the subformula relation.
Clearly true for atomics by assumption.
We trade on the following observations:

- If $A \vee \neg A$ and $B \vee \neg B$ then distributivity gives us

$$(A \wedge B) \vee (A \wedge \neg B) \vee (\neg A \wedge B) \vee (\neg A \wedge \neg B).$$

The first three disjuncts give us $A \vee B$ and the last disjunct gives us $\neg(A \vee B)$, whereby we obtain $(A \vee B) \vee \neg(A \vee B)$. And $\wedge$ and $\rightarrow$ similarly. This means that the propositional fragment obeys classical logic.

- We can eliminate the quantifiers using the two T-theorems

$$(\exists x)(\psi(x)) \longleftrightarrow \bigvee_{0<i\leq n} \psi(c_i)$$

and

$$(\forall x)(\psi(x)) \longleftrightarrow \bigwedge_{0<i\leq n} \psi(c_i)$$

and then appeal to what we have just proved about the propositional fragment.

■

Another story is that paying insufficient attention to the distinction between finite and infinite leads one to think that AC is true everywhere because it is true in the finite world. (See theorem 1.) And of course there is this theorem of Diaconescu:

REMARK 2 *(Diaconescu [7]) AC $\rightarrow$ Excluded Middle.*

Proof:
Take AC in the form that every inhabited set of inhabited sets has a choice function. (We say x is inhabited iff $(\exists y)(y \in x)$ – rather than $\neg(\forall y)(y \notin x)$). $\bot$ and $\top$ are two distinct objects, thought of as `false` and `true`.
Let p be an arbitrary proposition. We assume AC and deduce $p \vee \neg p$.

Let $a = \{x : x = \bot \vee (p \ \wedge \ x = \top)\}$ and
$b = \{x : x = \top \vee (p \ \wedge \ x = \bot)\}$ and
$X = \{a, b\}$.

Then X is an inhabited set of inhabited sets and must have a selection function f. Therefore $f(a) \in a$ and $f(b) \in b$. Further we know $(f(a) = \bot) \vee (f(a) = \top)$ and $f(b)$ similarly. Thus there are four possibilities, so we can use proof by cases. If $f(a) = \top$ then p; if $f(b) = \bot$ then p. If neither of these happens—so $f(a) = \bot$ and $f(b) = \top$ – then at least $f(a) \neq f(b)$ so $a \neq b$. But $p \to (a = b)$, so we infer $\neg p$. Thus proof by cases gives us excluded middle.

■

However i offer the suggestion that the questionable confidence that AC and excluded middle hold in the Infinite World is not the result of recklessly extrapolating from the Finite to the Infinite but rather the result of recklessly extrapolating from the Concrete to the Abstract.

6.2 A Concrete/Abstract Distinction

This section is speculative: i am not claiming that there is a hard-and-fast distinction that can be used to provide a rigorous explanation. The hope is that what i say here will resonate with a kindred distinction that the reader's mind already houses, and that they may be able to use it in building a picture of their own. When Quine and Goodman [14] wrote "We do not believe in abstract entities." nobody quibbled that they didn't define what 'abstract' meant and what the difference was between the abstract things they didn't believe in and the concrete things that they did believe in. I am hoping that my readers can evaluate my observations *lazily* up to the point where the elucidation starts to shed some light on their picture of the axiom of choice. Then they can go their own way.

It's a bit like the celestial/secular distinction, which is not a good start.

1. The concrete world has agency; the abstract world does not.

2. The concrete world has a time axis; the abstract world does not.

3. The abstract world is deterministic and reproducible; the concrete world is *noisy*.

4. The world of the concrete has complete information (you can ask any question and get an answer);

   There is an eerie parallel with fictional worlds. It is famously underdetermined how many children Lady Macbeth had[1]). There are fictionalist

[1] `https://archive.org/details/in.ernet.dli.2015.215175`

Philosophies of Mathematics ... tho' i have not heard this parallel used as an argument in their favour.

the world of the abstract isn't noisy but it lacks complete information – in the sense that it is strongly typed: there are questions that just can't be asked or answered: ¿$3 \in 5$?

5. Truths about *concreta* are contingent; truths about *abstracta* are necessary.

6. *concreta* are located in time and space; *abstracta* are not.

Ad (1). Part of the appeal of AC is agentive. *Homo Faber* evolved in the world of *concreta* and likes to think in terms of agency. But the world of abstracta is not agentive so the appeal is spurious. *Homo Faber* think they can wellorder the universe beco's they are in the grip of the agentive fallacy. If you don't assume that you can wellorder the universe then the only way you will persuade yourself of AC is if you think it was wellordeed anyway/already. And why would you believe that?

Ad (3). Typically it is nondeterminism that causes us to reach for AC; however the world of abstracta is deterministic.

Types are abstract; tokens are concrete.

Part of the appeal of the old protestant gibe about angels dancing on the head of a pin is the way in which it imputes to the target a confusion between abstract (angel) and concrete (pin).

If you confuse the concrete-abstract distinction you can end up losing sleep over the Banach-Tarski paradox and the suite of hat-colouring problems.

Banach-Tarski arose from consideration of the dissection puzzles. Can you chop up a cube into finitely many pieces and reassemble them to get a regular tetrahedron of the same volume? Questions like this are not questions about infinite sets; they are questions about finitary combinatorics.

One of the things one wants to say about Banach-Tarski is that the real problem it throws up is not AC but the idea that regions of space are sets (of ordered triples of real numbers), an error that my colleague Jeremy Butterfield calls *pointillism*, but which might less graphically be characterised as the error of set-theoretic foundationalism, of thinking that everything is a set. If you stain pointillism with AC, you see these Banach-Tarski–shaped splodges under the microscope. AC is not the problem: AC is merely the stain that makes the pathology visible. Once you realise that Banach-Tarski is the result of pointillism you no longer care whether it follows from AC or not.

But another take on this is that you might be finding Banach-Tarski odd because you are failing to distinguish properly between the concrete and the abstract. It is possible to wonder (and some comic material has been developed along these lines) why there isn't – somewhere in gold-mining hill country – a reclusive mathematician (one thinks of Theodore Kaczynski `https:`

`//en.wikipedia.org/wiki/Ted_Kaczynski`) who for the sake of appearances sometimes pans gold but mostly practices his dark mathematical arts in doubling the size of nuggets ... and occasionally going down to a trading post in the valley to turn the nuggets into cash, to then buy groceries with the proceeds. The point about the decompositions in Banach-Tarski is not that they rely on the axiom of choice; the point is that they live in the world of *abstracta* rather than *concreta*; the pieces that the nuggets get chopped into are not even measurable! In contrast the dissection problems of the nineteenth century have clear roots in the concrete world, and the solutions can be applied to actual scissors and paper. Banach-Tarski beguiles us by sounding like the perfectly respectable dissection problems we know of old.

There are also hat problems. See [16]. Here is one i found in an example sheet i once taught from.

There is a counted set of people, each wearing a hat. There are two colours of hat. Each person can see all the other hats but they cannot see their own. Can the wearers devise a strategy that will result in all but finitely many of them being able to deduce the colour of their own hat? AC says yes! There is an equivalence relation of finite difference on the family of two-colourings of the set of hats: two colourings are equivalent iff they differ on only finitely many heads. By the axiom of choice there is a selection function on the set of equivalence classes. (In fact there are lots.) The hat-wearers agree to fix one such choice function. A hat wearer can look around them and observe all the other hats. This doesn't give them the *exact* colouring but it does at least tell them which equivalence class the actual colouring is in. The choice function will direct their attention to one particular colouring χ belonging to that equivalence class, and they then bet that the colour of their hat is the colour assigned by χ. They all bet on the same χ. The actual colouring in which the hat wearers find themselves differs from the colouring χ at only finitely many places, so only finitely many of them can be wrong.

This looks very odd indeed. (That is why it is a good example sheet question). It certainly looks at first blush as if the oddity is something to do with the axiom of choice. But it might be more helpful to see the oddity as arising from the trick of narrating a fact about uncountable choice as if it were a fact about concrete well-lit middle-distance medium-sized objects in the real world, wearing hats. Reflect that the hat wearers – who are supposed to be ordinary people after all, finite beings – nevertheless are supposed to be able to assemble in finite time the infinitely many assignments-of-colours-to-hats for their individual contemplation. And then they have to agree; how do i communicate to you that i want us to go for this colouring rather than that? There is no way an arbitrary colouring χ is a finite object so it is not clear how a finite being can comprehend it. No concrete creature has access to an infinite amount of information. The oddity is nothing to do with the axiom of choice; the oddity is the idea that the hat-wearers can process an infinite amount of information.

A bit vague from here on

Bits of Mathematics that concern the interface between concrete and abstract are always problematic. Probability! Computability!

People always say that reproducibility is the key notion in scientific practice; on this picture it's the key notion in mathematics. But then mathematics is the Queen of the Sciences.

Being seduced by the apparent possibility of supertasks is a consequence of not distinguishing the concrete from the abstract. Supertasks require agency, and there is agency in the world of concreta but not the world of abstracta.

Not grasping the concrete/abstract distinction results in taking supertasks seriously. Also thinking that the axiom of choice is obviously true. (the error lies not in thinking that it's *true*; the error lies in thinking that it's *obviously*[2] true.

6.3 Try not to confuse them

In this section we consider the errors of misidentification, and their consequences

[It's not blindingly obvious that the world of *concreta* is finite, tho' it might be ... it might be compact. But we should expand on the two mistakes, how they *are* mistakes, but not the *same* mistake, because the world of *concreta* is not the same as the world of finite things.]

Equipped with these two distinctions one finds a raft of possible mistakes to make. One might neglect to distinguish the concrete from the abstract, or neglect to distinguish the finite from the infinite. At times one might think that one of the distinctions is in play when it is in fact the other: All these mistakes have consequences, and some of those consequences involve the axiom of choice.

Let us start with Russell's parable of the countably infinite collection of pairs of shoes and the countably infinite collection of pairs of socks. The striking feature of this parable is that it seems obvious that the family of pairs of socks has a transversal. But the reason why that seems obvious isn't that the family is finite: it isn't! It is that socks are concrete (well, cotton or wool) objects and concrete objects can always be distinguished by grubby everyday details such as their position in space. In the everyday world we can wellorder everything. The pack of cards with which i play bridge with my friends is not an abstract object in the way that the game of bridge itself is an abstract object: the cards are grubby physical objects with creases and taco-sauce stains on the reverse that cardsharpers can use to tell which card is K♠ and which is A♣. So the thought is that the axiom of choice is true in the everyday world not beco's the everyday world is finite (tho' it presumably is) but because it is *concrete*, and we are all of us cardsharpers[3]. In the world of *abstracta* (by which i mean really hard-core abstracta, like the real numbers or the natural numbers) one can make a case for saying that it's not at all obvious that one can wellorder everything. The real line is the unique complete ordered field. Now there is nothing in that intelligence that tells you how to wellorder it. So, if you can wellorder it, it must

[2]There might be things that make it obvious, but this isn't one of them.

[3]Need to say something about P+NP in this context. Most interesting NP problems have extra structure which can be exploited.

be for some completely other reason. (Such as the axiom of choice, one might think. But if *that* is why the reals are wellordered then we can't use the fact that the reals are wellordered in an IBE argument for AC without begging the question.) But why might we expect there to be such a completely other reason? We wellordered the socks beco's they live in a noisy world of blooming buzzing confusion replete with extraneous detail that we cardsharpers can exploit. The reals don't live in that kind of world. So the point is not that AC is true-in-the-finite-world-but-not-true-in-the-infinite-world, the point is rather that it is true in the concrete world but not true in the world of *abstracta*.

[this may be where the connection to grue comes in. What are these on them! completely other reasons for believing that $\mathbb{R}$ can be wordered? Everything that we've examined can be wordered? But the only things that we can examine are in the world of concreta, where we can worder things by contemplating them.]

H I A T U S

Need to collate all the material on concealment, agency, formalisation and abstraction into one document which can then be inserted into ACpedagogy and dialethismarticle.tex. Mind-projection fallacy, tournament chess, supertasks, AxInf true only in the world of abstract objects. Believing you can perform supertasks (or even believing that performing a supertask is a thought-experiment worth thinking) is an error you make when you forget that you are in the world of abstract objects rather than in the world of *concreta* in which we actually live. No nondeterminism in the world of abstract objects, no *noise*. And no time axis: there are linear orders all right, but no time. Nondeterminism is failure of reproducibility.

One might be deploying in the world of *abstracta* intuitions that were evolved to help you cope with the world of *concreta* and which work well there. (And I do mean evolved: natural selection can work on any inherited strategies we might have for dealing with the concrete world; it is only at one remove that it can work on strategies for reasoning about abstracta.)

Homo Faber think they can wellorder everything by hand. Admittedly in the finite world they can (we saw in theorem 1 how AC is true in the finite world) but that isn't quite the point. The point is that AC seems to be true in the world of *concreta*. (Notice that Mathematical Physics never mentions AC). The world of concreta seems at first blush to consist of things that are not only finite but also quite comically small (for example it will apparently take only 10^{106} years for all the black holes in the universe to evaporate, and no self-respecting Ramsey theorist will so much as get out of bed for a number that puny) but one wouldn't want to bank on it being *exclusively* finite. After all, we might want to conceptualise *abstracta* in such a way that some of the things that do *not* get swept up into it (and therefore get left in *concreta*) are not finite: in principle the concrete world might be infinite. The point is rather that in the world of *concreta* in which we live there is always enough grubby detail to enable us to make the choices we want to make. It's not just Old Masters

We just drop *Homo Faber* in it

that have decayed varnish and coffee-stains that give them individuality. This is why it seems obvious (for example) that there is a choice function on the set of pairs of socks. Every sock has a *patina*. Russell's thought-experiment is a concretisation of the axiom of choice for socks, I mean pairs. It is the axiom of choice for pairs narrated in the world of *concreta*. Russell's thought-experiment makes us think about the difference between the world of *concreta* and the world of *abstracta*.

> What is so clever about Russell's parable is that it exploits our confusion of the abstract eith the concrete to make AC_2 look extremely plausible. It then pulls the rug out from under us by pointing out the difference between the shoes and the socks.

rhetoric has agency; logic doesn't.

When you abstract away from everyday life to the arcane world of abstract entities you leave behind (you *conceal*!) the fussy details that enabled you to wellorder things in the real world.

Two mistakes:

(i) Overestimating the extent to which an understanding of the infinite can be had by extrapolation from the finite;

(ii) Overestimating the extent to which an understanding of the abstract can be had by extrapolation from the concrete.

These two errors overlap because 'finite' and 'concrete' overlap. (ii) results in trying to express a lot of abstract mathematics as constructions. . . a mistake to which *Homo Faber* is particularly drawn. *H. Faber* evolved to live in the world of *concreta* and has habits that reflect that history; *H. Faber* likes constructions.

Both errors result in a blithe assumption that an infinite monotone abstract construction (a supertask) can be conceptualised in the same way as a finite concrete one. This assumption is wrong but not necessarily disastrous, at least as long as one is considering monotone deterministic constructions (supertasks). Things start to go wrong with your mathematics once you insist on thinking of it in terms of supertasks and thinking that those supertasks are nondeterministic. Cantor's supertask is deterministic.

(This is not a reason for having a go at constructivists, or at least i don't think so. Should Say something about this.)

Constructions are a useful concept in the world of *concreta*, so *Homo Faber* likes to think in terms of constructions.

Somewhere here we need a chapter boundary

If you make the mistake of overestimating the resemblance between the world of *abstracta* and the world of *concreta* then you will be vulnerable to the seductions of supertasks. Transfinite recursion. The proof Cantor gave of his theorem that every closed set of reals has a largest closed subset without isolated points involves a transfinite recursion, and that is the way the story is always told. But it can be told in a "top-down" *at-a-stroke* way.

Recall, when X is a closed set of reals, that $D(X)$ is X minus its isolated points. Start with a closed set X, do D at successor stages and take $\bigcap$ at limit stages... and eventually obtain a perfect closed set.

This is the way the story is told, and the story of ordinals grew out of the telling of it. It is ordinals that we use to number the pages of the worksheet that we follow in order to construct our answer. *Homo Faber* likes this sort of thing very much. It not only tells us that the set we desire exists, it even tells us how to find it, while getting our hands upliftingly dirty in the process – we have *earned* the answer! Honest toil, indeed.

There are some inconvenient and annoying questions one can ask about this process ... "How do we know that we don't run out of ordinals?" for one. Notice that there is nothing in Cantor's story to promise us that this supertask concludes successfully. That's just assumed.

All good stuff. However there are plenty of people who are unaware that the story can be told without ordinals. The perfect closed set sought by Cantor can be obtained by a single set existence axiom...

$$\bigcap\bigcap\{Y \subseteq \mathcal{P}(X) : X \in Y \wedge D``Y \subseteq Y \wedge (\forall A \subseteq Y)(\bigcap A \in Y)\} \qquad \text{(A)}$$

If you have the axiom scheme of separation then it is obvious that this object is a set[4]. There is an actual set abstract because the construction is deterministic.

Thinking that it's not only *possible* to prove the existence of perfect closed sets by means of this kind of transfinite recursion but that this is the *correct* or perhaps even the *only* way is a mistake. It's the mistake of thinking that the abstract resembles the concrete. It's not a grave mistake because the result we are trying to prove can be proved without making it. We are relying on the hope that monotone discrete deterministic transfinite processes always halt – just as discrete deterministic finite processes always do. And our hope is not disappointed: as it happens we are in luck. Monotone discrete deterministic transfinite processes do, indeed, always halt. (This is beco's the output of such a process can always be described by a set abstract along the lines of (A) above.) So far our mistake has not got us into trouble. The key ingredient is 'deterministic'. If we drop it then we find that blah Discrete nondeterministic finite processes in the world of concreta always halt. blah

In this section we have seen how one might come to believe the Axiom of Choice by being confused about the abstract/concrete distinction and the finite/infinite distinction. In the following section 6.4 we consider somes lines of thought that might cause one to not believe the axiom.

[4]It may look as if you need power set but you don't: you quantify over all subsets of X but you don't need the collection of them to be a set.

6.4 Why might we not believe it?

It is possible to make the axiom of choice look highly implausible, or perhaps a better word is *unappealing*. I am not going to claim that the patter that follows is a convincing refutation, but it is worth thinking about.

If the Wellordering Principle is true then in particular $\mathbb{C}$ is wellordered. Now the complexes have an automorphism generated by swapping i and $-i$, but no wellordering of the complexes can have an automorphism: wellorderings are rigid.

This doesn't mean that the complexes cannot have a wellordering, but it does mean that no wellordering of $\mathbb{C}$ can be described within that part of complex analysis for which complex conjugation is an automorphism.

Now the reals (and the complexes) are a rather special case. One might feel that the fact that $\mathbb{R}$ can be completely characterised as the unique complete ordered field, and that $\mathbb{C}$ is its algebraic closure, means that somehow everything we know about them must arise from that characterisation. And if that characterisation doesn't deliver a wellordering then that must be because there is none. This argument (such as it is) relies on the special character of $\mathbb{R}$ and $\mathbb{C}$ and isn't susceptible of wider application.

Some sets, like $\mathbb{N}$ and the second number class, are naturally wellordered, and we don't need AC to wellorder them. Many other sets are not naturally wellordered in this way, and for them is neither obvious that they should be naturally wellordered nor obvious that they shouldn't. For example, there doesn't seem to be anything in the definition of $H_{\aleph_1}$ to say that it should be wellordered, but neither does it have any characterising structure that admits an automorphism in the way that $\mathbb{C}$ does and would tell us that it isn't.

The intuition behind this point about the automorphism of $\mathbb{C}$ tells us that if there is such a thing as a wellordering of $\mathbb{R}$ or $\mathbb{C}$ this fact is not really a fact about the reals, not something that emerges from the innermost nature of the reals; rather it is something dropped on $\mathbb{R}$ by a passing seagull ... *not part of the Mathematics*. And where did the seagull get it from, one might aask?

This was very much the kind of response the axiom of choice received a hundred-odd years ago when it was sprung on us by Zermelo. *If you think the axiom of choice is plausible then you are thinking of mathematics in a completely different way from me.*

In short:

If $\mathbb{C}$ is wellordered then – since $\mathbb{C}$ has an automorphism – the wellordering must be completely invisible to the arithmetic – indeed the *Analysis* – of $\mathbb{C}$. However in some sense there is no more to $\mathbb{C}$ than Complex Analysis. There is no more to $\mathbb{C}$ than being the algebraic closure of $\mathbb{R}$, and *that* doesn't tell us how to wellorder $\mathbb{C}$.

Of course this doesn't tell us that AC is *false* ...

On this view asking whether $|\mathbb{R}| = \aleph_1$ or $\aleph_2$ is as daft as asking ¿$3 \in 5$? And don't forget that is surprisingly fiddly even to show that $|\mathbb{R}| = 2^{\aleph_0}$. How easy is it to prove constructively?? It's not straightforward to even *state* it constructively!

There are lots of stories about where the reals come from: abstraction is a wondrous and many splendoured thing. However we don't need to spell them all out and decide which one is the best beco's it won't matter: *none* of them prepare the ground for the news that $\mathbb{R}$ can be wellordered. Doesn't mean that it can't of course but, to the extent that the abstraction stories – whatever they are – are the roots of the mathematics of $\mathbb{R}$, we are drawn to the conclusion that the wellordering – if there is one – isn't part of that mathematics.

It may be that this shrinking away is part of the same phenomenon as the widely felt distaste for Set Theory: *it isn't Mathematics* . . . a distaste that is strongly felt but never articulated sufficiently clearly to become a source of insights.

In the – concrete – world from which abstract entities are abstracted there is a huge amount of detail (some of which can be used to wellorder things) but a lot of that information is discarded (concealed) in the process of the abstraction. The wellorderings are not relations for which the equivalence relations we take the quotients are congruence relations, so they don't appear in the quotients.

H I A T U S

If you implement the reals as sets then there is a fighting chance that you might import information about the internal structure of those sets that enables you to distinguish between them and thereby wellorder the set of them. This is in fact precisely what happens in ZF + V=L.

Natural numbers arise from finite sets thru' equipollence. There's nothing about numbers that tell you which of them should be members of which others. Theere's nothing about where reals come or what they do that tells you how to wellorder them.

It's always a bit risky to pronounce on the contents of other minds but it's probably safe to say that a lot of people believe AC beco's they believe they can just wellorder any set by brute force – in a *supertask*. This raises questions about Agency which is problematic beco's Pure Mathematics has no agents. But let's engage with this idea anyway and see what emerges[5].

If you can wellorder everything then in particular you can worder $\mathbb{R}$. When people spell this out we are told that there are models of set theory in which $\mathbb{R}$ is wellordered and also models in which it isn't. These models are good and useful things, but they don't show that it is both consistent that $\mathbb{R}$ be wellordered and consistent that it not be wellordered. The thing that they say is wellordered (or not, as the case may be) is not $\mathbb{R}$ but an implementation of it. An implementation of the reals in our preferred foundational system may be

[5]This is the kind of situation that the paraconsistentists would say invites us to drop non-contradiction

wellordered – or made of green cheese, or whatever – but that doesn't tell us anything about $\mathbb{R}$ itself. If $\mathbb{R}$ had a natural wellordering (in the way that it has a dense total ordering, for example) then this would surely be captured by any implementation, so the fact that there are implementations of real arithmetic that make the set of reals not wellordered is pretty compelling evidence that the reals, in at least one good sense, simply aren't wellordered.

Or do they want to say that a purported implementation of $\mathbb{R}$ into set theory isn't an implementation unless it's wellordered? That being wordered is part of the spec? It clearly doesn't follow from the specification of $\mathbb{R}$ as a complete ordered field. So how does it come to be part of the spec? But nobody seems to want to say that an implementation of a complete ordered field must be wellordered – ZFistas certainly say that there are "models of set theory" in which $\mathbb{R}$ is not wellordered.

What are the reals anyway? Set theoretic foundationalists would have you believe that $\mathbb{R}$ is a pure set, a set of pure sets, that every real number is a set. Pointillism! But this is a mistake. Reals are not sets, they are numbers. The question is not "can we wellorder the pure set that implements $\mathbb{R}$ in Set Theory? The answer to this *yes* if our set theory has Gödel's axiom $V = L$ (which nobody believes, as it happens) but otherwise possibly not. The reals are the unique complete ordered field, and the question is "Is the unique complete ordered field wellordered?"

****

OLD STUFF BELOW HERE

6.4.1 Grue Emeralds

Does every perfect binary tree have an infinite path? Think of the tree $\{0, 1\}^{<\omega}$ of finite sequences of 0s and 1s partially ordered by end-extension. That is the prototypical perfect binary tree and the stream of zeroes is an infinite path in it. We haven't examined *every* perfect binary tree of course, but what we can say is that every perfect binary tree *so far examined* has had an infinite path. Isn't that evidence that every perfect binary tree has an infinite path? There is a curious echo here of a famous puzzle of Nelson Goodman's: the grue emeralds. An emerald is grue (see [13]) if either

(i) it is examined before 1/i/2500 and found to be green; or

(ii) is unexamined before 1/i/2500 and is blue.

There is much to debate in this gruesome scenario, but one thing is clear: the fact that every emerald so far examined has turned out to be grue is not evidence that all unexamined emeralds are grue.

I offer the thought that the mere fact that every perfect binary tree so far conceived has an infinite path is not evidence that all perfect binary trees have such paths. Granted, every perfect binary tree so far examined has had an infinite path ... just as every emerald so far examined has been grue. The act of examining the emerald makes it grue, and the act of examining a tree expands it from an object of type `naked-tree` to an object of datatype `tree-with-an-embedding-into-E`^{3}.

Expand this section; think it through properly

[The parallel to keep an eye on is: examining a set wellorders it in the same way that examining the emerald makes it grue.]

A potentially useful thought ... The examined (and threrefore wellordered) set is a different ADT from the unexamined (and therefore naked) set. But the unexamined and examined emeralds are of the same ADT.

There are many subtleties in the problem of the grue emeralds, and this is no place for a thorough examination of them (even were I competant to do it). The key point for us is that the
(i) grueness (or otherwise) of the emerald, and
(ii) our investigation of it for grueness

are not independent events in the way that
(iii) the greenness (or otherwise) of an emerald and
(iv) our investigation of it for greenness

are independent. We do not render an emerald green by examining it, tho' we do prevent it from being grue by examining it before 2500.

Examining a perfectly ordinary emerald makes it grue.

Conceiving of a set makes it wellordered. So 'wellordered' is a grue predicate.

[speculation here]

A set is wellorderable* if it is examined and found to be wellorderable, or is unexamined and not wellorderable.

If you examine a set and find it to be wellorderable then you have perforce wellordered it ... expanded it into a wellordering... haven't you? In fact, if you examine it closely enuff to ascertain whether or not it is wellorderable then you perforce wellorder it. So every set is wellorderable*. Ooops, not quite. You might be a wellordered set that hasn't been examined. Perhaps the definition we want is "x is wellorderable* if it is either unexamined or discovered to be wellordered"

What is going on here? We can't imagine a set without imagining it as a wellordered set? Surely that's not true – think of $\mathbb{R}$. But perhaps by imagining it we turn it into something that might be wellordered at some later stage? Perhaps we are simply mistaken in thinking that we can imagine it. It can be argued that we cannot imagine any uncountable wellorderings.

The suggestion is that

> (i) thinking that the fact that all perfect binary trees so far encountered have infinite branches is evidence for AC_2

is the same mistake as

> (ii) thinking that there is inductive support for "all emeralds are grue".

Can we conceive of perfect binary trees with no infinite branches? Certainly there are models of set theory containing such special trees, but of course they do not *really* lack infinite branches. There are infinite branches all right, just not in the special corner of the universe that is the model containing the special tree. So we haven't succeeded in conceiving of a perfect binary tree with no infinite branch; what we have managed to conceive is a perfect binary tree all of whose infinite branches can be overlooked or mislaid ... are in other words *deniable*.

imagining is *concretising*

Does this mean that a perfect binary tree lacking an infinite branch is inconceivable? And does its inconceivability mean it's impossible? These avenues of enquiry will remind some readers of Berkeley's Master Argument for Idealism. Berkeley leads his readers by the nose through a thicket of

"it's inconceivable that anything should exist unconceived"

and its like to

"it is impossible that anything should exist unconceived".

The parallels are strong, and they are not encouraging for the advocate of AC, since the general view nowadays is that Berkeley's Master Argument is deeply flawed, and that successful repairs – if any – won't capture the sense of the original exercise[6]. There are parallels between the Master Argument on the one hand, and – on the other – the thought that AC is obvious because one cannot imagine a set that can't be wellordered; an exploration of these parallels may be useful to both parties. I don't think anyone has considered the act of expanding (in the model theoretic sense) a mathematical object to be a mental construction in the way one would in this context, but – altho' it might be helpful – I do not have the stomach for it. But i'll have a tentative stab in section 6.4.2 which now follows.

Get the reference right

Thinking about the project of counting a union of countably many countable sets might cause a reader to start thinking about supertasks. My position on this is that it is not a helpful move. *supertask* is not a useful concept (beguiling tho' some of its instances are) and – even if it were – it isn't helpful in coming to grips with AC. Indeed the idea that it is helpful in that setting is a result of another error of mistaking something for something else.It comes of thinking of that project in a too concrete –too *agentive* – way.

However, the *aperçu* that moves one to make the mistake of thinking that AC is connected with supertasks is a good one. AC indeed turns out to be a principle about transfinite processes. It is the principle that every monotone nondeterministic process concludes successfully.

H I A T U S

Why might we believe it? Why might we not believe it? I think there is a concrete/abstract distinction to be made, and that one of the most widespread

[6] I am endebted to Maarten Steenhagen for directing my attention to [27] (specifically pp 127ff.)

causes for people to believe in AC is neglect of this distinction. So, before we tackle either horn of the do-we-(not)-believe dilemma, we should have a discussion of this distinction.

We discuss the two distinctions. Concrete/Abstract and Finite/Infinite. Ignoring the second results in thinking there can't be infinite sets; ignoring the first results in believing AC. These are different mistakes.

We are forced to consider supertasks beco's the 'keep on going' (KBO[7]) argument for countable choice relies on the plausibility of a supertask story.

Supertasks smuggle in agency but that's OK as long as you are on your guard.

We want our concept of supertask to be of a discrete kind of thing lest anything done in time become a supertask and we get trapped by a Zeno-like paradox.

So a supertask is something discrete, run over infinitely many ordinals.

Many supertasks are formatted to look seductively like infinitely long versions of things of the kind one might actually do, in time. However the separability of $\mathbb{R}$ means that the realistic feel of supertasks of uncountable length is illusory.

Thus a supertask is a task that involves doing infinitely many things. But not *merely* infinitely many things: obtaining $\{n^2 : n \in \mathbb{N}\}$ from $\mathbb{N}$ by squaring everything in $\mathbb{N}$ is not a supertask: since no single act of squaring interferes with any other, all the squarings can be done independently and simultaneously – in parallel. You have a supertask on your plate only when the infinitely many things cannot be done in parallel but have to be done in succession.

[other considerations? Insert here]

So a supertask is a discrete transfinite process, indexed by ordinals. So it has to halt and when it has halted it's supposed to have achieved something. Output something.

What features do we need to consider in trying to ascertain whether or not a supertask will conclude? Various things to consider: deterministic/nondeterministic, monotone/nonmonotone, cts/not-cts

There will be theorems telling us that a s'task satisfying some of these conditions will halt and succeed. AC will be one of those theorems.

Supertask of trying to build a free complete boolean algebra with countably many generators. (one can build a free complete Heyting algebra with countably many generators.)

They concern us on two fronts:

(i) transfinite processes: can we complete them.

(ii) But also supertasks of *expansion* expanding a naked set into a counted-set

[7] And it doesn't mean *Korean Baseball Organisation*. Ask Google, and mention Churchill.

AC tells us that certain supertasks which o/w might not conclude successfully do in fact conclude successfully. To state its consequences clearly we need to identify 3 features.

'Supertask' is a wonderful piece of Adman's jargon. More prosaically a supertask is a discrete task of transfinite length. It is a task spread over infinitely many stages, and those stages are clocked by ordinals. (one has to insist on the ordinals lest any continuous process over the real line should count as a supertask).

So perhaps we want to tease apart two things. We want to construct a Wombat; it looks as if the Wombat can be obtained as a result of a supertask. We design the supertask, and it completes jez' fine, but sadly we were mistaken: the output is not the Wombat. An order-preserving injection from the second number class into $\mathbb{R}$ is a case in point.

The Axiom of Dependent Choices (DC) is the principle that every [non-deterministic] supertask of length ω can be completed. In its usual formulation it says that,
for any set X with a binary relation R satisfying $(\forall x \in X)(\exists y \in X)(R(x, y))$ there is a sequence $\langle x_1, x_2 \ldots n_n \ldots\rangle$ where, for all i, $R(x_i, x_{i+1})$.

Hang on, what if $n > \omega$?

Consider the supertask in [20] p 26, which we recapitulate here. We have a bag, and infinitely many beads. Our points in time and our beads are both indexed by countable ordinals. At time $t = 0$ the bag is empty; at time $t = n$ beads with numbers $n \cdot 10$ to $n \cdot 11$ are put into the bag, and the bead with number n is removed. At time $t = \omega$ the bag is empty again; every bead that has been put in before $t = \omega$ is removed before time $t = \omega$. ("let b be an arbitrary bead ..." then b is not in the bag at time $t = \omega$). In fact the bag is empty uncountably often. There is no real mathematical significance to this; the point to the trick is that the mathematically naïve can be spooked by the fact that, athough at every stage you put in more beads than you remove, nevertheless there are stages at which you have removed everything that you have put in. There is nothing wrong with this really, except the fact that the function $n \mapsto$ the cardinality of $\{k :$ the bead with number k is in the bag at time $n\}$ is not a monotone function from the class of countable ordinals to $\mathbb{N}$.

I bring supertasks up really only in order to wave them away – I think they are a conjuring trick: they entertain but do not enlighten. However one cannot simply ignore them, since appeal to supertask intuitions underlies many people's belief in the truth of the Axiom of Choice. They derive their plausibility from beguilingly familiar features which are actually mathematically entirely irrelevant. Using a supertask argument is like working a conjuring trick, or telling a joke well: they have to direct – or rather *mis*direct – the audience's attention. This is something we shall see again. They also trade on an assumption that the default is for the infinite to resemble the finite. Not in *all* respects it doesn't – clearly! – so this is a *false* assumption and it needs to be smuggled in subliminally rather than adopted openly so we can all see it. Better still – as in all good joke-telling – the audience should be induced to make the necessary background assumptions *themselves*. Complicity!

The Separability of $\mathbb{R}$

It is central to the idea of a supertask that the subtasks be done *in succession*. But succession in what? Both the Thompson's lamp supertask and the countable choice supertask seem to be (notionally) executed in time – actual, physical time. It is this reassuring air of concreteness that lends them what plausibility they have. But one shouldn't be led by this concreteness to an acceptance that all supertasks are executable. It is a consequence of the separability of $\mathbb{R}$ that we cannot embed any uncountable wellordering into the reals in an order-preserving way, and this means that we cannot conceive of any transfinite process (supertask) of uncountable length as taking place *in time*.

This has implications for the possibility of arguing for AC on the basis of supertasks. I want to argue that if you sensibly believe AC – in particular that an arbitrary uncountable set can be wellordered, then it isn't beco's of supertasks. You can't wellorder $\mathcal{P}(\mathbb{R})$ by a supertask since supertasks take place in (notional) time. Once one has taken that on board it seems hardly necessary to follow up with the special case that you can't wellorder even the reals by supertasks beco's if you did, then, the reals being uncountable, the dates at which you pointed to the various reals would form an increasing ω_1 sequence in the time line, and that is impossible.

If you want to say that your supertasks take place in some other kind of time then you are severing the last link to intuitive motivation.

6.4.2 Supertasks: Expansions and Forcible Wellordering

In this section we discuss the possibility of datatype expansions being performed by supertasks. These are 'expansions' in the model-theory sense in which the rationals as an ordered field are an expansion of the rationals as an ordered set[8] We expand a countable `naked-set` into a structure of type `counted-set` by performing the (mental) supertask of counting it... or do we?

Actually we have to be **very** careful here. If the supertask consists in non-deterministically pulling members out of the set like rabbits out of a hat, in a discrete wellordered sequence of pulls clocked by the ordinals, then there is no reason to expect that we will exhaust it in ω steps (tho' we do know we will exhaust it in *countably many* steps). To be sure of exhausting it in precisely ω steps we'd have to know the enumeration in advance, and be merely *reciting* it, and of course such a recitation effects nothing. In contrast, embedding a perfect binary tree into the plane, or the socks in E^3, are both supertasks of length ω.

It might be worth noting the following dispiriting facts.

(i) Can't count a countable set just on being told that it's countable;
(ii) Can't count a countable set on being given a wellordering of it;
(iii) Can't count a countable set even if all its members are reals.

need references for these

[8] See `https://en.wikipedia.org/wiki/Model_theory`.

We consider them in turn.

(i) Every proper initial segment of the set of countable ordinals (the second number class) is countable. If we could count a countable set on being told that it is countable we would have a function that, to each countable ordinal α, replied with a counting of the ordinals below α. That would be enough to prove that the set of countable ordinals has no countable unbounded subset, and it is known (since at least [12] and surely earlier) that any proof of this fact must use at least some AC. It's not even as if there is some strategy we can use on a set X, some project on which we embark, which is guaranteed – *if X is countable* – to produce a counting of it (and which produces something uninformative if X is not countable). We can try picking elements from X as we run through the ordinals. If X is countable we will run out of members of X – and therefore stop – at some countable stage, but there is no guarantee that the output of this process is a counting of X. It'll be a *wellordering* all right, but it might not be a *counting*. It is a deep fact that there is no definable way of recovering a counting of a countable set from a wellordering of it.

(ii) If we could count a countable set on being given a wellordering of it we would have a function that, to each countable ordinal, replied with a counting of the ordinals below it, and this, as we have just seen, needs some AC.

(iii) If we could use the structure of the reals to count a countable set of reals then we would be able to prove that $\mathbb{R}$ is not a union of a countable set of countable sets, and it is known that we can't.

Even once one has taken on board the idea that `naked-set` is a different type from `counted-set` one can still fall into the trap of thinking (and i have heard people say this) that some objects [$\mathbb{N}$ is the obvious example] are obviously counted sets whereas some are obviously merely countable. That's the wrong way to think. It's not that $\mathbb{N}$ is obviously a `counted set`, it starts off as a `naked-set` like everyone else; it's that the `naked-set` $\mathbb{N}$ *happens to* have a counting [the identity map will do nicely] that is rather more salient than the counting of – for example – $\mathbb{Q}$, or – to take a more extreme example – the set of recursive ordinals.

need more discussion here

Interestingly to describe this situation properly we seem to need a concept of a structure – $\mathbb{N}$– which we are at liberty to think of as set, a worder, a . . .

Stuff deleted from here and moved to counting.tex

Perhaps there is some profit to be derived from the exercise of applying an analysis like this to the counting of countably infinite sets. This gets very murky indeed, since it involves supertasks and the axiom of choice. It's a situation where one has to make risky assumptions simply to get off the ground.

OK, so i have a set X which happens to be countable. How do i count it? I attempt to build a bijection between it and my collection of natural-number-representatives, of counter sets. This is legitimate as long as `set` is the kind of datatype that supports random access and replacement (which is OK) but we also need to be able to perform the supertask of indicating members of X (novel members of X indeed) infinitely often. The assumption that we have this kind of ability is a nontrivial choice principle, since it implies that every infinite set has a countable subset.

And even if that is all right (which i don't think it is, but never mind) we are still not satisfied, and here's why. Let X be an infinite set, and let us pair off members of X with counter sets. That shows that X has a countable subset. It doesn't show that X is countable, because there might be stuff left over *even if X is – in fact – countable*. How are we to ensure that the process of counting, of matching up members of X with counter sets, exhausts X and the set of counter sets simultaneously? Clearly the only way of ensuring that is for X to come equipped with a wellordering of length ω. Notice that even having X being actually countable, plus being equipped with a wellordering doesn't help us. There is no effective route from a wellordering of a countable set to a counting of that set.

6.4.3 A Parallel with Berkeley's Master Argument?

One conceives the socks (like the shoes) as scattered through 3-space (how else is one to conceive them?). But once one has done that, one has perforce wellordered them. Can one conceive a counterexample to the axiom of choice? Doesn't that sound rather like the task of conceiving "of a tree or house existing by itself, independent of, and unperceived by any mind whatsoever"? A parallel with Berkeley's Master Argument is something one could do without. There seems to be general agreement in the literature that Berkeley's argument is fallacious, and there seems to be general agreement, too, about roughly where the fallacy is located, but no agreement on its precise nature.

This suggests that one can (attempt to) argue for the axiom of choice in the same way that Berkeley argued for idealism: it is impossible to conceive of a counterexample. One can argue about how good Berkeley's argument was in the first place, but it is also worth thinking about how good the parallel is.

Can't conceive any uncountable wellorderings

If we conceive of the countably many pairs of socks then we conceive the set of socks as countable. So the thought is not that Russell's parable smuggles in spurious information, the thought is rather that the information which is smuggled in was inevitably going to be dragged in by the process of imagining the socks. Suppose you don't imagine the socks as socks, but as proxies for the members of the pairs in the countable set of pairs (for which they were of course proxies all along). Then you are imagining a countable family of pairs, in effect a countable family of pairs of *reals* since we imagine these things as living in space (don't we?) and since 3-space is in bijection with $\mathbb{R}$ and $\mathbb{R}$ has a total order this gives us a choice function on the pairs. So a countable set of pairs that lacks a choice function is inconceivable.

However, we should beware. If the plan is to promote the axiom of choice by saying that counterexamples are inconceivable, then the notion of inconceivability we will need is too broad and the argument proves too much. The point is that if *conceivable* means anything remotely like *visualisation* then we have to remember that all uncountable wellorderings are inconceivable . . . in that sense. (It is a point worth making to beginners in set theory, particularly strong beginners, that there is no point in trying to imagine the set of countable ordinals; being used to real analysis they need to be told, right from the outset, that the second number class is a very different beast from $\mathbb{R}$). And we don't need the axiom of choice to prove the existence of uncountable wellorderings. All that is needed is the axiom of power set and some minimal amount of separation such as can be found in Mac Lane set theory or even KF. So this argument proves too much. Well, too much for *this* writer's taste. There may be people who draw up their skirts in horror at the thought of uncountable wellorderings, but one would prefer to keep inconceivability-of-counterexamples arguments for AC uncoupled from sensitivities about the existence of uncountable wellorderings. After all, there are plenty of people who want to be able to argue for the axiom of choice and simultaneously believe in the existence of uncountable wellorderings. Arguing for AC by claiming the inconceivability of counterexamples is not an option for them.

We haven't described these theories. . .

But i suppose there might be a difference between the way in which counterexamples to AC are inconceivable and the way in which uncountable wellorderings are inconceivable. The obvious people to tease out this difference are people who want to both argue that AC is true (because counterexamples to it cannot be conceived) and that uncountable wellorderings exist. I am not such a person.

This needs to be developed

Summary: If we can find a notion of conceivability according to which uncountable wellorderings are conceivable but counterexamples to AC are not then one can run an analogue of Berkeley's Master Argument to show that AC is true. A lot of potholes!

6.5 Some Subtleties

So what is one to make of the line of patter routinely sold to undergraduates, the purported proof that a union of countably many countable sets is countable? It's not a proof [that a union of countably many countable sets is countable] because we know that there is no such proof: it's possible for the premisses to be true but the conclusion false. So what is it?

Is it an ellipsis? If it were an ellipsis, it would be flagged as such, and a subsequent unpacking would be promised. But no such explanation is provided, just a pretence that everything is going to be OK, like the Blonde Expedition to the Sun "It'll be OK, we're landing at night".

Telling them that is a proof doesn't just give them false beliefs about the axiom of choice [which is bad enough] it gives them false ideas about the nature of proof, and that is much more serious.

Since it's not an ellipsis for a valid proof it's a defective proof, otherwise known as a *fallacy*. What nature of fallacy? A: Fallacy of equivocation.

It's very striking that there should be people who think that AC is obviously true and obviously indispensible but nevertheless don't want to tell their students anything about it beyond the bare minimum needed to secure their compliance. This is rather the way that priestcrafts treat their lay flock.

Some things are simultaneously too important to be altogether withheld from the lay flock yet also so fraught with complications that one shouldn't risk trying to explain them to the lay flock. A delicate balancing act is called for.

However, if the institutions are to persist through time, at some point you have to identify those members of the lay flock that are going to go on to be priests, and let them into the secret. Most religions have got the hang of it. Mathematics hasn't. Edit this

Christianity would never have got where it is today if the first christians had said to themselves "The death of Jesus on the cross is this hugely important event that gives us all the possibility of Eternal Life – *but we're not going to tell anyone about it*".

This policy of not telling students about AC is not a considered result of a set of deliberations; it's a continuation of what would have been a sensible policy before we understood the rôle of AC, compounded by a *post-hoc* rationalisation of bad practice that should have been abolished by the discovery of the axiom. rewrite this para

6.5.1 Banach-Tarski

Also Vitali, $\mathbb{R}$ as a VS over $\mathbb{Q}$

This has been moved...

6.5.2 Infinite Exponent Partition Relations

There is a theorem [find references] that says that infinite exponent partition relations violate choice. Conversely, if choice fails, one can sometimes find models in which some infinite exponent partition relations hold. What is going on? If one is undecided about AC what is one to believe? What is the mathematical content of these nonexistence proofs? I think the correct response is to say that the possibility of the truth of AC means that there are no algorithms [even in an extended sense] for finding infinite monochromatic sets for partitions of infinite sets... no constructive proof of their existence. Indeed there is a useful parallel here with the critique of classical logic by the constructivists (the exhibitionists). You don't have to discard the classical proof altogether, you just have to start thinking of it as a proof of something else.

6.5.3 $\mathrm{AC}_\omega^\omega$

Nathan says this should be incorporated into the section on supertasks

If we make the countably many choices in advance then, in executing the zigzag algorithm, all we are doing is passively executing a deterministic process; (we are the machine on which it runs, and *we* are not making any choices at all) we are merely proving that a counted union of counted sets is counted. At stage $\binom{m+n}{2} + m$ we take the mth thing from the nth counted set.

However it looks as if there is another way of doing it, by making infinitely many choices *in succession* rather than simultaneously. There are people who explain DC have taken on board the fact that one needs countable choice to prove that a union of countably many countable sets is countable, but haven't fully grasped the manner in which AC is put to use in proving it. If you are trying to prove that a union of countably many countable set is countable then, you might think, when you visit the xth set to get its yth member you have to invoke the axiom of choice to obtain that yth member – because that xth set is merely countable not counted. As we've just seen, that is not in fact how the axiom of choice is used in this proof. The endeavour to give a formal description of this strategy results in a story along the following lines.

> "First you count the index set, so you have a family $\{A_i : i \in \mathbb{N}\}$. As we have observed, this costs nothing. Then at stage 1 you ask all the A_i for an element, using a choice function f_1. Where does this f_1 come from? Well, there is this choice principle called $\mathrm{AC}_\omega^\omega$ that says that if I have a countable family $\{A_i : i \in \mathbb{N}\}$ of countable sets then there is a function picking one element from each. This function can be thought of as an ω sequence $i \mapsto f_1(A_i)$ of elements from the union of the A_i. Replace each A_i by $A_i \setminus \{f_1(A_i)\}$ and do the same thing, this time using f_2, which is a choice function that $\mathrm{AC}_\omega^\omega$ tells you is to be had for the family $\{A_i \setminus \{f_1(A_i)\} : i \in \mathbb{N}\}$.
>
> Subsequently you iterate, at each stage using a function defined on the (set of the) remains of the A_is, concatenating the ω-sequence you have just obtained onto the end of the sequence you have been constructing so far, so that after n steps you have a wellordering of length $\omega \cdot n$ of a subset of $\bigcup_{i\in\mathbb{N}} A_i$. You keep doing this – possibly transfinitely – until the A_i are all used up. Of course there is no guarantee that the A_i all run out at stage ω, so the process might be of transfinite length. But at least, when it stops, you have a wellordering of $\bigcup_{i\in I} A_i$."

Interestingly this doesn't prove that $\bigcup_{i\in I} A_i$ is countable. *Wellordered* yes, but that isn't enough to show that it is countable. We will return to this later. For the moment our concern is to understand how AC *is* used in the proof that a union of countably many countable sets is countable.

It turns out that here we are using more than merely $\mathrm{AC}_\omega^\omega$. The axiom we are using in this construction is actually the (presumably much stronger) "There is a global function that assigns to each countable family of countable sets a choice function". But observe that even if you have this your desired end will not be reliably achieved. Suppose I use this function ω times, what have I achieved? I have a wellordering of length ω^2 that, for each i, contains infinitely many elements of A_i. I don't know that I have got everything in A_i. I can persist with this process, and run it transfinitely, and eventually I will have wellordered the whole of $\bigcup_{i\in\mathbb{N}} A_i$ – but there is no visible countable bound on how long this process will run. It is true that each A_i will be exhausted in countably many stages – at stage α_i, say, to give it a name – but how do we know that the set $\{\alpha_i : i \in \mathbb{N}\}$ is bounded below ω_1? That allegation follows from countable choice, as we know, but the obvious proof (try it) exploits the power to pick representatives from countable families of *uncountable* sets. All we have proved is that a countable union of countable sets is wellordered and of size $\aleph_1$ at most.

Thus it seems that $\mathrm{AC}^{\omega}_{\omega}$ doesn't (at least not straightforwardly) prove that a union of countably many countable sets is countable. What can we actually do with it? The obvious thing to do is to try the doomed strategy above and see how far we can go with it. Consider the special case where our countable family of countable sets is in fact a family of *finite* sets. Socks! Clearly our axiom will tell us that we can count the set of socks in the attic. We use the axiom once to simultaneously pick one sock from each of the $\aleph_0$ pairs. Each pair then has only one sock left, and we are done.

Theorem 3
$AC^{\omega}_{\omega} \vdash$ For every $n \in \mathbb{N}$, every countable family of sets all of which are of size n at most has a sumset of size $\aleph_0$.

Proof:

By induction on n.

The theorem is is clearly true for $n = 1$. For the induction step suppose $\mathcal{F}$ is a countable family of sets all of size $n+1$ at most, and suppose that any countable family of sets all of which are of size n at most has a sumset of size $\aleph_0$. By $\mathrm{AC}^{\omega}_{\omega}$ we have a selection function f that picks one element from every set in $\mathcal{F}$. $\mathcal{F}' = \{x \setminus \{f(x)\} : x \in \mathcal{F}\} \setminus \{\emptyset\}$ is now a family to which the induction hypothesis can be applied (all of its members are of size n at most) so it has a sumset of size $\aleph_0$. But $\bigcup \mathcal{F}$ is $\bigcup \mathcal{F}' \cup f``\mathcal{F}$, and $f``\mathcal{F}$ is clearly countable, So $\bigcup \mathcal{F}$ is the union of two countable sets and is countable. ■

What if $\mathcal{F}$ is a countable family of sets all of them finite, but with no finite bound on their size? This theorem tells us nothing about this situation at all!

Nathan says we can prove thm 3 as follows. Suppose we have a countable family of finite sets. Associate to each set the (countable!) set of its total orderings. Use $\mathrm{AC}^{\omega}_{\omega}$ to pick an ordering for each, and then concatenate them. This actually shows that a union of countably many finite sets is countable.

This works because each set of total orders is finite. If each set in the family is infinite then it won't work.

Suppose we know that a union of countably many countable sets is wordered. (This is compatible with failure of countable choice beco's ω_1 can be singular.) Then $\mathrm{AC}^{\omega}_{\omega}$ follows, because a global wellordering on the union provides a choice function on the summands. So, clearly, $\mathrm{AC}^{\omega}_{\omega}$ doesn't imply that a countable union of countable sets is countable. If it did, we would have an outright proof that ω_1 were regular.

6.5.4 Agency

See the chapter logicrave.tex a Agency

Most of the ways of pointing up the use of AC involve making fine distinctions that seem to invoke the concept of agency, and this is something of which mathematicians are suspicious, and rightly so: Mathematics is agent-independent. It's one of the reasons why the constructivist critique of classical mathematics gets a cool reception. When we say "it may be that it can be counted, but not by you" it looks as if we are relativising mathematics to agents. On the face of it agency is clearly involved in the constructive critique. . . . but it isn't really; what we are actually doing is invoking a concept of *information*.

Perhaps this belongs with supertasks

Curry-Howard reeks of agency; anything to do with computability reeks of agency. But remember that a function can be computable even if the agent doesn't know how to compute it. If computation involves agency, who is the agent? The Turing machine?

AC looks plausible in some versions and implausible in others. There are plenty of people who find it entirely plausible that every surjection should have a right inverse but baulk at the tho'rt of every set being wellordered. How can you wellorder $\mathbb{R}$, after all? If you think propositions A and A' have different features then clearly you think they are different propositions. If you find one version of AC plausible and the other one implausible then these two alleged versions of AC cannot both be versions of AC, and you are misidentifying at least one of them – and, if one, then perhaps both. . . ? Why not?

If you think it is implausible that every set can be wellordered then you are probably thinking that the wellordering must be in some sense definable. If you think it plausible that every surjection has a right-inverse then you are probably thinking of the set as having useful added structure. In general you are probably equivocating over different concepts of set.

Of course it might also be that the reason why you don't think that $\mathbb{R}$ can be wellordered is that you can't imagine a wellordering of the reals, and the reals look so familiar that you expect that if there were one, you would be able to imagine it.

Paradoxically you might find yourself more inclined to believe that arbitrary sets can be wellordered than that $\mathbb{R}$ can be wellordered! Is this an example of the conjunction fallacy?

Envoi

With the best will in the world it is hard to see a project to justify AC by appeal to supertasks as anything other than a fancy way of depicting an uncritical and unjustifiable extrapolation of finite behaviour to the infinite. The infinite doesn't resemble the finite closely enough to justify AC.

Using the axiom of choice isn't bad mathematics so much as bad *practice*.

Banach-Tarski is loaves-and-fishes.

Why do we ignore AC? It's not because (like Ax Power set) it's straightforwardly true and there's nothing to stress about. AC is not straightforwardly true in that way.

When confronted with entirely novel stimuli we reach for the tools that we have to hand, however inadequate they be, and try to see the new data as substrates for those old tools. This results in our applying perfectly good intuitions to material for which those intuitions were not designed. It also results in us performing fallacies of equivocation.

Dually when presented with new tools we try to apply them to old problems. When some clever bugger invented the hammer there was a mad rush to go through old outstanding problems to see if any of them were nails.

As Ben Garling says, we often find that old foundational crises leave behind them scars in the form of expository/pedagogical problems.

As Oron says: "Why didn't mathematicians work this out ages ago? Why do they understand so little?" I think the answer is that for most of mathematical practice a clear understanding of the axiom of choice is not really required. Most of mathematics is the study of the finite, and AC holds in finite domains. Some things that are easy to understand are also easy to *mis*understand and – unless there is an immediate and dire cost to misunderstanding that thing – one can continue to misunderstand it for a long time, while nevertheless continuing to enjoy the nice warm feeling brought to one by that misunderstanding-which-one-mistakes-for-an-understanding – which of course is *phenomenally* the same as the nice warm feeling one obtains from *actual* understanding. In these circumstances there are no cues to tell one that one is going down the wrong path.

But perhaps this is a good moment to remind ourselves that AC is not a proposition but a licence: it is not in *proofs* that choices are to be found: it is in *constructions*. Mathematics is not a body of truths/propositions, but a body of constructions, and that is the only way to understand AC. It's not declarative but performative : it confers a licence. Think of Euclid's *Elements*: it's not a body of theorems but a *recipe book*, a body of *instructions for doing things*. You are allowed various tools: for example you are empowered/authorised to draw a line through two points; to draw a circle whose centre is at x and has y on the circumference, and so on. In that spirit AC allows you to wellorder anything.

Explain 'performative'

6.6 A section on Skolemisation?

HIATUS

blend these two

This is an important topic, and there is an extensive technical literature on it. A good place for the determined interested reader to start would be `https://plato.stanford.edu/entries/epsilon-calculus/`

Let $\phi(,)$ be a binary relation such that $(\forall x)(\exists y)\phi(x, y)$. A skolem function for ϕ is a function f such that $(\forall x)\phi(x, f(x))$. The assertion that for any such ϕ we can find a skolem function does look very much like an application of the axiom of choice. Remarkably one does not need the axiom of choice if one wishes to pretend that such ϕ have Skolem functions. What is going on is this. Suppose T is a first-order theory in $\mathcal{L}(\phi)$, the language that contains the expression ϕ, and $T \vdash (\forall x)(\exists y)\phi(x, y)$. Suppose further that we expand $\mathcal{L}(\phi)$ by adding a symbol 'f' and an axiom $(\forall x)\phi(x, f(x))$, giving us a new theory T' in the expanded language. Then T' is consistent if T is, and there is no use of the axiom of choice in the proof!

Sadly the matter involves some fairly technical logic (proof theory in particular) and is probably not to the taste of most readers of this book. However the need clarification cannot be disregarded altogether, since the thoughtful reader will immediately want to apply this relative consistency result to the "countable union of countable sets is countable" situation. Let us suppose we are reasoning in some theory T that empowers us to perform certain manipulations on sets. Let $\{X_i : i \in \mathbb{N}\}$ be a counted family of countable sets. Saying that the X_i are all countable is to say that $(\forall i)$(there is a counting of X_i). But then we can consistently suppose that there is a Skolem function f sending each i to a counting of X_i, and we can use f in the zigzag construction to obtain a counting of the union. What's not to like? What's not to like is that the authorisations T gave us to do whatever-it-was that it authorises do not extend to manipulating f since f is not mentioned in the language that T lives in.

Say something about skolemisation in resolution proofs in first-order logic!

6.7 Isn't it simplest just to believe it?

The case for the axiom of choice is made by appeal to thought-experiments involving scenarios that turn out on close inspection to be implausible in the extreme. The perfect binary tree must have a branch. To contort this into a case for AC you have to have a perfect binary tree about which you know so little (in which you have taken so little interest) that you haven't even got a Hasse diagram for it ("shows how much you care!") but which is nevertheless so important to you that you ardently desire an infinite path for it, and for which you must therefore be allowed to use AC. I say: show me such a tree and I'll start thinking there is a case for AC as an axiom. I'm still waiting.

"Suppose there is a countable family of countable sets that we don't know how to count ..." Why should i suppose such a thing? How likely is it that i might run into one?

I say to believers in AC: yes, you occupy the rhetorical high ground, so you don't need any arguments. But suppose you didn't occupy the rhetorical high ground; how do you – a believer in AC – occupy and defend it?

6.7.1 AC keeps things simple

So that everybody is called 'Bruce'. [2] Finite sets obey AC, and mostly the infinite sets we encounter in mathematics do too. Isn't it a reasonably sensible simplifying assumption to make that unobserved sets will behave like observed sets? There are two replies to this. One is that it might be that if we made more strenuous efforts to observe the unobserved sets one might observe them and make interesting discoveries about them. The second is that not every observed set is observed to be wellordered anyway! Don't forget that the best information we have about wellordering the reals is that we *know* there is no definable relation that can be proved to wellorder them. (This allows there to be definable relations that might, in suitable models, wellorder the local *simulacrum* of the reals, and it turns out that this is in fact the case.) blend these two paragraphs

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

One reply that one would like to use, but can't, is that this flies in the face of a widely used (and thoughly bad) line of talk about set-theoretic axioms, namely that one should populate the mathematical universe with everything one can. This line of talk is terrible, because *Extremalaxiomen* of this kind basically never make sense. But this reply may be worth using nevertheless, since there are people who are susceptible to maximisation principles of this kind, and might be induced to adopt . . . infinite sets without countable subsets, countable sets of pairs of socks without a counting of the socks, and so on, as above.

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

It may well be that the wisest course in relation to the axiom of choice is the same course as the $\sqrt{2}^{\sqrt{2}}$ story leads us to in relation to the law of excluded middle. Use it sometimes, but bear in mind that there may be other times when the news it brings you is useless to you. And to always – *always* – prefer proofs that do not use it to proofs that do.

The current situation with AC is that the combatants have agreed to differ. People who are fully signed up to the modern consensus realist view of sets as arbitrary objects-in-extension believe – almost without exception – that the axiom of choice is true. There is a smaller party – consisting largely of constructivists of various flavours – who have a subtly different – and more intensional – concept of set and who in consequence do not accept the axiom of choice. However it has to be said that the philosophy of mathematics generally subscribed to by constructivists does not accord Set Theory the foundational role (i nearly wrote *leading role* there) many others do. In consequence they do not have a lot to say about the axiom of choice in its set-theoretic garb.

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

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

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

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

On this view the axiom of choice is just plain true, and the intuitive argument for it is that one can boldly go and straightforwardly just bloody well wellorder the universe *by hand* as it were. To be more precise, the axiom of choice (on this story) follows from realism about mathematical objects. (We saw earlier – p. 9 – how it looks plausible that AC follows from second-order set comprehension) The force of this story derives from the 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.

Supertask

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

But this is just the supertask mistake

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

There are two things wrong with this story. The first is that the imagery of picking things out of a set *in time* is restricted to sequences of choices whose length can be embedded in whatever it is that measures [our conception of] time, presumably $\mathbb{R}$. We cannot embed into $\mathbb{R}$ any wellorderings of uncountable length so this story never tells us how to wellorder uncountable sets.[9] This doesn't mean that the story is wrong, but it does demonstrate that its intuitive plausibility – beguiling tho' it is – is entirely spurious – even if you believe in supertasks.

The other problem is this. For it to be plausible that we can wellorder the universe by brute force we have to be sure that as long as we can pick 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 $(\alpha + 1)th$ thing. That's just straightforwardly true, and it's the argument we saw in the proof of theorem 1. Our realist intuitions get us this far, and thus far they are correct. The problem is that this is not enough: we still have to consider the case when 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) then a side effect is that the difference between the successor stage and the limit stage is made to seem *less* significant than it really is.

We are back in exactly the situation we saw on page 38. Realism doesn't get you the axiom of choice: what it gets you is the right to tell the story on page 38 in beguilingly concrete terms. This argument for the axiom of choice derives all its plausibility by artfully concealing the assumption that *the infinite resembles the finite in the way required*. The italicised assumption turns out to be precisely the axiom of choice. This is not to say that the platonists are wrong when they claim that AC holds for their conception of set, merely that this story isn't an argument for it.

Finally here is something one can say to people who think the axiom of choice is true for sets. *Take very seriously the idea that there might be lots of datatypes of set; what you are currently thinking of as one datatype is in fact many.*. Is their concept of set the absolutely rock-bottom concept of `naked-set`? Or is it not perhaps one of the assorted richer datatypes of `decorated-set`? [decoration unspecified for the present] If there is a rock-bottom, minimalist type of `naked-set` then it is plausible that AC might fail for that type even if it holds for others, so the intuition that AC holds for sets might instead be an intuition about one of the datatypes of `decorated-set`. That's not to say that, of all the various concepts of set we need in mathematics, the rock-bottom concept of `naked-set` is the one that will loom largest in the thinking

[9] 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.

of mathematicians, but it does offer a way of representing the believers and the unbelievers as not actually disagreeing. Of course this the old point about a fallacy of equivocation from section 3.2.

The Consistency of the Axiom of Choice?

Does this belong in the other volume?

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

There are various weak versions of the axiom of choice that the reader will probably need to know about. **The axiom of countable choice** ("AC_ω") says that every countable set of (nonempty) sets has a choice function.

Both these versions are strictly weaker than full AC. DC seems to encapsulate as much of the axiom of choice as we need if we are to do Real Analysis – well, the minimal amount needed to do it sensibly. DC does not imply the various headline-grabbing pathologies like Vitali's construction of a nonmeasurable set of reals nor the Banach-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.

6.8.1 IBE and some counterexamples

Can we argue for AC by IBE? There is a *prima facie* problem in that there are some consequences of AC that people have objected to at one time or another. We have already mentioned Vitali's theorem that there is a non-measurable set of reals, and the more recent and striking Banach-Tarski paradox[10] on the decompositions of spheres. Nor should we forget that when Zermelo [31] in 1904 derived the wellordering theorem from AC the reaction was not entirely favourable: the wellordering of the reals was then felt, initially, to be as pathological as Banach-Tarski was later.

However, one can tell a consistent and unified story about why these aren't really problems for AC. There is, granted, a concept of set which finds these results unwelcome, but that concept is not the one that modern axiomatic set theory is trying to capture. The view of set theory that objects to the three results mentioned in the last paragraph is one that does not regard sets as fully extensional and arbitrary. How might it come about that one does not like the idea of a non-measurable set of reals, or a Banach-Tarski–style decomposition of the sphere, or a wellordering of the reals? What is it that is unsatisfactory about the set whose existence is being alleged in cases like these?[11] It's fairly clear that the problem is that the alleged sets are not in any obvious sense definable.[12] If you think that a set is not a mere naked extensional object but an extensional-object-with-a-description then you will find some of the consequences of AC distasteful. But this means that in terms of the historical process described in section **??** you are trapped at stage (2). Once you have achieved the enlightenment of stage (3) these concerns evaporate. Nowadays mathematicians are happy about *arbitrary* sets in the same way that they are happy about *arbitrary* reals.

Constructive Mathematicians do not like AC

There are communities that do not accept the axiom of choice, and the reasons they have are diverse.

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

duplication

First we deny excluded middle

First we illustrate why constructivists repudiate the law of excluded middle.

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

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

[12]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 $\mathbb{R}$.

Some readers may already know the standard horror story about $\sqrt{2}^{\sqrt{2}}$. For those of you that don't – yet – here it is.

Suppose you are given the challenge of finding two irrational numbers 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.[13]

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

The Axiom of Choice implies Excluded Middle

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

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

Let us assume AC in this form, and deduce excluded middle. Let p be an arbitrary expression; we will deduce $p \vee \neg p$. Consider the set, the *doubleton*, $\{0, 1\}$, and the equivalence relation $\sim$ defined by $x \sim y$ iff $x = y \vee p$. Next

Check this definition: it got garbled at one point.

[13] 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 $(\sqrt{2})^{(log_{\sqrt{2}}(3))} = 3$.

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 $[0]_\sim$ and $[1]_\sim$ – which admittedly may or may not be the same thing – but they are at any rate inhabited. Since the quotient is an inhabited set of inhabited sets, it has a selection function f. We know that $[0]_\sim \subseteq \{0,1\}$ so certainly $(\forall x)(x \in [0]_\sim \to x = 0 \vee x = 1)$. Analogously we know that $[1]_\sim \subseteq \{0,1\}$ so certainly $(\forall x)(x \in [1]_\sim \to x = 0 \vee x = 1)$. So certainly $f([0]_\sim) = 0 \vee f([0]_\sim) = 1$ and $f([1]_\sim) = 0 \vee f([1]_\sim) = 1$.

This gives us four possible combinations. $f([0]_\sim) = 1$ and $f([1]_\sim) = 0$ both imply $1 \sim 0$ and therefore p. That takes care of three possibilities; the remaining possibility is $f([0]) = 0 \wedge f([1]) = 1$. Since f is a function this tells us that $[0]_\sim \neq [1]_\sim$ so in this case $\neg p$. So we conclude $p \vee \neg p$.[14]

Observe, however, that if we define the family of N-finite sets recursively by:

> The empty set is N-finite;
> if X is N-finite and $x \notin X$ then $X \cup \{x\}$ is N-finite.

then we can prove by structural induction on the N-finite sets that every N-finite set of inhabited sets has a choice function. This is theorem 1.

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

Uplifting though this moral is, it is not the point that I was trying to make. The fact that AC implies excluded middle and that there are principled reasons sometimes to eschew excluded middle means that there are principled reasons for (sometimes) wishing to eschew the axiom of choice.

6.9 Weak forms

The difference between various forms becomes obscured. Countable choice is a very different beast from full choice.

- Analysis without AC_ω is a disaster area. Analysis without full AC but with AC_ω is a very interesting prospect, particularly if we add nice things like LM – there are even people who advocate it; (Imre says that all such people are logicians);

- WQO theory without countable choice is a train-wreck – but very very few theorems in WQO theory need full choice;

[14] Thanks to Douglas Bridges for the right steer on this exercise! The theorem is due to Diaconescu [7].

- NF refutes full choice but doesn't seem to refute AC_ω. (Not sure about the various choice-contradicting nice conditions on models of TZT);
- We need AC_ω to identify the two definitions of wellfoundedness – and thereby to make sense of games of finite length. Games of length ω are supertasks.
- Don't we need AC_ω to do forcing?

6.10 Some thoughts about certificates

[a lot of the talk below is about freeness. There is also the point that, on one way of declaring the von Neumann ordinals, the vN ordinals is the same as the rectype of its certificates. Certainly true for vN naturals: the certificate that n is a natural is the set $[0, n]$, which is of course n itself. For transfinite ordinals there is the constructor that takes arbitrary increasing sequences, so of coiurse we get more certificates]

My point of departure is the idea of a recursive datatype or *rectype* for short. A rectype has *founders* and is built up by *constructors*. All the usual examples are **free** in the sense that each object in the rectype is denoted by a unique word in the constructors. Examples are the natural numbers, or lists and trees. Such rectypes are always initial objects in a suitable category. Computer scientists, for obvious reasons, tend to be interested only in rectypes of **finite character**: finitely many founders and finitely many constructors each of finite arity. However there is no mathematical reason not to consider rectypes of infinite character, and the cumulative hierarchy of sets is a natural example. It slight change here has no founders at all, and has one constructor – `set-of` – of unbounded arity. This is a free rectype and is well-behaved.

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

	Free	Not Free
Finite Character	The naturals lists, trees	?? ??
Infinite Character	The cumulative hierarchy of sets	The ordinals

Need to fill in the question marks.

Next I need the idea of a *certificate* or *proof*. If you are a member of a rectype there is always a good reason for you to be, and a certificate-or-proof is that reason – presented as a mathematical object. If the rectype is free (so it's an initial object in a suitable category) every object in it has a unique certificate. If the rectype is not free there may be a multiplicity of certificates. (Or there may even be none, as we shall see). Notice that even if the rectype R is not free, the rectype of certificates-for-R is always free. Perhaps I should be a bit more explicit about what a certificate is to be. A certificate-that-x-belongs-to-the-rectype is: the constructor used in the last step in the construction of x, together with a list of arguments to that constructor, with certificates for each of those arguments. So a certificate is a word in the constructors and founders. And the family of certificates for a rectype is another rectype – indeed a free rectype.

Now we need a slightly finer distinction, within the family of rectypes of infinite character. Specifically I shall be interested in the following rectypes.

1. The collection of wellfounded hereditarily countable sets. The single constructor is countable-set-of. This collection is often called HC[15];

2. The rectype whose founder is the ordinal number 0, with constructors `successor` and `sup-of-`ω`-sequence-of`. This is a substructure of the ordinals;

3. The rectype whose founders are all the countable sets, and whose constructor is `union-of-countable-set-of`;

4. The rectype whose founders are all the ω-sequences and whose constructor is ω`-sequence of`;

5. The cumulative hierarchy of sets.

(1), (4) and (5) are free. (2) and (3) are not.

Now any rectype admits a wellfounded quasiorder which in my introductory book [10] "Logic Induction and Sets" I call the **engendering relation**, and which is the transitive closure of the relation that x bears to each of things it is immediately constructed from. For example, in $\mathbb{N}$ the engendering relation is $<$. (I am a bit worried by the fact that there doesn't seem to be a standard name for this engendering relation in the literature .... If one isn't needed, when I think it is, then I must have misunderstood something very badly). The engendering relation is wellfounded, and has a canonical rank function, which is a map to the ordinals, whereby the rank of any object in the rectype is the least ordinal bigger than the ranks of all the things that bear the engendering relation to it. In case (5) the rectype rank is literally the same as the set-theoretic rank.

Now let's think about free rectypes of infinite character, but *bounded* character, so their constructors have bounded appetites.

Jech [18] has a wonderful theorem that says that every set in HC has rank less than ω_2. It's very important that the proof does not use AC at all. It exploits the fact that the rectype HC is free: each object has a unique certificate. I think that in general Jech's theorem shows that in any free rectype of countable character every object must have rank $< \omega_2$.

The freeness is important here. It is a theorem of Gitik [12] that the rectype (2) can contain all ordinals.

There is another result that is useful in this connection. I noticed it myself, but i'm sure it's folklore. If AC_ω holds, then $|HC| = 2^{\aleph_0}$. In a sense this isn't really the theorem; the theorem that underlies it goes like this:

[15] I prefer '$H_{\aleph_1}$'

Each of these rectypes is the least fixed point for a suitably chosen operation $\mathcal{O}$. So if you can find another fixed point ("pick a fixed point, any fixed point"!) or even something x with $|x| = |\mathcal{O}(x)|$ you should be able to embed the rectype in it and thereby bound its size. (There's a certain amount of small print to this: not all of which have I checked). Consider not HC but the rectype (3). The reals is the same size as the set of ω-sequences of reals. That means that we can define by recursion on the rectype (3) an injection into the reals. We need the freeness of (3) to get an *in*jection. If we can choose a certificate for each hereditarily countable set then we can embed HC into the reals. Hence the fact that $|HC| = 2^{\aleph_0}$.

I don't think this depends on special properties of countable sets; i think Jech's argument can be generalised to apply to all free rectypes of bounded character. I think it will say something like: if κ is an aleph then in any free rectype generated by fewer than κ founders and fewer than κ constructors each of arity less than κ every object has rank $< \kappa^{++}$ and the rectype itself is of power 2^κ. Something like that, anyway.

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

But what about the non-free rectypes? One would expect that even in a non-free rectype every object should have a certificate. How could this not be true? Since everything in the rectype is there for a good reason, there must be a good reason one can point to. Although this is true for rectypes of finite character it appears not to be straightforwardly true for rectypes of infinite character. It seems that unless we assume AC we have no reason to suppose that a rectype of infinite character is a projection (in the obvious way) of its rectype of certificates. For example, in the model of Gitik's where every limit ordinal has cofinality ω the rectype (2) generated from 0 by `succ` and ω-sups contains all ordinals, and the rectype of certificates for it is a free rectype of countable character, so every certificate has rank $< \omega_2$. In those circumstances we cannot rely on ordinals beyond ω_2 having certificates.

Free rectypes of infinite bounded character are well-behaved, but we need AC to show that every infinite rectype is a surjective image of a free one. So in the absence of AC the task of establishing the sethood of a non-free rectype of infinitary-but-bounded character is nontrivial. For example in NF we do not know if the rectype (2) is the universe. And this despite the fact that we know that not every set can be a projection of a member of rectype (3).

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

So I think my questions to you are along the lines: (i) how much of this is known? Can I improve bits of it by expressing it in a more category-theoretic way..? Any helpful comments gratefully received...

Dear Thomas,

Thanks for your "Letter to Jamie". If I've understood it correctly, an answer is this:

Note that AC in the category of sets is that every surjection has a right inverse. I.e. if $f : A \to B$ then there exists a $g : B \to A$ such that $f \circ g : B \to B$ is the identity.

There's a powerful theory of initial algebras which gives functions from free objects (essentially, your certificates) to other objects (your non-free rectypes). AC implies (and almost certainly is equivalent with) the property that every one of these functions has a right inverse.

So what you describe probably can all be expressed in categorical language. I think what you've written amounts to observing (correctly or falsely I cannot judge off-the-cuff) that in the category of sets without AC, there are certain functions which do not have initial algebras, but they do have initial algebras in the category of sets with AC. This makes sense; in the category of sets without AC there are simply fewer functions!

There's one little niggling thing. An object in the category of sets is a set because it's an object in the category of sets. You might have to set up two categories; a category of sets ("small things") and a category of collections ("big things").

Dear Thomas,

FYI here's the page about the axiom of choice in Set (the category of sets)

`http://books.google.co.uk/books?id=KaXmMjwBulgC&pg=PA17&lpg=PA17&`
`dq=axiom+of+choice+epi+split+epi&source=web&ots=kuyJgzb9_v&sig=UJdRbYAOHZVXbzxiYjWE9pXr`
`hl=en&sa=X&oi=book_result&resnum=4&ct=result`

6.10.1 AC and Certification

Blend these two sections properly

Once we have taken on board the rôle played the concept of *datatype* in explaining the difference between the theorem that needs choice and the theorem that does not need choice, one can see that the axiom of choice weaves its magic by showing how, when we are given an object of one datatype (a `naked set` that happens to be countable), we can see it as a reduct of an object of the richer datatype `counted set`.

The next step after consciously acknowledging that mathematical objects typically and usefully have identifiable datatypes is the step of thinking of those datatypes as mathematical objects themselves. When we do this we find another rôle for the axiom of choice.

Let us help ourselves to the concept of *certificate*. It is useful primarily in connection with recursive datatypes [...] but it is actually slightly more general.

A certificate that a particular object is a member of a particular datatype is something that will convince a skeptical reader that the object in question is, indeed, an object of the datatype it is alleged to belong to. If we want to set up a subtype of `naked-set` called `countable-naked-set` a certificate for an object of that type would be a counting of it. Similarly, a certificate for a counted set is a counting of it. This makes it sound as if the two types `counted-set` and `countable-set` are the same, but they aren't, and the certificate-talk gives us a way of illustrating the difference. If x is a `counted-set`, the certificate that

x is so is part of the object x; if x is `countable-set` it isn't. A `countable-set` is a `naked-set` that *could* be expanded by decorating it with a counting (and that counting is a certificate that it is of type `countable-set`), but it remains a `naked-set` and the counting is not a part of it – it hovers around attentively but is not part of the kit; in contrast a `counted-set` is a `naked-set` *that has been* expanded by decorating it with a counting. . . and that counting is a certificate that the expanded object – of which it is a part – is of type `counted-set`).

Clearly objects of either of these types have certificates.

Now consider the datatype

`set-that-is-a-union-of-countably-many-countable-sets`.

It is a subtype of the type `naked-set`. Let's call this type `C` for short. A certificate that an object X really is of type `C` must be a counted set $\{\langle C_i, X_i\rangle : i \in \mathbb{N}\}$ of pairs where each X_i is a countable set with C_i a counting of it, such that $\bigcup_{i\in\mathbb{N}} X_i = X$.[16]

Now this certificate will give rise to a counting of X, by means of the zigzag construction on the C_i. So if every object of type `C` has a certificate it follows that a union of countably many countable sets is always countable, so every element of `C` is actually countable.

There are two more illustrations, slightly less unnatural. There is $H_{\aleph_1}$, the (wellfounded) hereditarily countable sets, aka HC; there is also the class of hereditarily wellordered sets.

A certificate that x is a member of HC is a counted subset X of HC such that $x = \bigcup X$, equipped with a function that assigns to each $y \in x$ a certificate that $y \in$ HC.

Remark 3 *If every element of HC has a certificate then every member of HC has a countable transitive closure.*

Proof: By induction on set-theoretic rank. ■

The significant feature common to all these cases is of course the fact that these rectypes are not free. Explain freeness

Thus, existence of certificates implies choice principles! Consideration of other, more complex datatypes will show [i think!] that the principle "for every datatype, every object of that datatype has a certificate" will imply full AC. AC should follow from the assertion that there is a global function assigning to each hereditarily wellordered set a wellordering of it. First step would be to show that AC follows from the assertion that there is a function assigning to each wellordered set a wellordering of it.

H I A T U S

[16] Or perhaps just a countable set of such certificates. . . ?

Dependent Choice

You need to be clear about what you are picking *from*. You can go on picking from a set as often as you like – through all the ordinals, even – as long as you don't remove them once you've picked them. After all, there's nothing wrong with a function from a wellordered set X to a set Y! If you remove your chosen element each time (so you pick a different member of X every time) then you are constructing a wellordered subset of X, and of course the size of any such subset is bounded by $|X|$.

"But" (i can hear the reader exclaiming) "the second choice is made from a set that is *different* from the first set!" If the first set is X, and x is chosen from it then the second choice is made from $X \setminus \{x\}$. This leads us to principles of *dependent* choice, where the set on which the choice function is being defined has some structure that arises from the choice function itself.

But this has no bearing on the axiom of choice, because AC talks about making choices from lots of *different* sets.

6.11 Leftovers

There is the point that the counterexamples to AC are things that it's impossible to describe completely, simply beco's of the order structure built into our language.

That is to say, the negation of AC is a sort-of self-refuting sentence like "I can't say 'breakfast"' which cannot be true if uttered and "It is raining and I do not believe it" which cannot be true if believed by the speaker. $\neg$AC resembles them in that the point is not that it can't be true, it's that it cannot be understood – or perhaps that if understood cannot be believed.

H I A T U S

It might be claimed the picture above misrepresents the thought processes of the people who think that the axiom of choice is obvious. Yes – it will be admitted – there is a danger of a fallacy of equivocation as sketched above, but the argument for choice relies on the cases where it is provable. It's a different kind of IBE: the reason why we can prove all these instances of the axiom of choice is simply that the axiom of choice is true. We can't prove that the set of socks is wellordered but that's only because we have not been given enough information about it. Any set about which we know enough reveals itself – under the close examination that we are able to give it – to be wellordered. Why is this? Is this just coincidence on a cosmic scale? Of course not! There is a simple explanation: the truth of the axiom of choice.

However this line of talk isn't really supported by the data. Not all observed sets are observed to be wellordered. Some sets provably have selection functions: the power set of the naturals for example. But some don't *provably* have them: the power set of $\mathbb{R}$ for example. (One could try claiming that the power set of the reals is not observable in the relevant sense, but since the only reason for arguing this is that it fails to support this argument, this would be too obviously circular for most tastes).

What is the correct concept of "observable" here? (We obviously don't mean *literally* "observable"! (It's worth thinking about whether or not the only things that are observable in the relevant sense are things that have enough order structure: you perforce wellorder a set in the course of observing it.)

We might mean something like

observed-to-be-ϕ = provably ϕ

observable set = definable set

Clearly we are thinking here of sets-in-intension, or descriptions of sets. Perhaps we can here put to good use the expression from possible-world rhetoric. 'V_ω' is a **rigid designator** (it denotes the same thing in all standard models); '$\mathbb{R}$' is not.

So what precisely is the general observation whose truth is to explained by the axiom of choice? It's not the fact that every observable set is known to be wellordered, since that is not a fact. Nor is it the fact that every definable set can consistently be wellordered, since AC would explain a lot more than that, and Inference to the *Best* Explanation would point us not at AC itself but rather at *the statement that AC is consistent*. Sadly that last observation is something we already know and don't need any arguments for. If we want an argument for AC we won't find it here.

The argument isn't really IBE at all (in contrast to the genuine IBE argument used for replacement, for example) but is a kind of induction by simple enumeration, or whatever is the argument that we use to refute the scepticism that says that unobserved objects might suddenly go out of existence, or misbehave in other ways, like the unobserved wallpaper in the drawing room of the magician Mr. Leakey in [15].

This is an attempt to tar with a radical sceptic's brush the people who say that you know AC to hold only for sets (finite, definable etc) for which you have privileged information. All observed sets are wellordered, so all unobserved sets are wellordered too.

(Is there a parallel here with attempts to prove that all emeralds are grue?)

There is a problem with arguments for the truth or falsity of set theoretic axioms. It is fairly general, but we can illustrate it here with the axiom of choice, since that is the axiom under discussion.

If you believe that the axiom of choice is the kind of thing that has a truth-value then you probably believe that it is noncontingent. If it's true it's necessarily true and if it's false it's necessarily false. If you have house room for such ideas of metaphysical neccessity then you probably try to capture them by talk about possible worlds. Conveniently there are obvious candidates for the possible worlds we would use to explain the necessary truth or necessary falsehood of AC, namely models of set theory, or perhaps *standard* models of set theory only. This terminology also gives us vocabulary to say things like " 'V_ω' is a rigid designator" and " '$\mathbb{R}$' is not a rigid designator" which (if our possible worlds are standard models) enable us to capture some things that set theorists recognise as facts.

How inconvenient it is, therefore, that on this account AC turns out to be true in some possible worlds and false in some others, and therefore not to be noncontingent after all. Not only that, but we don't know which of these models is the actual world, so we have no idea whether it is true *simpliciter* or not.

Clearly there is some explaining to be done

Can we argue that it is false? Argument to the effect that if it were true then counterexamples would be unimaginable?

OK, even if we cannot argue that the axiom of choice is true (at least by arguments like this) is there nevertheless a case to be made for adopting it as an axiom? (You would have reached this stage long ago if you had never been that kind of realist and never believed you had any epistemic access to arbitrary infinite extensional objects).

What are the pros and cons? On the pro side is the point that it makes the arbitrary infinite extensional objects behave like the cuddly familiar, nonarbitrary finite ones and thereby makes the world a tidy place. (Well, there is still this fact about $\mathbb{R}$ but believers in AC are greedy) It is true that $\mathbb{R}$ is not naturally wellordered, but if anything this is a point in AC's favour, since by wellordering $\mathbb{R}$ it gives us another way of reasoning about $\mathbb{R}$ and proving things about it.

On the con side is the fact that models in which every set of reals is measurable are quite cute in various ways.

I want to make a connection here with what i was telling you in the first lecture about the three stages entities go through on their way to becoming mathematical objects. We have noted that AC for *finite* objects is true. So it's only *infinite* objects we are ever going to get our knickers in a twist about. And we didn't start manipulating/calculating-with infinite objects until the days of Cantor and Dedekind. At the earliest stages of this process the infinite objects we had to deal with were all naturally motivated, naturally occurring, objects with enough internal structure – so we find that all the instances of AC that we wanted were true. Well, almost all of them: $\mathbb{R}$ has no definable wellordering as we have seen.

What this means is that, with hindsight, we should have expected people to notice that they needed the Axiom of Choice as an extra principle at precisely that stage when they started reasoning about/calculating-with *arbitrary infinite objects-in-extension*. And this is indeed exactly what happened.

Let's take an example: Vitali's construction of a non-measurable set. This could only arise once one had the concept of an arbitrary set of reals.

So if you only ever deal with finite sets and sets with enough internal structure you will hardly ever encounter one that doesn't come ready wellordered, and the issue doesn't arise. The question about whether or not AC is *true* can arise for you only if you are a realist about arbitrary infinite extensional objects.

Tie together Grue emeralds with expansion and self-refutation connect with Berkeley

AC and regimentation. No accident that you use BPI to prove a representation theorem

AC implies the existence of God

The following comes from Meyer [21].

One direction: God implies Choice, since if God existed, it would be possible to construct a choice set for each set, since God could just think about it for a bit and do the choosing, being omnipotent and all. The other direction: take a causal sequence - by Zorn's lemma (an equiv. of Choice), it will have a unique first member. Thus we have a cosmological argument which establishes a First Cause. (God, of course).

Another example of people using AC when they don't need to.

How do you prove that there is no subset of $\mathbb{R}$ that is of order-type ω_1 in the inherited order? Suppose there were such a set, X. Then $\mathbb{R} \setminus X$ is partitioned into open intervals, each of which must contain a rational, so pick a rational from each one, using AC. Then we have an uncountable set of rationals, which is impossible. (I have actually had students say this to me). But AC is not needed: since $\mathbb{Q}$ is countable we can (one choice!) pick an enumeration of it and select the first rational in each interval according to that enumeration.

Connect with logic-and-rhetoric the point that: just beco's the p i am telling you about isn't (as it happens) going to be a problem for you (beco's of your choice of pathway) it doesn't follow that p wasn't true!

Be sure to find some rude things to say about axioms of plenitude. What is an axiom of plenitude anyway? What is an axiom of restriction, a *Beschränkheitsaxiom*?

Conversation with Peter Smith 7/iii/2018

EXplain propetly why it was only after we acquired the idea of arbitrary set/function-in-extension that AC could blow up in our faces (B-T)

It's beco's of the belief that the infinite resembles the finite that they believe AC. All else is post-hoc rationalisation.

Look up traffic on stackexchange

Talk about skolem functions?

If $\sim$ is a congruence relation for an infinitary [total] operation f then in general you need AC to show that f is total on the quotient. Two examples:
(i) Infinitary sums of cardinals and ordinals – "multiplicative" axiom!
(ii) The Cauchy reals are order-complete. (perhaps we can do this without choice)
(iii) Also power set axiom in APG models

There are natural examples where you can't *just keep buggering on.* There are games where Player II can choose to stay alive for n steps, for any n, but is doomed to lose sooner or later.

The fact that WF is rigid sets you up for AC, because the existence of a definable wellorder of V enforces rigidity.

Chapter 7

Appendices etc etc

7.1 Glossary

Wellordering
performative
Contrastive explanation

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