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Thomas Forster

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The unexamined course is not worth attending, but come along anyway.

1 Introductory Lecture

What is philosophy for? New wave capitalism has appropriated the word 'philosophy'. Every damned company has a philosophy: even drug dealers and payday lenders have a philosophy. I typed 'our corporate philosophy' into google and got 80 million hits. It's just a fancy way of saying what they think they are trying to do. So perhaps philosophy of mathematics is about what mathematicians are trying to do. That's certainly my take on it. Lots of philosophers think that philosophy of mathematics is an autonomous discipline, separate from mathematics, an activity for which the chief qualifications is skill in philosophy, the thought being that a degree in philosophy is a certificate of posession of a portable skill, rather the way in which an MBA enables you to go and run, well, anything.

This idea isn't entirely incompatible with the view that philosophy of mathematics is all about what mathematicians are trying to do. What can philosophers bring to this endeavour? One idea (and this one goes back to Plato) is that the job of philosophy is Conceptual Analysis: taking care to carve nature at the joints. Questions like "What is the right way to think about these phenomena?" Are protium and deuterium distinct chemical elements[1]? Should Pluto be a planet? Flew: "Essays in Conceptual Analysis". Beautifully parodied by Frayn. Think of the house as an extension of the car rather than the other way round. Thus Philosophy-of-X is the study of the conceptual issues that arise in the practice-of-X.

You might feel (i certainly do) that—for any investigation—asking questions like "what is the right way to think of these phenomena that we are trying to investigate?" is part of the practice of that investigation. We think about such questions (as mathematicians, astronomers, chemists etc) but altho' we teach the answers, the outcomes of the discussions, in u/g courses we don't teach the discussions themselves. Which of course is one of the reasons why there is a demand for course with titles like the one i am giving.

[1]The have distinct chemistry: if you take to drinking D_2O instead of H_2O it will kill you.

There is a literature known to educationalists about the stuff that students are supposed to know but are never taught. You may find the phrase 'hidden curriculum', but this tends to refer to things that get concealed from the students because the professionals would prefer not to avow them. But of course the students pick them up anyway. Usually this refers to discreditable things like institutional racism or sexism, but it can also refer to innocent things of which the professionals are not sufficiently aware to know how to teach them properly.

One such topic is the endogenous typing of mathematics, and i shall be talking about that in a later lecture.

If you think, as i do, that philosophy of mathematics is the study of—and the dealing with—the methodological problems encountered by mathematicians in the course of their work as mathematicians, then you will find that a lot of your philosophising about mathematics is highlighted by the experience of teaching and explaining mathematics. This is one of the reasons why i enjoy teaching it. A Cambridge colleague of mine[2] once said that expository/pædogogical problems in mathematics often sit on top of the scars of old foundational/conceptual problems in mathematics. There's a lot to think about there.

And what is Mathematics for?

This is not an airy-fairy question at all. If you don't know what you are trying to achieve then you won't know whether you are succeeding or not. And there are some things one can say about what the purpose of mathematics is.

The first—perhaps slightly surprising point—to make is that mathematics (uniquely among the sciences) is not defined by its subject matter. One can make this point in a brutal and simple way by merely pointing out that the subject matter of Mathematics grows over time. Things that weren't mathematical subject matter become Mathematics as Mathematics grows and engulfs things. The Greeks didn't think that knots were mathematical objects; we do. Proofs were not mathematical objects for the mediævals, but they are now. (How else could we even *state* Gödel's Incompleteness theorem mathematically?) But what am i saying? Mathematics is eternal, it can't possibly change over time! What changes over time is the stuff we study, what we apply it to. Mathematics itself is not defined by its subject matter but by its methods. Anything done to a mathematical standard becomes part of Mathematics.

> Q: So what is it to do stuff to a mathematical standard?
>
> A: Anything done with sufficient rigour and sufficient abstractness is (becomes) part of Mathematics.

A friend of mine[3] went on holday in Bali once, and came back with what she alleged is a Balinese *bon mot*: "In Bali we have no art, we just try to do things as well as we can". So—in Bali—Art is what happens when you do things

[2] Ben Garling

[3] `https://en.wikipedia.org/wiki/Odette_de_Mourgues`

properly. Well, Mathematics is what happens when do—whatever it is you are doing—with sufficient rigour and abstractness.

People sometimes make the same point about Science (a nineteenth century word, that). Science is simply the endeavour of trying to understand Mother Nature, and (this is the point) trying to do it properly. You can only be opposed to science if you don't want to understand Mother Nature, or don't want to do it properly, or wish to prevent other people from understanding her. You might object to the activities of some scient*ists* but it doesn't make much sense to object to science *per se*; the people who think they do simply haven't thought it through.

1.1 So: what is Rigour and what is Abstractness?

1.1.1 Rigour

Rigour demands that our concepts be hard-edged: it demands transparent identity criteria.

> The chief danger to our philosophy, apart from laziness and woolliness, is *scholasticism*, the essence of which is treating what is vague as if it were precise . . .

Ramsey 'Philosophy' 1929.

Have to get your primitive concepts unambiguously defined. Quine has a slogan '*No entity without identity*' to which we will return later. The message is that if you do not have reliable identity criteria for—as it might be—*widgets*, then you simply don't have a concept of widget, at least not a mathematical concept of widget, you can't have variables ranging over widgets—and you certainly can't have a formal theory about them.

Attempts to reason with poorly defined entities results in various characteristic failures in the process of theory-building. Fallacies of equivocation

> Let G be an arbitrary Government.
> Since G is arbitrary, G is surely unjust.
> But G was arbitrary.
> Therefore all governments are unjust.

> All metals are elements
> bronze is a metal
> therefore bronze is an element.

and proofs of falsehoods, which you definitely don't want.

But there is worse.

Let me give you an example. Suppose you want to reason about a phenomenon that involves some quantity taking a value between 0 and 1. Naturally you want to measure this quantity on a daily basis. There might be a threshold of interest for some purposes, so that if the quantity is above threshold you do one thing, and if it is below, you do another. Suppose the quantity is fuzzy, in the sense that if the value is close to threshold your measuring apparatus might measure it as above threshold and then—without the value having changed—measure it as below threshold. In these circumstances it is not safe to attempt to reason rigorously about the system. Were you to try, you would find yourself trying to set up a logic that allows a value to be simultaneously both above and below threshold, a logic that allows that $p \wedge \neg p$ might be true. I know this sounds absurd, but people have done it. It can happen if you try to reason formally about things that are not hard-edged: it is the danger of *hasty formalisation*.

I leave rigour with a thought ...

The need for rigour misidentified as a need for foundations. I will talk about foundationalism at length later. For the moment i am just warning you against it.

1.1.2 Abstractness

When i was 12, and packed off to boarding school, my father gave me a subscription to SCIENTIFIC AMERICAN and i remember being very struck by an article about globular clusters (of stars) that mentioned that there was this theorem—the *Virial Theorem*—that was in the first instance a fact about molecules in a gas cloud, that could be used to tell us about movements of stars. I was very struck by the fact that there was this thing which was supposed to be an insight about one set of phenomena but turned out to say something about other quite different phenomena. It was an *abstract gadget*. I suppose i could have learnt the same lesson from reflecting on the fact that the natural numbers can be used to count, well, *anything*, really. But perhaps numbers make the point less well, because one can always say that natural numbers are properties of sets (of whatever-it-is) so—to a set theorist at least—they don't come across as portable in quite the way the virial theorem seemed to me to be.

Abstractness leads us to another important idea

1.1.3 Concealment

[mathematical modelling always involves deciding what to conceal] blood pressure. When measuring blood pressure you do not record body posture, you conceal it, so you'd better make sure it's always the same.

abstraction requires Concealment. When you abstract "away" (as they say) from something you are concealing the stuff that is left behind. We all know that mathematics is about abstracting and generalising, but i sometimes think that we should be emphasising the process of concealment. Think of the exercise not in terms of what you want to abstract away and concentrate on, but in terms of what you are deciding to ignore.

The trick is conceal the right amount.

Lloyd Reinhardt was (perhaps still is) a philosopher of science at Sydney U. When a student he was a flatmate of George Boolos, Boolos was a logician, Lloyd was not, but he was doing a baby logic course. He came home with an assignment which involved formalising "You can fool all of the people some of the time ..." etc. He had no idea what to do, so he asked Boolos. Boolos said: "what system did they tell you to use?" (meaning: first-order logic, propositional logic etc). "They didn't say" said Lloyd. "Then do it in propositional logic" said Boolos, "and the answer is p".

Boolos concealed too much!

Can conceal the process of evaluation in propositional logic but not if you have undefined values. Suppose p evaluates to true and q doesn't evaluate—is undefined. What are we to make of $p \vee q$? If we evaluate lazily we can detect the fact p is true and that therefore $p \vee q$ must be true. However if we evaluate strictly (so that we evaluate all subformulæ of an expression before we evaluate the expression itself) we get trapped in a nonterminating evaluation of q and we never get round to evaluating the next level up—$p \vee q$—and we never learn that it is true.

Notice that for programming languages the difference between lazy and eager evaluation absolutely can*not* be ignored. If you have the `if-then-else` construction (and what self-respecting programming language does *not* have such a construct?) then you *have* to evaluate it lazily. What are you to do with

$$\texttt{if}\ p\ \texttt{then}\ \text{do}\ f\ \texttt{else}\ \text{do}\ g$$

? It's a total waste of time to have f and g both up and running and ready to go when you *know* you are only going to use one of them. Indeed it might not even be *possible* to have both f and g up and ready to go simultaneously—they might make conflicting demands on your resources.

Some egregious Real-life Examples

Propositional logic is a triumph of ellipsis. We can get away with writing 'p' instead of '$p_t^{ch,co}$' (which would mean that Chap *ch* in context *co* asserts p at time t) as long as we can hold all these other parameters constant. In settings where the other parameters cannot be held constant the ellipsis is not safe. Yet it is precisely this kind of ellipsis we have to perform if what we want is a *logic* rather than a first-order theory of deduction-tokens-in-context.

We have a catch-phrase for this need for the other parameters to be constant, and the fact that we have it in latin shows how old this idea is: *ceteris paribus*—other things being equal.

Here is an example of how *not* to do it, taken from a standard text (names changed to preserve anonymity). Kevin (not his real name) and his friends have been having some fun in the chemistry lab, and they wrote:

$$\mathtt{MgO} + \mathtt{H}_2 \to \mathtt{Mg} + \mathtt{H}_2\mathtt{O}$$

(This would require extreme conditions but never mind[4]; this is at least standard chemical notation.)

Then assume we have some $\texttt{MgO}$ and some $\texttt{H}_2$. They (Kevin and his friends) end up representing reactions by means of *logical* formulæ like

$$(\texttt{MgO} \wedge \texttt{H}_2) \to (\texttt{Mg} \wedge \texttt{H}_2\texttt{O}) \tag{K1}$$

This is on the basis that if one represents "I have some $\texttt{MgO}$" by the propositional letter '$\texttt{MgO}$' (and others similarly)[5] then the displayed formula does not at all represent the reaction it is supposed to represent. $p \to q$ does not say anything like "p and then q" (at which point p no longer!) but once one "has" $\texttt{Mg}$ and $\texttt{H}_2\texttt{O}$ as a result of the reaction allegedly captured by the displayed formula one no longer "has" any $\texttt{Mg}$ or $\texttt{H}_2\texttt{O}$: it's been used up! In contrast, p and $p \to q$ are not in any sense "used up" by modus ponens. And nothing will be achieved by trying to capture the fact that the reagants are used up by writing something like repetition

$$(\text{MgO} \wedge \text{H}_2) \to ((\text{Mg} \wedge \text{H}_2\text{O}) \wedge \neg\text{MgO} \wedge \neg\ \text{H}_2)$$

Consider what this would mean. It would mean that from the assumption MgO $\wedge$ H_2 we would be able to infer $\neg$MgO $\wedge$ $\neg$ H_2. Unfortunately this conclusion contradicts the assumption, so we would infer $\neg(\text{MgO} \wedge \text{H}_2)$, and that is clearly not what was intended. The problem—an important part of it at least—is that we have tried to get away without datestamping anything.

Now if we spice up the formalism we are using by means of datestamping, then it all becomes much more sensible. Rather than write 'MgO' to mean "Kevin has some magnesia" we write 'MgO(t)' to mean "at time t Kevin [or whoever it happens to be] has some magnesia"—and the other reagents similarly—then instead of (K1) we have

$$\text{MgO}(t) \wedge \text{H}_2(t) \to \text{Mg}(t+1) \wedge \text{H}_2\text{O}(t+1) \tag{K2}$$

which is altogether more sensible. Notice that just as we left the datestamps out of the original formulation, here we have left out the name of the poor helot in the lab coat. That is perfectly OK, because the chemistry doesn't depend on the chemist. No harm comes from concealing the technician in the lab coat!

In writing 'MgO(t)' we have taken the (possession-by-Kevin of) magnesia to be a predicate, and points-in-time as arguments. We could have written it the other way round: 't(MgO)' with time as the predicate and magnesia as the argument. That way it more obviously corresponds to "at time t there is some magnesia". Or we could make the lab technician explicit by writing something like '$K(\text{MgO}, t)$' with a two-place predicate $K(,)$ which would mean something like "Kevin has some magnesia at time t". Indeed we could even have had a

[4] I suppose it just *might* work if you roasted magnesia in a stream of hydrogen at a temperature above the melting point of magnesium metal. However I suspect not: Kevin probably not only knows no logic but no chemistry either.

[5] Do not be led astray by the fact that 'MgO' is three letters in English! It's only *one* in the propositional language we are setting up here!

three-place predicate and a formulation like 'H(k,MgO,t)' to mean that "k has some magnesia at time t". All of these can be made to work.

The moral of all this is that if there are important features—such as datestamping—that your formalisation takes no account of, then you shouldn't be surprised if things go wrong.

To forestall the charge that I have tried to burn a straw man instead of a heretic, I should point out that this example (of how *not* to do it) comes from a textbook (which should be showing us how we *should* do it), to wit [censored][6]

[Mind you, there might be a useful parallel one can draw here between logical-truth-plus-evaluation-strategies on the one hand and chemical-reactions-with-mechanism on the other. The arrow syntax that one remembers from school chemistry

$$NaOH + HCl \to NaCl + H_2O$$

conceals mechanism.]

1.2 The Intension/Extension distinction

We need the intension-extension distinction for a variety of reasons. For one thing we need it quite explicitly if we are to understand computable functions, but we also need it in a less direct and explicit way if we are to understand the evolution of our thinking about many mathematical entities.

The intension/extension distinction is not hard-and-fast, so i can't give you a crisp definition. What i can offer you is a *contrastive explanation*.

"contrastive explanation"?

An interesting expression. Suppose there are two related concepts that you want to explain. They are connected at least to the extent that you always think of them together, perhaps you even confuse or conflate them. A contrastive explanation is something that bundles together two explanations, one for each of the two things you are trying to explain, in such a way that each explanation relies on the other. The hope is that by highlighting the differences between the two concepts you will make it easier for your hearer—by looking at the differences being enumerated—to match the picture you are painting with a pair of concepts of which they already have some apprehension.

footprint extent (in the world)

Function-in-intension/function in extension. Log tables. graph of the function

intension is meaning; extension is extent

[6]Most of us have at one time or another committed to paper *sottises* like this—if not worse—and nevertheless subsequently gone on to lead entirely blameless lives. The law providing that spent convictions should be overlooked is a good one, and it is there to protect others as well as me. Kevin has been granted name suppression.

The intension/extension distinction is central to modern Logic. Statements there are in intension and in extension. Logic since the Greeks has been the study of ways of combining statements, and of inferences between statements. But do we mean statements in intension or in extension? What are these two things? Here we encounter another feature of the intension/extension distinction: intensions evaluate to extensions. A statement in intension is a thing that has meaning. Once it has been evaluated (to a statement-in-extension) in a situation, a context, it has only a truth-value: it has been reduced to being either the `true` or the `false`. A statement-in-intension is a meaning; a statement-in-extension is a truth-value. Statements can be combined into complex compound statements by means of various copulæ. How does the truth-value of the compound depend on the truth-value of the statements being compounded? We say that a copula (the linguists would call those things *conjunctions* and logicians would call them *connectives*) `blah` is **ex**tensional if the truth-value (the extension) of A `blah`B depends only on the truth-values of A and of B. If blah requires one to look at the *intensions* of A and B then the copula is intensional. $\wedge$ and $\vee$ are extensional. Implication feels much more intensional.

Pretty clear what the logical principles are that govern $\wedge$ and $\vee$. What about implication? Implication is clearly intensional. If i want to know whether or not A implies B I need to know the two meanings (of A and of B), not just the two truth-values (whether they are true or false).

Some principles governing implication are clear: Transitivity of implication, and contraposition, are unproblematic. But there are other assertions about implication which are obscure indeed. Here is a famous one, to which we will return.

Is $A \to (B \to A)$ a good principle? That is: does "B implies A" follow from A? (And don't say "If i knew A already why would i worry whether or not I could infer it from B?"! The question is not whether or not the inference is *useful*; the question is whether or not it is *sound*). And it's not at all clear. Much ink was being spilt on this particular question 100 years ago.

One of the reasons why it is hard to see whether or not $A \to (B \to A)$ is a sound logical principle is that one has the feeling that $A \to B$ belongs to a different (as it were) *level* from A and B—unlike $\neg A$, $\neg B$, $A \vee B$ and $A \wedge B$ which are all assertions about ... whatever-it-was-that A and B were about. In contrast, $A \to B$ appears to be about not whatever-it-was-that A and B were about, but rather to be about A and B *themselves*. That doesn't by itself make it impossible to see what sound logical principles there are concerning $\to$, but it does mean that it's *prima facie* a different problem from trying to work out what logical principles control $\vee$, $\wedge$ and $\neg$.

Another reason for this uncertainty is that there are squillions of different notions of implication between intensions and it's typically not clear which one is in play. It's easy to get ensnared by fallacies of equivocation. Suppose we have persuaded ourselves that $A \to (B \to A)$ should be true (or false, whatever) by examining our intuitions about $\to$... we need to check: have we taken sufficient care to ensure that we are reading the two occurrences of the arrow in the same way? There are so many things it could mean, and they are so slippery. Can we be sure we haven't changed our reading of it in mid sentence?

This is an example of a general phenomenon: identity criteria for intensional entities are generally pretty obscure and this makes rigorous theories of intensional entities very hard to come by. This is a theme to which we will return.

This is why C20 logic gave up on the attempt to develop a logic for reasoning with statements-in-intension and stuck to reasoning about statements-in-extension. If we want an extensional copula $\to$ for statements, we require only that as long as $A \to B$ and A are both true then B is true. If all we know about statements-in-intension is their evaluation-behaviour (whether they come out true or false) then this requirement is just about the only one we can formulate. We allow that $A \to B$ is true as long it isn't the case that A is true and B is false. This definition almost invariably upsets beginners, who complain that it doesn't capture any connection of meaning between A and B. But then it was never intended to.

Incidentally, on this account of $\to$, $A \to (B \to A)$ comes out as a sound principle. Check it.

Of course this decision to stick to extensional logic didn't please everyone, and in reaction to it there arose a tradition of studying intensional logic. But it is very striking—and not much remarked on—that Mathematics has taken no interest whatever in these new intensional logics. By that i don't mean that mathematicians don't *study* them—mathematicians study everything (with sufficient rigour and abstractness)!—what i mean is that mathematicians don't *use* these intensional logics in their own praxis. We will return to this point later.

Those are two direct applications of intension/extension. However we also need it, as i say, to understand the evolution of how we think about mathematical entities. Actually the need for the intension/extension distinction for doing history isn't really peculiar to mathematics. It works for Science in general. Comte-an progression.

1.2.1 Three Stages

Objects on their way to becoming mathematical objects go through (i'm oversimplifying) three stages. These stages seem to echo roughly the three stages in the evolution of Science as outlined by Comte. You should look him up on Wikipædia. It was he who gave us the expression *positivism*, a phrase that was taken up by the coterie of materialists in early 20th century Vienna, around Moritz Schlick, who called themselves *Logical positivists*, because they wished to augment Comte's ideas with ideas from formal logic.

These stages are not clear-cut, and i can only give a contrastive explanation of them.

• First stage: prescientific, informal, notion. They are not mathematical notions at all. Knots for the Ancient Greeks, for example. This corresponds to what Comte called the *theological phase* when events were explained by reference to deities. Storms happen beco's the rain gods are angry. We shouldn't be too snooty about explanations of this kind. As Nietszche points out somewhere, for people to explain things in this way is actually good scientific practice; they were trying to explain stuff they *didn't* understand in terms of stuff that they (thought they) *did* understand.

• Second stage: the widget is an object-in-intension only. Functions $\mathbb{R} \to \mathbb{R}$ for the C18. Then widgets are not first-class objects (as the CompScis say) *and you cannot quantify over them*. At that intermediate stage, when we just had widgets-in-intension, every widget was an idea in the mind of God, and the apple of his eye.

To a significant extent school mathematics is trapped at this stage, and students experience a shock when they encounter the third—more abstract—stage. I remember in my final year at school my father sending me for coaching to Richard Watts-Tobin—a JRF at his college—who was a tame mathematician employed by Crick and Watson, He tried to explain Rolle's theorem to me, but i was having none of it, not (at that stage) having the concept of arbitrary real-valued–function-in-extension[7]. All farmers are poor; George is a farmer; Is George poor? There are cultures that cannot draw this inference: they say "Dunno, guv . . . who is this George anyway?". Hardy said of Ramanujan that all the natural numbers were his personal friends. I saw 'Divergent Series' at school, and contemplating the series whose general term is $n! \cdot x^n$. . . a highly intensional object

I remember when i was a graduate student reading the following passage in a textbook written in 1963 (sounds recent to me even if not to you!)

> "It seems to me that a worthwhile distinction can be drawn between two types of pure mathematics. The first—which unfortunately is somewhat out of style at present—centres attention on particular functions and theorems which are rich in meaning and history, like the gamma function and the prime number theorem, or on juicy individual facts like Euler's wonderful formula
>
> $$1 + 1/4 + 1/9 + \cdots = \pi^2/6$$
>
> The second is concerned primarily with form and structure."

George F Simmons, *Topology and Modern analysis p ix.*

• Fully fledged existence. "First class object", as the CompScis say. Key catchphrase "to be is to be the value of a variable". You can quantify over them, as the logicians say. At this stage you have the concept of an arbitrary widget. It also becomes possible to believe there are empty ones. Empty sets; empty lists; empty functions. cf the acknowledgement that 0 was a number.

A logician is a mathematician who thinks that a formula is a mathematical object

[7] It now—nearly 60 years later—looks odd to me that he offered me Rolle's theorem rather than the Intermediate Value Theorem, which must surely come first. . . ?

The transition between stage ii and stage iii was a painful one for mathematics at the end of the 19th century. All those pathological sets and functions, which the 18th century would not have countenanced, and which the mathematicians of 100 years ago had to be forced, kicking and screaming, to acknowledge. And it's a painful transition, too, for students making the transition from school mathematics (which—to a first approximation—is C18 mathematics) to university mathmatics.

As the objects of our thought become less intensional it becomes easier to have variables ranging over them. Here is an example I use on my students. "Think of a number; square it; add twice the number you first thought of; add 1...". You might have thought of a particular number, but you didn't have to; you might have just said to yourself: let the number be x. Then you would have elaborated the expressions x^2, $x^2 + 2x$, $x^2 + 2x + 1$ and so on. But if i'd said to you "Think of a binary relation, compose it it with its converse ..." you would not have been able to do the same trick with the variable, because (unless you are a mathematician or a compsci of a particular stamp) you do not have the concept of an arbitrary binary relation. "Let x be an arbitrary whole number" is acceptable. "Let R be an arbitrary binary relation" is probably not. Binary relations are too intensional. Every binary relation you can think of—*lover, sister, square root*—has a particular reason for being there, a *thisness* (for which the mediævals used the word *haecceity*) just as planets and chemical elements did until recently. If widgets are sufficiently intensional then you can't *think-of-a-widget* without thinking of it as being a particular widget, so you do not have the concept of an arbitrary widget. Indeed you may even have the concept 'binary relation' sufficiently clearly set out in your mind for you to be able to see that *brother-of, sister-of, lover-of, square-root-of* are all examples of ... binary relations! Certainly Euler and the Bernoullis wouldn't.

In summary: If widgets are too intensional, then you cannot conceive of an object as a widget without conceiving it to be a particular widget; this means that you cannot have the concept of an arbitrary widget, you cannot have variables ranging over widgets, and widgets cannot be the subject matter of a fully-fledged scientific theory with equations or inequations, conservation laws and suchlike.

Natural numbers don't have souls (even tho' each of them was Ramanujan's personal friend); Numerology is grounded in the belief that (natural) numbers *do* have souls, and it is an error. Part of the difference between astrology and astronomy is that astrology still thinks that planets and constellations have souls. This is one of the ways in which astrology is not science. It's not just that it doesn't use evidence-based learning or the scientific method, it's that all its entities are intensional.

Progress in science has a long-term tendency to replace intensional concepts with their extensional mates. (Comte and positivism) e.g. 17th century mathematics did not have the concept of arbitrary real-valued-function-in-extension; 19th century didn't have (but acquired) the notion of arbitrary set. Romantic nonsense gives way to abstract nonsense. (Mordell called it "the theory of the empty set")

Rice's theorem.

Intensions are denoted by proper nouns; extensions by common nouns.

This preference for objects-in-extension is shown also by the progress of science. Science—at least the way we do it *now*—doesn't like intensional objects. Science in the Ancient world was much more intensional. [Nietzsche's remark about how the ancients did their philosophy of science.] Gods are highly intensional objects. But it wasn't just Gods. (Chemical) Elements; planets. All of them individual things, rich in meaning. They were proper nouns not common nouns. The objects that are the subject matter of modern science are not like that.

Ethics, life, language. These are *intensional* concepts, but it's their *ex*tensions that we have to live with. Essence-Implementation.

Some objects are only ever objects-in-intension. We can have variables ranging over numbers, but we do not have variables ranging over Gods. No proof of a fact about God ever begins "Let Δ be an arbitrary deity". I know it sounds funny, but it's worth asking *why* it's wrong, so *obviously* wrong. As i am forever telling my students, explanations of the obvious can be illuminating.

Generally intensional entities can't be values of variables ("let Δ be an arbitrary deity") Computable functions-in-intension... algorithms? Programs? Computable functions-in-extension are graphs, lookup tables.

1.3 Operationalism

An operationalist theory of widgets characterises widgets in terms of their reaction to your investigative apparatus. DNA and RNA were called acids beco's they dissolve in dilute alkali. REM sleep is characterised by something detectable on EEG. The superego is operationally defined as that part of the ego that is soluble in ethanol. Later one gets a more (i think the word is) *categorical* (in contrast to *dispositional*) analysis of the concept.

states and species

Where do we get these identity criteria from? Operationalism.

No entity without identity. So acquiring a mathematical concept of widgets involves finding identity criteria for widgets. One thing this might involve (tho' the problem might in principle arise in different ways) is finding an equivalence relation on the primordial slime of pre-widgets which has nice properties, and then we take that equivalence relation to be identity-of-widgets. Let's take a simple example. So simple that it doesn't even get noticed.

We wish to formalise a notion of (discrete) machine. We do this because we started with a reasonable concept of input and output (they are observables after all) and we want to explain the connection between the two. What's needed is a good concept of state.

What do states do? (By this i mean both

(i) what do they do to inputs and output; and
(ii) what do they do for the theorist?).

They mediate between input and output. That is to say, for each state s there is a transition function T_s that accepts an input as argument and returns an output and a new state (to which the machine moves). (The answer to (ii) is that they are the theoretic construct that explains the relation between input and output)

Clearly we want to individuate states in such a way that whenever two transition functions T_s and $T_{s'}$ have the same graph, then the two states s and s' are the same state. Informally, we want our states to be extensional not intentional: two states that do the same thing are—for our purposes—the same state. Notice that this is not a definition of equality between states, since it would be circular. But it is a useful hurdle that candidate definitions can be made to jump. Are there definitions that meet it? Yes there are, but we won't go into the details here.

Well, we now have such an analysis. How do we know it's right? Well, we wanted a mathematical concept of machine in order to get a mathematical concept of algorithm and of computable function. There are other approaches to that problem, and, fortunately for us, they all give the same concept of computable function. That means we're doing something right!

There is a similar point to be made about the definition of species. You want to say something like: any two members of a species (if they are of different genders) can be mated to produce offspring of the same species. Again this isn't a definition, beco's it's circular.

This is a dispositional analysis of state. People also use the word 'operational' for this. In the sciences dispositional analyses are generally felt to be less profound than categorical analyses and—more to the point—to represent an earlier stage of understanding than that represented by categorical analyses. (Comte again). However in mathematics it seems pretty clear that the idea of a widget-in-extension is usually something that comes *later* than an idea of a widget-in-intension.

2 Lecture 2: Abstract Data Types and the endogenous Typing of Mathematics

> "Ideas from Computer Science will be as important in Mathematics in the next century as ideas from mathematical physics have been in the present one".
>
> John McCarthy, writing in the 20th century.

Mathematics is basically quite strongly typed. This is never talked about. Hidden curriculum. Not quite in the way that AC is hidden curriculum: you're supposed to know how to use it—there are *actual proofs* that use it—but in the sense that it colours your whole way of thinking, but it's never brought out. It's never used in a proof.

Charles Pigden's challenge; the reconstruction of $\mathbb{Q}$, $\mathbb{R}$ etc from $\mathbb{N}$ makes the real numer 1, the rational number 1, the integer 1 and the natural number 1 all distinct onjects. Surely this is a bug, he says?

No, say i!

Mohan says: "I defy you to come up with a theorem and a proof of it wherein the real number 0 and the integer 0 both appear and where it matters that they are different". Randall's riposte to this is : "I defy you to come up with a theorem and a proof of it wherein the real number 0 and the integer 0 both appear and where it matters that they are the same!" I don't see how it can ever matter that they should be the same. After all, there is a canonical casting function that will turn $0_{\mathbb{Z}}$ into $0_{\mathbb{R}}$, and isn't that as good as having them be the same thing?

Anyway!

'Type theory' is a word heard much more frequently on the lips of mathematicians nowadays than it was even 20 years ago. And nowadays it means something different from what it meant when the phrase first obtained currency. A century ago 'type theory' referred to the system in Russell-and-Whitehead's *Principia*, something that nowadays nobody except a few sad antiquarians gives tho'rt to. Back then it was all the rage. Do not forget that the full title of Gödel's incompleteness theorem paper is "*Über formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme I*". Russellian type theory is a very different beast from the kind of modern type theory that i want to talk about.

Let's get Russellian type theory out of the way. The general idea is that it shares only the name with the modern type theory that is our concern here. This isn't entirely true, but the connections are too murky to go into here. I am trying to write about them. I've already written one book about this stuff (and the only subsequent literature on it is beco's i made a mistake in chapter 1) and i'm trying to write another. . . [8]

Russellian type theory is best described—to a first approximation—as a strongly typed Set Theory. And the strong typing is fairly explicitly (at least that is the way the story is always told) a way of getting rid of the set-theoretic paradoxes. The most famous of these is of course the paradox that Russell

[8] Dear Sir or Madam will you read my book
it took years to write won't you take a look

⋮

It's a thousand pages, give or take a few
I'll be writing more in a week or two
I could make it longer if you like the style
I can change it 'round
And I wanna be a paperback writer
Paperback writer

himself discovered and which bears his name. It's also the one that is most easily despatched by the strong typing that Russell cooked up.

[discuss Russell's paradox]

Types explicit in the syntax

many-sorted language like Vector Spaces. (Possibly graphs and geometries too)

Despite the fact that Russell-style typing has roots that are entirely distinct from endogenous mathematical typing Turing says somewhere that Russellian Type theory is much closer to actual mathematical practice than set theory is. [details here]

I am trying to write a book—a paperback, with any luck!—about how Russellian typing and the much richer endogenous strong typing of mathematics are intimately connected. But i must resist the temptation to explore those parallels here.

So let's talk about the endogenous typing of mathematics. It's always been there, but mathematicians are only just becoming conscious of it. It was borrowed—from mathematics—by early theoretical computer scientists (most of whom were expat mathematicians, there being no CS degrees in those days) in the 1970s, from roots in C20 model theory, Tarski etc. It was those Theoretical Computer Science people who first made it explicit. There is now a rich literature on type theory in the CS community.

Altho' the concept of ADT has been made explicit only within living memory, there have been tell-tale signs in our mathematical notation for hundreds of years, really going back as far as we have had specifically mathematical notation at all... which actually isn't very long. Number theorists always use lower case roman letters to range over natural numbers; with vector spaces people always write the scalars as lower case Greek letter and the vectors as boldface lower case roman letters. Trig functions: typically your arguments are lower case Greek letters and the values lower case roman letters, hardly ever the other way round. The fact that these distinctions are visible in the notation doesn't mean they were explicit, but they were there, semiconsciously. These rules are not formal and explicit, the way the typing rules in Russellian type theory are, but they do reveal something of what those typing rules would look like were we to make them explicit. But if you don't respect them people won't respect you. It's a bit like Gilbert Ryle, long-time editor of the philosophical journal MIND saying that he always rejected articles written in green ink. Only nutters write in green ink. Only nutters fail to respect the informal conventions i have alluded to.

The compscis' idea has its roots in a notion from mathematical logic called *signature*. It's a notion that you have to have if you are to do semantics for first-order logic, but that is a concept of narrower scope. Let's start with that. It was first made explicit in the second third of the last century by Tarski (The Compsci's concept of ADT comes from the last third)

The idea behind *signature* is that if you are to do semantics for a language then the models you construct have to have within them bits—theatre people would call them *props*—that stand in 1-1 correspondence with the *props* of the language. A function symbol in the language has to be married up with an actual function in the model; a constant symbol in the language has to be married up with a designated object in the model.... The *signature* of a language (or a model) is the itemised list of props. They have to match up: the model has to have the same props as the language and in the same places. This information about the props is recorded in the *signature*. If you are going to be explicit and formal aboout semantics, you have to bring out all this signature stuff out into the open.

Let's have some illustrations, at least situations where the idea of a signature is useful. These are examples i collected to explain to philosophy sudents what a signature is.

- Cricket and baseball resemble each other in a way that cricket and tennis do not. One might say that cricket and baseball have the same signature. Well, more or less! They can be described by giving different values to the same set of parameters.

- It has been said that a French farce is a play with four characters, two doors and one bed. This *aperçu* is best expressed by using the concept of signature.

- Mail order kitsets: IKEA and suchlike. When your parcel of pieces arrives, somewhere buried in the polystyrene chips you have a piece of paper (the "manifest") that tells you how many kinds of objects there are and how many you have of each, but it does not tell you what to *do* with them. Loosely, the manifest is the *signature* in this example. Instructions on what you do with the objects come with the *axioms* (instructions for assembly), on a separate sheet.

- Recipes correspond to theories: lists of ingredients to signatures.

Contrastive explanation again: signatures do not correspond to *axioms*.

Structure	**Signature**	**Axioms**
French Farce	4 chars, 2 doors 1 bed	Plot
Dish	Ingredients	Recipe
Kitset	list of contents	Instructions for assembly
Cricket/baseball	innings, catches, etc	Rules
Tennis/table tennis		

The signature of groups is: one two-place function, one one-place function, and one constant symbol;

The signature of rings is: two two-place functions, one one-place function, and two constant symbols.

Signature of fields is the same as that of rings

Signature of graphs is a single two place relation.

All these things have equality as well, so we sort of don't count that.

The signature of bipartite graphs has a two-place (edge) relation plus a colour predicate.

the signature of set theory is equality plus one binary predicate;

The signature of the language of first-order Peano arithmetic has slots for one unary function symbol, one nullary function symbol (or constant) and equality.

rings and fields have the same signature but different axioms. graphs and digraphs have the same signature but different axioms. There are various ADTs of graphs. Graphs with decorated edges, graphs with decorated nodes, multigraphs. Many of these ADTs you need because they are useful in connection with real-life data that you want to reason about abstractly and rigorously—which is to say mathematically. You can put max-flow min-cut theorems to good use in reasoning about how to move stuff around networks.

Let's have some examples:

Numbers of various kinds. Lists. Arrays. Matrices, graphs

Meal bookings!!!!

polymorphic and monomorphic. Lists are polymorphic, but the natural numbers that measure their length are monomorphic.

[This is the place to explain expansions and reducts.]

ADT of sets is minimal wrt expansion/reduction. Play strip-poker with an algebra, eventually you get a set. This status of Set Theory facilitates the modern view—foisted on mathematics by logicians—that a mathematical structure can always be thought of as a set with knobs on.

But of course not every ADT is such that its members can be naturally thought of as sets-with-knobs-on. Fields can be naturally thought of as sets-with-knobs-on, but their elements aren't. Natural numbers and real numbers are both ADTs but neither of those entities can be naturally thought of as sets-with-knobs-on.

2.1 Datatype Specification

We need the expression *specification*. A datatype has a specification, which tells you which predicates and operations the datatype supports. Specifications are connected to the idea we saw above of a *signature*.

We need the concept of operations or actions supported by an ADT. The object-oriented programming people are heavily into this. Mathematicians less so, tho' it can be pædogogically helpful.

$\mathbb{N}$ supports addition multiplication and exponentiation; integers mod p support addition and multiplication but do not support exponentiation. Why not? It can be helpful to your students to get them to exlain why not.

$\mathbb{N}$ is simply those ordinals that you can construct without use of the `supremum` constructor.

Object-oriented programming.

The restriction on the operations pulls back to constraints on the syntax. These constraints resemble the constraints in Russellian type theory

2.2 Why are they useful? Why do we need to know about them

Why should we embrace the idea of an ADT? They were invented by Compscis to bring some discipline into the writing of programs, and to prevent us making silly mistakes; for example, we never find ourselves writing a program that attempts to do an arithmetical operation to something that isn't a number. Look up the famous ATT brown out, caused (in part) by a missing semicolon in some C code which would have been picked up by a type-checker[9] Enriching a language with a typing discipline adds redundancy (and i mean redundancy in the information-theoretic sense): if you mistype a command the chances are that you will turn it into something that is not well-typed and the type-checker will detect your error. That's what *didn't* happen with the ATT brown-out.

Something parallel happens in Mathematics. There are mathematical silly mistakes which typing disciplines enable us to avoid making. You don't have to wonder whether or not $3 \in 5$, or whether or not the number 6 might be Julius Cæsar[10]. But there are more serious applications we can talk about. But—before that—a couple of tangential comments.

This is an idea of which of course the Vienna school's idea of *category mistake* is a precursor. "This stone is thinking about Vienna" is Carnap's standard example. It is an absurd statement of course. What matters is: wherein does the absurdity reside? You have an explanation of the absurdity if you have a notion of 'category' (not the right word to use now, though it was the word the Viennese positivists used) or 'sort'. So you can explain it by saying that stones are of the wrong *sort* of thing to be doing any thinking. Thus the Vienna school had a concept of datatype (tho' they didn't use the word) but they didn't have a theory of what a datatype was, nor did they have the idea that datatypes can be characterised by the operations they support. That is as much as to say that, for the Viennese positivists, a datatype was very much an object-in-intension, each one a one-off, like planets for the Ancient World. And beco's the Vienna school were attracted to a monist physicalism they weren't going to be receptive to the idea of datatypes as different types of stuff, so they never really took the idea of ADTs seriously. We are ready for it now that we have recovered from "two dogmas".

[9] You might have to try harder than i have done, co's i couldn't find anything on the web—ATT have presumably done their best to suppress it. I dug up this info from old correspondence of mine nearer the time

[10] There are people who worry about this. Look at `https://oxford.universitypressscholarship.com/view/10.1093/oso/9780199641314.001.0001/oso-9780199641314-chapter-9`.

Some people have stronger type-theoretic intuitions than others. I remember being *very* bothered when i encountered trig functions for the first time. How can you possibly have a function from angles to real numbers? Surely angles don't contain that sort of information? I cannot now reconstruct my unease (i wish i could) but i think it could be expressed in terms i would now understand as: the structure supported by the reals must somehow be present in the angles if there is to be a function from angles to reals. I can see what it is to add two angles, but what is it to multiply them? I don't think that was my unease at the time (i wouldn't have been able to articulate it even if it had been) but there was something i was very unhappy about. Probably something highly intensional! You may have had a similar experience. Or so may your students!

But back to applications in mathematics.

2.2.1 Some Parts are not typed

There are parts of mathematics that are unexpectedly *not* typed, and students need to be warned.

The theory of Computable functions $\mathbb{N} \to \mathbb{N}$ concerns itself with functions, machines for computing those function, inputs and outputs for those machines (which are of course tuples of from $\mathbb{N}$) but of course also running times, core dumps of the machines and possibly some more things i haven't thought of. On the face of it these are all of different ADTs. But actually they aren't. Machines can be coded as natural numbers, and this isn't just a curiosity but an absolutely central fact, crucial to the proof of the unsolvability of the halting problem. You have to feed a machine its own code as an input. At other times you have code up a pair $\langle p, i\rangle$ of a program p and an input i to that program, and feed that as an input to another program. It is also crucial that n-tuples of natural numbers can be coded up as natural numbers. Register machines manipulate bit-strings; bit strings can support both arithmétic and boolean operations, and they can be addresses, and times. In Computation theory there is an area called *problem reduction* which concerns itself with questions like "Suppose i had a gremlin that could solve questions like this (as it might be: does p halt on i?), can i use it to solve questions like this (as it might be "are there infinitely many distinct inputs on which p halts?"). Answering problem-reduction exercises often requires one to treat a given data object as being of more than one of these apparent types simultaneously. Beginners find this most unnatural, and initially have great difficulty with questions of this kind: there is kind of ADT censorship that prevents them from finding the coding tricks that are essential for problem reduction. Of course if they haven't been brought up to know about typing disciplines then they aren't going to be in touch with their intuitions and they won't know that it's their typing intuitions that are holding them back. It can be quite helpful to them if their guide (that means you!) points out to them that what is holding them back is their typing intuition.

2.2.2 Cases where you really need the concept

We collect here several cases where appreciation of the concept of ADT can spare us some embarrassing pratfalls.

A simple group is one with no normal subgroups other than itself and the unit. Well, the singleton of the unit. The type-checker, once you've put it into auto-correct mode, will correct 'unit' to 'singleton of the unit'. The reason why it's OK to write 'unit' instead of 'singleton of the unit' is not because they are the same (as some people have—incredibly—alleged) but beco's they are so obviously of different types that it is OK to use the same notation for both beco's the difference in context will disambiguate the notation for us[11]. We can economise by using the same notation for both.

Burali-Forti

"Every ordinal is the order type of its predecessaors in their natural order"

Conway on countable choice

countable-set vs counted-set.

two-colourable graph and two-coloured graph.

2.3 Implementation *vs* Definition

One illustration of McCarthy's point that ideas from Computer Science will be important in mathematics in this century is the useful role to be played by the CompSci's concept of *implementation*. The obvious point of departure for understanding the idea of implementation is the relation between an operating system (Windows, or perhaps LINUX) which is in some sense an abstract object, and the various manifestations of it on different brands of computer.

Mathematicians do not use the word 'implementation', and it's a real pity, because we *do* have ideas of implementation in mathematics; they use the word 'definition' instead. They conflate implementation and definition because they do not have the concept of an ADT. And implementation and definition are completely different things, different ideas.

Implementations are called for when you wish to constrain your universe of discourse to be one particular suite of entities, but there are nevertheless still some extra entities that you need. A typical example of extra entity that one might want is ordered pair. If you are doing arithmetic then you can code up ordered pairs of naturals as naturals, using a suitable quadratic bijection $\mathbb{N} \times \mathbb{N} \longleftrightarrow \mathbb{N}$. If you want ordered pairs when you are doing set theory then you can use the Wiener-Kuratowski scheme, under which every ordered pair of two sets is another set. That is, you *implement* the extra entities in the entities that are officially authorised. Under the Wiener-Kuratowski scheme not every set codes an ordered pair, but there are other ways of coding up pairs in such

[11] Strictly it isn't even the singleton of the unit but a structure whose carrier set is the singleton of the unit, with one designated element and the identity relation on that singleton (to be the inverse operation) plus the relation $\langle 1, \langle 1, 1\rangle\rangle$ to be the graph of the multiplication relation. But i suspect this subtlety is lost on the people who want to identify sets with their singletons

a way that every set does code a pair. Does this matter? Probably not; what does matter is that there is more than one way of coding up pairs of sets as sets.

What we are defining in these cases is not an ordered pair. It's not entirely clear to me that what we are doing should even be described as *defining* anything at all. If we *are* defining something then it's not ordered pairs but an *implementation* of ordered pairs.

I am going to talk later in some detail about the error of foundationalism. For the moment i just want to make the point that it tends to lead to the error of confusing implementation and definition. The second error is not an *inescapable* consequence of the first, but it's but a short step from the first to the second.

Here is a highly topical pet hate of mine. If you are a foundationalist about set theory you probably think that ordinals are transitive sets totally ordered by the membership relation, and that the order relation on them is set-membership. Indeed you probably say that ordinals are *defined* to be ... transitive sets totally ordered by the membership relation (don't ask!). Using the axiom of foundation it follows immediately that the order relation on ordinals *thus defined* is wellfounded. You, the foundationalist, think you have proved thereby that the order relation on ordinals is wellfounded. You think you've proved that beco's you think that that's what ordinals *are*. If, however, you think merely that transitive sets wellordered by $\in$ are a cute way of *implementing* ordinals in set theory (as indeed they are) then what you have done can't be described as a proof that the order relation on ordinals is wellfounded, merely that the order relation *on your implementation* of ordinals is wellfounded. That would be a bit like me trying to establish the security of an operating system by proving—somehow—that the binary that runs on my laptop is secure. Your laptop might have a completely different architecture and the binary that runs on your laptop might not be secure!

The situation is a bit like one i shall talk about later: when Russell and Whitehead famously prove ([**?**], *54.43) that $1 + 1 = 2$ they of course aren't really doing that at all. What they *are* doing is illustrating how their implementation of arithmetic inside their ramified typed set theory is compliant with the specification—at least to that extent. The difference is that in the Russell-Whitehead context everybody knows that that is what they are doing. The shocking thing about the ordinal case is that you will find the argument i have just pilloried in actual textbooks written by real mathematicians who ought to know better. Shame on them.

If you neglect to acquire the concept of ADT then you become more likely to fall into the trap of confusing definition with implementation.

[Another possible point of departure: whether you write on a blackboard or on a piece of paper, the handwriting looks the same! This is very striking because at the two different scales you are using different muscle groups! From the point of view of your motor module these are completely different actions.]

3 A Lecture on The Error of Foundationalism

> *Irrefragibility, thy name is Mathematics. Mathematics is where the proofs are. Scientific standards have turned austere indeed [...] if anyone is to fuss about foundations [for her]. Where might he find foundations half so firm as what he wants to found?*

Quine, "The Foundations of Mathematics" in "The Ways of Paradox.[12]"

Foundationalism is the error of thinking that mathematics needs foundations. It's not universally recognised as an error. In fact there is one version of this error that is widely subscribed to, namely set-theoretic foundationalism.

Pick up most textbooks of Analysis or Topology and you will find Chapter 0 containing a raft of "definitions" of things in terms of sets—a nod to set-theoretic foundationalism—which is then completely ignored in the rest of the book. And quite right too. Chapter 0 was a holding operation to ward off things you didn't want to think about so you could get on with your mathematics *without thinking about them*.

So it's less than 100% clear *quite* how sincere this subscription is.

Think what this lofty indifference is telling us. If you can ignore these supposed definitions altogether then they clearly weren't definitions. After all, if you are going to reason about whales you can't ignore the definition of whale. You can ignore *implementations* all right; and in some sense that's the whole point. When i am running (an implementation of) LINUX on my laptop i pay no attention to the implementation[13]. All i care about is the operating system that i am running. The users' manual for the OS does not begin with Chapter 0 on the implementation for your laptop.

> *One source of the error of foundationalism is the mistake of thinking that providing foundations is the way to achieve rigour.*

Why do we say foundationalism is an error? Why might it not be the case that everything is a set? I suppose the point is not that it's the foundationalism that is an error, but rather the patter in favour of it. Foundationalism about sets doesn't furnish what the prospectus says it does. You don't get rigour from foundations. Or—if you do—it is at the cost of some bizzarre assumptions. Try telling Ramanujan. . . .

The mindset that is attracted to foundationalism is vulnerable to an infinite regress argument. If you find yourself vulnerable to an infinite regress argument then you are doing something wrong. Google the Münchhausen trilemma. `https://en.wikipedia.org/wiki/M%C3%BCnchhausen_trilemma`

What is it about the things you have founded your mathematics on that makes them so special that they don't need foundations in turn?

[12] I love the way Quine writes. . . *irrefragibility*—what a lovely *lovely* word . . .

[13] That is why it is in parentheses in this last sentence!

Do the foundations tell you why it works? Do they move you to say ‘Ah! *Now* I understand!” Definition of acid (electron acceptor) contrasted with definition of ordered pair.

It’s turtles all the way down. Set theoretic foundationalism is the belief that the whole of mathematics rests on the back of a gigantic empty set.

Conway. Chapter 0 of [2] “Mathematicians’ Lib!”

The unease about complex numbers wasn’t banished by any reductionist programme so much as by a decision to stop worrying.

The commonest foundationalist error is set-theoretic foundationalism. However, Set Theory is genuinely useful, so let’s record its useful features before we start rubbishing its claim to be a foundation.

3.1 A Digression on the special Role of Set Theory

There is a recurring problem in Mathematics of how to conceptualise the new things that we invent: ideal divisors, points at infinity, complex numbers. I don’t know as much about the history of these discussions as i would like to. In fact i was totally unaware of the literature on this until one year in Canterbury i had the good fortune to meet Mark Wilson of Pittsburgh—then an Erskine fellow—who is a very good person to read. There is an extensive 19th century literature on this which i would love to know more about, and which nobody nowadays seems to read. How much you worry about how to concretise/conceptualise novel entities is, to a certain extent, a question of personal preference. Oliver Heaviside was of the view that you shouldn’t spend time worrying about how to conceptualise complex numbers, points at infinity, ideal divisors and the other novelties of that time. Just get on with the mathematics, all those problems will come out in the wash. Mark quotes one remark of Heaviside that sticks in my mind: “logic is eternal—it can wait”.

Set theory arrived conveniently in the last third of the nineteenth century with an answer—of sorts. My understanding from the people who know the history is that this was one reason why Dedekind was interested in Set Theory. Why is Set Theory useful? Set theory enables one to concretise stuff, and in a rather operationalist way:

(Set theory probably has roots in expressions like “a circle is the locus of point a fixed distance from a point. All the conic sections have definitions as the locus of points satisfying some condition in terms of distances. But the Greeks didn’t have set theory even tho’ they had conic sections.)

Set theory enables us to concretise novel entities.

Dedekind cuts

field of fractions construction;

point at infinity as a pencil of parallel lines (so a point p at infinity lies on a line l iff $l \in p$);

ideal divisors. $\mathbb{Z}[\sqrt{-5}]$

Spell these out

Divisors stand in 1-1 correspondence with certain sets of numbers closed under linear combinations—namely the set of multiples of a divisor. So an "ideal" divisor will correspond to a set of numbers closed under linear combinations.

You can see that there are holes in the rationals. Counterexamples to the intermediate value theorem, Cauchy sequences without limits. You have to find a way of thinking of the holes and the places where there aren't holes as two things of the same kind.

(Must track down Frayne's *Bon Mot* about how one could identify a work of art with the set of opinions that are held about it. Though i suspect he should have said: the set of ordered pairs of: an opinion with someone-who-holds-that-opinion.)

Set theory has another goodie for us. It enables one to formalise inductive definitions which o/w violate Aristotle's desideratum "definitions cannot be negative" (which of course—by its own lights—can't be part of a definition of definition!)

> "0 is a natural number; the successor of a natural number is a natural number; nothing else is a natural number."

You may have been offered the above as a definition of natural number. You *sort-of* know what is meant. But—altho' it isn't totally unhelpful—it is of course defective, and defective in a way that was first characterised (as far as i know) by Aristotle. Definitions cannot be negative, so the third clause is illegitimate. However, with set theory we can get round this. We declare that the set of natural numbers is the intersection of all sets that contain 0 and are closed under successor. The thought is that, if a is some entity that is *not* a natural number, then there will be some set containing 0 and closed under successor which does not contain it[14] ... so it doesn't belong to absolutely *all* the sets containing 0 and closed under successor, and therefore is not a natural number according to this definition. This definition is the modern definition of natural number—and it makes essential use of set-theoretic gadgetry.

Another defect of the original definition is that it is circular: the *definiendum* (the thing being defined) appears within the *definiens* (the thing doing the defining). This feature is more strikingly evident in the definition (perhaps the CompSci word 'declaration' is a better word here) of functions by recursion. The definition using sets is noncircular.

This definition also underpins the principle of mathematical induction. Suppose 0 is a frog, and every successor of a frog is a frog. Then the set of frogs contains 0 and is closed under successor. But we have defined $\mathbb{N}$ to be the intersection of all sets that contain 0 and are closed under successor. So every natural number is a frog!

[14]It might be helpful to think of such a set as a *certificate* that a is not a natural number...

The definition is inspired by the hope that whenever a is *not* a natural number then there will be a certificate to this effect, which is to say: a set that contains 0 and is closed under successor but that doesn't contain a. Quite how good a definition it is depends on how good our set-existence axioms are at coming up with these certificates. If they don't come up with certificates for all things that aren't natural numbers then those things which lack certificates will pass the test of belonging to all sets containing 0 and closed under successor, and will therefore pass as natural numbers. That's a long and interesting story of which more perhaps later. For the moment we record merely that the use of set-gadgetry enables us to give a definition of natural number that is at least legitimate (doesn't violate the non-negativity clause) and underpins mathematical induction. You can't do it without set theory.

Worth recalling that the Greeks did not have a definition in this style of natural number, and they did not know proofs by induction on the naturals. (Their proof that $\Sigma_{i<n} 2n + 1 = n^2$ was not by induction, but by waving arms over a picture.)

People worry about impredicativity in this connection, but they shouldn't.

Notice that altho' this definition of $\mathbb{N}$ *makes essential use of* sets, it nevertheless doesn't require us to assume that natural numbers *are* sets. And the things we want to prove about them by induction might not mention sets at all ...

The idea that not only do we need to talk about sets when assembling our definition of natural numbers, but that we want natural numbers to actually *be* sets ... now! *that* is a mistake. Or at least—since there may be some foundationalists present—if you want to adopt that position you are going to have to have an entirely new set of arguments.

So Set Theory is useful, just not as a foundation.

A striking example of the error of set-theoretic foundationalism in action is this instance of what my colleague Jeremy Butterfield calls *Pointillism* (Banach-Tarski). It's usually sold to us as a pathological consequence of the axiom of choice, but it's really a result of pointillism.

3.2 The error of Foundationalism, continued

• Rigour requires the axiomatic method. So if you start axiomatising things you thnk you are being rigorous. Providing foundations is not the same as providing rigour.

• Your primitive assumptions don't have to be *fundamental*, they have to be *uncontroversial*.

• Mistaking the need for *Understanding* for the need for *Certainty*. Goes back to Luther, of all people.

• Mathematicians have a long record of feeling uncomfortable about new entities, in the sense that they feel that it's a problem if they don't know how to conceptualise them. Complex numbers a famous and long-lived problem. Easy to think that once you have concretised things you understand them better. This is an impetus to foundationalism. Mathematicians worried a great deal about complex numberss. What are they? What is the right way to think about them?

One defect of foundationalism—at least those foundationalisms that reduce everything to monist theories (theories that maintain that there is ony kind of thing)—is that they have difficulty reproducing the type distinctions that we are looking for in the theories being founded. Specifically: foundationalism about sets tries to persuade us that all mathematical objects are sets. In so doing it erases—fails to capture/reproduce—the type distinctions in mathematics that i was talking about last week. $3 \in 5$? is a stupid question to ask, but set-theoretic fondationalism has no way of preventing an innocent user of their foundationalist project from asking it. If everything is a set then either $3 \in 5$ or not: it's a perfectly reasonable question. And yet the fact that it is a stupid question is a *mathematical* fact. And set-theoretic foundationalism cannot deliver that mathematical fact: the stupidity of the question "is 3 a member of 5?' is a mathematical fact that is not a fact about sets. Therefore not all mathematical facts are facts about sets. This failure of set-theoretical foundationalism to reproduce the type distinctions we know and love is a point against set theory often made nowadays by people in a modern category-theoretic tradition.

One remedy or palliative for foundationalism is operationalism. You don't worry about what things are, only about what they do.

Category theory is explicitly operationalist in flavour.

People talk about category theory as providing an altenative foundation for mathematics, a foundation that is alternative to set theory. I don't think the categorists themselves see it that way. I think they see category theory as a way of doing mathematics without foundations.

4 A Lecture on Constructivism

It was started by mathematicians not philosophers.

All to do with constructive proof. What one of my students calls exhibitionism.

[explain the difference]

There is a God so there must be a magic sword and a magic key.

The point of departure for the constructive critique of conventional mathematics is the idea of constructive proof. A constructive proof of the existence of a thing that is F is a proof that can be processed into a recipe for constructing a thing that is F. Does constructive proof matter? Depends on what you are trying to do.

If you are concerned about constructive proof—certainly if you are concerned about it to the extent of not wanting to use nonconstructive proof—then you will be looking askance at the law of double negation. Let me explain.

Suppose i prove $\exists x F(x)$ by assuming $\neg\exists x F(x)$ and deducing a contradiction. What have i done? In general whenever i deduce a contradiction from q i have proved $\neg q$. So in this case i have proved $\neg\neg\exists x F(x)$. This proof doesn't tell me where to find the promised x which is F. So it clearly contains different information from that to be found in a proof that contains a recipe for finding such x. Different information? Not merely *less* information. . . ? Probably merely *less*, yes, but . . . the constructive proof might tell you that there is an F and tell you where to find it, but yet give you no overarching reason why there should be such a thing. It just produces the F thing out of a hat. There are such proofs, as we all know. Every mathematician can tell you a troubling story of a theorem that they know to prove, but whose truth is somehow not satisfactorily explained; they know it's true but they don't know *why*. In contrast your non-constructive proof of the same theorem might be a communication from an aloof deity that gives you deep reasons why the theorem should be true—(as it might be) that there should be things that are F—without troubling to tell you where to find them. Some of these nonconstructive proofs can be very appealing—there are nonconstructive proofs in Aigner-Ziegler's famous piece of mathematical pornography *Proofs from the book*. A rich *cache* of such proofs is to be found in probabilistic arguments in finite graph theory[15]. This reticence on the part of the Gods is why one sometimes hears nonconstructive proof described as *Prophesy*. These two proofs clearly give different information. *Since* you individuate propositions by the information they contain, and the information in a proposition is put there by its proof, then *if you have two proofs that contain different information they must be proofs of different things*. In this case these two different things must be $\neg\neg\exists x F(x)$ and $\exists x F(x)$. So a proof that $\neg\neg\exists x F(x)$ is not so much a proof of $\exists x F(x)$ (which would involve exhibiting an x which is F) but rather a prophecy that there will be such a proof, or perhaps a prophecy that there will be such an x, or that such an x will turn up even tho' we can't (or don't know how to) turn one up at the moment.

Thus you might come to believe that $\neg\neg\exists x F(x)$ and $\exists x F(x)$ are different propositions. And—if they are—the law of double negation is not legitimate. Thus you might come to deny the law of double negation.

It's not just double negation. We also have problems with excluded middle: $p \vee \neg p$. Let me explain.

You are sitting in your shed, your workshop, at your bench, building a widget. Now it happens that at some point in the instructions on how to make a widget you have to ascertain whether or not p holds. If it does, your construction of a widget procedes along one path; if it doesn't, it proceeds along a different path. As long as you have the law of excluded middle you're OK: you're OK down either branch, and the law of excluded middle reassures you that you will, in fact, go down one of the two branches and won't get jammed in the doorway. Thus you have an existence proof: there is (or there will be) a widget. You don't know which of the two (or more—there may be more than one fork in the road) widgets you get, but you know you will get one. Clearly it's

[15]Thank you Dillon Mayhew!

use of the law of excluded middle that has enabled us to produce this somewhat pathological thing, this nonconstructive proof.

So you at least get your existence proof, even tho' it isn't constructive. Notice the significance of the law of excluded middle here. The foregoing means that if excluded middle didn't hold then you wouldn't *have* an existence proof. You come to a fork in the road but not only do you not know which branch you will be taking you don't even know that there is a way through it. So, in particular ('*a fortiori*'!) you don't get a *nonconstructive* existence proof. The point is not that if you didn't have excluded middle your proof of the existence theorem would have been constructive; the point is that if you didn't have excluded middle you wouldn't have a proof at all. (At least not *this* proof). But this does at least mean that if you eschew excluded middle then you have closed off *one* way, at least, in which nonconstructive existence theorems can happen. Moral: if you want all your proofs to be constructive, don't use excluded middle.

There is in fact a standard example of a nonconstructive proof that features the law of excluded middle in precisely this way. Suppose you are given the challenge of finding two irrational numbers α and β such that α^β is rational. It is in fact the case that both e and $log_e(2)$ are transcendental but neither of these is easy to prove. Is there an easier way in? Well, one thing any fule kno 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?[16] 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.[17] Here we are applying excluded middle to the proposition that $\sqrt{2}^{\sqrt{2}}$ is rational.

What does this prove? It certainly doesn't straightforwardly show that the law of excluded middle is *false*; what it does show is 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 there are reasonable people who are engaged in activities where it matters, so i suppose—to that extent and in that sense—then it does matter. And we should care about it.

"But what" i can hear you asking "is special about excluded middle? Surely this is actually a more general problem about 'or'? What happens if the fork in the road is $p \vee q$ where we know that one of them must hold (so we know that the process continues) but we don't know which? Don't we have the same

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[16]As it happens, it isn't (but that, too, is hard to prove).

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

problem?" A very good point. This is why we have to design constructive logic so that whenever you prove $p \vee q$ it is beco's you have proved (or can prove) one of p and q. Of course this isn't a logical principle that one can set out as an axiom; rather it is a property of one's axiomatic deductively system that has to be baked in by a clever choice of axioms. What we are trying to ensure is a metatheorem not a theorem. The metatheorem that we are trying to protect is the metatheorem that says that constructive logic has **the existence property**. Merely not-assuming-excluded-middle isn't enough. However i shall spare you the details.

I've tried to recount this sympathetically, but you can probably tell that i think it's an error. A quite specific error, and one that is quite hard to spot because it is never brought out—at least not in the way i am trying to here. The people who are committing this error naturally don't see it in this light and don't express it as i have expressed it.

What this tells us is that the concept of a proposition in the mind of the True Believer in Constructive Mathematics is different (specifically more fine-grained—remember that constructively you don't always want to identify p with $\neg\neg p$) than the concept of a proposition in the mind of the "classically" inclined mathematician. For the classical mathematician a proposition is something that evaluates to `true` or to `false`. For the constructive mathematician it's much more nuanced.

This gives rise to a phenomenon known to philosophers as a *Radical Translation Problem*. The challenge, given the radical nature of their diagreements, is—well, it's *two* challenges really—is for each to find a way of making sense of what the other is saying. In a sense the challenge has been met satisfactory. A pair of such ways has been found. Each party can give a semantics for what the other party is saying. The semantics is acceptable to the party that uses it, but it is not a semantics that the other would consider all right, not a semantics that captures what *they* (the other party) mean.

> " That is not what I meant at all
> That is not it, at all."

You don't want to let sensible decisions about mathematical practice ("I'm not going to use excluded middle or double negation!") spill over into metaphysical disputes about the legitimacy of certain logical principles. This is what i have in mind when i say that i am a sympathiser but not a believer. Metaphysics (logic) is always the last thing to alter/mutilate. The difference between constructive and nonconstructive proof is part of what linguists call *pragmatics*, and it absolutely has to stay there.

I am not a believer, tho' i am def a sympathiser. I think the issues they raise are important even if they get them wrong. Apparently people with my take on this are called *cohabitationists*.

And, being a working mathematician, i am quite pathetically grateful for the fascinating and beautiful mathematics that has been uncovered by the people who are in the grip of these errors.

5 Lecture IV: loose ends

There are some things that i know i ought to talk about, but about which i have nothing helpful to say. Homotopy type Theory is all the rage, but i haven't beeen able to get into it. Reading the HoTT Book is like reading Proust. In both cases i had the experience of coming to the foot of a page—having read it and enjoyed it— and turning it over, only to be struck by the thought that i had taken in absolutely nothing of what i had just read. It was soothing and uncontroversial and went in painlessly but left no trace or mark whatever.

I have already talked about the Axiom of Choice.

5.1 The Axiom of Choice

AC is obviously nonconstructive in its own right: it tells us that every set can be wellordered without telling us how. But it is in any case nonconstructive in another way, in that it implies logical principles known to be nonconstructive, such as excluded middle. But a demonstration of that is a bit too technical for a philosophical talk.

But there are three rather offbeat things i would like to talk about: *Synonymy, Obsolete Entities, and Agency.*

5.2 Synonymy

No entity without identity, so we need to know when two widgets are the same widget.

But we might be confused about whether or not widgets and gadgets might not be the same thing.

Synonymy and the implications for individuation of mathematical subject matter

Doing things mathematically means doing them with suff rigour and suff abstractness. This means setting up a formal langauge for doing whatever it is you are doing. You might be able to mathematise you subject matter in more than one way. Register machines and Turing machines but that's not a problem as long as you can move painless back and forth between the two (or more) formalisms. If you want to reason formally about partial orders you can do it in a language with a symbol for strict inequality: '$<$', or a symbol for nonstrict inequality '$\leq$'. Or you can write things the other way round, using '$>$' or '$\geq$'

Another, perhaps less familiar, example of material that we can formalise in more than one way is the stuff that can be described either as boolean algebra (with $\wedge$ and $\vee$) or as boolean rings $\cdot$ and $+$.

What do we mean by moving painlessly between one and the other? We mean that there are interpretations both ways. More grades of mutual interpretability than you might think.

Two theories T_1 in $\mathcal{L}_1$ and T_2 in $\mathcal{L}_2$ are synonymnous iff there interpretations $i_1 : \mathcal{L}_1 \hookrightarrow \mathcal{L}_2$ sending T_1 into T_2 and $i_1 : \mathcal{L}_2 \hookrightarrow \mathcal{L}_1$ sending T_2 into T_1 which are mutually inverse "up to logical equivalence".

Now it can happen that you can have mutually interpretable (indeed synonymous) formalisms which, on the face of it, are about different things. Not just like the virial theorem. rather more radical.

Kaye-Wong and Ackermann's translation: $n \, E \, m$ iff the nth bit of m is 1. Makes $\mathbb{N}$ iso to the hereditarily finite sets. PA is now synonymous with ZF minus infinity plus neg-infinity plus transitive containment.

So what are these two synonymous theories really about? The theory of partial order and the theory of strict partial order are about the same thing! So these two theories are presumably about the same thing. What is that thing?

Another poser happens when the two synonymous theories are in the same language... and they are mutually contradictory! Can this happen? Yes! There are three examples known to me (and probably more that are not known to me!) If the two theories are in the same language and contradict one another then at least one of them must be false. But if they are synonymous then they stand-or-fall together ... so they must both be false! That doesn't seem a sensible conclusion. Perhaps in these circumstances we should say that this shows that we are using a notion of synonymy that is too permissive, and we need something tighter. Trouble is, there doesn't seem to be any way of tightening up the definition that isn't entirely trivial. The third (and most interesting) analysis—which i believe to be the correct one—is that the two apparently contradictory theories about widgets are actually not contradictory theories about widgets at all, but are in fact entirely consonant—indeed *identical*—theories about... something entirely different!

Let us have a couple of examples.

Benedikt Loewe's proof that ZF and ZFU are synonymous

Church-Oswald synonymous with ZF

In each case neither point of view is privileged.

Is there anything we can say in general about what the noumena are that lie behind the entities we started reasoning about? A pair of synonymous theories describe the same *noumenon*. One might feel like saying that they contain the same information. This is a different notion of information from that of Shannon-and-Weaver. Carnap–and–Bar-Hillel tried to inaugurate the study of what they called *semantic information* in a famous article in BJPS in 1953 but nothing ever seemed to come of it.

Thus the entities that ZF and CUS are attempts to describe have a dual aspect. They "support" two kinds of set membership. But these two membership relations are not independent, they are *interdefinable*.

5.3 Obsolete entities

Phlogiston is the wrong way to think about combustion. What is the status, therefore, of phlogiston theories? They're not entirely vacuous, beco's one can actually say true things with them ... can't one? What happens to the entities postulated by obsolete scientific theories? There is an extensive literature on this question in the philosophy of science community. Curiously, people who interest themselves in philosophy of mathematics take very little interest in the status of the entities postulated by dysfunctional or obsolete mathematics. It may be that they have too much respect for mathematics to wish to say anything that so much as *sounds* like a suggestion that there might be such a thing as obsolete or dysfunctional mathematics. However, the view that mathematics is an activity certainly leaves open the possibility that some ideas might be better suited than others for progressing that activity. Much of mathematics (like much of science in general, for that matter) arises from projects to find the correct way to think of a particular phenomenon. Or, sometimes, the right way to apply a particular insight or technique ... to a phenomenon. Mathematics has its false starts—its phlogiston theories—too. Boolean algebra is the correct way to think of (classical) propositional logic algebraically. Tarski, reflecting on this, felt that there should be an algebraic way of thinking of first-order logic (predicate calculus, whatever you want to call it). His response was to develop what he called *cylindrical algebras*. Various people (one thinks of Donald Monk) did good work on these things, but after the 1960s they fell out of favour, and the historical record makes it clear why. New approaches became available with the spread of category theory, and nowadays anyone who is both (i) interested in the question and (ii) familiar with category theory will say that the correct way to algebrize first-order logic is by means of the device of *classifying topos*. Indeed most people who are familiar with the expression 'classifying topos' have never even heard of cyclndrcial algebras.

Cylindrical algebras are not the only example of developments in mathematics that have either not lived up to the expectations that were held of them or have come to be seen as misconceived. Quaternions were an attempt to reason algebraically (about rotations? dunno, but anyway) about things that were better done by vector spaces. Octonions even more so.

Similar to the phenomenon of obsolete entities is the problem of what one is to make of *blind alley entities*. One hears people say that—for example—fuzzy logic is the wrong way to think about nondeterminism; that dialetheism (the belief that some things can be both true and false) is the wrong way to think about reasoning about noisy data (or, for that matter, the wrong way to think about the ineffable); that "quantum" logics are the wrong way to think about partial valuations, and three-valued logic is the wrong way to think about nontermination. And intuitionistic mathematics is the wrong way to connect with the important distinction beween constructive and nonconstructive proof. And—in all these cases—saying that the allegations made by their proponents are untrue is the wrong way of repudiating them! But it does raise the question: if these—apparently undeniably mathematical gadgets—are the wrong answers to these questions, how can they still be mathematics? Wasn't Mathematics supposed to be anything-done-properly. This stuff isn't being done properly so how can it be Mathematics!

Neither Dialetheism nor "fuzzy" logic has resulted in any significant mathematics, so in a sense the problem doesn't arise there. Not sure about quantum logic, but constructive logic has given rise to quite a lot of mathematics of incontestable beauty and importance. We absolutely cannot dispense with the gadgetry of constructive logic in the way that we can dispense with phlogiston[18]

Or perhaps we can. The legacy of constructive logic that we wish to preserve—possible world semantics, Curry-Howard and so on—is in some sense not part of constructive logic at all, not part of what the constructivists/intuitionists wanted us to buy, but is instead part of a strategy on the part of traditionally-minded mathematicians to make sense of the activities of the intuitionists. It's a *rational reconstruction* of Constructive Mathematics (or perhaps rather of the thought-processes of its advocates) dreamt up by traditionally-minded mathematicians who do not want to throw out the baby with the bathwater. Constructive mathematicians do not think of possible world semantics as describing the meaning of what tbey do. *They* don't correspond to phlogiston. The stuff that corresponds to phlogiston is the patter about lawless sequences, about the creative subject.

One is left wondering what this means for the status of cylindrical algebras—the *entities* not the theory. Given that phlogiston-talk is the wrong way to understand combustion, what are we to do with the phlogiston texts that remain? Do they have meaning? Do they succeed ...? And how good is the parallel between cyclindrical algebras and phlogiston anyway? Phlogiston has gone away—indeed it was never there in the first place. But the cylindrical algebras haven't gone away. Is it OK to just ignore them?

This problem in the philosophy of mathematics is quite unlike the problems usually addressed by philosophers who avow an interest in philosophy of mathematics: its parallels are rather with questions better known in general philosophy of Science. There is a philosophy-of-science literature on this but i haven't been able to find very much, and what little i have found doesn't seem to me to be very insightful. It may be that providing a focus to questions of this kind by thinking about mathematical instances may help concentrate the mind[19].

Perhaps as well as obsolete or illconceived mathematics one should consider the status of poorly motivated experimental mathematics. Complex numbers were invented in order to solve a mathematical problem. But Conway numbers (which people nowadays seem to call "surreal numbers") were dreamt up purely for the hell of it. But nobody thinks any the less of them—nobody thinks of them as *delegitimated*—on that account.

There is a nice example in my very own area of Set theory. Kiselewicz's idea of double-extension set theory. [4], [3], [5]. It's a cute syntactic trick. Does it have any mathematical meaning? Only time will tell. Meanwhile *we don't know whather or not it is part of Mathematics*

So, one thought to take away from this is the following. Is there any genuine

[18] *cf* Hilbert: "We will not be expelled from the Paradise that Cantor has created for us." So much for The Fall: Mathematicians are not going to take any crap from any mere deity.

[19] See [6].

mathematical difference between the mathematics of obsolete or blind-alley entities and the mathematics of genuinely useful entities? There's a difference all right, but is it *mathematical*? Do the obsolete/blind-alley entities somehow fail to be genuine mathematics?

5.4 Agency

Mathematics is famously (to coin a phrase) a *third person* activity not a *first-person* activity: there is no agency in mathematics. And yet the meddling omnivores with precision grips that engage in mathematics are certainly *doing* something. Doing things is what they do. And of course it's the way they talk, and sing, indeed—*be doo be do*.

So is there a useful notion of agency in mathematics? One has to observe immediately that there is clearly a lot of agency involved in the *application* of mathematics, and in its *acquisition* ("Do exercise 15"). A working mathematician may be described as *creative* (or may not) and there is clearly no creativity without agency. In the early days of computability the word 'computer' meant 'a human being who computes' and of course the '*er*' suffix denotes agency.

Wilfrid Hodges said to me years ago that the use of game-theoretic imagery in Logic is thoroughly meretricious: we find it appealing beco's we like to think in terms of interactions—our brain architecture inclines us that way—but it's not there in the Mathematics; the Mind Projection Fallacy perhaps. This resonated with me beco's i had always nurtured the same unworthy suspicion. It is suggestive, i think, that Mathematics houses three quite distinct and unconnected disciplines all called Game theory. There is the study of discrete (usually finite) combinatorial games (board games, chess, go etc) studied by combinatorists and logcians, continuous games which is really a branch of Control theory, and economists' games. They are united only by the fact that we approach them all with our God-given machinery for understanding *interactions*, be they the mind-games played by social animals or the pursuit-evasion games played by prey animals. They share nothing more than the name.

Mathematics is not alone in having no concept—or at best a very attenuated concept—of agency. None of the sciences have languages with personal pronouns. Many share with mathematics the lack of any concept of *experiment*. For example: Astronomy, epidemiology . . . They have observers but no agents.

No first-person mathematics: never do expressions in mathematical languages make any statements about the utterer of the expression. "there is no proof of me" is a nice snappy formulation of the Gödel sentence, but it can't be right. The Gödel sentence isn't talking to us!

But of course agency in the application of mathematics is not what is meant. What is interesting is the possibility of idealised agency *within the mathematics*. This thought was offered to me in the first instance by Nick Denyer, who gave an interesting talk on the nature of the agent to whom Euclid is offering his (their?) recipes. We need to remind ourselves that Euclid's treatise is not a body of *theorems*; it's a body of *recipes*; it's a body of things-to-be-done. By someone. An agent. What is this agent (whom Denyer calls *Valentina*) deemed to be capable of? Valentina can draw a straight line through any two points[20]—a line as long as you like in either direction, but perhaps not infinite.

However there are a number of situations in mathematics where it is natural to reach for a notion of agency.

> Deductions are acts. Lewis Carroll on Achilles and the tortoise [1]
>
> Group actions
>
> Berkeley's master Argument
>
> permutations always thought of in terms of moving things around.
>
> Agents in dissection problems.
>
> Topologists use the image of an ant walking around on the surface of a space as an intelligent agent. The point of the ant-talk is that the ant is supposed to be able to ascertain things. It doesn't actually *act* but it is able to investigate, and to investigate "local" properties only. However the ant is an agent in a thought-experiment and is therefore a *fictional* agent.
>
> Brouwer's creative subject
>
> Observers in Quantum theory.

. . . but these last two examples are a bit scary, and in any case Quantum theory is part of mathematical physics and should perhaps not be on our radar. And Brouwer's creative subject may be part of the misconceived or obsolete mathematics i was talking about earlier.

If you have agents you have a time axis of some kind. Agents *act*: and *after* they have acted things are different. The time dimension isn't $\mathbb{R}$, because the processes that the agents execute can be of length an uncountable ordinal, and no uncountable wellordering can be embedded in the reals. Even something as banal as wellordering the Reals (a life's work!) is something that cannot be done in physical time *even with arbitrarily small time intervals between actions*.

Not that anyone supposed the time axis on which idealised mathematical agents live is $\mathbb{R}$, but it's worth asking what it is. Is it a proper class version of the Long Line? Something like the ordinals $\times\mathbb{R}$. . . ? (`https://en.wikipedia.org/wiki/Long_line_(topology)`)

Is there *anything* one can say about the time axis on which mathematical agents act?

Are processes what agents execute? And are they all discrete and well-founded?

[20]However far apart, as Lindsay Groves points out

Are agents deterministic? Is free-will of mathematical agents a way of understanding nondeterminism?

Is the axiom of choice an assertion about the existence of an agent who can choose a member from every set in a family of sets?

If mathematics really is agent-free, then we should be able to rephrase it in a way that rids it of all this agentive talk. Would that really be progress? *Would we miss anything?*

References

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